Conflict Adaptation Depends on Task Structure

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The dependence of the Simon effect on the correspondence of the previous trial can be explained by the conflict-monitoring theory, which holds that a control system adjusts automatic activation from irrelevant stimulus information (conflict adaptation) on the basis of the congruency of the previous trial. The authors report on 4 experiments showing that conflict adaptation is mediated by task structure as well as previous congruency. Experiment 1 showed that for 2 completely segregated sets of stimulus–response pairs there was no conflict adaptation across sets. However, if the stimulus sets overlapped for 2 separate response sets, conflict adaptation could be observed across the response sets. Experiment 2 showed that this effect was not due to the use of stimulus–response sets lateralized to 1 hemisphere each. Experiment 3 showed that if response sets are common for 2 separate stimulus sets, then conflict adaptation can again be observed across sets. Finally, Experiment 4 showed local control effects in the absence of confounding feature-overlap effects. These results indicate that deployment of control as evidenced by conflict adaptation reflects task structure.

Keywords: conflict monitoring, Simon effect, cognitive control, task structure, executive control

The environment is usually brimming with multiple sources of potentially relevant and useful information. The appropriate selection of information for the control of action is a feat the cognitive system achieves readily. However, how the cognitive system is able to do this is not well understood. The control of action is often attributed to a system called the central executive (e.g., Logan, 2003; Monsell & Driver, 2000; Norman & Shallice, 1986). To understand how the central executive works without resorting to a homunculus, researchers seek to isolate components of this system and identify multiple smaller processes, each performing a specific function of a limited scope (Monsell & Driver, 2000). The idea is that an army of smaller, dumber processes working together can give rise to intelligent behavior, which on the surface looks like the act of a unitary process.

Jonathan Cohen and colleagues (Botvinick, Braver, Barch, Carter, & Cohen, 2001; MacDonald, Cohen, Stenger, & Carter, 2000; Yeung, Botvinick, & Cohen, 2004) proposed one approach for dividing executive processes into smaller components. Focusing on the dynamic effects of irrelevant information during response conflict tasks, they examined how the cognitive system knows when to engage in more controlled processing. In their view, conflict is a useful source of information about when to recruit control. Conflict occurs whenever irrelevant information from the environment interferes with the processing of the relevant information and thus provides a good indicator of need for more controlled processing.

For example, conflict can occur in tasks in which participants are asked to respond to a nonspatial feature of the stimulus (e.g., color) with spatially defined responses (e.g., left vs. right key in a typical two-choice task). The stimulus also varies on the spatial dimension so that its location can either correspond or not correspond with the correct response. Participants are instructed to ignore the stimulus location and respond on the basis of the relevant stimulus feature. Despite this, reaction times (RTs) are shorter on trials when the stimulus location corresponds with the correct response location (corresponding trials) as compared to trials when the stimulus location does not correspond with the response location (noncorresponding trials). That is, people are faster in pressing a left key in response to, say, the color of the stimulus, when the stimulus appears on the left side of a stimulus display than when it appears on the right side. The RT advantage for corresponding trials compared to noncorresponding trials is termed the *Simon effect*.

One popular account of the Simon effect that is consistent with Botvinick et al.'s (2001) model is that the irrelevant stimulus information automatically activates the response that corresponds to that location. That is, the location of the stimulus primes the response through a direct pathway that does not engage the usual response selection processes. When the corresponding response is not the same as the correct one, it conflicts with the correct response, and the conflict takes some time to be resolved. Alternatively, if the automatically activated response and the correct response are the same, facilitation can occur.

The role of conflict in recruiting control is supported by the finding that the Simon effect gets diminished following noncorresponding trials (Stürmer, Leuthold, Soetens, Schröter, & Sommer, 2002). Irrelevant stimulus information seems to have less of an effect if the immediately preceding trial was a noncorresponding trial. This dependency on the preceding trial is termed *sequential modulation* and is seen in a wide range of response conflict tasks,

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including the Stroop task (Kerns et al., 2004) and the flanker task (Gratton, Coles, & Donchin, 1992).

