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Accounting for sequential trial effects in the flanker task: Conflict adaptation or associative priming?

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The conflict–control loop theory proposes that the detection of conflict in information processing triggers an increase in cognitive control, resulting in improved performance on the subsequent trial. This theory seems consistent with the robust finding that conflict susceptibility is reduced following correct trials associated with high conflict: the conflict adaptation effect. However, despite providing favorable conditions for eliciting and detecting conflict-triggered performance adjustments, none of the five experiments reported here provide unequivocal evidence of such adjustments. Instead, the results corroborate and extend earlier findings by demonstrating that the conflict adaptation effect, at least in the flanker task, is only present for a specific subset of trial sequences that is characterized by a response repetition. This pattern of results provides strong evidence that the conflict adaptation effect reflects associative stimulus–response priming instead of conflict-driven adaptations in cognitive control.

Recent research has suggested that the degree of cognitive control over information processing may be dynamically adjusted on the basis of information from mechanisms dedicated to the online monitoring of performance (Ridderinkhof, Ullsperger, Crone, & Nieuwenhuis, 2004). One of the most influential contributions to this research area is the conflict–control loop theory (Botvinick, Braver, Barch, Carter, & Cohen, 2001; Botvinick, Cohen, & Carter, 2004). According to this theory, the anterior cingulate cortex is involved in the online detection of conflicts in information processing. The detection of such conflict acts as a signal for other brain areas, and in particular the prefrontal cortex, to increase cognitive control, thereby improving subsequent task performance. An attractive feature of the conflict–control loop theory is that it posits a plausible solution to the question of how cognitive control processes are themselves controlled.

The conflict–control loop theory has received support from findings indicating that behavioral and neural manifestations of conflict are associated with systematic adjustments in performance on the subsequent trial (e.g., Botvinick et al., 2001; Kerns et al., 2004). An important empirical paradigm in which such findings are obtained is the flanker task (Eriksen & Eriksen, 1974). In an often used version of this task, participants are required to give a speeded left- or right-hand response to a central target arrow while ignoring congruent (e.g., <<<<<) or incongruent (e.g., >><>>) flanker arrows. The efficiency of cognitive control in this task is indexed by the *congruency effect*, the decline in performance on incongruent relative to congruent trials. This effect is caused by the fact that the flanker stimuli, though task irrelevant, often receive a considerable amount of processing (even up to the level of the primary motor cortex; see, e.g., Mattler, 2003), resulting in a processing conflict on incongruent trials.

Gratton and colleagues reported a finding with the flanker task that would appear to support the notion of conflict-sensitive modulations in the level of cognitive control (Gratton, Coles, & Donchin, 1992). They found that the congruency effect was smaller for trials that were directly preceded by a high-conflict (i.e., incongruent)

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trial than for trials that were preceded by a low-conflict (congruent) trial. More specifically, reaction times (RTs) on congruent trials preceded by a congruent trial (CC) were faster than those on congruent trials preceded by an incongruent trial (IC). Similarly, RTs on incongruent trials were faster following incongruent trials (II) than following congruent trials (CI). Similar results were obtained for error rates. This pattern of results is consistent with predictions of the conflict–control loop theory, according to which conflict on trial $n-1$ should trigger an increase in cognitive control, yielding reduced susceptibility to conflict on trial n. As further support for the theory, Botvinick and colleagues identified a region of the anterior cingulate cortex in which activity was directly related to the post-conflict reduction of the congruency effect (Botvinick, Nystrom, Fissell, Carter, & Cohen, 1999).

Because of its apparent relationship with conflict-sensitive control adjustments, the sequential modulation of the congruency effect is often referred to as the *conflict adaptation effect*. The size of the effect can be computed as follows:

$$\text{Conflict adaptation effect} = (RT_{CI} - RT_{CC}) - (RT_{II} - RT_{IC}). \quad (1)$$

A similar equation describes the conflict adaptation effect on error rates. Not surprisingly, this simple measure has come to be a popular index of the integrity and/or efficiency of cognitive control processes—useful for studying group differences in cognitive control (e.g., Bish, Ferrante, McDonald-McGinn, Zackai, & Simon, 2005; de Bruijn, Hulstijn, Verkes, Ruigt, & Sabbe, 2004; McNeely, West, Christensen, & Alain, 2003) or investigating the neural bases of conflict detection and cognitive control (Botvinick et al., 1999; Stürmer, Leuthold, Soetens, Schröter, & Sommer, 2002).

However, Mayr, Awh, and Laurey (2003) have recently challenged the conflict–control loop account of the conflict adaptation effect, arguing that the effect is confounded with the presence of stimulus–response (S–R) repetitions. This can be seen in Table 1, which lists the eight possible stimulus transitions in the flanker task that are defined by the factorial combination of levels of the variables current trial type (congruent vs. incongruent), previous trial type (congruent vs. incongruent), and response type (response change vs. response repetition). Table 1 indicates that S–R repetitions occur on 50% of the CC and II trials but on none of the IC or CI trials. As Mayr et al. pointed out, this pattern is critical in light of the substantial performance benefits that are obtained with exact S–R repetitions (Pashler & Baylis, 1991). As indicated by Equation 1, decreased RTs on CC and II trials resulting from S–R repetition priming may account for an increased conflict adaptation effect. An additional confound reflects the fact that 50% of the IC and CI trials but none of the CC and II trials involve a response repetition in the absence of a stimulus repetition. According to feature integration theory (Hommel, 2004), such *partial repetitions* should be associated with elevated RTs in comparison with trials on which neither the stimulus nor the response is repeated. Feature integration theory proposes that if a stimulus and response co-occur in time, their features spontaneously form a transient association, such that reactivating one member of this association tends to activate the other member (explaining S–R repetition benefits). Partial repetitions are associated with slowed responses because the repeated member of the recently formed association activates the member that is inappropriate in the current context, resulting in a processing conflict. The resulting increase in RTs for IC and CI trials would further contribute to the conflict adaptation effect (see Equation 1; Hommel, Proctor, & Vu, 2004; Notebaert, Soetens, & Melis, 2001).

These considerations suggest that the conflict adaptation effect may reflect, at least in part, the consequences of associative priming rather than of a conflict-triggered increase in cognitive control. Mayr et al. (2003) reported two experiments that provided strong initial evidence for this possibility. In one experiment, they analyzed the conflict adaptation effect separately for response repetition trials (including all S–R repetitions and partial repetitions) and response change trials (see Table 1). They found that the effect was present for the repetition trials but entirely absent for the change trials. In a second experiment, they employed a modified version of the flanker task in which response repetition trials were eliminated altogether, allowing an unconfounded measure of the conflict adaptation effect. Again, the congruency effect did not change as a function of conflict on trial $n-1$. These and other findings led Mayr et al. to conclude that conflict-triggered regulation of control is not necessary to explain the conflict adaptation effect, thereby challenging a key assumption of the conflict–control loop theory.

