

Running Head: CONFLICT ADAPTATION WITHIN AND ACROSS TASKS

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Generality and Specificity in Cognitive Control: Conflict Adaptation within and across
Selective-Attention Tasks but not across Selective-Attention and Simon Tasks

Antonio L. Freitas

Sheri L. Clark

State University of New York at Stony Brook

Address correspondence to Antonio L. Freitas, Department of Psychology, State University of
New York, Stony Brook, NY 11794-2500. email: antonio.freitas@stonybrook.edu.

Abstract

To explain how cognitive control is modulated contextually, Botvinick, Braver, Barch, Carter, and Cohen (2001) proposed that detecting information-processing conflict attenuates the disruptive influence of information-processing conflicts encountered subsequently, by which time appropriate cognitive-control mechanisms already will have been engaged. This *conflict-adaptation* hypothesis has motivated extensive programs of research while also attracting vigorous methodological critiques that highlight alternative accounts of trial n x trial $n-1$ sequential effects in cognitive-control tasks. Addressing those alternatives through precluding analyzing stimulus repetitions without creating any sort of confounds among any stimulus or trial characteristics, the present research observed significant conflict-adaptation effects within and across several selective-attention tasks. Moreover, across-task conflict-adaptation effects were largest when spanning tasks (i.e., a newly developed Stroop-trajectory task and a flanker task, which both require resolving conflict among stimulus elements) that presumably depend on the same mechanism of cognitive control (selective attention) than when spanning tasks that do not (i.e., the Stroop-trajectory task and a Simon task, the latter—but not former—of which requires resolving conflict between stimulus and response elements). These findings contribute to advancing beyond examining whether or not conflict adaptation exists to clarifying the conditions under which it is and is not observed.

Adapting the control of action and cognition to fluctuating environmental demands is a hallmark of effective human behavior. An influential account of the contextual modulation of cognitive control has been Botvinick, Braver, Barch, Carter, and Cohen's (2001) conflict-monitoring theory, which proposes that signals of information-processing conflict, as when different processing streams implicate incompatible responses, engage cognitive control. Following that theory, detecting an incident of information-processing conflict should attenuate the disruptive influence of information-processing conflicts encountered subsequently, by which time appropriate cognitive-control mechanisms already will have been engaged. This *conflict-adaptation* prediction has generated a substantial body of research on human behavior and brain function (for review, see Carter, & Krug, 2012), giving rise to novel conceptual approaches to understanding such disparate phenomena as racial and gender stereotyping (Kleiman, Hassin, & Trope, in press), affective and hedonic experiences (Braem, Verguts, Roggeman, & Notebaert, 2012; Van Steenbergen, Band, & Hommel, 2009, 2010), neural development (Waxer, & Morton, 2011), and relations among electrocortical and behavioral indices of cognitive control in large-scale correlational studies (Clayson & Larson, 2011). Alongside these fascinating applications of conflict-monitoring theory, however, questions have persisted pertaining to whether the conflict-adaptation effect itself is a basic principle of cognitive control or instead a methodological artifact, as discussed below.

From the outset, research on conflict adaptation has taken steps to address alternative explanations. Gratton, Coles, and Donchin (1992) first demonstrated that increases in response time and decreases in response accuracy on incongruent relative to congruent trials are smaller following incongruent trials than following congruent trials of cognitive-control tasks, such as an Eriksen flanker task (Eriksen & Eriksen, 1974). That initial paper reported that the adaptation

effect it had identified was not moderated significantly by whether or not subjects viewed identical stimulus arrays across trials n and $n-1$, such as “HSHH” on both trials, relative to “HSHH” at trial $n-1$ and “SSHSS” at trial n (Gratton et al., 1992). In contrast, Mayr, Awh, and Laurey (2003) found that conflict-adaptation effects, also on the flanker task, were *not* observed when exact stimulus repetitions were removed from analysis, suggesting that repetition priming might be a more parsimonious account of their data than conflict monitoring theory.

Addressing those conflicting findings, subsequent work expanded the number of stimuli used as flankers and as targets (Ullsperger, Bylsma, & Botvinick, 2005; see also Notebaert & Verguts, 2006). Significant conflict-adaptation effects were observed despite limiting analyses to trial pairs in which no target, distractor, or response elements repeated across successive trials (e.g., “77677,” followed by “33233”; Ullsperger et al, 2005). As noted by Schmidt and DeHouwer (2011), however, many conflict-adaptation studies with greater than two responses have created stimulus-contingency confounds. For example, in a flanker task with more than two responses, each central cue of a flanker trial would appear more often with identical flankers (because there is only one type of congruent array for that target, e.g., “77777”) than with any other stimuli (because there are many types of incongruent arrays for that target, e.g., “11711,” “22722,” “33733,” etc.). Those authors showed that eliminating stimulus-contingency confounds can eliminate the conflict-adaptation effect (Schmidt & DeHouwer, 2011). In our view, it is important to note that unconfounding all stimulus and response elements in a four-response design requires .75 incongruent trials and .25 congruent trials across, leading to unequal proportions of the different sequences that form the primary independent variable in conflict-adaptation studies (e.g., $.75 * .25 = .1875$ incongruent following congruent trials; and $.75 * .75 = .5625$ incongruent following incongruent trials). Correlating a manipulation of trial sequences’

levels of information-processing conflict with a manipulation of trial sequences' frequencies makes it impossible to attribute any result solely to one manipulation or the other.

Accordingly, novel tests of the conflict-adaptation hypothesis are needed that (a) eliminate exact stimulus repetitions without (b) introducing stimulus-contingency confounds, while (c) holding congruency rates to .50. Toward this end, our first experiment examined conflict adaptation in a four-response selective-attention task (a newly developed "Stroop-trajectory" task) in which stimulus arrays were oriented vertically or horizontally, thereby allowing analysis of trial pairs associated with different responses and different stimuli. Our remaining two experiments addressed the above three criteria by interspersing different tasks across trials, as described next.

In our view, and in accord with Egner (2008), a straightforward implication of Botvinick and colleagues' (2001) conflict-monitoring theory is that conflict-adaptation effects should be observed across different tasks to the extent that performance at trials n and $n-1$ depends on the operation of a common component process of cognitive control. To the extent that a component process of cognitive control (e.g., selective attention) is needed to resolve information-processing conflict at trial n , engaging that process at trial $n-1$ should facilitate resolving information-processing conflict at trial n (whether or not same task is performed at trials n and $n-1$), thereby leading to attenuated neural signals of information-processing conflict and attenuated increases in response time and decreases in response accuracy as a result of encountering incongruent relative to congruent trials of cognitive-control tasks. Importantly, if such (conflict-adaptation) effects were to be observed despite differences in stimuli perceived and rules followed in different tasks across trials n and $n-1$, the results would appear impossible to attribute to stimulus repetitions or to any introduction of stimulus-contingency confounds. However, variability in

whether conflict-adaptation effects have been observed across different tasks (e.g., Freitas, Bahar, Yang, & Banai, 2007; Kan et al., in press; Kleiman et al., in press; Kunde & Wuhr, 2006) or have not been observed across different tasks (e.g., Akçay & Hazeltine, 2011; Funes, Lupiáñez, & Humphreys, 2010; Wendt, Kluwe, & Peters, 2006) has yielded uncertainty regarding the implications of those studies for understanding the contextual modulation of cognitive control. The present work aimed to help resolve this uncertainty by bringing under experimental control factors determining whether or not across-task conflict adaptation is observed.

More specifically, given the general proposal (Egner, 2008; Funes et al., 2010) that across-task similarity may help determine whether or not across-task conflict-adaptation is observed, our final two experiments respectively manipulated two bases of across-task similarity, relating to the structure of stimuli encountered (i.e., whether trials of different tasks encountered at trials n and $n-1$ were comprised of single or multiple objects, Experiment 2) and to the nature of information-processing conflict (i.e., whether trials of different tasks encountered at trials n and $n-1$ entailed resolving conflict between stimulus elements or between stimulus and response elements, Experiment 3). A novel methodological property of these experiments is that each included three tasks that varied along the above-noted dimensions, thereby affording the first experimental tests of which we are aware of whether the magnitude of conflict-adaptation effects would be greater across some task combinations than across others. Clarifying the conditions under which the contextual modulation of cognitive control generalizes across tasks that utilize different stimuli would provide clear support for the conflict-adaptation hypothesis while also helping elucidate the processes by which such effects unfold.

Experiment 1

One approach to examining sequential effects in cognitive-control tasks is to present stimulus arrays on alternating vertical and horizontal dimensions and then to restrict analyses to trial pairs with alternating orientations, thereby eliminating stimulus and response repetitions without introducing stimulus-contingency confounds. However, there has been variability in whether conflict-adaptation effects have been observed across vertical/horizontal orientations (Freitas et al., 2007, Experiment 1; Kunde & Wuhr, 2006) or have not been observed across vertical/horizontal orientations (Mayr et al., 2003), making it difficult to reach firm conclusions regarding the implications of the alternating-orientations studies for the conflict-adaptation hypothesis. Methodological factors differing across previous experiments may play some role in that heterogeneity. As reported by Mayr and Awh (2009), for example, a re-analysis of the early blocks of the Mayr et al. (2003) data found small conflict-adaptation effects on a flanker task that were not significant statistically but that were similar in size to the statistically significant conflict-adaptation effects on a flanker task reported in Freitas et al. (2007, Experiment 1, in which each experiment was comprised of three blocks of trials), which included relatively large samples of subjects, thereby yielding relatively high statistical power. Given this possibility that variability in statistical power across previous experiments may account for some of the variability in their confirmation or disconfirmation of the conflict-adaptation hypothesis using the alternating-orientations design¹, it is important to identify experimental paradigms that yield

¹ Another potentially important difference between the experiments of Freitas et al., 2007, and those of Mayr et al., 2003, is that the former experiments selected trials for presentation randomly without replacement at the level of blocks, whereas the latter experiments selected trials for presentation randomly with replacement (U. Mayr, personal communication, 2008). As reported herein, all experiments in this investigation selected trials for presentation randomly with replacement.

conflict-adaptation effects robust enough to be replicated reliably in experiments using the smaller sample sizes typical of cognitive-control studies.

Toward this end, we have developed a new cognitive-control task, which we term the “Stroop-trajectory” task, given that it requires responding to non-symbolic information independent of congruent or incongruent symbolic information, yielding a stimulus-stimulus conflict (as a Type IV “Stroop-like” task in the terminology of Kornblum, Hasbroucq, & Osman, 1990; Kornblum & Lee, 1995). In the Stroop-trajectory task, pointing triangles are presented one-at-a-time, cumulatively yielding an array of slightly overlapping triangles on each trial. Lastly, a smaller triangle, pointing in the same direction as all of the others, appears at either the top or the bottom of vertically oriented arrays or at the left or right of horizontally oriented arrays (see Figure 1). Participants’ task is to indicate the location of the smaller triangle. Trial congruence reflects whether or not the smaller triangle’s location matches the direction indicated by all triangles in the array (therefore matching also the direction in which new triangles in the array have appeared).