Botvinick et al. (2001) accounted for sequential modulations in response conflict tasks by hypothesizing a conflict-monitoring module that monitors the activation of competing responses. When competing responses are simultaneously activated (on a noncorresponding trial), the conflict between responses is detected, and the monitoring module activates a control system. To resolve the conflict between the competing responses, the control system in turn activates an attention module that increases the influence of the relevant stimulus dimension relative to the irrelevant dimension. This attentional state persists so that the irrelevant information has a reduced influence on response selection processes on the subsequent trial. This simple model of sequential modulations is automatic in the sense that no intervention from a higher cognitive process is needed once the connection between the conflict monitor and the attention module is set up.

Conflict Monitoring Versus Feature Integration

Although this model is well-supported by behavioral, electrophysiological, and neuroimaging data (the latter indicating that conflict monitoring might be a function of the anterior cingulate cortex), some researchers have argued that sequential modulations need not be attributed to a control process. For example, Mayr, Awh, and Laurey (2003) proposed that sequential modulations can be attributed solely to response (or target) priming. In their study, when they eliminated sequences with response repetitions, they did not see any sequential modulations in the flanker task, suggesting that response priming is sufficient to explain sequential modulations.

Hommel, Proctor, and Vu (2004) proposed a more detailed theoretical account that shared the central tenet that control processes as proposed in the conflict-monitoring account are not necessary to account for sequential modulations. They held that sequential modulations in the Simon effect could be attributed to the operation of a feature-integration process (Hommel, 1998). This feature-integration process would lead to binding of features of the stimulus–response episode into a representation termed an *event file*. When some but not all features of the stimulus–response episode repeat from the previous trial, the repeating features automatically activate other features in the event file from the previous trial. These activated features compete with the current features and lead to longer RTs. When every feature repeats or alternates, there is no such problem, because no competing features are activated. When two-choice tasks are used, feature overlap between the previous trial and the current trial would decrease the Simon effect after noncorresponding trials and increase it after corresponding trials. Thus, the feature-integration process could account for the sequential effects accounted for by the control model, without the need of conflict detection or control.

To test these alternative hypotheses, researchers have looked at sequential modulations in sequences where neither the stimulus nor the response was repeated. In these *complete-change sequences,* priming or feature integration cannot play a role; hence, any sequential modulations should be attributed to a control process. In one study of the Stroop effect, Notebaert, Gevers, Verbruggen, and Liefooghe (2006) found that both priming (or feature integration) and conflict monitoring played a role in sequential

modulations (see also Kerns et al., 2004). Because these researchers looked at sequential modulations in trial sequences in which none of the features repeated from the previous trials (complete-change sequences), the modulations could not have stemmed from the priming or integration of specific features.

In a similar vein, Akçay and Hazeltine (2007) found significant sequential modulations in a four-choice Simon task looking only at complete-change sequences. Furthermore, they carried out a test of feature-integration theory. As explained earlier, the feature-integration theory holds that partial repetitions, in which some of the features repeat and the rest change, should suffer from interference caused by the irrelevant information in the event file (Hommel, 2005; Hommel et al., 2004). Thus, feature-integration theory predicts that both complete repetitions and alternations should be faster than partial repetitions, where only one of the features repeat. This prediction was not borne out by the data. When the researchers controlled for the effects of correspondence on current and previous trials, the effect of stimulus location repetition and response repetition was additive, with complete alternation trials being the slowest sequences. This test of feature-integration theory in a task with more than two values of the features suggests that the basic model of feature integration warrants more testing.

To summarize, control adaptation has been demonstrated repeatedly for many types of conflict. Although repetitions of features account for some of the sequential effects, they are insufficient to account for all of the effects observed, thus establishing the existence of conflict adaptation (see earlier references and also Notebaert & Verguts, in press; Ullsperger, Bylsma, & Botvinick, 2005; Wühr, 2005).

Conflict Monitoring and the Scope of Control

Because the model proposed by Botvinick et al. (2001) accounts for patterns of data across a wide range of experimental tasks, conflict monitoring is generally envisioned as a general purpose module. This conceptualization is consistent with the idea that conflict monitoring is a function of the anterior cingulate cortex, which is activated in a variety of contexts involving response conflict (Botvinick et al., 2001).