In the present research, we evaluated the validity of this conclusion: Although the results of Mayr et al. (2003) provide a compelling demonstration of the importance of associative priming for sequential modulations of the congruency effect, we were less convinced by their theory that, in general, cognitive control does not contribute to the conflict adaptation effect. In particular, we hypothesized that the flanker task version used by Mayr et al. may have elicited relatively low levels of processing conflict. For example, Mayr et al. used task instructions that placed an important emphasis on accuracy of responding ("Go as fast as you can while not letting accuracy drop below 95%"; U. Mayr, personal communication, March 22, 2005); these instructions encouraged tight focusing on the central target arrow, and hence reduced processing conflict. Their relatively low error rates in comparison with other flanker task studies (e.g., Gratton et al., 1992) are consistent with this observation. Therefore, the failure to detect a "true" conflict adaptation effect might be attributed to the weakness of the conflict signals, resulting in a reduced mobilization of cognitive control processes. Furthermore, there are clear indications that conflict-sensitive control adjustments contribute significantly to the conflict adaptation effects observed in other interference tasks, such as the Stroop and Simon tasks (e.g., Kerns et al., 2004; Stürmer et al., 2002). It is not obvious why these tasks should differ in this regard from the flanker task. These considerations raise the possibility that the extreme pattern of results obtained by Mayr et al., suggesting that the conflict adaptation effect reflects associative priming *exclusively*, might be observed only under a limited set of circumstances.

The goal of the present research was to test the robustness and generality of the findings reported by Mayr et al. (2003). In Experiments 1–3, we investigated whether increasing flanker-induced conflict on incongruent trials would reveal evidence of conflict-sensitive modulations in the efficiency of cognitive control. In Experiment 4, we examined whether the findings of Mayr and colleagues would generalize to the frequently used letter version of the flanker task. And as a final test, Experiment 5 involved a reanalysis of previously published flanker task data from 892 community-dwelling individuals from various age groups. In each of the five experiments, we investigated the conflict adaptation effect separately for response change and response repetition trials. Any evidence for a conflict adaptation effect on response change trials would provide support for the predictions of the conflict–control loop theory. In contrast, if the conflict adaptation effect were present only for response repetition and not for response change trials, this would provide further evidence for a pure associative priming account of the conflict adaptation effect (Mayr et al., 2003).

EXPERIMENT 1

In this experiment, participants performed an arrow version of the flanker task very similar to the one used by Mayr et al. (2003). The replicated task parameters included the employed stimuli, stimulus duration, intertrial interval, and task instruction. One essential aspect of the task was changed, however: To induce more processing conflict, we presented the flanker arrows 100 msec before the central target arrow (see, e.g., Mattler, 2003; Wascher, Reinhard, Wauschkuhn, & Verleger, 1999). As a result of this manipulation, the flankers became more salient and received a head start in activating the (possibly incorrect) response (Kopp, Rist, & Mattler, 1996), leading to increased conflict on incongruent trials, and thus reinforcing the need for cognitive control.

Method

Participants. Fourteen students (9 women) from the Vrije Universiteit Amsterdam were paid for their participation. They ranged in age from 17 to 28 years ($M = 20.1$) and had normal or corrected-to-normal visual acuity. All participants in this and subsequent experiments took part in only one experiment.

Stimuli, Design, and Procedure. Stimuli were presented in white against a black background on a computer screen placed at a distance of 75 cm from the participant. Each stimulus array subtended a visual angle of 6.1° × 1.0° and consisted of seven horizontally arranged arrows: <<<<<<<, >>>>>>>, <<<><<<, or >>><>>>. The participants were instructed to respond to the central target arrow by pressing a spatially compatible key on the computer keyboard (“z” or “/”) with their left or right index finger, respectively. They were told to respond as quickly as possible while avoiding errors. A distinction was made between congruent (i.e., target arrow and flankers associated with the same response; e.g., <<<<<<<) and incongruent (i.e., target arrow and flankers associated with different responses; e.g., <<<><<<) stimulus arrays. In this experiment, the flanker arrows were presented 100 msec before the target arrow. The entire stimulus array remained on the screen until the participant’s response was registered. Following a 1,000-msec blank screen, the next trial started.

The participants received 40 practice trials before entering the experimental phase, which consisted of 16 blocks of 40 trials each. Each block contained 10 trials with each of the four possible stimulus arrays. Presentation order of the stimulus arrays was randomized. All instructions were automated and presented on the screen. Apart from the final payments, there were no further interactions with the experimenter, who was a lab assistant naive to the main purpose of the experiments.

Data analysis. We classified each trial *n* according to its congruency, the congruency of the preceding trial $n-1$, and whether the trial dyad involved the same (left/right) response or different responses (Mayr et al., 2003; see also Table 1). The individual mean RTs and error rates for the resulting eight categories were submitted to repeated measures ANOVAs with the variables current trial type (congruent vs. incongruent), previous trial type (congruent vs. incongruent), and response type (response repetition vs. response

Table 1
Stimulus Transitions in the Flanker Task

Stimulus Array		Trial Type		Repetition (+) vs. Change (−)		
Trial $n-1$	Trial *n*	$n-1$	*n*	Congruency	Stimulus	Response
>>>>>	>>>>>	C	C	+	+	+
>>>>>	<<<<<	C	C	+	−	−
>>>>>	<<><<	C	I	−	−	+
>>>>>	>><>>	C	I	−	−	−
<<><<	>>>>>	I	C	−	−	+
<<><<	<<<<<	I	C	−	−	−
<<><<	<<><<	I	I	+	+	+
<<><<	>><>>	I	I	+	−	−

Note—There are 16 different stimulus transitions in the flanker task. Eight are shown here; the remaining eight are their mirror images. C and I refer to the trial type (congruent or incongruent) of the previous ($n-1$) and the current (*n*) trial. Repetitions (+) and changes (−) of congruency, stimulus, and required response are indicated. Note that 50% of the CC and II trials involve an exact stimulus–response repetition (typically associated with faster responding) and that 50% of the CI and IC trials involve a partial repetition (i.e., a response repetition in the absence of a stimulus repetition, typically associated with slower responding).

change). Conflict adaptation effects were expressed in a significant interaction of current and previous trial types. The three-way interaction was assessed to determine whether the conflict adaptation effect was different for response repetitions and response changes. If the three-way interaction effect was significant, we examined whether the conflict adaptation effect was reliably present at each level of response type by computing simple interaction effects of current and previous trial type, separately for response repetitions and changes. The Results section for each of the reported experiments focuses on these interaction effects; the test statistics associated with the three main effects and the remaining interaction effects are summarized in Table 2. Error trials and the trials following errors were excluded from the analyses.

Results and Discussion

Mean RTs and error rates for each combination of current and previous trial type are shown in Figure 1, separately for response change trials (left) and response repetition trials (right). For response repetition trials, the RT data and error rates show a clear reduction of the congruency effect following incongruent trials. This conflict adaptation effect is not evident for response change trials.

