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Attention Modulation by Proportion Congruency: The Asymmetrical List Shifting Effect

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Proportion congruency effects represent hallmark phenomena in current theorizing about cognitive control. This is based on the notion that proportion congruency determines the relative levels of attention to relevant and irrelevant information in conflict tasks. However, little empirical evidence exists that uniquely supports such an attention modulation account; moreover, a rivaling account was recently proposed that attributes the effect of proportion congruency to mere contingency learning. In the present study, the influences of shifts in list-wide (Experiment 1) or item-specific (Experiment 2) proportion congruency were investigated. As predicted by attention modulation but not by contingency learning, strong asymmetries were observed in such shifting: An increase in the proportion of congruent trials had only limited impact on the size of the congruency effect when participants were initially trained with a mostly incongruent list, but the impact was substantial for an equivalent increase of incongruent trials when participants were initially trained with a mostly congruent list. This asymmetrical list shifting effect directly supports attention modulation by proportion congruency manipulations and as such provides a novel tool for exploring cognitive control. Implications of our findings for existing theories of cognitive control are discussed.

Keywords: attention, cognitive control, contingency learning, proportion congruency

Over the last decades, various influential frameworks of cognitive control have emerged, such as conflict-monitoring theory (Botvinick, Braver, Barch, Carter, & Cohen, 2001; see also Blais, Robidoux, Risko, & Besner, 2007), the dual-mechanisms-of-control account (Braver, 2012; Braver, Gray, & Burgess, 2007), and the adaptation-by-binding account (Verguts & Notebaert, 2008, 2009). Although differing in important ways from one another, these frameworks share the key principle of *attention modulation*; that is, in response to conflict, attention is biased away from task-irrelevant information toward the task-relevant information. This mechanism allows for controlling behavior in situations that challenge goal-directed performance.

A major (and robust) observation that underlies this principle of attention modulation derives from manipulating the proportion congruency (PC) in conflict paradigms such as the Stroop task. In this task, participants must report the color of a color word; for example, the word *blue* printed in either blue (congruent) or red (incongruent) ink. The difficulty in ignoring the color word, which is typically attributed to the fact that words are processed relatively automatically (e.g., Brown, Gore, & Carr, 2002), results in performance differences between congruent and incongruent trials (the congruency effect) and requires cognitive control. Logan and Zbrodoff (1979) were among the first to report on the list-wide proportion congruency (LWPC) effect: the observation that the congruency effect decreases with higher list-wide proportions of incongruent trials (Bugg, Jacoby, & Toth, 2008; Bugg, McDaniel, Scullin, & Braver, 2011). The LWPC effect has also been reported for flanker (e.g., Gratton, Coles, & Donchin, 1992) and Simon tasks (e.g., Hommel, 1994), and it can be explained by the sustained and strategic weighting of attention paid to the task-relevant and -irrelevant information (Logan & Zbrodoff, 1979).

The manipulation of PC has thus become a benchmark tool in the study of cognitive control and has recently even inspired a reevaluation of cognitive control. Cognitive control is traditionally understood as a strategic, proactive, and effortful (top-down) modulation of selective attention processes, and the LWPC effect fits well with this tradition. In more recent years, however, the effect of PC has been successfully implemented in an item-specific manner. Jacoby, Lindsay, and Hessels (2003) assigned particular items to be mostly congruent and others to be mostly incongruent and observed that the congruency effect was larger for the former item type—the item-specific proportion congruency (ISPC) effect. This observation was received with excitement because, in contrast to LWPC effects, it cannot be based on sustained and proactive attention modulation (i.e., one does not typically know on a trial-

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by-trial basis which item will occur next and thus which settings are required). From the notion of attention modulation, then, ISPC effects require a form of stimulus-driven, reactive implementation of attention settings. For example, early visual processing of the word (e.g., shape, size, orientation) or of the color could modulate attention settings for further within-trial word-reading and/or color-naming processes. This considerably changes the traditional perspective on cognitive control (see Bugg & Crump, 2012).

Overall, the LWPC and ISPC effects have thus left their marks on current thinking about cognitive control. However, this may be premature in the sense that an alternative account has been proposed that relates PC effects to simple contingency learning rather than attention modulation. As such, there is current debate on the question of whether PC effects are rightfully seen as hallmark phenomena of cognitive control. This debate is far from settled, and we now outline it in more detail before elaborating on the current study.

List-Wide Proportion Congruency

As noted above, an *attention modulation account* of LWPC effects attributes these to a modulation of attention to the relevant and irrelevant dimensions (Bugg & Chanani, 2011; Bugg, McDaniel, et al., 2011; Lindsay & Jacoby, 1994; Logan & Zbrodoff, 1979): As the ratio of congruent trials to incongruent trials increases, attention to the relevant (color) dimension can be relaxed such that the influence of the irrelevant (word) dimension increases. Although attention modulation has been the standard explanation since the effect's discovery, only little empirical evidence exists that uniquely supports it.

Moreover, attention modulation accounts of the LWPC effect have recently been seriously challenged by event-based learning accounts (Risko, Blais, Stolz, & Besner, 2008; Schmidt & Besner, 2008; see also Musen & Squire, 1993; Schmidt, Crump, Cheesman, & Besner, 2007; Wendt & Luna-Rodriguez, 2009). These alternative accounts relate part (Risko et al., 2008) or most (Schmidt & Besner, 2008) of the influence of LWPC effects to nonattentional factors, and their major representative may be referred to as the *contingency learning account* (Schmidt, 2013a; Schmidt & Besner, 2008; Schmidt et al., 2007). Based on the notion that PC manipulations are typically confounded with contingency, the contingency learning account claims that the modulation of congruency effects (which themselves can still be conflict based) by manipulating PC mostly reflects a form of contingency learning wherein participants learn to predict the correct response from the (irrelevant) word presented. This speeds the high-contingent responses (i.e., responses to particular color-word combinations that are more frequently presented than others) and produces a similar pattern as is typically attributed to attention modulation.

Although the contingency learning account presents a major challenge to pure attention modulation accounts, recent studies from Bugg and colleagues have reported on observations that cannot be easily accounted for by contingency learning. For example, Bugg and Chanani (2011) observed that the LWPC effect transfers to novel, PC-unmanipulated items (i.e., 50% congruent) that are intermixed with PC-manipulated (mostly congruent or mostly incongruent, depending on the list) items. This suggests that the LWPC effect is at least partly related to modulation of attention. Along the same line, Bugg, McDaniel, et al. (2011) showed that attention modulation of word-reading processes affected Stroop interference and color naming on PC-unmanipulated, neutral items. Thus, new evidence has started to accrue in support of attention modulation in LWPC designs.