Our theoretical motivations in designing this task were threefold. First, there is strong evidence that one result of encountering information-process conflict is to re-focus attention on the specific stimulus-response contingencies of the task with which one is engaged (Notebaert & Verguts, 2008 Braem, Verguts, & Notebaert, 2011). Accordingly, to generate conflict-adaptation effects general enough to transcend the specific stimulus-response contingencies required on different trial types, in our view it is important that the task have simple stimulus-response mappings. Otherwise, with more complex stimulus-response mappings, encountering conflict could lead to a re-focus on the stimulus-response contingency that is specific to the trial with which one is engaged presently but that then may conflict with the stimulus-response

contingency encountered on the subsequent trial. The Stroop-trajectory task's simple single rule, "indicate the location of the smaller arrow," should help avoid such a need to refocus attention on stimulus-response contingencies. Second, as often recognized (Bugg, 2008; Ullsperger et al., 2005), one likely impediment to detecting conflict adaptation is negative priming, as when participants repeatedly are obliged to respond to a stimulus they ignored recently (such as a central target stimulus that recently served as a flanker distractor stimulus), which is known to lead to slower, less accurate responses than responding to a stimulus that has not been ignored recently (e.g., Neill, Valdes, Terry, & Gorfein, 1992; Tipper, 1985). Accordingly, we designed the Stroop-trajectory task such that all triangles in all arrays always point in the same direction, and participants always respond to the smallest triangle; as a result, in no case are participants obliged to respond to a stimulus they recently ignored. Finally, extensive evidence indicates that experiences of perceiving and responding to specific stimuli generate episodic traces that bind perceptual and action features (Hommel, 1998, 2004). These episodic traces then guide subsequent action, facilitating performance on exact perceptual/action repetitions but impeding performance on partial perceptual/action repetitions (Hommel, 1998, 2004). The Stroop-trajectory task was designed to limit the influence of partial trial repetitions. During the early parts of each trial, participants view pointing stimuli (the large black triangles) that can be expected to activate particular behavioral responses (e.g., "left" for left-pointing triangles; Kopp et al., 1996) but that should not cue retrieval of representations of recent actions (given that participants never respond to the large black triangles). In this way, the temporal nature of the Stroop-trajectory task should allow the priming of (correct or incorrect) responses (on congruent and incongruent trials, respectively) without generating perceptual-action feature-binding effects. Moreover, by time participants respond to each trial's imperative stimulus (by indicating the

location of the smaller gray triangle), many separable perceptual events will have occurred (as each of the seven black triangles has appeared sequentially), thereby allowing decay of representations of prior responses to prior arrays, which should further limit the sensitivity of this experimental paradigm to the debilitating influence on performance of partial repetitions of perception-action events.

In summary, for the reasons stated above, and given our extensive pilot testing of the Stroop-trajectory task, we predicted that this task would yield significant conflict-adaptation effects when limiting analyses to trial pairs in which stimulus arrays alternated across horizontal and vertical orientations, with effects robust enough to be detected in an experiment with a relatively small sample size.

Methods

In exchange for course credit, 15 undergraduates (6 male), aged 18 – 44 years ($M = 21.33$), participated. Using their dominant hands (and the fingers of their own choosing), participants pressed the up, down, left, and right arrow keys of a standard computer keyboard (that had been positioned near the left hand of left-handed participants and near the right hand of right-handed participants) to indicate the location of a small gray triangle within an array of larger black triangles. Trials began with a 400 msec fixation cue (“.”) centered horizontally and vertically on the monitor. Following a 26.67 msec blank screen, vertical or horizontal arrays of seven black triangles (each 83 pixels high x 27 pixels wide) next were presented incrementally (see Figure 1). For upward-pointing arrays, a single upward-pointing triangle first was presented, centered horizontally, 41% of the distance from the bottom of the monitor. In successive intervals of 26.67 msec, each of five other identical triangles next was added to the array, with

each triangle appearing immediately above the one presented before it. Lastly, 40 msec after six black triangles in the array were visible, the seventh identical triangle appeared immediately above the others, and a smaller (24 pixels high x 14 pixels wide) upward-pointing gray triangle appeared inside either the top black triangle (on congruent trials) or the bottom black triangle (on incongruent trials) and were presented for 146.67 msec, after which the screen remained blank as the computer program awaited the participant's response. For right-pointing, downward-pointing, and left-pointing trials, the stimulus arrays were rotated 90°, 180°, and 270°, respectively. At the conclusion of each trial, the screen remained blank for an interval varying randomly between 125 and 250 msec, prior to the appearance of the fixation cue at the beginning of the next trial. Following computer-administered instructions and a 24-trial practice block, there were 8 blocks of 97 trials each.

Results

Latency data were not analyzed when erroneous responses were recorded at trials n or $n-1$ (9.44% of trials) or when latencies exceeded 800 msec (2.08% of remaining trials), and accuracy data were not analyzed when erroneous responses were recorded at trial $n-1$ (5.12% of trials). Moreover, response latency and accuracy data were analyzed only when the vertical/horizontal orientations of stimulus arrays differed across trials n and $n-1$, thereby precluding from analysis any exact stimulus repetitions.

Response times and accuracy rates were analyzed in 2 (trial n congruence) x 2 (trial $n-1$ congruence) repeated-measures ANOVAs (see Table 1 for cell means). For responses times, there was a significant effect of trial n congruence, $F(1, 14) = 5.26$, $MSE = 1482.77$, $p < .05$, partial $\eta^2 = .27$, but not of trial $n-1$ congruence, $F(1, 14) = 3.39$, $MSE = 49.85$, $p = .09$, partial η^2

= .19; most important to this investigation, the trial n x trial $n-1$ congruence interaction was significant, $F(1, 14) = 16.83$, $MSE = 116.81$, $p < .001$, partial $\eta^2 = .55$. For accuracy rates, there were significant effects of trial n congruence, $F(1, 14) = 15.74$, $MSE = 0.0034$, $p < .002$, partial $\eta^2 = .53$ and of trial $n-1$ congruence, $F(1, 14) = 6.00$, $MSE = 0.0008$, $p < .05$, partial $\eta^2 = .30$; most important to this investigation, the trial n x trial $n-1$ congruence interaction also was significant, $F(1, 14) = 8.95$, $MSE = 0.0009$, $p < .01$, partial $\eta^2 = .39$. As reported in Table 1, consistent with the conflict-adaptation hypothesis, these interactions indicate that differences in response accuracy and latency across incongruent relative to congruent trials (at trial n) were significantly greater following congruent trials than following incongruent trials (at trial $n-1$).

Discussion

In a cognitive-control task developed to minimize negative-priming effects, perceptual-action feature-binding effects, and any need of participants to keep in mind complex task rules, significant conflict-adaptation effects were observed despite limiting analyses to trial pairs in which stimulus arrays were presented on alternating (vertical versus horizontal) orientations, thereby precluding analysis of exact stimulus repetitions without introducing stimulus-contingency confounds. Moreover, the effects were sufficiently robust to be detected in a sample size about as small as that which seems to be preferred in many cognitive-control studies, including those that have reported influential null effects (e.g., Funes et al., 2010; Mayr et al., 2003; Schmidt & DeHouwer, 2011), suggesting that the Stroop-trajectory task may serve as a useful tool for further efforts to understand conflict adaptation. Indeed, Experiment 3 will return to this task in an examination of across-task conflict adaptation.

Experiment 2

In light of earlier conflicting findings on the role of stimulus repetitions in putative conflict-adaptation effects, another approach to understanding the contextual modulation of cognitive control has been to combine different tasks, with different stimuli and different response contingences, across trials. However, variability in whether or not conflict-adaptation effects have been observed across different tasks has yielded uncertainty regarding the implications of such findings. For example, in one experiment that intermixed flanker trials (which required responding to the center of three arrows) and “spatial Stroop” trials (which required indicating the location of a box relative to an arrow), conflict adaptation in response time was observed despite limiting analyses to trial pairs in which different tasks were presented at trials n and $n-1$ (Freitas et al., 2007, Experiment 3). Funes and colleagues (2010) proposed that putative conflict adaptation across spatial Stroop and flanker tasks might instead reflect specific strategic adjustments, given similarities in how those tasks can be structured. More specifically, those authors suggested that conflict adaptation across spatial Stroop and flanker tasks would be less likely to be observed with spatial Stroop stimuli comprised of a single object (rather than of multiple objects, as in Freitas et al., 2007), thereby reducing the possibility that participants could use a similar strategy (focusing on one of several objects) on trials of that task as used on flanker trials (which also contain multiple objects). Those authors then interspersed flanker trials with spatial Stroop trials in which a single arrow (rather than two objects, as in Freitas and colleagues’, 2007, Experiment 3) appeared on the left or right side of participants’ fixation point. Their lack of a finding of conflict adaptation across those two tasks (Funes et al., 2010, Experiment 1) is consistent with the possibility that conflict adaptation across flanker and Stroop trials may be observed only when Stroop stimuli are composed of multiple, separate stimulus

elements. Given that the Stroop and flanker tasks are among the most widely used tasks in studies of cognitive control, this would be a valuable conclusion.

However, it is important to note that the spatial Stroop task used by Funes and colleagues differed from that used by Freitas and colleagues not only in the number of its stimulus elements but also in the nature of its information-processing conflict. Rather than requiring responding according a stimulus's spatial location independent of its symbolic meaning (as in Freitas et al.'s, 2007, "spatial Stroop" task; see also Palef, 1978, for a similar definition of that task), the task that Funes and colleagues termed a "spatial Stroop" task required that participants respond to the left/right direction in which arrows pointed (irrespective of congruent or incongruent left/right hand response). That design, requiring attending to stimulus elements despite congruent or incongruent response elements, has been referred to as a "reverse spatial Stroop" task (which is how we will refer to that task in the remainder of this paper; O'Leary & Barber, 1993), or more generally as a "reverse Stroop" task (Chmiel, 1984; MacLeod, 1991). Accordingly, it remains unknown whether the conflict-adaptation effects reported by Freitas and colleagues (2007) across the flanker and spatial Stroop tasks will apply only to Stroop tasks with separable stimulus elements, or whether they also will also apply to standard (i.e., non-reversed) spatial Stroop tasks in which participants respond to a single spatial Stroop stimulus as a function of its location rather than its meaning.