The statistical analyses revealed a significant interaction of current and previous trial type in both the RT [$F(1,13) = 15.1$, $MS_e = 282.7$, $p = .002$] and the error [$F(1,13) = 24.0$, $MS_e = .1$, $p < .001$] data, indicating an overall conflict adaptation effect. The three-way interaction was also significant in the RT [$F(1,13) = 8.1$, $MS_e = 739.3$, $p = .014$] and the error [$F(1,13) = 9.8$, $MS_e = .1$, $p < .01$] data, indicating that the conflict adaptation effect was significantly larger for response repetition trials. Follow-up analyses revealed that the conflict adaptation effect was reliably present for the response repetition trials in both the RT [$F(1,13) = 24.2$, $MS_e = 420.4$, $p < .001$] and error [$F(1,13) = 28.7$, $MS_e = .1$, $p < .001$] data. In contrast, there was no reliable conflict adaptation effect for response changes in the RT [$F(1,13) = 0.1$, $MS_e = 601.6$, $p = .74$] or in the error [$F(1,13) = 0.8$, $MS_e = .1$, $p = .39$] data.

The pattern of results in Experiment 1 replicated that of Mayr et al. (2003): Although the introduction of a delay between flanker and target presentation presumably increased the level of experienced conflict (Mattler, 2003; Wascher et al., 1999), the conflict adaptation pattern was present only for response repetition trials. The absence of the effect for response change trials indicates that conflict-induced sequential modulations in cognitive control did not contribute to the overall conflict adaptation effect. Instead, the results are consistent with a pure associative priming account of the conflict adaptation effect. However, it is of course possible that the level of induced conflict in Experiment 1 was still insufficient to elicit detectable modulations in cognitive control. Therefore, in Experiment 2, we employed a different, often used, and powerful manipulation for increasing processing conflict.

EXPERIMENT 2

In Experiment 2, we promoted an increase in processing conflict through the use of instructions and end-of-block feedback that emphasized speed over accuracy of responding. Thus, participants were encouraged to respond on the basis of an incomplete analysis of the stimulus array, which increased the probability of activation of the incorrect response representation on incongruent trials (see, e.g., Gratton, Coles, Sirevaag, Eriksen, & Donchin, 1988). According to the conflict–control loop theory, the resulting increase in conflict should be associated with clear performance adjustments following incongruent trials.

Method

Details of the method were the same as in Experiment 1, except as noted below.

Participants. Fourteen students (3 women) from the Vrije Universiteit Amsterdam were paid for their participation. They ranged in age from 17 to 29 years ($M = 20.2$) and had normal or corrected-to-normal visual acuity.

Stimuli, Design, and Procedure. In Experiment 2 and subsequent experiments, each stimulus array consisted of five arrows (i.e., a target with two flankers on either side) that all appeared on the screen at the same time. Each trial consisted of the presentation of the stimulus array for 100 msec, followed by a 1,400-msec blank

Table 2
Summary of *F* Values in Experiments 1–5

	E1 (1,13)	E2 (1,13)	E3 (1,13)	E4 (1,12)	E5, Children (1,290)	E5, Young Adults (1,313)	E5, Older Adults (1,267)
				Reaction Times			
Response type	0.1	1.3	6.3^{*}	0.6	95.6^{***}	3.1	5.2^{*}
Current trial type	153.1^{***}	316.6^{***}	90.8^{***}	5.0^{*}	$1,295.0^{***}$	$4,002.0^{***}$	$2,310.6^{***}$
Previous trial type	0.0	10.8^{**}	0.2	0.0	0.1	0.1	11.3^{**}
Response × current	11.1^{**}	3.7	0.0	0.2	30.1^{***}	35.9^{***}	16.2^{***}
Response × previous	7.8^{*}	1.9	1.5	0.4	42.8^{***}	243.2^{***}	139.9^{***}
				Error Rates			
Response type	7.0^{*}	7.2^{*}	34.3^{***}	12.0^{**}	44.8^{***}	6.6^{*}	7.0^{**}
Current trial type	21.9^{***}	174.5^{***}	155.0^{***}	69.0^{***}	182.6^{***}	286.2^{***}	278.8^{***}
Previous trial type	36.3^{***}	43.6^{***}	8.9^{*}	4.4	40.5^{***}	55.5^{***}	85.3^{***}
Response × current	19.7^{**}	36.2^{***}	27.6^{***}	6.2^{*}	61.6^{***}	2.9	3.3
Response × previous	7.5^{*}	21.1^{**}	21.3^{***}	0.3	39.5^{***}	119.6^{***}	103.7^{***}

Note—Numbers in parentheses represent degrees of freedom. See the text for additional statistical results. $^{*}p < .05$. $^{**}p < .01$. $^{***}p < .001$.

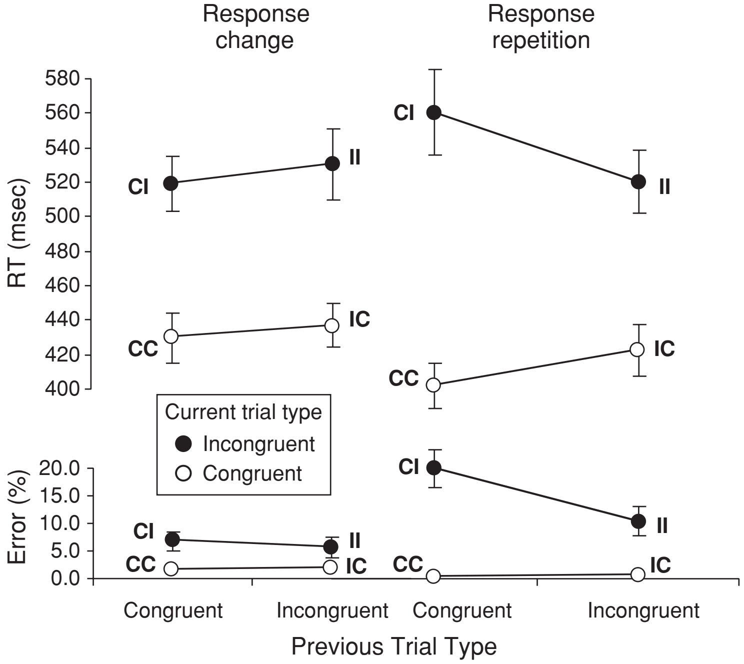

Figure 1. Mean reaction times (RTs) and error rates in Experiment 1 for each combination of current trial type and previous trial type, presented separately for response change trials (left) and response repetition trials (right). Error bars reflect standard errors of the mean.

screen. Instructions in advance of the experiment emphasized the need to respond "as quickly as possible," with no mention of the need to respond accurately. The participants were reminded to pay close attention to additional instructions provided at the end of each block and to follow these instructions in the subsequent block. The purpose of these instructions was to maintain the participants at an accuracy level of 80%–85%. Thus, if the error percentage for a particular block was below 15%, the end-of-block instruction was: "You didn't respond fast enough. Try to respond faster in the next block." If the error percentage was larger than 20%, the instruction was: "You made too many errors. Try to make fewer errors. But also try to keep responding as fast as possible."