Item-Specific Proportion Congruency

From the notion of attention modulation, ISPC effects are necessarily related to a form of stimulus-driven implementation of attention settings. The appeal of the rivaling contingency learning account, however, is even stronger for ISPC effects than is the case for the LWPC effect. First, a pure contingency learning account may appear more parsimonious than attention modulation in that whereas the former draws upon a generally accepted (implicit) learning mechanism by which a response is primed through the detection of a feature of the stimulus, the latter requires this detection to be followed by the immediate modulation of word-reading processes—a process that is far from being explicated in detail. Second, the currently available empirical evidence—though divided—seems to lean toward the contingency learning account. Schmidt and Besner (2008) claimed that, at that time, the "weight of the evidence to date suggests that the ISPC effect is better explained by contingency learning" (p. 522), and they provided some convincing empirical support for this claim. In a study that deconfounded contingency and PC, they observed clear contingency learning effects while there was no indication for effects based on pure PC. Additionally, they tested and confirmed direct predictions from a contingency learning account. Various other studies have since then added empirical support to the claim that ISPC effects are largely attributable to contingency learning processes (Atalay & Misirlisoy, 2012; Hutchison, 2011; Schmidt, 2013a). Moreover, Schmidt (2013a) proposed a descriptive model (i.e., the parallel episodic processing, or PEP, model) that was shown in a simulation to easily produce ISPC effects without appealing to the notion of attention modulation.

Two recent studies from Bugg and colleagues (Bugg & Hutchison, 2012; Bugg, Jacoby, & Chanani, 2011) counter these claims. Bugg, Jacoby, and Chanani (2011) managed to implement an ISPC manipulation in a picture-word Stroop task without confounding it with contingencies (by using the relevant dimension as the PC signal) and still obtained ISPC effects (but see Schmidt, 2013b, for a critique). Moreover, Bugg, Jacoby, and Chanani (2011) observed a nonadditive relationship between contingency and congruency, which was proposed by Schmidt and Besner (2008) to not fit well with a contingency learning account. However, recently, Schmidt (2013a, 2013b) reconsidered the issue and showed that such a nonadditive relationship can actually be incorporated within a contingency learning account as well. Finally, the most problematic finding of Bugg and colleagues for a pure contingency learning account consists of their observed transfer of ISPC effects to novel, PC-unmanipulated items (Bugg, Jacoby, & Chanani, 2011). Bugg and Hutchison (2012) extended these findings to the classical color-word Stroop task and showed that noncontingency-based ISPC effects can be obtained also when using the irrelevant dimension as the PC signal. Hence, for the ISPC effect, too, the overall empirical evidence is currently divided. Here, we contribute to this debate by providing a direct test of attention modulation by PC while using a novel manipulation.

The Present Study: Shifting Between Lists of Different (Item-Specific) Proportion Congruency

The current paper provides a test of a direct prediction of attention modulation in LWPC and ISPC designs through the *list shifting* paradigm that we introduce here within the context of the color-word Stroop task.

Participants are provided with training on LWPC-manipulated (Experiment 1A) or ISPC-manipulated (Experiment 2) lists of Stroop trials. After the training phase, the proportion of congruent trials within these lists is reversed (either list-wide or within the item-specific context), and we assess the response to this shift. Such list shifting will provide important insights into how the cognitive control system operates in a nonstationary environment (Cohen, McClure, & Yu, 2007; Daw, O'Doherty, Dayan, Seymour, & Dolan, 2006). The flexible nature of cognitive control has recently drawn considerable interest in the form of trial-by-trial effects (e.g., Verguts & Notebaert, 2008), but little research has focused on this topic beyond such trial-by-trial modulations.

When one considers the potential impact of shifting between lists of different PC, a question that immediately emerges is whether shifting from a mostly incongruent (MI) to a mostly congruent (MC) list of trials is equivalent to shifting in the opposite direction. Attention modulation accounts of PC effects predict that this should not be the case. Rather, modulations of the congruency effect should be larger when transitioning from an MC list to an MI list relative to transitioning from an MI list to an MC list. This is because an initial MI list should lead to a focusing of attention on the relevant dimension (or a withdrawal of attention from the irrelevant dimension), thus reducing the likelihood that a future change in the relation between the relevant and irrelevant dimensions (i.e., a change in PC) is detected. Conversely, an initial MC list will relax attention focus on the relevant information, and this will increase the ability of the system to detect changes with respect to the relation between the relevant and irrelevant dimensions.

Hence, attention modulation accounts predict asymmetrical changes in the magnitude of the congruency effect for a shift from MC to MI compared to a shift from MI to MC. Given that this prediction is attention based, accounts of LWPC and ISPC effects that do not include attention modulation would not make such a prediction (Risko et al., 2008; Schmidt & Besner, 2008). Explanations in terms of simple contingency learning claim that certain color-word combinations are faster than others because of the fact that they are presented more frequently. When the list changes (i.e., shifting from MC to MI or from MI to MC), the color-word contingencies change in a symmetrical manner, and this should result in a symmetrical effect on congruency effects: Either these new contingencies are picked up and both list shifts have a similar effect on the congruency effect, or these new contingencies are not picked up and both list shifts have no effect. A contingency learning account does not consider congruency as important for learning.

In summary, list shifting provides a unique test of the feasibility of attention modulation accounts in comparison to the competing contingency account.

Experiment 1: List-Wide Proportion Congruency

In Experiment 1A, participants took part in a four-choice Stroop task. There were two phases of trials, each with a different proportion of congruent trials. In the MI phase, 75% of the trials were incongruent and 25% were congruent, and vice versa in the MC phase (see Table 1). Half of the participants started with an MI phase and shifted to an MC phase (MI-MC group), and the other half of participants did the opposite (MC-MI group). Attention modulation accounts predict a decrease in the magnitude of congruency effects when shifting from MC to MI that is larger than the increase in the magnitude of the congruency effect when shifting from MI to MC (i.e., asymmetrical changes). This is because MC and MI generate different attention control settings. Conversely, a contingency account makes different predictions: Because the shifts in contingency are similar in absolute terms for the MC-MI and MI-MC groups, it predicts similar—again in absolute terms—changes in the congruency effect as well (i.e., symmetrical changes).