The present experiment therefore included a single-item spatial Stroop task (the reverse of Funes et al.'s 2010, Exp. 1), henceforth termed "spatial Stroop I," and a two-item spatial Stroop task (adapted from Freitas et al., 2007, Exp. 3), henceforth termed "spatial Stroop II," to examine their potential conflict-adaptation interactions with one another and with a flanker task. All three tasks drew from the same stimulus set, comprised of block arrows, line arrows, and

pointing fingers. As illustrated in Figure 2, participants were assigned randomly one of the six possible combinations of stimulus type and task type. This design holds constant the degree of similarity of the three tasks' stimuli to one another. Given variability in findings from previous investigations of across-task conflict adaptation using variants of these tasks, the present experiment included a relatively large sample size, maximizing statistical power to detect any true population differences and therefore also maximizing the information value of any null results.

Most relevant to testing the conflict-adaptation hypothesis while precluding stimulus repetitions is whether or not conflict-adaptation effects would be observed despite the fact that none of the three tasks was presented successively across trials. As illustrated in Figure 2, each of the three tasks used different stimuli and different rules. Accordingly, if differences in response accuracy and latency across incongruent relative to congruent trials (at trial n) were found to be greater following congruent trials than following incongruent trials (at trial $n-1$), such findings would support the conflict-adaptation hypothesis and would appear impossible to attribute to repetition priming or to any sort of confound in stimulus or trial characteristics. The present design also allowed testing whether any conflict-adaptation effects observed on flanker trials would be greater following spatial Stroop II trials (which were comprised of multiple-element arrays, as are flanker trials) than following spatial Stroop I trials (which were comprised of single elements, unlike the flanker trials).

Methods

Eighty undergraduates (48 male), aged 18 – 30 ($M = 19.61$), participated in exchange for course credit. Participants held two-button response devices in their laps, pressing the left button

with the left thumb and the right button with the right thumb. All trials began with a 352.94 msec fixation cue (“.”) presented at the vertical and horizontal midpoint of the monitor. Following a 23.53 msec blank screen, flanker or spatial Stroop stimuli next were presented for 200 msec, after which the screen remained blank as the computer program awaited the participant’s response. At the conclusion of each trial, the screen remained blank for an interval varying randomly between 125 and 250 msec, prior to the appearance of the fixation cue at the beginning of the next trial.

The flanker task and the two variants of the spatial Stroop task all drew from the same set of left- or right-pointing block arrows (each 52 pixels high x 68 pixels wide), line arrows (each 29 pixels high x 66 pixels wide, rotated slightly upward by 14°), and hands with pointing finger (each 38 pixels high x 70 pixels wide, rotated slightly downward by 9°), all solid black. Each participant was assigned randomly a different stimulus type (block arrow, line arrow, or pointing finger) for each of the three tasks (the flanker task and the two variants of the spatial Stroop task), yielding the six possible stimulus/task groupings illustrated in Figure 2. Trials of the three tasks were selected for presentation randomly with replacement, with the exception that no task repeated on successive trials. Following computer-administered instructions and a 36-trial practice block, there were 10 blocks of 108 trials each.

On flanker trials, a group of three block arrows, line arrows, or pointing fingers was presented at the vertical and horizontal midpoint of the monitor. Participants pressed the response button corresponding to the direction in which the center stimulus pointed (see Figure 2, third column). If the center block arrow pointed left, for example, the participant would press the left response button. As determined through random selection with replacement across trials,

the center block arrow, line arrow, or finger pointed in either the same direction (on congruent trials) or the opposite direction (on incongruent trials) as did those surrounding it.

On the spatial Stroop I task, a single block arrow, line arrow, or pointing finger was presented left or right of the monitor's horizontal midpoint. Stimulus location was controlled by expanding horizontally, to 300 pixels, the stimulus images' backgrounds and placing each stimulus to the far left or right of its image. Participants pressed the response button corresponding to the stimulus's position relative to the monitor's horizontal midpoint (see Figure 2, second column). If the block arrow were left of the monitor's midpoint, for example, the participant would press the left response button. As determined through random selection with replacement across trials, the block arrow, line arrow, or finger pointed in either the same direction (on congruent trials) or the opposite direction (on incongruent trials) as its horizontal position.

On the spatial Stroop II task, a block arrow, line arrow, or pointing finger and a rectangle, rounded rectangle, or circle, respectively, were presented at the horizontal midpoint of the monitor. To provide an informative comparison of conflict adaptation across the flanker task and the two versions of spatial Stroop tasks, variability among the two spatial Stroop tasks' basic perceptual relations to the flanker task was minimized: Because spatial Stroop I stimuli appeared at different locations from where the flanker stimuli appeared on the monitor, spatial Stroop II stimuli also were presented at varying locations (but in a task-irrelevant way). Whereas flanker stimuli appeared at the monitor's vertical and horizontal midpoints and spatial Stroop I stimuli appeared left or right of the monitor's horizontal midpoint, spatial Stroop II stimuli appeared above or below the monitor's vertical midpoint (as determined randomly with replacement across trials). Stimulus location was controlled by expanding vertically, to 300 pixels, the

stimulus images' backgrounds and placing each stimulus to the far top or bottom of its image. Participants pressed the response button corresponding to the horizontal position of the rectangle, rounded rectangle, or circle, relative to the block arrow, line arrow, or pointing finger, respectively (see Figure 2, fourth column). If the rectangle were to the left of the block arrow, for example, the participant would press the left response button. As determined through random selection with replacement across trials, the block arrow, line arrow, or finger pointed in either the same direction (on congruent trials) or the opposite direction (on incongruent trials) as the horizontal position of the rectangle, rounded rectangle, or circle, respectively.

Results

Latency data were not analyzed when erroneous responses were recorded at trials n or $n-1$ (13.61% of trials) or when latencies exceeded 1000 msec (3.15% of remaining trials)². Accuracy data were not analyzed when erroneous responses were recorded at trial $n-1$ (7.36% of trials). As noted above, the present experiment contained no exact stimulus repetitions.

Overall Conflict Adaptation (across Flanker, Spatial Stroop I and Spatial Stroop II Tasks)

² This 1000-msec cutoff, rather than the 800-msec cutoff used in Experiment 1, was needed in this experiment and in Experiment 3 given the relatively slower responses in this experiment and in Experiment 3 (which is not surprising given that these two experiments each interspersed three different task manipulations across trials) and given our laboratory's general practice of not removing from analysis more than around of 3% of response times. To ensure that the substantive conclusions reported in this paper were not dependent on using different cutoff criteria across the three experiments, we re-analyzed the data from Experiment 1 using the 1000-msec cutoff, which resulted in excluding 1.08% of response times and yielded trial n x trial $n-1$ congruency interaction on response time ($F(1, 14) = 22.55$, $MSE = 97.41$, $p < .001$, partial $\eta^2 = .62$) that did not differ appreciably from those reported in the main text (when using the 800-msec cutoff).

Response times and accuracy rates were analyzed in 2 (trial n congruence) x 2 (trial $n-1$ congruence) repeated-measures ANOVAs (see Table 2 for cell means). For responses times, there were significant effects of trial n congruence, $F(1, 79) = 535.28$, $MSE = 369.40$, $p < .0001$, partial $\eta^2 = .87$ and of trial $n-1$ congruence, $F(1, 79) = 45.94$, $MSE = 120.87$, $p < .0001$, partial $\eta^2 = .37$; most important to this investigation, the trial n x trial $n-1$ congruence interaction also was significant, $F(1, 79) = 25.49$, $MSE = 124.59$, $p < .0001$, partial $\eta^2 = .24$. For accuracy rates, there was a significant effect of trial n congruence, $F(1, 79) = 193.59$, $MSE = 0.0016$, $p < .0001$, partial $\eta^2 = .71$, but not of trial $n-1$ congruence, $F(1, 79) = 2.78$, $MSE = 0.0004$, $p < .10$, partial $\eta^2 = .03$; most important to this investigation, the trial n x trial $n-1$ congruence interaction was significant, $F(1, 79) = 73.75$, $MSE = 0.0004$, $p < .0001$, partial $\eta^2 = .48$. As summarized in Figure 3 (see Table 2 for specific cell means), consistent with the conflict-adaptation hypothesis, these interactions indicate that differences in response accuracy and latency across incongruent relative to congruent trials (at trial n) were significantly greater following congruent trials than following incongruent trials (at trial $n-1$).

To examine overall across-task conflict adaptation, an additional analysis excluded data when the two variants of spatial Stroop task were presented successively at trials n and $n-1$. These analyses of conflict adaptation from the spatial Stroop tasks to the flanker task and vice versa were examined in 2 (direction of trial sequence: flanker \rightarrow spatial Stroop or spatial Stroop \rightarrow flanker) x 2 (trial n congruence) x 2 (trial $n-1$ congruence) repeated-measures ANOVAs. Consistent with the hypothesis of across-task conflict adaptation, there were significant trial n x trial $n-1$ congruence interactions on response time, $F(1, 79) = 8.35$, $p < .01$, partial $\eta^2 = .10$, and on accuracy, $F(1, 79) = 18.45$, $p < .0001$, partial $\eta^2 = .19$, which were not moderated by whether flanker trials preceded or followed spatial Stroop trials (for response time, $F(1, 79) = 2.39$, $p >$

.12; for accuracy, $F=0.04$). Having established conflict-adaptation effects in a design that precluded exact stimulus repetitions without introducing any confounds in stimulus or trial characteristics, we next explored whether the amount of conflict adaptation observed on each of the three task types was modulated by the type of task presented at trial $n-1$.

Conflict Adaptation on Flanker Trials

Response latency and accuracy on flanker trials were analyzed via repeated-measures ANOVAs as a function of trial n congruence, trial $n-1$ congruence, and trial $n-1$ task type (spatial Stroop I or spatial Stroop II). Consistent with the hypothesis of conflict adaptation, there were significant trial n x trial $n-1$ congruence interactions for response time, $F(1, 79) = 10.03$, $MSE = 532.84$, $p < .01$, partial $\eta^2 = .11$, and for accuracy, $F(1, 79) = 11.69$, $MSE = 0.0016$, $p < .01$, partial $\eta^2 = .13$. These two-way interactions were not moderated further by the type of spatial Stroop task presented at trial $n-1$: the three-way trial n congruence x trial $n-1$ congruence x trial $n-1$ task type (spatial Stroop I or spatial Stroop II) interactions were not significant for response time, $F(1, 79) = 0.95$, or for accuracy, $F(1, 79) = 0.54$.