Results and Discussion

Mean RTs and error rates for each combination of current and previous trial type are shown in Figure 2, separately for response change trials (left) and response repetition trials (right). The pattern of results is similar to that in Experiment 1: There is a conflict adaptation effect for the response repetition trials but not for the response change trials.

The statistical analyses revealed a significant interaction effect of current and previous trial type in the RT data [$F(1,13) = 10.9, MS_e = 161.0, p = .006$] and the error data [$F(1,13) = 55.4, MS_e = .2, p < .001$], indicating an overall conflict adaptation effect. The three-way interaction was also significant in the RT [$F(1,13) = 24.1, MS_e = 161.2, p < .001$] and the error [$F(1,13) = 17.6, MS_e = .3, p = .001$] data, indicating that the conflict adaptation effect was significantly larger for response repetition trials. Follow-up analyses revealed that the conflict adaptation effect was reliably present for response repetition trials in both the RT [$F(1,13) = 22.3, MS_e = 243.7, p < .001$] and the error [$F(1,13) = 43.6, MS_e = .4, p < .001$] data. In contrast, there was no reliable conflict adaptation effect for response changes, neither in the RT data [$F(1,13) = 2.7, MS_e = 78.6, p = .13$] nor the error data [$F(1,13) = 4.4, MS_e = .2, p = .056$].

In this experiment, participants responded under time pressure, as induced by instructions and end-of-block feedback, and as evidenced by fast RTs and high error rates. Although speeded responding is associated with high processing conflict, the results revealed no evidence for conflict-sensitive modulations of cognitive control: The conflict adaptation effect was only present for response repetition trials, consistent with the pure associative priming account.

EXPERIMENT 3

In Experiment 3, we used a financial bonus system in combination with trial-by-trial feedback to motivate participants to the fullest to respond quickly, while at the same time encouraging them to adjust their performance following errors and high-conflict trials. Specifically, following each response that was correct and within a deadline (determined separately for each individual and dynamically adjusted during the experiment), participants received positive feedback, indicating a small financial reward. All other responses were followed by negative feedback, signaling the absence of reward. Thus, negative

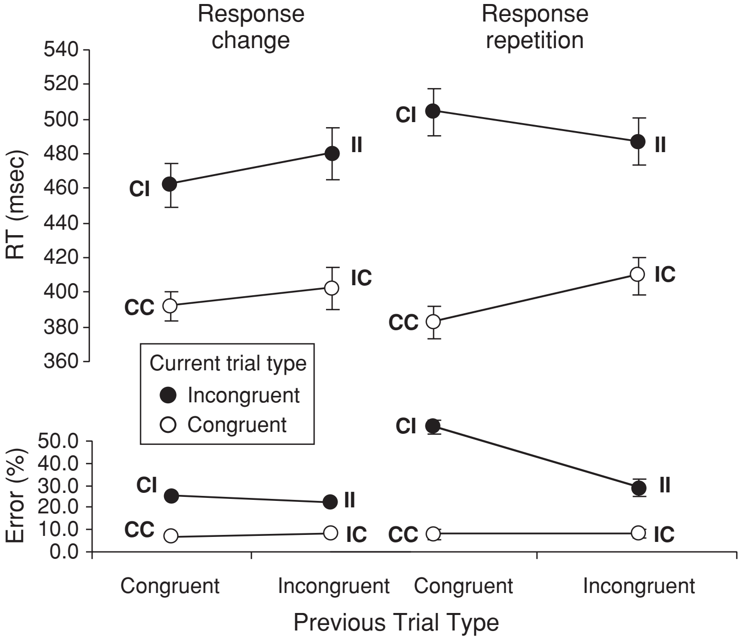

Figure 2. Mean reaction times (RTs) and error rates in Experiment 2 for each combination of current trial type and previous trial type, presented separately for response change trials (left) and response repetition trials (right). Error bars reflect standard errors of the mean.

feedback was provided following errors but also following "late" responses, which presumably would occur more often on incongruent (i.e., high-conflict) trials. As such, the feedback explicitly indicated the need to adjust performance specifically following incongruent trials. We reasoned that if participants are at all capable of dynamically adjusting their susceptibility to the flankers, the conditions in Experiment 3 should reveal a conflict adaptation effect for response change trials.

Method

Details of the method were the same as in Experiment 2, except as noted below.

Participants. Fourteen students (9 women) from the Vrije Universiteit Amsterdam participated in the experiment. They were paid a basic salary, plus a performance-related incentive bonus, as described below. The participants ranged in age from 18 to 32 years ($M = 21.7$) and had normal or corrected-to-normal visual acuity.

Stimuli, Design, and Procedure. The participants were informed that on each trial they had "only a fraction of a second to respond," that they would earn a 1-eurocent bonus for each response that was correct and on time, and that they would not earn money for incorrect or late responses. The distinction between responses that were "on time" and "too late" was determined by a deadline that was adjusted on a trial-by-trial basis, with the aim of maintaining the percentage of "too late" trials at 20%. The deadline was initialized at 400 msec and then regulated using a staircase tracking algorithm: +40 msec for each "too late" response and −10 msec for each "on time" response. In the practice block, each trial started with the presentation of the stimulus array for 100 msec, followed by a 900-msec blank screen. Subsequently, a feedback stimulus was displayed for 1,000 msec: "Correct!," "Too late!," or "Error!" Following a 1,000-msec blank screen, the next trial started. During the experimental phase, the written feedback stimulus was replaced by a colored fixation dot that was presented for 1,000 msec and immediately followed by the next trial. A green fixation dot indicated that the response was correct and on time, a red fixation dot that it was incorrect or too late. The fixation dot subtended 0.7º and was presented 0.6º below the location at which the central target arrow was presented. After each block, detailed feedback was provided on the screen, indicating the number of correct, incorrect, and too-late trials, along with the bonus money won in the preceding block and the total amount of bonus money won so far.

Results and Discussion

Mean RTs and error rates for each combination of current and previous trial type are shown in Figure 3, separately for response change trials (left) and response repetition trials (right). As in the previous two experiments, the congruency effect is reduced following incongruent trials, but only for the response repetition trials. In Experiment 3, this pattern is most pronounced for the error data, presumably because the emphasis on response speed decreased the variance in the RT data but increased variance in the error data.