Experiment 1B was identical but without the PC list shifting manipulation (i.e., MC-MC and MI-MI transitions were used) in order to rule out explanations in terms of practice: Across phases the congruency effect might decrease (e.g., MacLeod & Dunbar, 1988), and this general decrease in the magnitude of the congruency effect could artifactually supplement the magnitude of the decrease in the congruency effect when shifting from an MC to an MI list. In addition, this general decrease in the magnitude of the congruency effect could artifactually diminish the magnitude of

Table 1
Color/Word Combinations in Percentages for Experiments 1A and 1B and Experiment 2

		Color/word															
Experiment	PC	R/R	R/Gr	R/Y	R/B	Gr/Gr	Gr/R	Gr/Y	Gr/B	Y/Y	Y/B	Y/R	Y/Gr	B/B	B/Y	B/R	B/Gr
1A/1B	MC	18.75	2.08	2.08	2.08	18.75	2.08	2.08	2.08	18.75	2.08	2.08	2.08	18.75	2.08	2.08	2.08
	MI	6.25	6.25	6.25	6.25	6.25	6.25	6.25	6.25	6.25	6.25	6.25	6.25	6.25	6.25	6.25	6.25
		R/R	R/G	R/Y	R/B	G/G	G/R	G/Y	G/B	Y/Y	Y/B	Y/R	Y/G	B/B	B/Y	B/R	B/G
2	MC	20	5			20	5			12.5	12.5			12.5	12.5		
	MI	5	20			5	20			12.5	12.5			12.5	12.5		

Note. Please note that the item-specific PC manipulation in Experiment 2 is depicted here with the combinations of red and green as manipulated items and the combinations of yellow and blue as unmanipulated items. This was actually counterbalanced such that the reverse was true for half of the participants. PC = proportion congruency; R = red; Gr = gray; G = green; Y = yellow; B = blue; MC = mostly congruent; MI = mostly incongruent.

the increase in the congruency effect when shifting from an MI to an MC list.

Method

Participants. Forty-two participants from Arizona State University participated in Experiment 1A ($n = 20$) or 1B ($n = 22$) for course credit or pay ($5.00).

Design. For Experiment 1A, a 2 (Congruency: Congruent vs. Incongruent) × 2 (Phase: Phase 1 vs. Phase 2) × 2 (Order: MC-MI vs. MI-MC) mixed design was used with Order manipulated between-subject. For Experiment 1B, a 2 (Congruency: Congruent vs. Incongruent) × 2 (Phase: Phase 1 vs. Phase 2) × 2 (Proportion Congruency: MC vs. MI) mixed design was used. Here, Proportion Congruency was manipulated between-subject and remained constant across blocks.

Apparatus and stimuli. Experiment Builder software (SR Research Ltd.) controlled the timing and presentation of stimuli and logged responses and response times. Stimuli were presented on a standard 15-in. color monitor, against a black background. The stimulus display consisted of a fixation cross at the center of the screen. The color words *red, blue, gray, and yellow* (in Courier font) were presented in colors red, blue, gray or yellow. Responses were registered by means of a *QWERTY* keyboard.

Procedure. Participants were instructed that on each trial a colored color word would appear, and they were to respond to the color in which it was printed. The *S, D, K,* and *L* keys were mapped on the colors red, blue, gray, and yellow, respectively. Each trial began with the presentation of the fixation cross for 500 ms. The target was presented and remained on the screen until a response was made. Participants performed two blocks of 288 experimental trials. Table 1 depicts the precise configuration of blocks.

Results

Incorrect trials and trials with response times (RTs) more than 3 standard deviations below or above average RT (per subject, Phase, and Congruency) were excluded from RT analyses. A 2 (Congruency: Congruent vs. Incongruent) × 2 (Phase: Phase 1 vs. Phase 2) × 2 (Order: MC-MI vs. MI-MC) mixed-design analysis of variance (ANOVA) was performed for both mean RTs and mean error percentages (PEs) for Experiment 1A, and a 2 (Congruency: Congruent vs. Incongruent) × 2 (Phase: Phase 1 vs. Phase 2) × 2 (Proportion Congruency: MC vs. MI) mixed-design ANOVA was performed for Experiment 1B. Effect size estimates (partial eta squared) are provided. See Table 2 for an overview of mean RTs, PEs, and congruency effects. See Figure 1 for an overview of the development of congruency effects across phases.

Experiment 1A.

RT. There were significant main effects of Congruency, $F(1, 18) = 59.8$, $p < .001$, $\eta_p^2 = .77$; Phase, $F(1, 18) = 28.7$, $p < .001$, $\eta_p^2 = .62$; and Order, $F(1, 18) = 6.1$, $p < .05$, $\eta_p^2 = .25$. There were significant two-way interactions between Congruency and Phase, $F(1, 18) = 8.5$, $p < .01$, $\eta_p^2 = .32$; between Congruency and Order, $F(1, 18) = 5.8$, $p < .05$, $\eta_p^2 = .24$; and between Phase and Order, $F(1, 18) = 5.4$, $p < .05$, $\eta_p^2 = .23$. Finally, there was a significant three-way interaction among Congruency, Phase, and Order, $F(1, 18) = 17.1$, $p < .001$, $\eta_p^2 = .49$. This three-way interaction indicated a substantial decrease of the congruency effect over phases (congruency effects of 290 ms and 101 ms for Phases 1 and 2, respectively) for the MC-MI group, $F(1, 9) = 13.9$, $p < .001$, $\eta_p^2 = .61$, whereas for the reversed direction (i.e., the MI-MC group) we observed a marginal increase of the congruency effect ($p = .09$; congruency effects of 86 ms and 119 ms for Phases 1 and 2, respectively).[1] Critically, the decrease (MC minus MI) of the congruency effect for the MC-MI group was significantly larger than the increase (MC minus MI) for the MI-MC group, $F(18) = 8.5$, $p < .01$, $\eta_p^2 = .32$, indicating an asymmetry of effects in list shifting.

PE. Overall PEs amounted to 6.5%. Analyses on PEs paralleled the above analyses on RTs. Again, a significant three-way interaction was observed among Congruency, Phase, and Order, $F(1, 18) = 7.1$, $p < .05$, $\eta_p^2 = .28$, that was rooted in a significant decrease of the congruency effect over phases for the MC-MI group (congruency effects of 9.8% and 4.8% for Phases 1 and 2, respectively), $F(1, 9) = 20{,}5$, $p < .001$, $\eta_p^2 = .70$, whereas there was no significant change in congruency effects across phases for the MI-MC group (congruency effects of 4.2% and 4.7% for Phases 1 and 2, respectively; $p = .82$). The decrease of the congruency effect for the MC-MI group was significantly larger than the numerical increase for the MI-MC group, $F(1, 18) = 5.1$, $p < .05$, $\eta_p^2 = .22$, again indicating an asymmetry in effects of list shifting. Moreover, a significant main effect was observed for Congruency, $F(1, 18) = 17.8$, $p < .01$, $\eta_p^2 = .50$, whereas a significant two-way interaction between Phase and Order, $F(1, 18) = 21.6$, $p < .001$, $\eta_p^2 = .55$, signaled increased overall performance across phases for the MC-MI group and decreased overall performance for the MI-MC group.