Conflict Adaptation on Spatial Stroop I Trials

Trial n spatial Stroop I responses were analyzed via repeated-measures ANOVA as a function of trial n congruence, trial $n-1$ congruence, and trial $n-1$ task type (flanker task or spatial Stroop II task). Consistent with the hypothesis of conflict adaptation, there were significant trial n x trial $n-1$ congruence interactions for response time, $F(1, 79) = 18.72$, $MSE = 540.53$, $p < .001$, partial $\eta^2 = .19$, and for accuracy, $F(1, 79) = 35.03$, $MSE = 0.0027$, $p < .0001$, partial $\eta^2 = .31$, which were moderated further by trial $n-1$ task type for response time, $F(1, 79) = 9.89$, $MSE = 411.27$, $p < .01$, partial $\eta^2 = .11$, and for accuracy, $F(1, 79) = 14.09$, $MSE = 0.0028$, $p <$

.001, partial $\eta^2 = .15$. Clarifying the nature of those three-way interactions, the trial n congruence x trial $n-1$ congruence interactions were of greater magnitude following the spatial Stroop II task at trial $n-1$ (for response time, $F(1, 79) = 22.49$, $MSE = 600.73$, $p < .0001$, partial $\eta^2 = .22$; for accuracy, $F(1, 79) = 40.16$, $MSE = 0.0032$, $p < .0001$, partial $\eta^2 = .34$) than following the flanker task at trial $n-1$ (where the trial n congruence x trial $n-1$ congruence interactions were not significant for response time, $F(1, 79) = 1.93$; or for accuracy, $F(1, 79) = 2.31$).

Conflict Adaptation on Spatial Stroop II Trials

Trial n spatial Stroop II responses were analyzed via repeated-measures ANOVA as a function of trial n congruence, trial $n-1$ congruence, and trial $n-1$ task type (flanker task or spatial Stroop I task). Consistent with the hypothesis of conflict adaptation, there were significant trial n x trial $n-1$ congruence interactions for response time, $F(1, 79) = 15.03$, $MSE = 715.71$, $p < .001$, partial $\eta^2 = .16$, and for accuracy, $F(1, 79) = 48.04$, $MSE = 0.0015$, $p < .0001$, partial $\eta^2 = .38$, which were moderated further by trial $n-1$ task type for response time, $F(1, 79) = 14.81$, $MSE = 550.68$, $p < .001$, partial $\eta^2 = .16$, and for accuracy, $F(1, 79) = 8.84$, $MSE = 0.0015$, $p < .01$, partial $\eta^2 = .10$. Clarifying the nature of those three-way interactions, the trial n congruence x trial $n-1$ congruence interactions were of greater magnitude following the spatial Stroop I task at trial $n-1$ (for response time, $F(1, 79) = 23.55$, $MSE = 799.24$, $p < .0001$, partial $\eta^2 = .23$; for accuracy, $F(1, 79) = 55.52$, $MSE = 0.0013$, $p < .0001$, partial $\eta^2 = .41$) than following the flanker task at trial $n-1$ (where there was a significant trial n congruence x trial $n-1$ congruence interaction for accuracy, $F(1, 79) = 6.74$, $MSE = 0.0017$, $p < .05$, partial $\eta^2 = .08$; but not for response time, $F(1, 79) = 0.19$).

Discussion

By interspersing three selective-attention tasks across trials, with each task associated with perceptually distinct stimuli, and with no single task presented on successive trials (thereby precluding exact stimulus repetitions without introducing any sort of stimulus-contingency or trial-frequency confound), the present experiment yielded clear support for the conflict-adaptation hypothesis.

These findings also are the first of which we are aware to reveal a pattern of *gradations* in conflict adaptation, with the amount of conflict adaptation observed across the flanker and two spatial Stroop tasks was significantly different from zero but significantly less than that observed across the two spatial Stroop tasks. There are at least two plausible explanations for this unanticipated finding. First, given that the stimulus-response contingencies of the two variants of spatial Stroop task were more similar to one another than to the stimulus-response contingencies of the flanker task, these results appear consistent with an associative learning account of conflict adaptation proposing that encountering information-processing conflict re-focuses attention on the specific stimulus-response contingencies of the task with which one is engaged (Verguts & Noteaert, 2009; Braem et al., 2011). A second possibility is that conflict-adaptation effects will be greatest in magnitude when performance at trials n and $n-1$ depends on the operation of common component processes, such those comprising the “where” relative to the “what” cortical visual system (see, e.g., Mishkin, Ungerleider, & Macko, 1983), which appears more relevant to both of the spatial Stroop tasks than to the flanker task. Because both of these accounts point to greater commonality among the two spatial Stroop task variants than between either of those task variants and the flanker task, the two possibilities cannot be distinguished here.

Finally, there was no evidence that the magnitude of conflict-adaptation effects on flanker trials was moderated by whether the spatial Stroop task at the previous trial was

comprised of multiple or single objects. This latter null effect, obtained in an experiment with an unusually large sample size and hence unusually high statistical power, argues against the possibility that the presently reported conflict-adaptation effects from the spatial Stroop tasks to the flanker task and vice versa reflect specific strategic adjustments, such as responding strategically to one of several objects on both trials n and $n-1$. Support for that possibility would have entailed larger-magnitude conflict-adaptation effects across the flanker and spatial Stroop II task than across the flanker and spatial Stroop I task, which was not observed. Rather, our interpretation is that resolving information-processing conflict on each of these three tasks requires selective attention, whether to a central rather than peripheral symbol (in the flanker task), to an arrow's location rather than its meaning (in the spatial Stroop I task), or to the location of a box rather than the meaning of an accompanying arrow (in the spatial Stroop II task). Accordingly, consistent with conflict-monitoring theory (Botvinick et al., 2001), we assume that engaging (at trial $n-1$) a mechanism of cognitive control will facilitate resolving (at trial n) information-processing conflict to which that mechanism is applicable. In light of this reasoning, the lack of conflict adaptation across the reverse spatial Stroop and flanker task observed by Funes and colleagues (2010, Experiment 1) can be seen to reflect those two tasks' dependencies on distinct cognitive-control mechanisms, with selective attention more relevant to the latter task (which entails resolving stimulus-stimulus conflicts) than to the former task (which entails resolving stimulus-response conflicts; cf. Kornblum et al, 1990). This explanation for differences in results across different experiments must be regarded as speculative until it can be tested within a single experiment that manipulates whether or not performance at trials n and $n-1$ depends on the operation of a common component process of cognitive control. Our final experiment pursued that objective.

Experiment 3

The Stroop-trajectory task that we introduced in Experiment 1 requires limiting attention to specific aspects of visual arrays, as does the flanker task (along with both spatial Stroop task variants examined in Experiment 2). Consistent with the results of Experiment 2, then, our application of Botvinick and colleagues' (2001) conflict-monitoring theory predicts that conflict-adaptation effects should be observed across the flanker and Stroop-trajectory tasks, even though the two tasks delineate distinct stimulus-response contingencies. More specifically, to the extent that a component process of cognitive control (e.g., selective attention) is needed to resolve information-processing conflict at trial n , engaging that process at trial $n-1$ should facilitate resolving information-processing conflict at trial n (whether or not same task is performed at trials n and $n-1$). If this reasoning is correct, moreover, the magnitude of conflict-adaptation effects across the Stroop-trajectory and flanker tasks should be greater than that observed across the Stroop-trajectory task and other tasks that do not depend on selective attention, such as the Simon task. The Simon task (much like the reverse spatial Stroop task examined by Funes et al., 2010, Experiment 1) requires responding to symbolic cues independent of their spatial locations, with faster and more accurate responses typically observed when a cue's spatial location matches rather than mismatches the spatial location of one's response (e.g., with faster and more accurate responses when using the left rather than right hand to respond to a stimulus on the left side of a display; Simon, 1969, 1990). In accord with Kornblum and colleagues (1990), Simon tasks entail resolving conflicts between stimulus and response elements, whereas the flanker and Stroop-trajectory tasks entail resolving conflicts among stimulus elements, such that the latter tasks depend on mechanisms of selective attention to a greater extent than do the former tasks.

In light of the above considerations, Experiment 3 was designed to test the hypothesis that the magnitude of conflict-adaptation effects across the Stroop-trajectory and flanker manipulations would be greater than that observed across the Stroop-trajectory manipulation and a Simon manipulation. Preliminary support for this hypothesis can be derived from past work. Several reports of an *absence* of across-task conflict adaptation (Akçay & Hazeltine, 2011; Funes, et al., 2010; Wendt et al., 2006) have been based on experiments interspersing trials of selective-attention tasks (i.e., involving stimulus-stimulus conflicts, which are type IV tasks in the nomenclature of Kornblum et al., 1990) with Simon-like tasks (i.e., involving stimulus-response conflicts, which are type III tasks in the nomenclature of Kornblum et al., 1990). In contrast, several reports of a *presence* of across-task conflict adaptation (Freitas et al., 2007; Kan et al., in press; Kleiman et al., in press) have been based on experiments interspersing trials of separate tasks that all depend in some way on selective attention. Although broadly consistent with our hypotheses, those differences in results across different groups of experiments could be attributed to a variety of factors that may have differed across the separately conducted experiments.

Experiments 3a and 3b therefore randomly interspersed trials that orthogonally combined flanker and Simon manipulations with trials that comprised Stroop-trajectory manipulations only. To broaden the potential evidentiary basis for any conclusions, we conducted, in Experiments 3a and 3b, near-exact replications of one another that differed only in whether or not stimulus arrays were oriented vertically or horizontally, respectively (see Figure 4). Given that this experiment entailed a relatively complex combination of three separate task manipulations across trials, we did not alternate stimulus-array orientations within-participants (as was done in Experiment 1). However, it is important to note that, by limiting across-task

analyses to trial pairs as a function of Stroop-trajectory and flanker manipulations or Stroop-trajectory and Simon manipulations (i.e., in no cases examining trial pairs comprised of flanker and Simon manipulations, which were combined parametrically on the same trials and therefore included stimulus repetitions of one another), all analyses of across-task conflict adaptation precluded any stimulus repetitions without creating any sort of confounds among any stimulus or trial characteristics.

It also is important to note that trial congruence on the Stroop-trajectory task, as described above in Experiment 1, reflects both (a) the congruence of the smaller triangle's location with the direction in which all arrows point on a given trial and (b) the congruence of the smaller triangle's location with the direction in which all arrows have appeared on a given trial. The multifaceted nature of that manipulation, while warranting future study to assess the specific processes underlying each of the two sources of congruence, does not appear to affect our above-stated predictions, given that selective attention appears needed to resolve both of these stimulus-stimulus conflicts (i.e., through attending to an object's location independent of conflicting symbolic information—the direction in which all triangles point—and independent of conflicting trajectory information—the direction in which all arrows have appeared on a given trial). Finally, given the relatively large within-task conflict-adaptation effects observed for the Stroop-trajectory task in Experiment 1, we recruited sample sizes for the present across-task studies that were about half the size of that used in Experiment 2's investigation of across-task conflict adaptation with the spatial Stroop task.

Methods

In exchange for course credit, 36 undergraduates (22 male), aged 18 – 31 years ($M = 19.83$), participated in Experiment 3a, and 39 undergraduates (18 male), aged 18 – 23 years ($M = 20.10$), participated in Experiment 3b.