In addition to the multitude of studies examining whether sequential effects can be explained without conflict monitoring, recent work has examined the scope of control recruited by conflict (Fernandez-Duque & Knight, in press; Kiesel, Kunde, & Hoffman, 2006; Kunde & Wühr, 2006; Stürmer, Seiss, & Leuthold, 2005). One straightforward possibility is that after the detection of conflict, control is imposed in a global fashion. An incompatible trial might signal to the system that the influence of automatic activation should be minimized and controlled and that rule-based response selection should be weighted more heavily. As reviewed earlier, several studies have indicated that an incompatible trial decreases compatibility effects on a subsequent trial even when no features are repeated (Akçay & Hazeltine, 2007; Kerns et al., 2004; Notebaert et al., 2006), suggesting that the effects of control are not restricted to a particular irrelevant feature. In summary, according to this proposal, the conflict-monitoring module is insensitive to the source of conflict, and control is recruited globally by engaging in more controlled processing throughout the cognitive system. Such an account is akin to the notion that individuals

simply engage in more effortful processing once they notice that the task has become more difficult.

Alternatively, it is possible that control is exerted in a task-specific manner. Consistent with this hypothesis, Mayr et al. (2003) switched participants between two versions of the flanker task, one with arrows pointing along the horizontal axis and requiring left–right responses and one with arrows pointing along the vertical axis and requiring up–down responses. They reported no effects of compatibility of the up–down flanker task on the following left–right flanker task, which they interpreted as evidence that the dynamic deployment of control was not responsible for the sequential effects. Instead, they assumed that these effects resulted from the priming of specific stimulus features within the display.

However, it is possible that Mayr et al. (2003) did not observe sequential effects because the changes in the control were local rather than global. According to the local control account, adjustments resulting from the detection of conflict occur within task-specific representations. In other words, after an incompatible trial, the control settings of the current task are adjusted, but other tasks are unaffected by the adjustment. Thus, detected conflict may change the influence of irrelevant information on subsequent trials, but only when the task is repeated. In this way, the local control account can reconcile the findings of Mayr et al. with those of studies in which sequential effects are observed even though no features are repeated on consecutive trials (e.g., Kerns et al., 2004).

Global and Local Control

An important feature of the global control account is that it has the potential benefit of computational simplicity. For instance, consider a case in which there are two tasks. In experiments in which individuals have to switch between two distinct tasks, psychologists have found robust task-switching costs even when neither stimulus features nor responses are repeated, suggesting that the tasks are represented rather than being an unstructured collection of stimulus–response mappings (Allport, Styles, & Hsieh, 1994; Rogers & Monsell, 1995). When there is conflict in one of the tasks, the conflict monitor must modulate the task representation that is appropriate for that task. According to the global control account, a generic signal is sent to all active task representations without identifying the current task. Such an approach would be computationally efficient because a single monitor could operate across tasks without having to track the source of the conflict. Evidence for this form of control would be obtained if conflict in one task affected performance in the other.

In contrast, according to the local control account, the conflict signal specifically modulates the active task representation. This might occur because the conflict monitor not only registers the amount of conflict but also identifies the source of the conflict. Alternatively, there might be no general purpose conflict-monitoring module, but each task might have its own independent conflict monitor. Thus, determining the extent of the control that is recruited by the conflict is critical for understanding the circuitry of conflict monitoring.

Although Mayr et al.'s (2003) study would seem to support the local account, it was not designed specifically to address this question. In fact, the authors did not conclude that control was local but instead favored a stimulus-repetition account to explain

their data. More recently, support for a local control account was found in a study by Kiesel et al. (2006). The researchers used a task-switching paradigm with numerical magnitude and even–odd tasks. Target stimuli in these tasks could call for the same response for both tasks (congruent) or different responses for the different tasks (incongruent; e.g., 6 would be even, which was mapped on the right key, and greater than 5, which was mapped on the left key). They found that when the task stayed the same from one trial to the next, the effect of congruency diminished after incongruent trials. However, in task-switch trials, the congruency effect was of the same magnitude after congruent and incongruent trials. This suggests that the conflict is recruited locally for the current task and does not carry on when the task switches.