Experiment 1B. There were no significant three-way interactions for RT or PE (ps $> .80$)—indicating that there was no significant difference between PC groups (MC-MC and MI-MI) in congruency effect changes between Phase 1 and Phase 2. Moreover, the congruency effect was similar between Phase 1 and Phase 2 for both PC groups (ps $> .40$). Finally, a normal PC effect was observed when the two PC groups were compared on RT, $F(1, 20) = 24.6$, $p < .001$, $\eta_p^2 = .55$, and on PE, $F(1, 20) = 7.5$, $p < .05$, $\eta_p^2 = .27$.

Finally, we performed an omnibus analysis with a 2 (Congruency: Congruent vs. Incongruent) × 2 (Phase: Phase 1 vs. Phase 2) × 2 (Order: MC-MI/MC-MC vs. MI-MC/MI-MI)[2] × 2 (Experiment: 1A vs. 1B) mixed-design ANOVA in which Order and Experiment were manipulated between-subject. This produced the desired four-way interaction among Congruency, Phase, Order, and Experiment, $F(1, 38) = 9.7$, $p < .01$.

[1] Some authors have explored the relation between proportion congruency effects and the effect of previous trial congruency (e.g., Funes, Lupiañez, & Humphreys, 2010), typically concluding that these are relatively independent. In line with this, when we included Previous Trial Congruency (congruent vs. incongruent) in Experiment 1A, this left intact the crucial three-way interaction among Congruency, Phase, and Order, $F(1, 18) = 8.0$, $p < .01$, while this three-way interaction did not further interact with Previous Trial Congruency ($p = .52$).

[2] Because of the difference in design between Experiments 1A and 1B, we matched the MC-MI and MI-MC groups of Experiment 1A with the MC-MC and MI-MI groups of Experiment 1B, respectively.

Table 2
Mean Response Times (RTs) and Mean Error Percentages (PEs) Across Experiments, Conditions, Phases, and (for Experiment 2) Trial Types

				RTs (ms)		PEs (%)		Congruency effect	
Experiment	Condition	Trial type	Phase	Con	Inc	Con	Inc	RT (ms)	PE (%)
1A	MC-MI	Manipulated	1: MC	921	1,211	2.6	12.4	290	9.8
		Manipulated	2: MI	794	895	2.8	7.6	101	4.8
	MI-MC	Manipulated	1: MI	741	828	3.1	7.3	86	4.2
		Manipulated	2: MC	637	756	5.0	9.7	119	4.7
1B	MC-MC	Manipulated	1: MC	830	1065	2.7	10.6	235	7.9
		Manipulated	2: MC	692	899	3.3	9.2	207	5.9
	MI-MI	Manipulated	1: MI	811	874	3.1	6.5	63	3.4
		Manipulated	2: MI	750	785	4.1	6.1	35	2.0
2	MC-MI-MC	Manipulated	1: MC	550	642	2.6	5.6	92	3.0
		Manipulated	2: MI	564	593	3.1	3.6	29	0.6
		Manipulated	3: MC	535	596	3.2	5.6	61	2.4
		Unmanipulated	1: MC	562	618	3.0	3.8	56	0.8
		Unmanipulated	2: MI	550	580	2.1	3.6	30	1.5
		Unmanipulated	3: MC	529	584	3.9	2.9	55	−1.0
	MI-MC-MI	Manipulated	1: MI	557	577	2.2	3.8	20	1.6
		Manipulated	2: MC	543	574	2.6	4.4	31	1.8
		Manipulated	3: MI	534	551	5.5	3.7	17	−1.8
		Unmanipulated	1: MI	551	591	3.4	4.0	40	0.6
		Unmanipulated	2: MC	551	571	2.5	5.7	20	3.2
		Unmanipulated	3: MI	539	567	2.4	4.1	28	1.7

Note. Congruency effects are added. MC = mostly congruent; MI = mostly incongruent; Con = congruent; Inc =incongruent.

Discussion

The results of Experiment 1 are clear. In terms of performance, shifting from an MC list to an MI list is not equivalent to shifting from an MI list to an MC list: Participants showed larger changes across phases in the magnitude of the congruency effect when they were initially exposed to an MC list. This observation, which we refer to as the *asymmetrical list shifting effect,* is consistent with the notion that attention modulation is triggered by the LWPC manipulation. Hence, it seems that when participants are initially exposed to an MI list, attention to the irrelevant dimension is reduced. This results in a reduction in the magnitude of the congruency effect but also in a reduction in the ability to detect a shift to an MC list. The results of Experiment 1B did not support an alternative explanation of Experiment 1A in terms of practice effects. That is, there was no significant decrease in the magnitude of the congruency effect across phases for either the MC-MC or the MI-MI group.

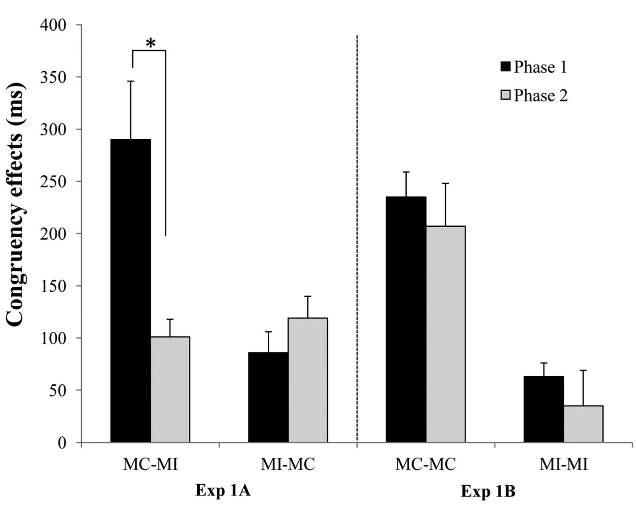

Figure 1. Mean congruency effects on response time for the different phases of Experiments (Exp) 1A and 1B. Error bars represent standard errors of the mean. MC = mostly congruent; MI = mostly incongruent. $^{*}p < .01$.