In Experiment 3a, half of participants used the “Ctrl” key at the keyboard’s bottom left corner for “down” responses and the number pad “-” key at the keyboard’s top right corner for “up” responses; the remainder used the “~” key at the keyboard’s top left corner for “up” responses and the number pad “Enter” key at the keyboard’s bottom right corner for “down” responses. In Experiment 3b, participants used the “left shift” key for left responses and the “right shift” key for right responses. Trials of the flanker/Simon task and the Stroop-Trajectory task were selected for presentation randomly with replacement across trials. Following computer-administered instructions and a 36-trial practice block, there were 8 blocks of 96 trials each. All trials began with a 352.94 msec fixation cue (“.”), centered horizontally and vertically, followed by a 23.53 msec blank screen.

Stroop-Trajectory Manipulation

The Stroop-trajectory manipulations were identical to those described in Experiment 1, with the following two exceptions. First, arrays always were aligned vertically in Experiment 3a and horizontally in Experiment 3b. Second, all stimulus elements were presented more briefly in this experiment than in Experiment 2 by a ratio of 11.76/13.33 msec, given this experiment’s use of an 85 MHz monitor refresh rate (rather than 75 MHz, as in Experiment 2). Accordingly, the first six black triangles as shown in Figure 2 were presented in successive intervals of 23.53 msec; 35.29 msec later, the final black triangle, along with the imperative gray triangle inside it,

was presented for 129.41 msec, after which the screen remained blank as the computer program awaited the participant's response.

Flanker Manipulation

Flanker stimuli (presented simultaneously in groups of three greater-than/less-than signs created in Calibri font, e.g., "<<<," or pointing fingers created in Microsoft Wingdings font, e.g., "☞☞☞," in images approximately 125 pixels high by 30 pixels wide when presented horizontally) were presented for 282 msec, after which the screen remained blank as the computer program awaited the participant's response. In Experiment 3a, flanker stimuli were oriented vertically, and participants pressed the response key whose vertical position on the keyboard corresponded to the direction in which the central character pointed (e.g., the top-left "~" or bottom-right "Enter" keys to indicate "up" or "down," respectively). In Experiment 3b, flanker stimuli were oriented horizontally, and participants pressed the response key whose horizontal position on the keyboard corresponded to the direction in which the central character pointed (e.g., the "left shift" or "right shift" key to indicate left or right, respectively). As determined through random selection with replacement across trials, the central character pointed in either the same direction (on congruent trials) or the opposite direction (on incongruent trials) as did those surrounding it.

Simon Manipulation

Comprising the Simon manipulation, flanker arrays were presented 20% of the distance from the monitor's horizontal midpoint to its left or right side (as determined randomly with replacement across trials). Independent of flanker congruence, stimulus arrays appeared on either the same or opposite side of the monitor as were participants' response keys. On congruent

Simon trials, response keys matched flanker arrays' horizontal monitor positions, as when an array appeared on the left side of the monitor, and a participant of Experiment 3a would press the top-left “~” key when the central character pointed up or a participant of Experiment 3b would press the “left shift” key when the central character pointed left. On incongruent Simon trials, response keys' horizontal keyboard positions mismatched stimulus arrays' horizontal monitor positions, as when an array appeared on the left side of the monitor, and a participant of Experiment 3a pressed the bottom-right “enter” key when the central character pointed down or a participant of Experiment 3b would press the “right shift” key when the central character pointed right.

Results

Latency data were not analyzed when erroneous responses were recorded at trials n or $n-1$ (14.93% of trials in Experiment 3a; 17.64% of trials in Experiment 3b) or when latencies exceeded 1000 msec (3.07% of remaining trials in Experiment 3a; 1.21% of remaining trials in Experiment 3b). Accuracy data were not analyzed when erroneous responses were recorded at trial $n-1$ (8.07% of trials in Experiment 3a and 10.02% of trials in Experiment 3b). As noted above, we precluded analyzing stimulus repetitions in these experiments by limiting across-task analyses to trial pairs as a function of Stroop-trajectory and flanker manipulations or Stroop-trajectory and Simon manipulations. That is, no across-task analyses examined trial pairs comprised of flanker and Simon manipulations, which were combined parametrically on the same trials and hence used the same stimuli; this insures that qualitatively distinct stimulus arrays always were presented at trials n relative to $n-1$ in all analyses of across-task effects. For the sake of completeness, we present in Tables 3a and 3b means and standard deviations of all possible across-task combinations across trials n and $n-1$ in Experiments 3a and 3b, respectively.

Because the Simon task was combined parametrically with the flanker task, analyzing the magnitude of conflict adaptation across task combinations required a different approach from that used in Experiment 2, which included a distinct manipulation of a single task at each trial. Accordingly, guided by our hypotheses stated above, we separately analyzed conflict-adaptation effects (a) across the Stroop-trajectory and flanker manipulations and (b) across the Stroop-trajectory and Simon manipulations. Lastly, by computing standardized indices of each conflict-adaptation effect for each participant, we compared the magnitude of conflict adaptation observed across the Stroop-trajectory and flanker manipulations to that observed across the Stroop-trajectory and Simon manipulations.

Conflict Adaptation across the Stroop-Trajectory and Flanker Manipulations

Response latency and accuracy rates were analyzed on the bases of the flanker and Stroop-trajectory manipulations at trials n and $n-1$, excluding trial pairs where either task was presented repeatedly across successive trials, in 2 (trial n task manipulation: flanker versus Stroop-trajectory) x 2 (trial n congruence) x 2 (trial $n-1$ congruence) repeated-measures ANOVAs. For the sake of brevity, we present only the most germane of the seven total main effects and interactions from each of the full ANOVAs.

For responses times, there were significant effects of trial n congruence in Experiment 3a ($F(1, 35) = 383.75, MSE = 704.16, p < .0001, \text{partial } \eta^2 = .92$) and in Experiment 3b ($F(1, 38) = 305.16, MSE = 1669.36, p < .0001, \text{partial } \eta^2 = .89$) and of trial $n-1$ congruence in Experiment 3a ($F(1, 35) = 25.17, MSE = 295.14, p < .0001, \text{partial } \eta^2 = .42$) and Experiment 3b, ($F(1, 38) = 33.93, MSE = 516.60, p < .0001, \text{partial } \eta^2 = .47$). The trial n task x trial n congruence interaction lay at the border of statistical significance in Experiment 3a ($F(1, 35) = 4.05, MSE = 273.68, p =$

.052, partial $\eta^2 = .10$) and was significant in Experiment 3b ($F(1, 38) = 33.38$, $MSE = 369.11$, $p < .0001$, partial $\eta^2 = .47$), suggesting that the flanker manipulation generally elicited a larger trial n congruence effect than did the Stroop-trajectory manipulation. Most important to this investigation, the trial n x trial $n-1$ congruence interaction was significant in Experiment 3a ($F(1, 35) = 4.29$, $MSE = 325.91$, $p < .05$, partial $\eta^2 = .11$) and in Experiment 3b ($F(1, 38) = 5.78$, $MSE = 567.57$, $p < .05$, partial $\eta^2 = .13$). These interactions were not moderated further by trial n task (in Experiment 3a, $F(1, 35) = 2.12$, $p > .15$; in Experiment 3b, $F = 0.07$), providing no suggestion that the magnitude of the conflict-adaptation effect on response time was significantly different when flanker trials were followed by Stroop-trajectory trials relative to when Stroop-trajectory trials were followed by flanker trials.

For accuracy rates, there were significant effects of trial n congruence in Experiment 3a ($F(1, 35) = 63.60$, $MSE = .004$, $p < .0001$, partial $\eta^2 = .65$) and in Experiment 3b ($F(1, 38) = 50.69$, $MSE = .013$, $p < .0001$, partial $\eta^2 = .57$) but not of trial $n-1$ congruence in Experiment 3a ($F = 0.20$) or in Experiment 3b, $F(1, 38) = 2.42$, $p > .12$). Moreover, there were significant trial n task x trial n congruence interactions in Experiment 3a ($F(1, 35) = 30.14$, $MSE = 0.004$, $p < .0001$, partial $\eta^2 = .39$) and in Experiment 3b ($F(1, 38) = 22.61$, $MSE = 0.008$, $p < .0001$, partial $\eta^2 = .37$), indicating that the flanker manipulation elicited a larger trial n congruence effect than did the Stroop-trajectory manipulation. Most important to this investigation, the trial n x trial $n-1$ congruence interaction was significant in Experiment 3a, $F(1, 35) = 22.55$, $MSE = 0.002$, $p < .0001$, partial $\eta^2 = .39$) and in Experiment 3b, $F(1, 38) = 38.57$, $MSE = 0.003$, $p < .0001$, partial $\eta^2 = .50$). The trial n task x trial n congruence x trial $n-1$ congruence interaction was not significant in Experiment 3a ($F = 0.55$), but it was significant in Experiment 3b ($F(1, 38) = 7.67$, $MSE = .003$, $p < .01$, partial $\eta^2 = .17$). Clarifying the nature of that three-way interaction in

Experiment 3b, separate ANOVAs showed that the two-way trial n x trial $n-1$ congruence interaction in Experiment 3b was statistically significant when flanker trials were followed by Stroop-trajectory trials ($F(1, 38) = 5.46, MSE = .002, p < .05, \text{partial } \eta^2 = .13$) and also when Stroop-trajectory trials were followed by flanker trials, but with a larger effect ($F(1, 38) = 29.68, MSE = 0.004, p < .0001, \text{partial } \eta^2 = .44$).

Conflict Adaptation across the Stroop-Trajectory and Simon Manipulations

Response latency and accuracy rates next were analyzed on the bases of the Simon and Stroop-trajectory manipulations at trials n and $n-1$, excluding trial pairs where either task was presented repeatedly across successive trials, in 2 (trial n task: Simon versus Stroop-trajectory) x 2 (trial n congruence) x 2 (trial $n-1$ congruence) repeated-measures ANOVAs.

For responses times, there were significant effects of trial n congruence in Experiment 3a ($F(1, 35) = 156.62, MSE = 843.69, p < .0001, \text{partial } \eta^2 = .82$) and in Experiment 3b ($F(1, 38) = 76.42, MSE = 1418.52, p < .0001, \text{partial } \eta^2 = .67$) and of trial $n-1$ congruence in Experiment 3a ($F(1, 35) = 9.82, MSE = 843.69, p < .0001, \text{partial } \eta^2 = .21$) and Experiment 3b, ($F(1, 38) = 30.39, MSE = 484.90, p < .0001, \text{partial } \eta^2 = .44$). Moreover, the trial n task x trial n congruence was significant in Experiment 3a ($F(1, 35) = 73.62, MSE = 477.64, p = .0001, \text{partial } \eta^2 = .68$) and in Experiment 3b ($F(1, 38) = 33.38, MSE = 369.11, p < .0001, \text{partial } \eta^2 = .47$), indicating that the Stroop-trajectory manipulation elicited a larger trial n congruence effect than did the Simon manipulation (see Tables 3a and 3b). Most important to this investigation, the trial n x trial $n-1$ congruence interaction was not significant in Experiment 3a ($F = 0.70$) or in Experiment 3b ($F = 0.04$). The three-way trial n task x trial n congruence x trial $n-1$ congruence interaction also was not significant in Experiment 3a ($F = 0.21$) or in Experiment 3b ($F = 0.63$).