In the current study, we tackle the same question of the scope of control and the factors determining it. In particular, we examine the role of task structure in recruitment of control, independent of the source of the conflicting information. Our approach was to manipulate overlap between stimulus and response sets of two subtasks. When there is no overlap in the stimulus–response sets of the two subtasks, the experimental task can be parsed into a separate representation for each subtask, and there is an opportunity to distinguish between local and global control. If sequential modulations (the signature of control processes) are seen only within a subtask, but not across subtasks, then we conclude that control is exerted at a local level. That is, control recruited by the conflict in one subtask does not carryover to the other subtask.

Note that we have used the same type of irrelevant information to generate conflict throughout the experiment, namely irrelevant stimulus location information (i.e., the Simon effect). This allowed us to isolate the role of task structure in determining the scope of control, independent of the role of different conflict resolution processes that might be involved for conflicts of different types (e.g., flanker and Simon effects). Hence, we emphasize that in the present study, the term *local control* means localized within a task and not necessarily localized within a control process or conflict type.

We report four experiments examining the effect of task structure on control recruited by conflict monitoring. All experiments use the Simon effect as the measure of conflict and use sequential modulations in the Simon effects as the signature of control recruited by the conflict. To preview the results, we show that if the task can be parsed into two nonoverlapping stimulus–response sets (subtasks), each lateralized to one hemisphere of the brain, there is no carryover of control (i.e., no sequential modulations) across these sets. This is evidence for local recruitment of control by conflict. However, making the stimulus set common to two response sets is sufficient to see carryover of control. In Experiment 2, we show that the same result can be obtained even when the subtasks are represented within a hemisphere. Experiment 3 shows that local sequential modulations are not solely due to priming and that having the response set in common is sufficient to lead to global sequential modulations. Finally, in Experiment 4, we couple a four-choice task with a three-choice task to replicate the local control findings of Experiment 1 and Experiment 2. The use of a four-choice and a three-choice task enables us to remove any confounding feature-overlap effects both within and across tasks. We conclude that the recruitment of control is local and sensitive to task structure.
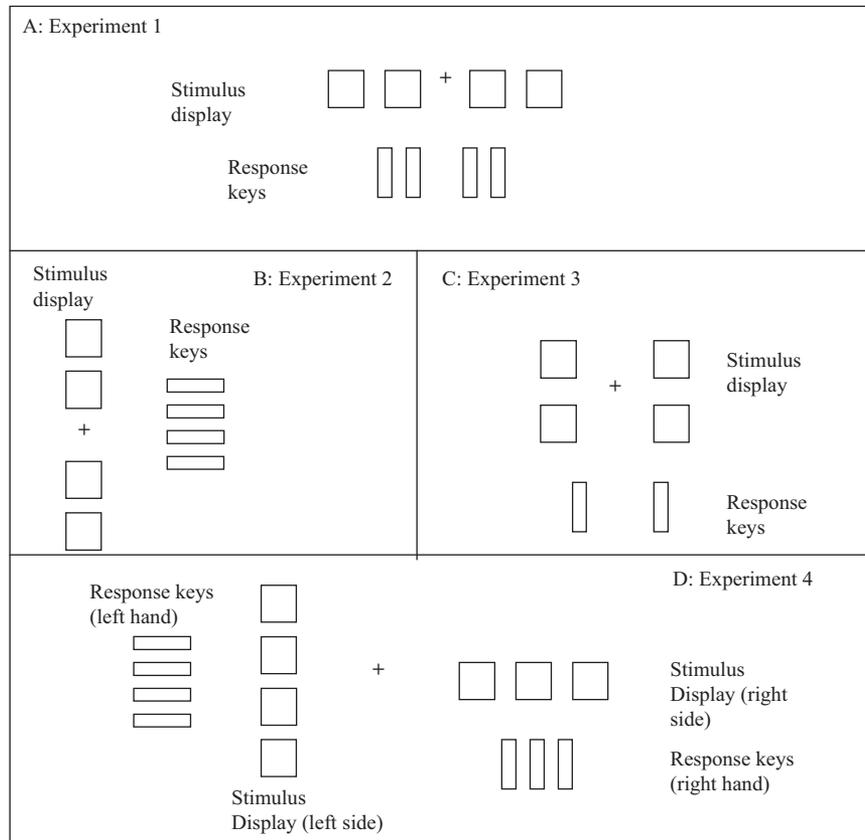
Figure 1. Schematic depiction of the arrangement of boxes and response keys for Experiments 1–4.