As outlined above, a contingency learning account of the PC effect would have predicted a symmetrical effect on the overall congruency effects because the overall contingency shift was symmetrical. However, as shown in Table 1, the potential for contingency learning was not symmetrical for the MC and MI lists: Whereas contingencies were present in the MC list, they were fully absent in the MI list. As such, whereas the adjustment to the MI-MC shift could involve contingency learning, the adjustment to the MC-MI shift would actually involve contingency unlearning. For now, this does not change the prediction of symmetrical shifting, as we focused here on a simple contingency learning account in which equal learning and unlearning rates are assumed. Indeed, Schmidt, De Houwer, and Besner (2010) showed that learning and unlearning of contingencies occur equally fast. However, if for some reason it would turn out that, for example, the learning rate can be affected by the continued absence of learning in the MI block of the MI-MC group, then possibly this could also explain the nonresponse to changing contingencies in this group.[3] For now we have no reason to believe this to be the case, and, in anticipation of what follows, we emphasize that the design of Experiment 2 provided contingency learning potential across MC and MI lists and still showed similar asymmetrical list shifting.

[3] We thank an anonymous reviewer for this suggestion.

The findings of Experiment 1 at first sight support a sustained and proactive mode of control or, in other words, a sustained focus of attention to the relevant information (Bugg, McDaniel, et al., 2011; Funes, Lupiañez, & Humphreys, 2010). This would be in line with the traditional view of cognitive control as a domain-general, top-down mechanism (Botvinick et al., 2001). Conversely, it has been proposed that LWPC effects are actually ISPC effects in disguise, with attention modulation being triggered at the item level (Blais & Bunge, 2010; Bugg et al., 2008). On the basis of the current data we cannot provide definite answers to the question about which of these two mechanisms was at play in Experiment 1, but we would like to note that Bugg and Chanani (2011; see also Bugg, 2012) claimed that with stimulus sets of four—like we adopted here—list-wide attention modulation is probably dominant over item-specific effects because associative learning is less effective. In the General Discussion we will return to this issue. For now it is important to note that the current data favor attention modulation (either list-wide or item-specfic) over contingency learning.

Finally, we would like to briefly address an apparent discrepancy between current results and those of Bugg and Chanani (2011). Though it was not the topic of study in their paper, Bugg and Chanani explored PC list shifting order and did not observe any significant impact of it. However, compared to those in our Experiment 1A, the participants in the study of Bugg and Chanani were provided with less training on the initial PC condition; this may explain the discrepancy. Attention must be sufficiently modulated to prompt differences in the ability to detect changes in conflict, and this requires sufficient training. Indeed, a study by West and Alain (2000) reported an asymmetry in shifting—here too not the topic of that study—after more than 700 trials on the initial MI list. A related issue concerns the amount of trials in Phase 2. We did not find a shifting effect in the MI-MC direction but believe that, with more extensive practice, we would have observed such an effect. The point is not that there is no shifting effect from an MI list to an MC list but rather that it is weaker (or takes more time) than the shifting effect in the MC-MI direction.

Experiment 2: Item-Specific Proportion Congruency

As noted above, there is considerable debate surrounding the mechanisms underlying the ISPC effect; in particular, with regard to whether the effect reflects item-specific attention modulation or "mere" contingency learning. In Experiment 2 we chose to use the same reasoning as in Experiment 1A in order to test an attention modulation account of the ISPC effect. PC was manipulated for only two out of four color items (*PC-manipulated items*), and the other two items were left PC unmanipulated (50% congruent; *PC-unmanipulated items*). After a training phase (Phase 1), item-specific PCs were reversed (the unmanipulated items remained set at 50% congruent) in Phase 2 so that we could explore if the asymmetrical list shifting effect can also be observed for ISPC effects (see Table 1).

In Experiment 2, we adapted the typical ISPC task to not include a within-subject manipulation of MC and MI items. Rather, we presented each participant with either MC or MI items, in addition to items that were not PC manipulated (50% congruent). We avoided the more traditional fully within-subject design (in which participants would have been trained with both MC and MI items and then tested with reversed proportions), because we wanted to avoid the possibility that detecting the MC-MI transition (which we predict) might increase the chances of participants detecting and adjusting to the MI-MC transition as well. As such, this would have decreased our chances of observing asymmetrical shifting and of distinguishing between attention modulation and contingency accounts.

One consequence of this choice of design is that Experiment 2 includes not only an ISPC but also an overall LWPC manipulation: The PC-manipulated items (80% congruent or incongruent) were complemented by PC-unmanipulated items (50% congruent, as opposed to the more typical case that involves items with reversed PCs) such that for each participant the overall LWPC was 65% congruent or incongruent (as opposed to the overall 50% congruent in the typical ISPC task). We expected, in line with previous studies (Blais & Bunge, 2010; Bugg et al., 2008), that this would still result mainly in effects at the item-specific level. We briefly return to this issue in the General Discussion.

A secondary aim in Experiment 2 was to establish the PC list shifting effect when initial training is controlled for (i.e., the fact that one group starts with MC and the other with MI). If indeed the asymmetrical list shifting effect is explained by differential attentional weighting of relevant and irrelevant stimulus features/dimensions, we would expect to also find a weaker or even absent adjustment to list shifting for participants who performed the MC-MI transition and are then retested in Phase 3 on an MC list (i.e., MC-MI-MC): During their intermediate MI list, attention should be strongly directed toward the relevant dimension, and subsequent changes in congruency should not be detected as easily (similar to the MI-MC group in Experiment 1A). If we observe such a relatively weak adjustment to list shifting from Phase 2 to Phase 3 for the MC-MI-MC group (compared to the shift from Phase 1 to Phase 2), this would rule out the argument that the effects are dependent on starting the experiment with an MI list.

Overall, for Experiment 2 the contingency learning account would predict symmetrical changes in the magnitude of the congruency effect for only the manipulated items (as only these contained word-color contingencies; see Table 1). Conversely, attention modulation would result in asymmetrical list shifting effects, as we should observe a stronger change in the magnitude of the congruency effect for the MC-MI transition of the MC-MI-MC group than for all other transitions (which should be accompanied by relatively small or absent changes in the magnitude of congruency effects). Moreover, this asymmetrical list shifting should be observed only for the PC-manipulated items from the notion of item-specific attention modulation, whereas we would observe asymmetrical list shifting independent of trial type (PC-manipulated vs. PC-unmanipulated) if attention modulation occurred in a list-wide, sustained fashion.

Method

Participants. Thirty-six participants from Ghent University participated in Experiment 2 for course credit. We excluded from the analyses one participant who stated to have been informed about the switches in PC by a colleague student and admitted to have used this information to search for and anticipate the switches.