For accuracy rates, there were significant effects of trial n congruence in Experiment 3a ($F(1, 35) = 66.65, MSE = .0056, p < .0001, \text{partial } \eta^2 = .66$) and in Experiment 3b ($F(1, 38) = 63.04, MSE = .012, p < .0001, \text{partial } \eta^2 = .62$) but not of trial $n-1$ congruence in Experiment 3a ($F = 0.96$) or in Experiment 3b, ($F = 0.75$). Moreover, there were significant trial n task x trial n congruence interactions in Experiment 3a ($F(1, 35) = 25.78, MSE = .002, p < .0001, \text{partial } \eta^2 = .42$) and in Experiment 3b ($F(1, 38) = 16.61, MSE = .007, p < .0001, \text{partial } \eta^2 = .30$), indicating that the Stroop-trajectory manipulation elicited a larger trial n congruence effect than did the Simon manipulation. Most important to this investigation, the trial n x trial $n-1$ congruence interaction was not significant in Experiment 3a ($F = 0.57$) or in Experiment 3b ($F(1, 38) = 2.13, p > .15$). The three-way trial n task x trial n congruence x trial $n-1$ congruence interaction was not significant in Experiment 3a ($F = 0.21$), but it was significant in Experiment 3b ($F(1, 38) = 12.08, MSE = .002, p < .01, \text{partial } \eta^2 = .24$). Clarifying the nature of that latter three-way interaction, separate ANOVAs showed that the two-way trial n x trial $n-1$ congruence interaction in Experiment 3b was not statistically significant when Stroop-trajectory trials followed Simon trials ($F = 0.88$), but it was significant—and in the opposite direction to that predicted by the conflict-adaptation hypothesis—when Simon trials followed Stroop-trajectory trials ($F(1, 38) = 9.92, MSE = .003, p < .02, \text{partial } \eta^2 = .15$). That is, the Simon trial n congruence effect was larger following trial $n-1$ incongruent Stroop-trajectory trials ($F(1, 38) = 23.17, MSE = .007, p < .0001, \text{partial } \eta^2 = .38$) than it was following congruent Stroop-trajectory trials ($F(1, 38) = 6.93, MSE = .004, p < .01, \text{partial } \eta^2 = .21$).

Comparison of Conflict-Adaptation Effects' Magnitudes

To compare the magnitude of conflict adaptation observed across the Stroop-trajectory and flanker manipulations to that observed across Stroop-trajectory and Simon manipulations,

we computed a single index of each effect for response time and accuracy. For each participant, we subtracted response-time and accuracy means when the level of information-processing conflict at trial n was consistent with that at trial $n-1$ (i.e., incongruent following incongruent trials and congruent following congruent trials) from response-time and accuracy means when the level of information-processing conflict at trial n was inconsistent with that at trial $n-1$ (i.e., incongruent following congruent trials and congruent following incongruent trials). We next divided each of these difference scores by its standard deviation within the sample, converting each of the trial n x trial $n-1$ congruence interactions reported in the preceding sections into a single conflict-adaptation score in the metric of the effect size Cohen's d . Conflict adaptation is indicated by positive scores on the response-time index (slower responses when the level of information-processing conflict at trial n was inconsistent rather than consistent with that at trial $n-1$) and by negative scores on the accuracy index (less accurate responses when the level of information-processing conflict at trial n was inconsistent rather than consistent with that at trial $n-1$).

For response times, in Experiments 3a and 3b the conflict-adaptation effects across the Stroop-trajectory and flanker manipulations ($M = 0.35$ standard deviations; and $M = 0.38$ standard deviations, respectively) were not significantly larger in magnitude ($F = 0.71$; and $F(1, 38) = 2.09, p > .15$, respectively) than were the conflict-adaptation effects across the Stroop-trajectory and Simon manipulations ($M = .14$ standard deviations, and $M = 0.03$ standard deviations, respectively). For accuracy, in Experiments 3a and 3b the conflict-adaptation effects across the Stroop-trajectory and flanker manipulations ($M = -0.79$ standard deviations; and $M = -0.99$ standard deviations, respectively) were significantly larger in magnitude ($F(1, 35) = 9.82, MSE = .0007, p < .01, \text{partial } \eta^2 = .22$; and $F(1, 38) = 17.43, MSE = .002, p < .001, \text{partial } \eta^2 =$

.31, respectively) than were the conflict-adaptation effects across the Stroop-trajectory and Simon manipulations ($M = -0.13$ standard deviations, and $M = 0.23$ standard deviations, respectively).

Tests of within-Task Sequential Effects Pertaining to the Simon Manipulation

Because conflict-adaptation effects were greater across the Stroop-trajectory and flanker manipulations than across the Stroop-trajectory and Simon manipulations, further analysis of data pertaining to the Simon task is needed to evaluate the sensitivity of the Simon effect, as realized in the present studies, to contextual modulation. We therefore analyzed response latency and accuracy rates on the bases of Simon manipulations at trials n and $n-1$ (which contained no exact stimulus repetitions, given the alternating classes of flanker stimuli, comprised of greater-than/less-than signs and pointing fingers). There were significant main effects of trial n Simon congruence on accuracy in Experiment 3a ($F(1, 35) = 25.15$, $MSE = 367.05$, $p < .0001$, partial $\eta^2 = .42$) and in Experiment 3b ($F(1, 38) = 14.76$, $MSE = 0.003$, $p < .001$, partial $\eta^2 = .28$) and on response time in Experiment 3a ($F(1, 35) = 22.19$, $MSE = 341.07$, $p < .0001$, partial $\eta^2 = .41$) but not in Experiment 3b ($F = 0.36$). Most importantly, there were significant trial $n \times$ trial $n-1$ congruence interactions on accuracy in Experiment 3a ($F(1, 35) = 12.40$, $MSE = 0.002$, $p < .01$, partial $\eta^2 = .26$) and in Experiment 3b, ($F(1, 38) = 19.05$, $MSE = 0.002$, $p < .0001$, partial $\eta^2 = .33$) and on response time in Experiment 3a ($F(1, 35) = 58.34$, $MSE = 336.68$, $p < .0001$, partial $\eta^2 = .43$) and in Experiment 3b ($F(1, 38) = 29.26$, $MSE = 509.16$, $p < .0001$, partial $\eta^2 = .44$).

Discussion

Because resolving information-processing conflict arising as a result of flanker and Stroop-trajectory manipulations (both of which generate conflict between stimulus elements)

depends on selective attention, whereas resolving information-processing conflict arising as a result of Simon manipulations (which generate conflict between stimulus and response elements) does not, our across-task application of conflict-monitoring theory provided a basis for predicting conflict-adaptation effects across trials comprised of Stroop-trajectory and flanker manipulations but not across trials comprised of Stroop-trajectory and Simon manipulations. As summarized in Figure 5 (which combines data from Experiments 3a and 3b), the present results supported that prediction, particularly with regard to accuracy, where the magnitude of conflict-adaptation effects across the Stroop-trajectory and flanker manipulations was significantly greater than that observed across the Stroop-trajectory and Simon manipulations. These results provide the first evidence of which we are aware that the magnitude of across-task conflict adaptation effects is determined by the nature of information-processing conflicts encountered within separate task manipulations, thereby supporting the prediction that engaging a mechanism of cognitive control will facilitate resolving subsequent information-processing conflicts to which that mechanism is applicable.

Two limitations of the present experiment also warrant discussion. First, the trial n Simon interference effects were smaller in magnitude than were the trial n flanker and Stroop-trajectory interference effects. The relatively small magnitude of the Simon effects raises the possibility that the three tasks' elicitation of different magnitudes of conflict, rather than their recruitment of different mechanisms of cognitive-control, could help explain the stronger conflict-adaptation effects observed across the Stroop-trajectory and flanker manipulations than across the Stroop-trajectory and Simon manipulations. In our view, this alternative explanation is unlikely to fully explain our findings. As reported above, the trial n flanker interference effects generally were larger than were the trial n Stroop-trajectory interference effects for both response time and

accuracy; despite those differences in trial n interference, conflict-adaptation effects on both response time and accuracy were observed across those two tasks. Nevertheless, future examinations of across-task conflict adaptation would benefit through efforts to equate the magnitude of trial n interference effects on the constituent tasks.

Second, examination of within-task sequential effects as a function of the Simon manipulation, while precluding exact stimulus repetitions, did include conceptual repetitions (e.g., “<<<” on right side of monitor; followed by “☞☞☞” on right side of monitor). Conceptual repetitions of this sort can facilitate behavioral efficiency (Gordon & Irwin, 2000; Kühn, Keizer, Colzato, Rombouts & Hommel, 2011). Accordingly, the processes underlying the presently reported within-task sequential effects on the Simon task remain unclear. This limitation notwithstanding, the significant trial n x trial $n-1$ Simon congruence interactions on response time and accuracy establish, in the least, that the Simon effect realized here was sensitive to contextual modulation. This evidence sharpens interpretation of the across-task findings, indicating that the relative lack of conflict-adaptation effects across the Stroop-trajectory and Simon manipulations does not likely reflect a general lack of sensitivity of the Simon manipulation to *any* contextual variables. Moreover, the relative absence of conflict adaptation across the Simon and Stroop-trajectory tasks in the present report also is consistent with previous reports of a general lack of conflict adaptation across Simon and selective-attention tasks (e.g., Akçay & Hazeltine, 2011; Funes et al., 2010; Wendt et al., 2006; but see Kunde & Wuhr, 2006, for an interesting exception) and with evidence that different neural processes are related to resolving conflicts among stimulus elements relative to resolving conflicts between stimulus and response elements (Soutschek, Taylor, Muller, & Schubert, 2013).

General Discussion

Given its parsimonious solution to the problem of how cognitive control itself is controlled, Botvinick and colleagues' (2001) conflict-monitoring theory has attracted considerable attention (as indicated by their paper's greater than 3000 citations in Google Scholar at the time of this writing) and has motivated research programs spanning many areas of psychology and cognitive neuroscience. Commensurate with its outsized influence, that theory also has attracted vigorous methodological critiques of the experiments that have purported to support it. The present work developed novel tests of the conflict-adaptation hypothesis that avoided methodological limitations of previous work. In three experiments that examined sequential effects within a selective-attention task (the Stroop-trajectory task, Experiment 1) and across several selective-attention tasks (a flanker task and two variants of spatial Stroop tasks, Experiment 2; a flanker task and a Stroop-trajectory task, Experiment 3), significant conflict-adaptation effects were observed despite that fact that each experiment precluded analyzing stimulus repetitions without creating any sort of confounds among any stimulus or trial characteristics.