The statistical analyses revealed a significant interaction effect of current and previous trial type in the error data [$F(1,13) = 8.8$, $MS_e = .4$, $p = .01$] but not in the RT data [$F(1,13) = 3.0$, $MS_e = 550.6$, $p = .11$], although the RT data did show a weak trend in the expected direction. The error data showed a significant three-way interaction [$F(1,13) = 23.2$, $MS_e = .2$, $p < .001$], indicating that the conflict adaptation effect was significantly larger for response repetitions than for response changes. Follow-up analyses of simple interaction effects on error rates revealed that the interaction of current and previous trial

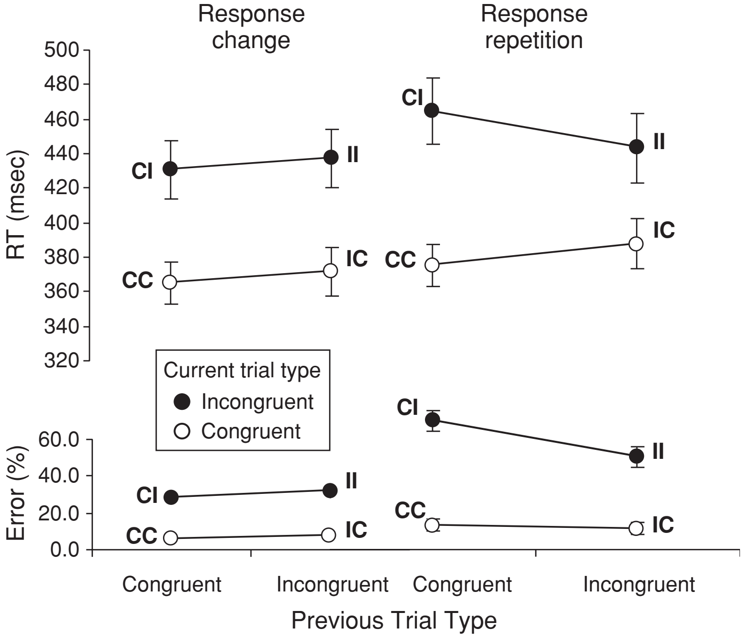

Figure 3. Mean reaction times (RTs) and error rates in Experiment 3 for each combination of current trial type and previous trial type, presented separately for response change trials (left) and response repetition trials (right). Error bars reflect standard errors of the mean.

type was significant for response repetitions [$F(1,13) = 21.3$, $MS_e = .4$, $p < .001$] but not for response changes [$F(1,13) = 0.4$, $MS_e = .2$, $p = .53$]. The three-way interaction effect for the RT data was not significant [$F(1,13) = 3.3$, $MS_e = 474.4$, $p = .09$].

As in the previous two experiments, the results indicate a clear conflict adaptation effect, but only for the response repetition trials, consistent with Mayr et al. (2003). This effect was most pronounced in the error data, presumably because in Experiment 3 participants were operating at a point of the speed–accuracy trade-off at which small changes in RT were associated with large changes in response accuracy. Together, the results from Experiments 1–3 provide strong converging evidence for the pure associative priming account of the conflict adaptation effect (Mayr et al., 2003). Indeed, despite providing favorable conditions for eliciting conflict-triggered sequential modulations in the degree of cognitive control, none of the three experiments provided any evidence of such modulations.

EXPERIMENT 4

As noted in the introduction, previous research using the Stroop task and the Simon task has found conflict adaptation effects even after controlling for the effects of stimulus repetitions (e.g., Stürmer et al., 2002). This discrepancy with the pattern of results observed with the arrow version of the flanker task (see Experiments 1–3; Mayr et al., 2003) is discussed further in the General Discussion. In Experiment 4, we examined whether our findings would extend to a different version of the flanker task: the letter version that was used in the original demonstration of the conflict adaptation effect (Gratton et al., 1992).

Method

Details of the method were the same as in Experiment 2, except as noted below.

Participants. Thirteen students (6 women) from the Vrije Universiteit Amsterdam were paid for their participation. They ranged in age from 19 to 30 years ($M = 23.5$) and had normal or corrected-to-normal visual acuity.

Stimuli, Design, and Procedure. Each stimulus array consisted of five horizontally arranged letters displayed in Arial font: HHHHH, SSSSS, HHSHH, or SSHSS. The participants were instructed to respond "as quickly and accurately as possible" to the central target letter and to try to ignore the four flankers. The H was mapped to the left and the S to the right response key. Each trial started with a 500-msec fixation cross, followed by the presentation of the stimulus array for 100 msec and a blank screen until the response was registered. The response was followed by another 500-msec blank screen, after which the next trial started. Each participant completed eight blocks of 80 trials each. After each block, the participants received verbal feedback encouraging them to speed up or slow down their responding, with the aim of maintaining accuracy between 80% and 85% correct.

Results and Discussion

Mean RTs and error rates for each combination of current and previous trial type are shown in Figure 4, separately for response change trials (left) and response repetition trials (right). Note that for response repetitions, the congruency effect was essentially absent following incongruent trials. Again, the response change trials did not show this pattern.

The statistical analyses revealed a pattern similar to the one in Experiment 3: In the RT data, no significant overall conflict adaptation effect occurred [$F(1,12) = 1.3$, $MS_e = 1{,}257.5$, $p = .27$], nor a significant three-way interaction [$F(1,12) = 1.1$, $MS_e = 3{,}678.2$, $p = .33$]. However, the error data showed the typical pattern observed in all of the previous experiments: a significant interaction effect of current and previous trial type [$F(1,12) = 19.8$, $MS_e = .2$, $p = .001$] and a significant three-way interaction [$F(1,12) = 8.0$, $MS_e = .2$, $p < .02$], indicating a larger conflict adaptation effect for the response repetition trials. Follow-up analyses revealed that the conflict adaptation pattern was reliably present for the response repetitions [$F(1,12) = 25.8$, $MS_e = .2$, $p < .001$] but not for the response changes [$F(1,12) = 0.5$, $MS_e = .2$, $p = .47$].

The pattern of results in Experiment 4 was the same as in the previous three experiments: The conflict adaptation effect was only present for response repetition trials. Thus, the two most frequently used versions of the flanker task (the arrow version and the letter version) suggest the same conclusion, namely that the conflict adaptation effect in the flanker task is entirely due to the effects of associative S–R priming.

EXPERIMENT 5

As a final test of the robustness of our findings, in Experiment 5 we examined the conflict adaptation effect in a group of 892 community-dwelling individuals from different age groups: children (~12 years old), young adults (~25 years old), and older adults (~50 years old). For this purpose, we reanalyzed two previously published data sets (see Stins, Polderman, Boomsma, & de Geus, 2005, for the children, and Posthuma, Mulder, Boomsma, & de Geus, 2002, for the young and older adults). These analyses allowed us to verify whether the findings of Experiments 1–4, obtained in young adults with above-average intelligence, would extend to different age groups from the general population. Importantly, the large number of participants in Experiment 5 yielded sufficient statistical power to detect even the smallest effects hinting at the contribution of cognitive control to the conflict adaptation effect.

Method

Participants. A total of 892 participants were recruited from the Netherlands Twin Register (Boomsma, 1998) as part of an ongoing project on the genetics of cognition and brain function. They were either monozygotic twins, dizygotic twins, or their siblings. All participants were part of one of three distinct age groups: children ($n = 291$, mean age = 12.4 years, $SD = 0.9$ years, 154 girls), young adults ($n = 332$, mean age = 25.6, $SD = 3.6$, 174 women), and older adults ($n = 269$, mean age = 48.8, $SD = 6.3$, 151 women). The flanker task (arrow version) was embedded in a battery of neuropsychological tasks (for details, see Posthuma et al., 2002; Stins et al., 2005). The study was approved by the local ethics committee. All participants gave their written informed consent prior to inclusion in the study. Written informed consent was also obtained from the parents or legal representatives of the child participants.