Apparatus and stimuli. E-Prime software (Schneider, Eschman, & Zuccolotto, 2002) was used for stimulus presentation and reaction time data acquisition. Stimuli were displayed on a 17-in. monitor of a Pentium processor, with a viewing distance of approximately 50 centimeters. The Stroop stimuli consisted of one out of four possible color words (*red*, *green*, *yellow,* and *blue*) printed out centrally on a black background in one of the four possible colors (red, green, yellow, or blue), with Courier font.

Design and procedure. Participants were randomly assigned to one of the two experimental groups, differing only in the sequence of the PC list shifts. Half of the participants started out with five MC blocks in Phase 1, followed with two MI blocks (Phase 2), and concluded with two MC blocks (Phase 3). The other half shifted from five MI blocks over two MC blocks back to two MI blocks. The experiment thus consisted of nine blocks of 80 trials each, amounting to 720 trials in total. On two of the four color words, a PC manipulation was applied (80% congruent or incongruent, depending on the list), whereas the other two color words were equally often presented in their congruent and incongruent color throughout the whole experiment. Counterbalanced across participants, *red* and *green* or *blue* and *yellow* served as either the PC-manipulated or the PC-unmanipulated color words. Table 1 depicts the precise item composition of Experiment 2. In mostly congruent blocks, 65% of trials were congruent overall (80% of the PC-manipulated and 50% of the PC-unmanipulated trials), whereas in mostly incongruent blocks 65% of trials were incongruent overall (80% of the PC-manipulated and 50% of the PC-unmanipulated trials). Overall, this resulted in a 2 (Congruency: Congruent vs. Incongruent) × 3 (Phase: Phase 1 vs. Phase 2 vs. Phase 3) × 2 (Trial-type: PC-manipulated vs. PC-unmanipulated) × 2 (Order: MC to MI to MC vs. MI to MC to MI) design.

The participants' task was to identify the color in which the words appeared on the screen as quickly as possible while ignoring the irrelevant word meaning. Speed and accuracy were equally stressed. The *C*, *V*, *B,* and *N* keys were mapped to the colors red, yellow, green, and blue, respectively. As a reminder to the participants, the keys were indicated with a colored patch, and a printout of the color-to-key mapping was attached to the bottom of the monitor. Each trial began with the presentation of a fixation cross for 500 ms, after which the target appeared on the screen until a response was registered. No error feedback was provided. The participants took a short, self-paced break between blocks.

We would like to note that we also manipulated PC awareness in Experiment 2. Hence, we informed half of the participants about the overall PC manipulation before the experiment started. The other half of the participants started the experiment in a naive mode. As this manipulation had no significant effects on the outcomes here presented (all *p*s > .20), we decided to not report on this any further. We emphasize that we did not inform the informed participants about the fact that only two colors were manipulated, nor about the switches in PC throughout the experiment; they were solely informed about the overall PC that they were to encounter (and which was thus strongest in Phase 1). Finally, including only the naive participants in our analyses produced a similar pattern of results compared to what we report here across naive and informed participants together.

Results

As in Experiment 1, we excluded from the RT analyses all incorrect trials and trials with RTs more than 3 standard deviations below or above average RT (per subject, Phase, and Congruency). A 2 (Order: MC to MI to MC vs. MI to MC to MI) × 3 (Phase: Phase 1 vs. Phase 2 vs. Phase 3) × 2 (Trial Type: PC-Manipulated vs. PC-Unmanipulated) × 2 (Congruency: Congruent vs. Incongruent) mixed-design ANOVA was performed on the mean RTs and PEs. Effect size estimates (partial eta squared) are provided. See Table 2 for an overview of mean RTs, PEs, and congruency effects. See Figure 2 for an overview of the development of congruency effects across phases.

RT. The analysis revealed significant main effects of Phase, $F(2, 66) = 16.9$, $p < .001$, $\eta_p^2 = .34$, and Congruency, $F(1, 33) = 43.0$, $p < .001$, $\eta_p^2 = .57$. The interactions between Phase and Congruency, $F(2, 66) = 6.6$, $p < .001$, $\eta_p^2 = .17$, and between Order and Congruency, $F(1, 33) = 5.3$, $p < .05$, $\eta_p^2 = .14$, were also significant. The three-way interaction among Order, Phase, and Congruency was significant, $F(2, 66) = 4.6$, $p < .01$, $\eta_p^2 = .12$, indicating that the difference in the congruency effect between the three phases significantly varied across the two Order groups. Moreover, the significant four-way interaction with Trial Type, $F(2, 66) = 6.3$, $p < .01$, $\eta_p^2 = .16$,[4] further indicated that this difference between the Order groups was observed only for the PC-manipulated items, $F(2, 66) = 8.0$, $p < .01$, $\eta_p^2 = .20$, and not for the PC-unmanipulated items ($p = .50$).

Further analyses on the PC-manipulated items showed that the congruency effect did not significantly vary across the three phases for the MI-MC-MI group ($p = .41$), although it differed between the three phases in the MC-MI-MC group, $F(2, 34) = 8.8$, $p < .01$, $\eta_p^2 = .36$. As in Experiment 1, the congruency effect first diminished from Phase 1 (MC; congruency effect = 92 ms) to Phase 2 (MI; congruency effect = 29 ms), $F(1, 17) = 19.9$, $p < .001$, $\eta_p^2 = .53$, but it only marginally increased from Phase 2 (MI) to Phase 3 (MC; congruency effect = 61 ms), $F(1, 17) = 3.4$, $p = .08$, $\eta_p^2 = .16$. Moreover, there was a significant difference in congruency effect between Phase 1 and Phase 3, $F(1, 17) = 5.6$, $p < .05$, $\eta_p^2 = .25$, indicating also that the decrease of the congruency effect over Phases 1 and 2 was significantly larger than the subsequent increase over Phases 2 and 3 (i.e., asymmetrical list shifting; within-subject). Finally, the decrease of the congruency effect over Phases 1 and 2 for the MC-MI-MC group was significantly larger than the numerical increase of the congruency effect over Phases 1 and 2 for the MI-MC-MI group, $F(1, 33) = 8.8$, $p < .01$, $\eta_p^2 = .21$ (i.e., asymmetrical list shifting; between-subject), whereas the latter increase did not significantly differ from the increase in congruency effect from MI to MC in the MC-MI-MC group ($p = .30$).

PE. Overall, PEs amounted to around 3.5%. A significant main effect of Congruency, $F(1, 33) = 14.2$, $p < .01$, $\eta_p^2 = .30$, and a significant interaction among Order, Trial Type, and Congruency, $F(1, 33) = 4.8$, $p < .05$, $\eta_p^2 = .13$, were observed. Follow-up analyses showed a marginally significant interaction

[4] As in Experiment 1A, we explored the role of previous trial congruency. This maintained the crucial four-way interaction among Order, Phase, Trial Type, and Congruency, $F(2, 68) = 6.8$, $p < .01$, while this four-way interaction did not further interact with Previous Trial Congruency ($p = .60$).