We hope that these findings contribute to advancing beyond examining whether or not conflict-adaptation effects exist to elucidating the processes by which conflict-adaptation effects may be observed under some conditions but not others. Toward that end, we found in Experiment 3 evidence of significantly larger conflict-adaptation effects on response accuracy across tasks that appear to depend on the same cognitive-control mechanism than across tasks that do not. Accordingly, the findings fit with the core logic of Botvinick and colleagues' (2001) conflict-monitoring theory, in that engaging a mechanism of cognitive control, such as selective attention, appears to facilitate resolving subsequent information-processing conflicts, whether on

the same task or a different task, to which that mechanism is applicable. Future work is needed to evaluate further this conclusion, which is based on the only existing investigation of which we are aware that has examined conflict adaptation across three separate tasks within a single experiment, thereby allowing a comparison of the amount of conflict adaptation observed across different task combinations.

A broader conclusion to draw from the present investigation is that conflict-adaptation effects in cognitive-control tasks likely reflect multiple underlying processes. In Experiment 3b, the effect of Simon congruence on response accuracy was significantly *larger* following incongruent Stroop-trajectory trials than it was following congruent Stroop-trajectory trials. This reversal of the typically observed conflict-adaptation effect is consistent with Verguts and Notebaert's (2009) associative learning model, which states that encountering information-processing conflict increases attention to task-specific stimulus and response dimensions. In accord with that model, refocusing attention on the stimulus-response contingencies of the Stroop-trajectory task in Experiment 3b entails attending to the left-right location of a stimulus and then responding with a left/right response, which should impede performance accuracy on incongruent Simon trials (where left/right stimulus locations mismatch participants' left/right responses) relative to congruent Simon trials (where left/right stimulus locations match participants' left/right responses), as indeed was observed. On the other hand, findings from the same experiment showed that conflict-adaptation effects also generalize across tasks with different stimulus-response contingencies (the flanker and Stroop-trajectory tasks) but that appear to depend on a common underlying mechanism of cognitive control.

Evidence that information-processing conflicts can (a) increase attention to task-specific stimulus and response dimensions while also (b) facilitating resolving information-processing

conflict on tasks with different rules and stimulus dimensions thus suggests the need for future work to examine the conditions under which these potentially opposing processes most strongly guide cognition and behavior. In this vein, the present evidence of across-task conflict-adaptation may appear inconsistent with previous evidence of conflict adaptation across task alternations that used consistent but not inconsistent stimulus dimensions (Notebaert & Verguts, 2008) and across alternate versions of a single task that used consistent but not inconsistent response mappings (Braem et al., 2011). Assuming that conflict adaptation may reflect multiple underlying processes, however, it is possible that methodological factors in any particular experiment may facilitate the operation of some underlying processes more than others. Accordingly, it is interesting to note that the aforementioned studies found very large effects on response time (i.e., at least twice as large as trial n congruence effects in the same studies) of switches in stimulus dimensions (Notebaert & Verguts, 2008) and in response mappings (Braem et al., 2011) across trials n and $n - 1$. These considerations suggest the hypothesis that, all else equal, conflict-adaptation effects should be most likely to transcend particular stimulus and response dimensions when the disruptive effects of switches in stimulus and/or response dimensions across trials n and $n - 1$ are minimized, thereby attenuating the need to refocus attention on those dimensions following information-processing conflicts. Systematic tests of that hypothesis may further the present paper's incipient progress toward integrating the presently rapidly accumulating methodological and theoretical advances that have been made from the standpoints of multiple theoretical perspectives on the contextual modulation of cognitive control.

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Authors' Note

Antonio L. Freitas and Sheri L. Clark, State University of New York at Stony Brook.

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Table 1. Mean response times (RT) and proportions of correct responses (Cor.) in trial n x trial $n-1$ combinations of a Stroop-trajectory task Experiment 1, limited to trial pairs in which stimulus arrays appeared at different (vertical or horizontal) orientations at trials n and $n-1$ (thereby precluding exact stimulus repetitions across trials n and $n-1$), SD in parentheses, $N = 15$.

Trial n Congruence		Trial $n-1$ Congruence	
		Congruent	Incongruent
Congruent	RT	362 (77)	377 (70)
Incongruent	RT	397 (54)	388 (57)
Congruent	Cor.	.986 (.04)	.981 (.03)
Incongruent	Cor.	.900 (.10)	.944 (.06)

Table 2. Mean response times (RT) and proportions of correct responses (Cor.) in trial n x trial $n-1$ combinations of Experiment 2, SD in parentheses, $N = 80$.

Trial n Task & Congruence			Trial $n-1$ Task & Congruence			
			Spatial Stroop I		Spatial Stroop II	
			Congruent	Incongruent	Congruent	Incongruent
Flanker	Congruent	RT	454 (72)	485 (78)	454 (74)	471 (76)
	Incongruent	RT	501 (72)	518 (85)	502 (70)	510 (77)
	Congruent	Cor.	.957 (.10)	.921 (.09)	.947 (.09)	.906 (.12)
	Incongruent	Cor.	.930 (.10)	.922 (.11)	.924 (.11)	.900 (.11)
Spatial Stroop I			Flanker		Spatial Stroop II	
			Congruent	Incongruent	Congruent	Incongruent
Spatial Stroop I	Congruent	RT	485 (84)	492 (84)	487 (78)	506 (88)
	Incongruent	RT	555 (89)	556 (81)	552 (88)	545 (88)
	Congruent	Cor.	.966 (.07)	.976 (.04)	.959 (.06)	.942 (.07)
	Incongruent	Cor.	.842 (.13)	.869 (.11)	.830 (.12)	.893 (.10)
Spatial Stroop II			Flanker		Spatial Stroop I	
			Congruent	Incongruent	Congruent	Incongruent
Spatial Stroop II	Congruent	RT	433 (76)	437 (79)	433 (74)	450 (85)
	Incongruent	RT	492 (89)	494 (90)	493 (91)	479 (88)
	Congruent	Cor.	.991 (.03)	.986 (.06)	.988 (.04)	.986 (.03)
	Incongruent	Cor.	.890 (.09)	.909 (.09)	.899 (.09)	.957 (.05)

Table 3a. Mean response times (RT, in msec) and proportions of correct responses (Cor.) in trial n x trial $n-1$ combinations of Experiment 3a, with standard deviations in parentheses ($N = 36$).

		Trial $n-1$ Manipulation and Stimulus-Array Congruence				
Trial n Manipulation	Stimulus-Array Congruence	D.V.	Stroop-Trajectory		Simon	
			Congruent	Incongruent	Congruent	Incongruent
Flanker	Congruent	RT	591 (70)	610 (73)	559 (67)	561 (65)
	Incongruent	RT	653 (66)	661 (66)	628 (70)	629 (65)
	Congruent	Acc.	.946 (.054)	.903 (.079)	.933 (.054)	.955 (.050)
	Incongruent	Acc.	.898 (.072)	.907 (.074)	.876 (.095)	.911 (.090)
		Flanker		Simon		
			Congruent	Incongruent	Congruent	Incongruent
Directional	Congruent	RT	425 (68)	435 (69)	430 (68)	430 (69)
Stroop	Incongruent	RT	493 (69)	495 (71)	496 (72)	491 (68)
	Congruent	Acc.	.975 (.033)	.967 (.048)	.969 (.039)	.973 (.041)
	Incongruent	Acc.	.856 (.096)	.888 (.080)	.865 (.095)	.876 (.082)
		Flanker		Stroop-Trajectory		
			Congruent	Incongruent	Congruent	Incongruent
Simon	Congruent	RT	577 (66)	592 (63)	611 (70)	627 (68)
	Incongruent	RT	600 (65)	604 (70)	631 (68)	647 (74)
	Congruent	Acc.	.934 (.066)	.947 (.053)	.945 (.045)	.923 (.055)
	Incongruent	Acc.	.891 (.095)	.900 (.077)	.898 (.071)	.884 (.100)

Table 3b. Mean response times (RT, in msec) and proportions of correct responses (Cor.) in trial n x trial $n-1$ combinations of Experiment 3b, with standard deviations in parentheses ($N = 39$).

		Trial $n-1$ Manipulation and Stimulus-Array Congruence					
Trial n Manipulation	Stimulus-Array Congruence	D.V.	Stroop-Trajectory		Simon		
			Congruent	Incongruent	Congruent	Incongruent	
Flanker	Congruent	RT	537 (79)	567 (95)	506 (77)	521 (79)	
	Incongruent	RT	638 (101)	653 (84)	618 (80)	612 (81)	
	Congruent	Cor.	.946 (.09)	.860 (.13)	.945 (.08)	.945 (.07)	
	Incongruent	Cor.	.849 (.16)	.870 (.11)	.855 (.13)	.875 (.15)	
		Flanker		Simon			
			Congruent	Incongruent	Congruent	Incongruent	
Directional	Congruent	RT	372 (73)	385 (75)	373 (69)	383 (79)	
Stroop	Incongruent	RT	446 (71)	447 (63)	444 (67)	448 (68)	
	Congruent	Cor.	.969 (.11)	.965 (.10)	.962 (.11)	.974 (.09)	
	Incongruent	Cor.	.815 (.15)	.845 (.14)	.817 (.15)	.845 (.15)	
		Flanker		Stroop-Trajectory			
			Congruent	Incongruent	Congruent	Incongruent	
Simon	Congruent	RT	558 (82)	563 (72)	585 (84)	603 (82)	
	Incongruent	RT	564 (76)	563 (96)	589 (87)	612 (113)	
	Congruent	Cor.	.907 (.08)	.938 (.08)	.913 (.11)	.912 (.08)	
	Incongruent	Cor.	.877 (.14)	.898 (.10)	.882 (.13)	.820 (.15)	

Table 4. Summary of conflict-adaptation effects in Experiments 1 – 3, expressed in the metric of Cohen’s d (averaged response time and accuracy on incongruent following congruent trials and congruent following incongruent trials minus averaged response time and accuracy on incongruent following incongruent trials and congruent following congruent trials, divided by the standard deviation of the difference score). Conflict adaptation is indicated by positive response-time values and negative accuracy values (i.e., slower and less accurate responses when the degree of information-processing conflict encountered at trial n mismatches rather than matches that encountered at trial $n-1$).

<u>Experiment</u>	<u>Task(s) at Trials n & $n-1$</u>	<u>Effect Size (M/SD)</u>	
		<u>Response Time</u>	<u>Accuracy</u>
1	Stoop-Trajectory (alternate orientations)	1.06	-0.77
2	Spatial Stroop (alternate variants)	0.56	-0.96
2	Flanker & Spatial Stroop	0.24	-0.57
3a	Stoop-Trajectory & Flanker	0.35	-0.79
3a	Stoop-Trajectory & Simon	0.14	-0.13
3b	Stoop-Trajectory & Flanker	0.62	-0.52
3b	Stoop-Trajectory & Simon	0.03	0.23

Note: As reported in the main text, each of the above effects was significant statistically ($ps < .05$) *except* for the response-time and accuracy effects across the Stroop-trajectory and Simon tasks (neither of which was significant statistically in either Experiment 3a or Experiment 3b).