Stimuli, Design, and Procedure. The procedural details differed for the child and adult participants. The children received a total of 80 trials (40 congruent and 40 incongruent). Each trial started with a fixation cross presented for 500 msec, followed by the flanker stimulus for 800 msec. The stimulus was followed by a 2,000-msec blank screen, after which the next trial started. The adult participants

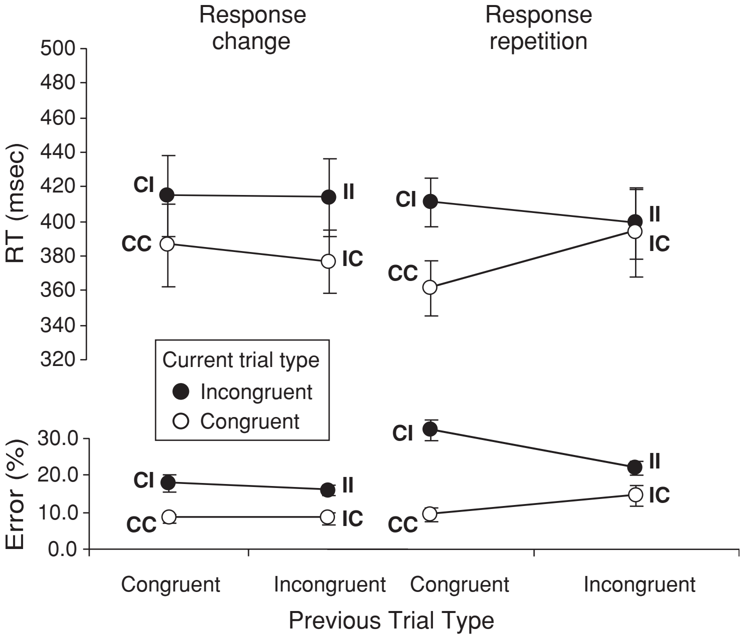

Figure 4. Mean reaction times (RTs) and error rates in Experiment 4 for each combination of current trial type and previous trial type, presented separately for response change trials (left) and response repetition trials (right). Error bars reflect standard errors of the mean.

received a total of 120 trials (60 congruent and 60 incongruent). Each trial started with the presentation of a fixation dot paired with a 100-msec warning tone. After 1,000 msec, the fixation dot was replaced by a flanker stimulus. The stimulus stayed on the screen for 100 msec and was followed by a 900-msec blank screen. Finally, the participants received a 1,500-msec feedback signal, indicating whether they had given the correct response. The instruction to participants in all three age groups was to "respond as fast and accurately as possible" and to ignore the flanking arrows.

Results and Discussion

Mean RTs and error rates for each combination of current and previous trial type are shown in Figure 5, separately for each age group and for response change trials (left columns) and response repetition trials (right columns). All three age groups showed the same pattern of results obtained in Experiments 1–4.

Children. The statistical analyses revealed a significant interaction of current and previous trial type in the RT [$F(1,290) = 100.8$, $MS_e = 2{,}334.7$, $p < .001$] and the error [$F(1,290) = 34.0$, $MS_e = 40.8$, $p < .001$] data, indicating an overall conflict adaptation effect. The three-way interaction was also significant in both the RT [$F(1,290) = 191.0$, $MS_e = 1{,}550.8$, $p < .001$] and the error [$F(1,290) = 51.6$, $MS_e = 34.4$, $p < .001$] data, indicating that the conflict adaptation effect was significantly larger for response repetition trials. Follow-up analyses revealed that the conflict adaptation effect was reliably present for response repetition trials in both the RT [$F(1,290) = 242.2$, $MS_e = 2{,}186.4$, $p < .001$] and the error [$F(1,290) = 63.0$, $MS_e = 50.0$, $p < .001$] data. In contrast, there was no reliable conflict adaptation effect for response changes, neither in the RT data [$F(1,290) = 1.0$, $MS_e = 1{,}699.0$, $p = .31$] nor the error data [$F(1,290) = 0.5$, $MS_e = 25.2$, $p = .49$].

Young adults. The statistical analyses revealed a significant interaction of current and previous trial type in the RT [$F(1,331) = 333.4$, $MS_e = 373.0$, $p < .001$] and the error [$F(1,331) = 60.3$, $MS_e = 39.8$, $p < .001$] data, indicating an overall conflict adaptation effect. The three-way interaction was also significant in both the RT [$F(1,331) = 278.3$, $MS_e = 333.8$, $p < .001$] and the error [$F(1,331) = 97.3$, $MS_e = 43.4$, $p < .001$] data, indicating that the conflict adaptation effect was significantly larger for response repetition trials. Follow-up analyses revealed that the conflict adaptation effect was reliably present for response repetition trials in both the RT [$F(1,331) = 521.6$, $MS_e = 413.6$, $p < .001$] and the error [$F(1,331) = 136.1$, $MS_e = 47.8$, $p < .001$] data. In contrast, there was no reliable conflict adaptation effect for response changes, neither in the RT data [$F(1,331) = 3.8$, $MS_e = 293.2$, $p = .052$] nor the error data [$F(1,331) = 3.6$, $MS_e = 35.5$, $p = .058$].

Older adults. The statistical analyses revealed a significant interaction of current and previous trial type in the RT [$F(1,268) = 161.0$, $MS_e = 492.1$, $p < .001$] and the error [$F(1,268) = 86.4$, $MS_e = 78.5$, $p < .001$] data, indicating an overall conflict adaptation effect. The three-way interaction was also significant in both the RT [$F(1,268) = 139.2$, $MS_e = 536.6$, $p < .001$] and the error [$F(1,331) = 107.1$, $MS_e = 68.5$, $p < .001$] data, indicating that the conflict adaptation effect was significantly larger for response repetition trials. Follow-up analyses revealed that the conflict adaptation effect was reliably present for response repetition trials in both the RT [$F(1,268) = 240.4$, $MS_e = 640.2$, $p < .001$] and the error [$F(1,331) = 185.6$, $MS_e = 76.1$, $p < .001$] data. In contrast, there was no reliable conflict adaptation effect for response changes, neither in the RT data [$F(1,331) = 0.1$, $MS_e = 71.0$, $p = .78$] nor the error data [$F(1,331) = 0.1$, $MS_e = 71.0$, $p = .78$].

The data are straightforward and confirm the results reported in Experiments 1–4. All three age groups showed a pronounced conflict adaptation effect for the response repetition trials but no such effect for the response change trials. These results are consistent with the pure associative priming account of the conflict adaptation effect in the flanker task.