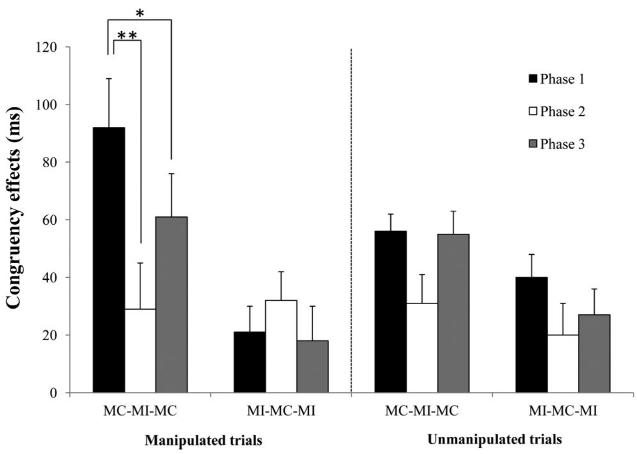

Figure 2. Mean congruency effects on RT for the different phases and trial types of Experiment 2. Error bars represent standard errors of the mean. MC = mostly congruent; MI = mostly incongruent. $^{*} p < .05$. $^{**}p < .01$.

between Order and Congruency for PC-manipulated items (congruency effects of 2% and .5% for the MC-MI-MC and MI-MC-MI groups, respectively), $F(1, 33) = 2.9$, $p = .09$, $\eta_p^2 = .08$, and a nonsignificant but reversed interaction for the PC-unmanipulated items ($p = .16$).

Discussion

The results of Experiment 2 clearly demonstrate item-specific attention modulation. For the PC-manipulated—but not the PC-unmanipulated—items we observed an asymmetrical list shifting effect similar to that reported in Experiment 1A, where a LWPC manipulation was used. These results are incompatible with either list-wide, sustained attention modulation or mere contingency learning accounts. For the PC-manipulated items in the MI condition, attention was focused on the relevant information, and this prevented the adequate detection of changes in the relation between the relevant and irrelevant information (i.e., shift to being MC). Impressively, this was even observed for the MI-MC transition of the MC-MI-MC group—a finding that counters any notion that the asymmetrical list shifting effects observed in Experiment 1A and in the Phase 1 to Phase 2 transition of Experiment 2 resulted from differences in initial training conditions (i.e., MC vs. MI).

On a final note, from Table 2 it becomes clear that the MC-MI and MI-MC groups in Experiment 1A are at two very different parts of the RT scale: Average RTs are higher for the MC-MI than for the MI-MC group. The fact that the asymmetrical list shifting effect was obtained on the manipulated trials in Experiment 2, which does not suffer from the scale differences observed in Experiment 1A, counters concern that the scale difference in Experiment 1A was responsible for the asymmetrical list shifting effect.

General Discussion

In the current paper we present a novel effect that we termed the *asymmetrical list shifting effect*. When shifting from an MC list to an MI list—either list-wide (Experiment 1A) or item-specific (Experiment 2)—there was a marked decrease in the magnitude of the Stroop congruency effect. Conversely, when participants shifted from an MI list to an MC list, such a change in the magnitude of the congruency effect was much weaker—and even absent with the amount of test trials used here. The absolute magnitude of the shift in PC per se was equivalent across transitions, indicating that the critical variable was the direction of the shift.

Attention Modulation

The asymmetrical list shifting effect fits well with the notion that different levels of PC (list-wide or item-specific) bring about different levels of processing of the relevant and/or irrelevant stimulus features through attention modulation (cf. Bugg, 2012; Bugg et al., 2008). Moreover, it directly opposes the symmetrical effects that one would have expected from a simple contingency learning account. As such, the current paper contributes to the ongoing debate on whether the various PC effects are related to attention modulation by the cognitive control system or merely to contingency learning—strongly favoring the former.

Whereas the current study provides empirical support for attention modulation in both LWPC-manipulated (Experiment 1) and ISPC-manipulated (Experiment 2) color-word Stroop tasks, the precise underlying mechanism requires further elaboration. The results of Experiment 2 support some form of attention modulation occurring at the item-specific level (see below). This result has tentative implications for the interpretation of Experiment 1. In particular, the current Experiment 2 implemented the item specificity of the PC manipulation in such a way that the LWPC was also manipulated (see above); yet, there was no effect on the PC-unmanipulated items (i.e., no effects of overall PC were observed even when we considered in a separate analysis the PC-unmanipulated items in only the first phase of the experiment; $p = .22$). This suggests that no sustained attention modulation developed on the basis of the LWPC in Experiment 2. Thus, the most parsimonious conclusion would be that in Experiment 1 the attention modulation was occurring at the item level, as it was in Experiment 2. That said, Bugg and Chanani (2011) have suggested that with stimulus sets of four, as in Experiment 1, list-wide attention modulation dominates item-specific effects because associative learning is less effective. Thus, we cannot exclude the possibility that list-wide, sustained attention modulation underlay the effects in Experiment 1, with its absence in Experiment 2 explained by, for example, the possibility that list-wide attention modulation can no longer be maintained once the system starts modulating attention at the item level.

With respect to the ISPC effect, the current study strengthens a recent claim by Bugg, Jacoby, and Chanani (2011) that it is "too early to lose control in accounts of item-specific proportion congruency effects" (p. 844) and balances the respective empirical evidence for attention modulation and contingency accounts. The fact that we obtained support for item-specific attention modulation in a classical color-word Stroop task corroborates the findings of Bugg and Hutchison (2012). This is important in light of the fact that it had previously been suggested that, in such a task, contingency provides the largest (if not the only) contribution to ISPC effects (Bugg, Jacoby, & Chanani, 2011; Hutchison, 2011).

How should item-specific attention modulation be understood? One possible explanation builds on the idea of "on the fly" retrieval and implementation of attentional settings in response to (early processing of) stimulus features (e.g., Bugg & Hutchison, 2012; Bugg, Jacoby, & Chanani, 2011; Crump, Gong, & Milliken, 2006; Heinemann, Kunde, & Kiesel, 2009; King, Korb, & Egner, 2012; Lehle & Hübner, 2008). For the current study, this would mean that word-reading and/or color-naming processes were rapidly and flexibly reconfigured after the stimulus onset based on the item-set of the stimulus. This would exemplify the enormous power and flexibility of the cognitive control system in aligning our actions with our goals. However, it is not clear how this would be further implemented at the neural level—an issue that is left untouched in most ISPC studies.