Experiment 1

In Experiment 1, we used a four-choice task to assess the effects of task structure on the implementation of control. The response set consisted of two pairs of responses, associated with the left and right hands, respectively. In separate halves of the experimental session, we manipulated the overlap between the stimulus sets for the two pairs of responses. In one half of the session, the stimulus set was composed of two colors, red and green, used for both response sets. That is, red and green could appear on both halves of the display, and the response side was given by the side on which the color appeared. Therefore, in this half, the response sets had the stimulus set in common. In the other half, the stimulus set was composed of four different colors. Two of the colors could appear only on the left of the display, and two could appear only on the right of the display. With this arrangement, we expected that participants would parse the task into two separate subtasks, because neither the response sets nor the stimulus sets were overlapping. If control is recruited globally, we should see sequential modulations no matter which subtask on the preceding trial occurred. If, on the other hand, we see local sequential modulations (subtask specific), then this would be evidence against global recruitment of control.

Methods

Participants. Twelve undergraduate students from the University of Iowa participated in the study in exchange for course credit.

All were right-handed and had normal or corrected-to-normal vision.

Apparatus and stimuli. The stimuli were presented on a 17-in. LCD monitor controlled by an IBM-compatible PC, which participants viewed from a distance of 50 cm. Stimuli were presented on black background. Four white boxes (two on each side of a fixation cross at the center of the screen) were continuously visible on the screen. The four boxes were arranged in two pairs on the horizontal meridian. Each box subtended 4.6° horizontally and 4.8° vertically. The distance of the centers of the inner two boxes from the fixation point was 11.9° of visual angle. The centers of the outer boxes were 6.6° apart from the center of the inner box on their side (Figure 1A). The stimuli were colored squares that filled up one of the boxes on a given trial. The four colors that were used in this experiment were red, green, blue, and yellow.

Responses were collected with two custom-made keyboards with millisecond accuracy. Participants used only the two keys under the index and middle finger of each hand. The keyboards were positioned so that the keys would be horizontally arranged, establishing a correspondence with the arrangement of boxes in the display. When the stimuli appeared to the left of fixation, participants were instructed to make the responses with the left hand; when the stimuli appeared to the right of fixation, participants made their responses with the right hand.

Task. Two versions of the task were given to each participant with the order counterbalanced across participants. During half of the 1-hr session, participants performed the four-color version of

the task. In this version, the colors red and green mapped to the left and right keys for the left hand, respectively. For the right hand, blue and yellow mapped to the left and right keys, respectively. Red and green were always presented in one of the left-side boxes. Similarly, blue and yellow were always presented in one of the right-side boxes. We introduced this restriction to facilitate the parsing of the task into two subtasks.

In the two-color version, only red and green were used. Consequently, red and green colors could appear on either side. If the color appeared on the left side, participants had to use their left hand for responding, and if the color appeared on the right side, they had to use their right hand. Red and green again mapped to the left and right keys, respectively, for both of the hands.

Note the definition of *correspondence* in our task. Correspondence was defined within a hemifield (i.e., not taking into account the side of the display). For example, a stimulus appearing on the left of the right pair of boxes that called for a response with the index finger of right hand would constitute a corresponding trial. The same color appearing in the right-most box would constitute a noncorresponding trial.

Procedure and design. Participants completed 8 blocks of 65 trials with each version of the task. The first block for each version was considered practice and was not used in the analyses. Stimuli were drawn from pseudorandomized lists, premade separately for each block to ensure each stimulus combination followed other stimulus combinations equally often. It was unpredictable whether the stimulus would switch side from one trial to the other.

Each trial began with a fixation display lasting 500 ms, in which the fixation cross and the boxes were visible on the screen. This was followed by the stimulus display for 150 ms. The stimulus display differed from the fixation display only in that one of the boxes was filled by a colored square. The stimulus display was followed by 1,850 ms of fixation screen, which also defined the allowed response interval. If the response was omitted within this interval (within 2,000 ms of stimulus onset) or if an incorrect response was made, the word *incorrect* was displayed just above the fixation cross for 2 s before the next trial began.