Figure Captions

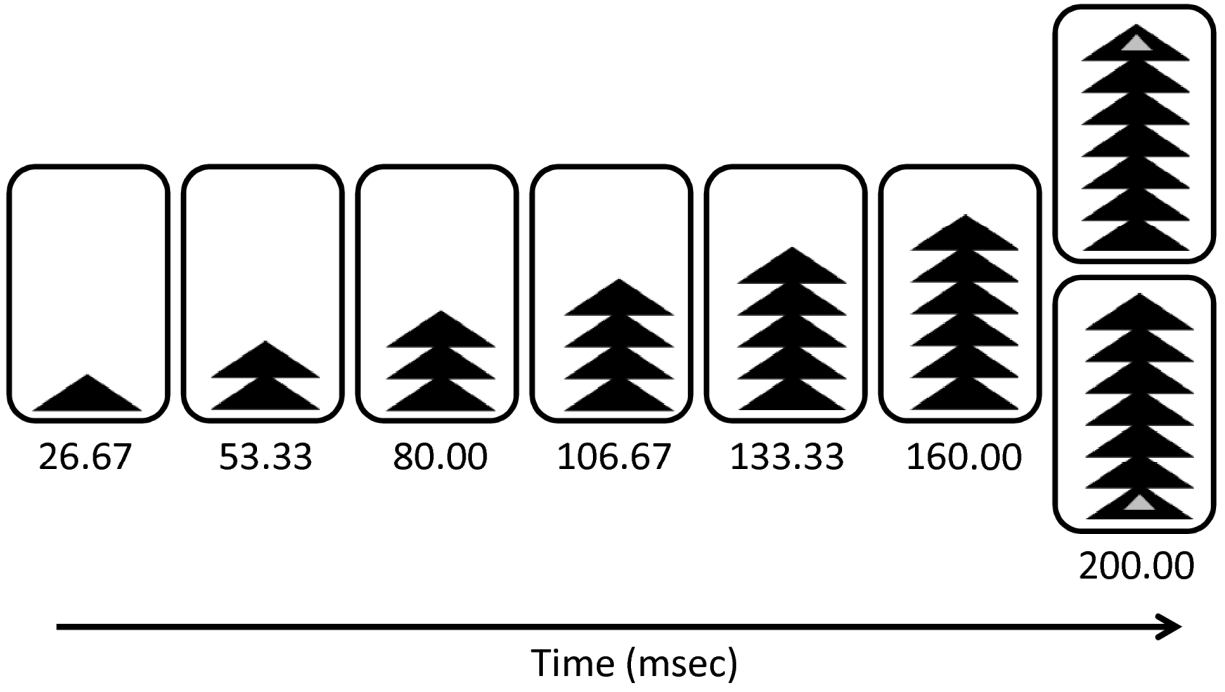
Figure 1. Illustration of an upward-pointing trial of the Stroop-trajectory task, in which the participant indicates the location of the smaller gray triangle among congruently (top right) or incongruently (bottom right) arrayed larger black triangles. For right-pointing, downward-pointing, and left-pointing trials, the stimulus arrays were rotated 90°, 180°, and 270°, respectively, and conflict-adaptation analyses were limited to trial pairs in which stimulus arrays appeared at different (vertical or horizontal) orientations at trials n and $n-1$, thereby precluding from analyses exact stimulus repetitions across trials n and $n-1$ in Experiments 2.










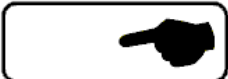








Figure 2. The six different mappings of stimulus types (pointing fingers, block arrows, line arrows) to the three tasks (spatial Stroop I, flanker, and spatial Stroop II) used in Experiment 3. Note that these are examples (with the top three rows exemplifying congruent/left and the bottom three rows exemplifying incongruent/right) of the broader stimulus sets that orthogonally combined all trial properties.

Figure 3. Conflict-adaptation effects on accuracy (top) and response time (bottom), combined across the flanker, spatial Stroop I, and spatial Stroop II tasks (with no task repetitions across trials n and $n-1$), Experiment 3, $N=80$.

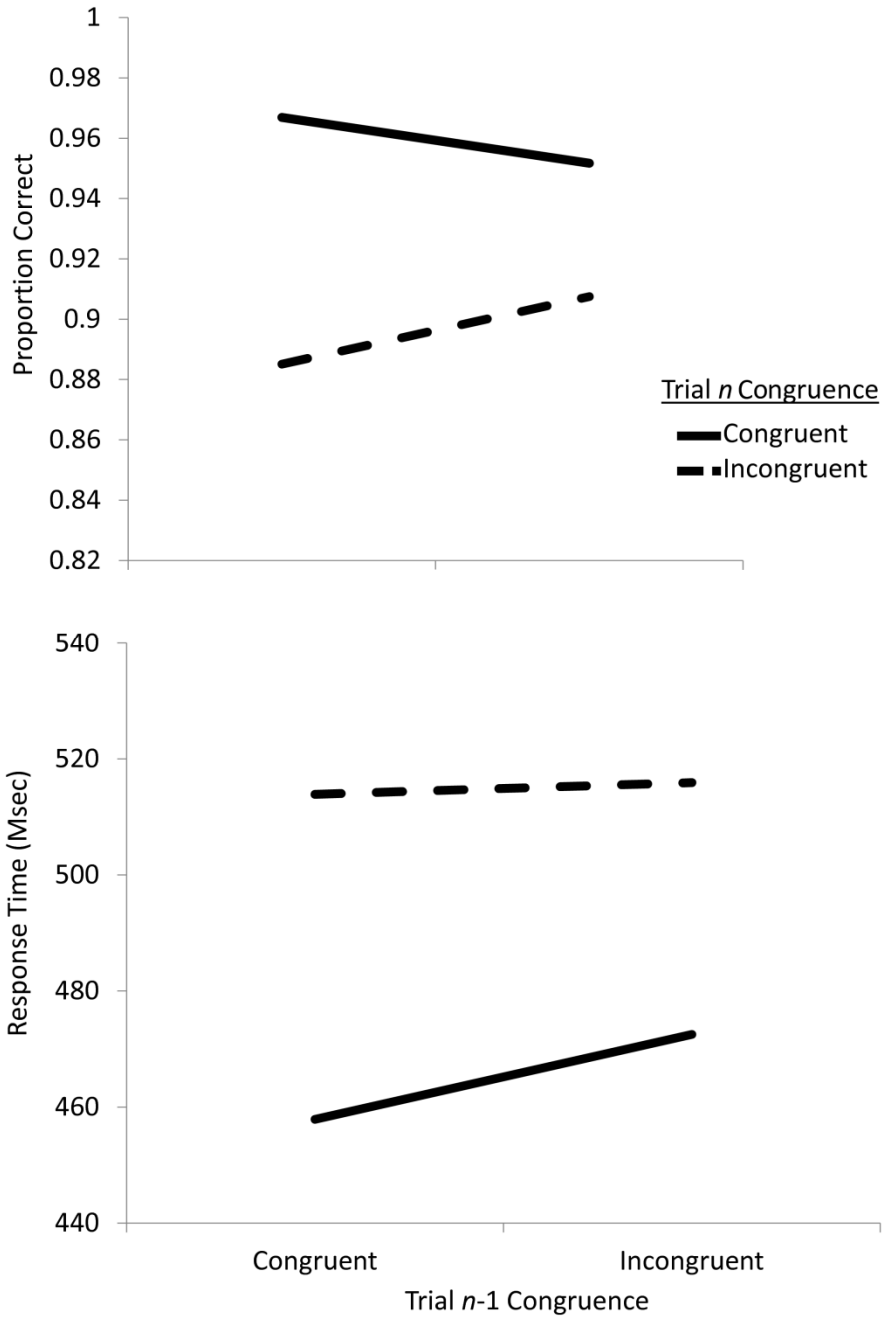
Figure 4. Illustration of trials interspersing flanker/Simon manipulations with Stroop-trajectory manipulations, Experiments 3a and 3b.

Figure 5. Conflict-adaptation effects on accuracy (top) and response time (bottom), across the Stroop-trajectory and flanker manipulations (left) and across Stroop-trajectory and Simon manipulations (right), data combined from Experiments 3a and 3b.



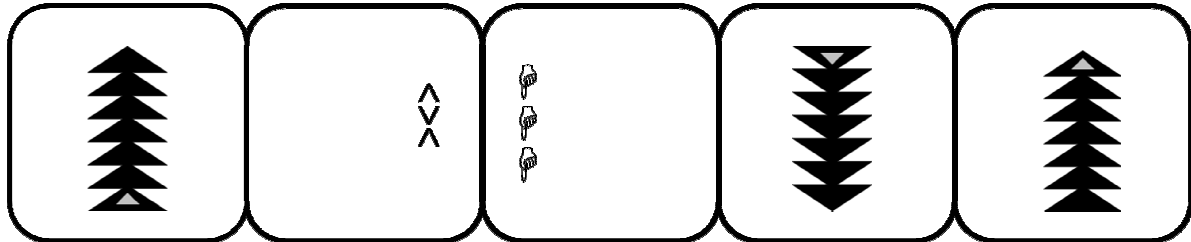
<u>Stimulus Grouping</u>	<u>Spatial Stroop I</u>	<u>Flanker</u>	<u>Spatial Stroop II</u>
1			
2			
3			
4			
5			
6			

Conflict Adaptation Within and Across Tasks 56



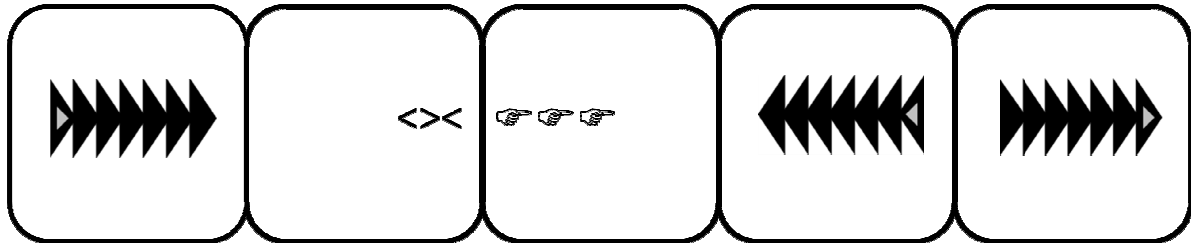
Exp. 3a (Press upper left key for up, lower right key for down.)

Stroop Inc. Flank Inc., Flank Con., Stroop Inc. Stroop Con.
 Simon Con. Simon Inc.



Exp. 3b (Press left key for left, right key for right.)

Stroop Inc. Flank Inc., Flank Con., Stroop Inc. Stroop Con.
 Simon Con. Simon Inc.



Trials