There is a theoretical alternative for implementing item-specific attention modulation that avoids the notion of rapid, on the fly adjustments within the trial. Verguts and Notebaert (2008, 2009) proposed the adaptation-by-binding account (ABBA), in which attention modulation is implemented by associating representations at the input level (e.g., color and word features) with representations at the task level (e.g., responding to the color) through Hebbian learning. They have shown in a computational model that, under the assumption that conflict enhances Hebbian learning (e.g., through release of noradrenalin), ABBA successfully accounts for the ISPC effect. Considered this way, MI items in the current Experiment 2 have stronger task-relevant connections due to the history of frequent conflict, which implies that the attentional settings are already in place before item presentation and need no rapid and dynamic adjustments after stimulus presentation. This renders the system rigid as opposed to flexible, because the associations that are acquired before a particular stimulus presentation will necessarily impact the processing of this stimulus. Still, the control system may be referred to as flexible in the sense that association strengths are continuously updated between trials. Although ABBA allows an implementation of attention modulation in the brain in a highly plausible manner, some crucial questions remain for this account. Most important for current purposes, it is still unclear whether the current computational implementation that underlies ABBA can actually account for the asymmetrical list shifting effect.

Item-Specific Attention Modulation, Contingency, or Both?

The findings of the current Experiment 2 balance the respective support for item-specific attention modulation and contingency learning (Atalay & Misirlisoy, 2012; Bugg & Hutchison, 2012; Bugg, Jacoby, & Chanani, 2011; Hutchison, 2011; Schmidt, 2013a; Schmidt & Besner, 2008). Overall, then, this seems to strengthen a recent claim by Bugg and Crump (2012; cf. Bugg, 2012) that possibly both attention modulation and contingency learning contribute to ISPC effects. This claim should inspire future exploration—across different experimental designs and across different paradigms (e.g., Stroop, Flanker, and Simon tasks)—of the interrelations between contingency and PC, such as when indications are observed for one but not the other.

Schmidt (2013a) reported clear contingency-based but not PC-based effects when these were deconfounded from each other in an ISPC task, and this, at first sight, is difficult to reconcile with the above claim. Bugg and Hutchison (2012) may have already provided a clear explanation in suggesting that contingency may be dominant when—as was the case in the study by Schmidt (2013a)—two item-sets (e.g., one set for MC and another for MI) are used, whereas PC-based effects may dominate in Stroop tasks with four item-sets, probably due to the absence of high-contingent incongruent responses with four item-sets. However, Experiment 2 of the current study also used a two item-set design and showed more or less the reversed pattern of Schmidt (2013a): No indications were observed to support contingency learning (i.e., there was no significant impact of the dramatic shift in contingency in the MI-MC group), and the overall data pattern clearly fitted the attention modulation account. As such, the current findings question the sufficiency of an item-set explanation for Schmidt's (2013a) observed absence of PC-based effects. Whereas the impact of contingency may be larger when high-contingent incongruent responses are available than when this is not the case (Bugg & Hutchison, 2012), this does not rule out that attentional modulation effects are at play in two item-set designs.

Finally, the question remains why the current design did not show clear indications for contingency effects, though various studies have reported them in similar designs (for a review, see Schmidt, 2013b). In particular, we observed no significant changes in the congruency effect when contingencies were reversed for the MI-MC transitions. We here speculate that this could be explained by potential additional interactions between attention modulation and contingency effects. That is, if an MI list prompts attention to be strongly focused on the relevant information, the response priming potency of the irrelevant information may be substantially decreased just because it is no longer processed as extensively. This reasoning may also imply for MC-MI shifts that at least some part of the dramatic impact may relate to contingency-based priming being updated after the shift.

The List Shifting Paradigm

The asymmetrical list shifting effect not only speaks to the debate on how to interpret PC effects but also constitutes a novel control effect per se. If one considers the development of PC effects as a useful adaptation to the current context, between-list changes in PC require readaptation to optimally adjust to the novel context. The observation that such readaptation is stronger with MC-MI than MI-MC shifts makes the list shifting paradigm an interesting one for future explorations of the cognitive control system. Moreover, the list shifting paradigm provides us with potential insight into adaptation mechanisms that work on larger timescales than currently explored; that is, it exceeds the timescale both of Gratton effects (effects of previous trial congruency; Gratton et al., 1992) and of LWPC and ISPC effects.

The experimental design that underlies the asymmetrical list shifting effect necessarily implies that a performance comparison is made between groups (comparing MC-MI and MI-MC) on a task that is not truly identical. One may argue that an alternative strategy would be to have individuals transfer to a PC neutral list (i.e., with 50% congruent items). Although this would make the second phase identical across groups, it would nevertheless leave a similar type of differences between groups (i.e., comparing a MC-neutral and a MI-neutral transition). Moreover, at least for current purposes, we would like to argue that our PC reversal is more convincing in showing the asymmetry, because even with more than 50% congruent trials, participants in the MI-MC group did not readapt to the shift in PC. Indeed, if no asymmetrical list shifting effect would be obtained with transfer to a neutral list, this could merely reflect the weaker signal for adjustment in the transfer phase.

It is clear that this and other choices (such as the above discussed adjustment of the ISPC task to not include within-subject transitions of both MC-MI and MI-MC) were made to optimally comply with our aims in the current study. Those doing future work on this paradigm may want to consider alternative settings. Ultimately, as we have already shown the MC-MI shift to differentially affect performance, the MI-MC shift is most interesting and may be studied without the inclusion of a MC-MI group. For example, researchers may want to explore if with longer test phases the MI-MC shift results in an eventual readaptation to the novel PC (and if this is possibly facilitated by being explicitly informed about it), and, if so, to elucidate the precise neural correlates and time course of the processes underlying this readaptation.

Conclusions

The present investigation has provided new support for attention-based accounts of the LWPC and ISPC effects. In particular, changes in PC are more aptly adapted to when shifting from MC to MI than when shifting in the reverse direction—the asymmetrical list shifting effect. This novel phenomenon cannot be easily explained by a pure contingency account. Moreover, it provides a promising tool both for future investigations of cognitive control and for service as a constraint for existing and future theories. We are curious to see if and how computational implementations of contingency learning (e.g., PEP model; Schmidt, 2013a) and attention modulation (e.g., conflict monitoring model or ABBA; Blais et al., 2007; Botvinick et al., 2001; Verguts & Notebaert, 2008, 2009) can ultimately deal with the asymmetrical list shifting effect and which model may be able to do so most parsimoniously.

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