GENERAL DISCUSSION

Sequential modulations in the size of the flanker congruency effect have been presented as key support for the conflict–control loop theory, according to which such modulations reflect conflict-triggered adjustments in the degree of exerted cognitive control. However, the research reported here provides strong evidence against this account by showing that the conflict adaptation effect is only present on a subset of trials that is confounded with a specific pattern of stimulus and response repetitions. The presence of the conflict adaptation effect on this subset of trials can be explained entirely in terms of associative priming between stimuli and responses. Although the conflict–control loop theory predicts a conflict adaptation effect also for the remaining trials (i.e., those involving a response change), none of the reported experiments supported this prediction, even though in general these experiments provided favorable conditions for detecting such effects. Thus, our results replicate those reported by Mayr et al. (2003; see also Leuthold, 2004), who were the first to challenge the notion that the conflict adaptation effect reflects conflict-triggered sequential control adjustments. Furthermore, our research demonstrates that (1) Mayr et al.'s results extend to different versions of the flanker task, including versions characterized by high processing conflict (and therefore versions promoting control adjustments), and (2) the results generalize to participants in different age groups drawn from the general population.

The present results allow us to evaluate the relative contributions to the conflict adaptation effect of performance improvements on exact S–R repetition trials (Hommel et al., 2004; Mayr et al., 2003; Pashler & Baylis, 1991) and performance impairments on partial repetition trials (i.e., those involving repetition of the response but not of the stimulus; Hommel et al., 2004; Notebaert et al., 2001), a possible contribution that was overlooked by Mayr et al. To this end, we averaged the data from all 947 participants in Experiments 1–5, and for each type of stimulus transition (CI, II, CC, and IC) compared performance on response repetition and response change trials. As illustrated in Figure 6, exact S–R repetitions resulted in improved performance for II trials (17-msec benefit) but not for CC trials (−4 msec; total

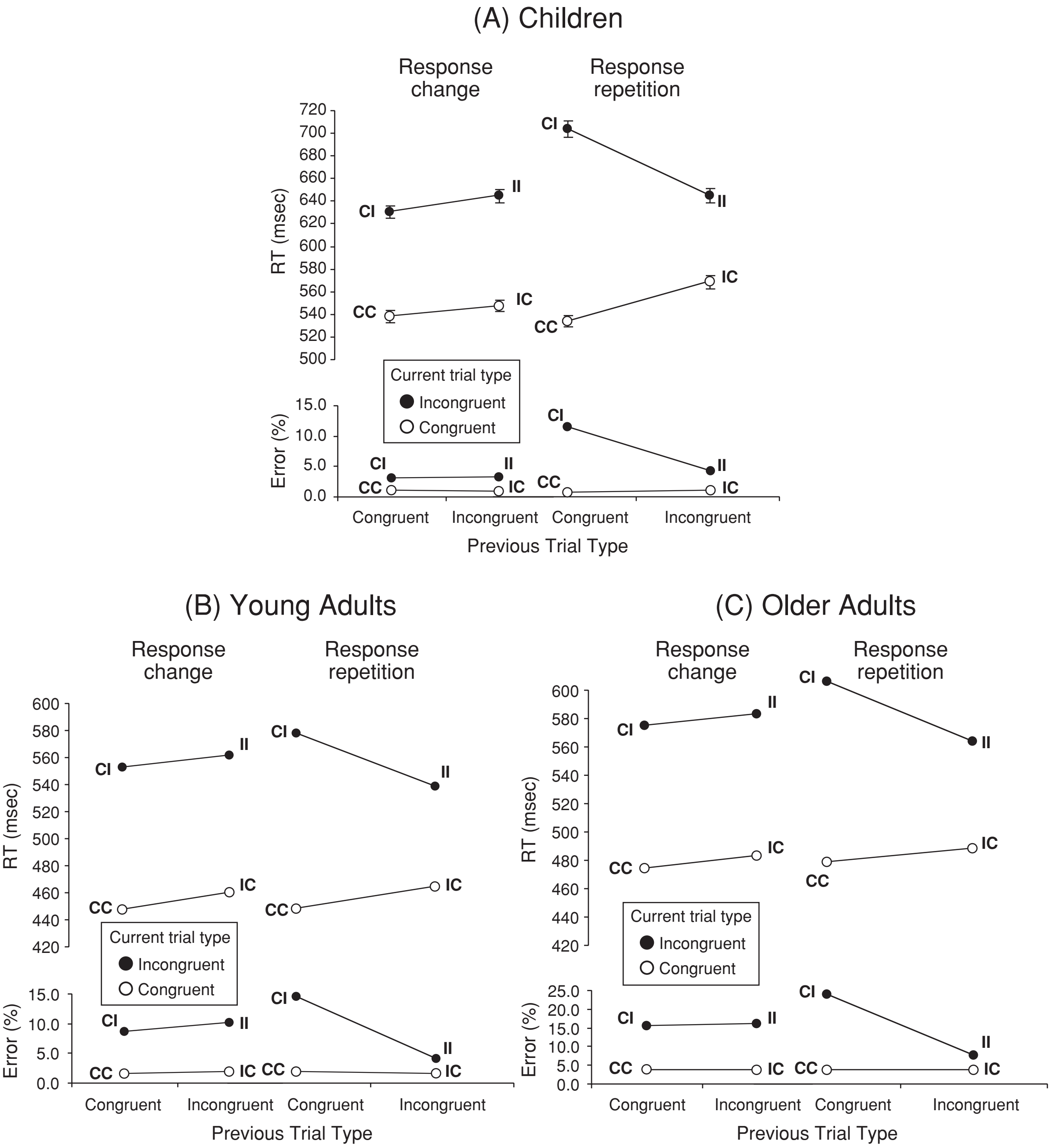

Figure 5. Mean reaction times (RTs) and error rates in Experiment 5 for each combination of current trial type and previous trial type, presented separately for response change trials (left columns) and response repetition trials (right columns). (A) Children ($n = 291$, mean age = 12.4 years). (B) Young adults ($n = 332$, mean age = 25.6 years). (C) Older adults ($n = 269$, mean age = 48.8 years). Standard errors were extremely small and are not indicated.

benefit = 13 msec). In contrast, performance impairments were evident for partial repetitions on both CI (35-msec cost) and IC (13 msec; total cost = 48 msec) trials. Statistical analyses revealed significant effects ($p < .005$, $df = 946$) for all pairwise comparisons, except for the comparisons involving CC and IC trials in the error data, which were nonsignificant ($p > .2$), possibly as a result of a floor effect. Thus, both factors mentioned above contribute to the conflict adaptation effect, but the largest contribution in the present data is an impairment in performance on partial repetition trials.[1]

The present results conflict with a recent report by Ullsperger and colleagues, who found a small but significant conflict adaptation effect in the flanker task, even after eliminating all response repetition trials (Ullsperger, Bylsma, & Botvinick, 2005). They argued that the dis-

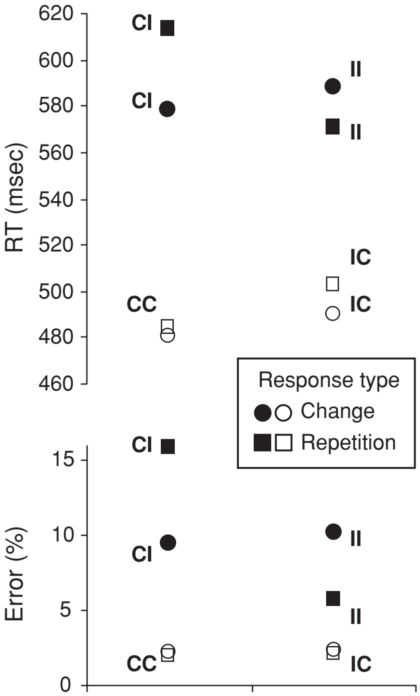

Figure 6. Mean reaction times (RTs) and error rates across all participants tested in Experiments 1–5 as a function of stimulus transition type (CI, II, CC, or IC) and response type (response change or response repetition). Open and filled symbols indicate congruent and incongruent current trials, respectively. The two columns mark the two levels of previous trial type, as in Figures 1–5.

crepancy between their findings and those of Mayr et al. (2003) might be attributed to various aspects of their task, including the use of a long intertrial interval, a short stimulus duration, and an instruction that emphasized speeded responding. However, the present study renders this explanation unlikely: The same pattern of results was observed across various intertrial intervals (e.g., ~1,500 msec in Experiment 4 vs. 3,500 msec in Experiment 5), various stimulus durations (e.g., 100 msec in Experiment 2 vs. 800 msec in Experiment 5), and different emphases on speeded responding (see Experiments 1 and 3). It is of course possible that there are specific circumstances, defined by an interaction of various task factors and/or participant population characteristics (and not encountered in the present set of experiments), under which a small but “true” conflict adaptation effect might be obtained. However, this possibility presents us with two problems if we want to use the conflict adaptation effect for studying cognitive control. First, we need to identify the limited set of circumstances under which conflict-triggered control modulations consistently occur. And second, the conflict adaptation effect on response change trials may be so small that it will fail to provide sufficient measurement space for detecting group differences in the integrity or efficiency of the conflict–control loop.

In contrast with the present findings for the flanker task, evidence for a control-driven conflict adaptation effect in two other conflict paradigms is steadily accumulating: In the Simon task, participants make spatially defined responses to a nonspatial attribute (e.g., color) of a spatially varying stimulus. In this task, conflict occurs on trials in which the response signaled by the task-irrelevant stimulus location is incompatible with the correct response indicated by the task-relevant stimulus attribute—a type of trial that is analogous to incongruent trials in the flanker task. It has been shown consistently that the Simon effect (i.e., the congruency effect in the Simon task) is smaller following high-conflict trials than following low-conflict trials, even when the analysis is restricted to trials on which neither the stimulus nor the response is repeated (see, e.g., Stürmer et al., 2002; Wühr & Ansorge, 2005). In addition, two studies have reported a conflict adaptation effect in the Stroop task after excluding exact S–R repetitions (Egner & Hirsch, 2005; Kerns et al., 2004). However, a limitation of both of these studies is that they failed to remove partial repetitions, another source of confound, from the analyses. Nevertheless, these findings raise the question whether there may be a fundamental difference between the various conflict paradigms that could explain the discrepancy regarding the presence of a “true” conflict adaptation effect. One evident difference is that in the Simon and Stroop tasks, the task-irrelevant stimulus attributes (location and word identity, respectively) belong to a different perceptual dimension than do the task-relevant stimulus attributes. In contrast, in the flanker task the task-irrelevant and task-relevant stimulus attributes (the flanker arrows and the central target arrow) belong to the same perceptual dimension. This suggests that the cognitive system may be able to actively select between perceptual dimensions but not between stimulus attributes within a dimension (cf. the dimensional weighting theory of Müller, Heller, & Ziegler, 1995).

Another distinct property of the flanker task is the small stimulus set (e.g., H and S or < and >), as a result of which trial-to-trial repetitions of stimulus attributes are unavoidable. Such repetitions may cause complex forms of priming even in addition to those described above. Consider, for example, the interaction between previous trial type and response type: On response change trials, participants responded faster if the previous trial was incongruent rather than congruent (10-msec difference averaged across all participants in Experiments 1–5, $p < .001$), whereas on response repetition trials, participants responded faster following congruent trials (12-msec difference, $p < .001$). The error rates showed a similar interaction, ruling out an explanation in terms of a speed–accuracy trade-off. These results suggest that on incongruent flanker trials (e.g., HHSHH), the cognitive system selectively inhibits a representation of the conflicting flanker items (i.e., H). If on the subsequent trial this item becomes the task-relevant item (i.e., a response change trial; e.g., HHHHH), performance suffers from residual inhibition associated with this item. Such negative priming may be particularly strong if an incongruent stimulus is followed by another incongruent stimulus in which the target and flanker items have been reversed (e.g., HHSHH → SSHSS; Stadler & Hogan, 1996). In contrast, if on the subsequent trial the inhibited flanker items remain task irrelevant (i.e., a response repetition trial; e.g., HHSHH → SSSSS), any residual inhibition will benefit performance. Priming effects of this form, though small in magnitude, are a source of variance in the data that could potentially mask small effects of interest. Other conflict paradigms, like the Stroop task, typically have a larger stimulus set and hence involve a smaller number of stimulus attribute repetitions, resulting in reduced nuisance effects in the data. One challenge for future research will be to develop tasks that have even larger stimulus sets and that yet elicit sufficient conflict to probe potential conflict adaptation effects (Ullsperger et al., 2005, Experiment 2).

In sum, the present findings provide further evidence for a pure associative priming account of the conflict adaptation effect in the flanker task (Mayr et al., 2003). We could find no evidence for the established notion that conflict-sensitive modulations of cognitive control are one of the sources underlying the typical pattern of performance adjustments observed in that task. In this regard, the flanker task may differ from other conflict paradigms, such as the Simon and Stroop tasks. The present research suggests that the flanker task is not suited for studying the type of sequential performance adjustments that are predicted by the conflict–control loop theory (Botvinick et al., 2001). This conclusion notwithstanding, brain imaging studies have yielded strong support for the predictions of the conflict–control loop theory regarding sequential effects on anterior cingulate cortex and prefrontal cortex activity—in both the flanker task and other conflict paradigms (e.g., Botvinick et al., 1999; Durston et al., 2003; Kerns et al., 2004; Nieuwenhuis, Yeung, Van den Wildenberg, & Ridderinkhof, 2003; but see Burle, Allain, Vidal, & Hasbroucq, 2005). An important question for future research is how this apparent discrepancy between behavioral and brain imaging findings can be resolved.

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NOTE

1. Gratton et al. (1992) reported a similar comparison of response repetition and response change trials, but only for CC and II trials. Their effects went in the same direction reported here but were not significant, possibly due to the small sample size ($N = 6$) and the resulting lack of statistical power. Because similar analyses were not reported for CI and IC trials, a full decomposition of the conflict adaptation effect observed in their study is not possible.

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