Results: Four-Color Version

RTs. We excluded the first trials of each block, trials with an incorrect response or a response omission, and trials following these (12% of all the trials). A three-way analysis of variance (ANOVA) was conducted on the remaining RTs with the following variables: side (same as previous trial vs. opposite), correspondence on previous trial (corresponding vs. noncorresponding), and correspondence on current trial (corresponding vs. noncorresponding).[1] The main effects of side, $F(1, 11) = 68.45$, $p < .0005$; correspondence on previous trial, $F(1, 11) = 13.76$, $p < .01$; and correspondence on current trial, $F(1, 11) = 31.73$, $p < .0005$, were significant. RTs were faster overall when the stimulus was on the same side as in the previous trial (531 ms vs. 660 ms), when the preceding trial was a corresponding trial (587 ms vs. 604 ms), or when the current trial was a corresponding trial (571 ms vs. 620 ms; i.e., a Simon effect of 49 ms). Side on previous trial interacted with correspondence on current trial, $F(1, 11) = 10.54$, $p < .01$, indicating a larger Simon effect for opposite-side sequences (66 ms) than for same-side sequences (31 ms).

Sequential modulations (i.e., interactions between correspondence on previous trial and correspondence on current trial) are of primary interest, because they reflect the recruitment of control by the conflict. We are especially interested in sequential modulations across subtasks (i.e., sequences in which the stimuli switched sides). The interaction of correspondence on previous trial with the correspondence on the current trial was significant, $F(1, 11) = 23.04$, $p < .005$. Critically, this interaction was further modulated by whether the stimulus switched sides from the previous trial, $F(1, 11) = 16.38$, $p < .005$, as shown in Figure 2.

To understand this three-way interaction, we conducted two-way ANOVAs with the following variables: correspondence on previous trial and correspondence on current trial for same-side and opposite-side sequences separately. For same-side sequences, there was a significant interaction of correspondence on previous trial with correspondence on current trial, $F(1, 11) = 33.14$, $p < .0005$, whereas there were no such interactions present for opposite-side sequences ($F < 1$). Thus, sequential modulations appeared only on same-side sequences (or within a subtask).

That sequential modulations were not present in opposite-side sequences indicates that the process responsible for the sequential effects works locally. Note that all the trials in opposite-side sequences involved a complete change of stimulus and response features. Therefore, it might be tempting to take this as evidence in favor of a priming account of sequential modulations. That is, the absence of sequential effects when neither the stimulus identity nor its location repeats is consistent with the feature-integration effects account of sequential effects (Akçay & Hazeltine, 2007; Kerns et al., 2004; Notebaert et al., 2006). However, before we favor this explanation for the present data, we examine the results of the two-color version.

Errors. We excluded the first trials of each block, because we are examining sequential effects. The accuracy data were entered into a three-way ANOVA with the following variables: side (same vs. opposite of previous trial), correspondence on previous trial (corresponding vs. noncorresponding), and correspondence on current trial (corresponding vs. noncorresponding). There were significant effects of side, $F(1, 11) = 6.47$, $p < .05$ (.04 and .08 for same- and opposite-side sequences, respectively) and significant effects of correspondence on current trial, $F(1, 11) = 13.29$, $p < .005$ (.03 vs. .09 for corresponding vs. noncorresponding trials, respectively). The only other effect that reached significance was the interaction between side and correspondence on current trial, $F(1, 11) = 6.49$, $p < .05$. This interaction resulted from the fact that correspondence effect was weaker on same-side sequences (.02 vs. .06 for corresponding vs. noncorresponding trials, respectively) than it was on the opposite-side sequences (.04 vs. .12 for corresponding vs. noncorresponding trials, respectively). In summary, the overall pattern of the error rates followed the pattern of the RT data, suggesting that this pattern was not due to a speed–accuracy trade-off (see Table 1).

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[1] In analyses not reported here, we found that hand (left vs. right) did not enter into any four-way interactions that would be important for our main point. Therefore, in this experiment and in the remaining experiments, we collapsed the data over two hands when the task involved more than one hand.
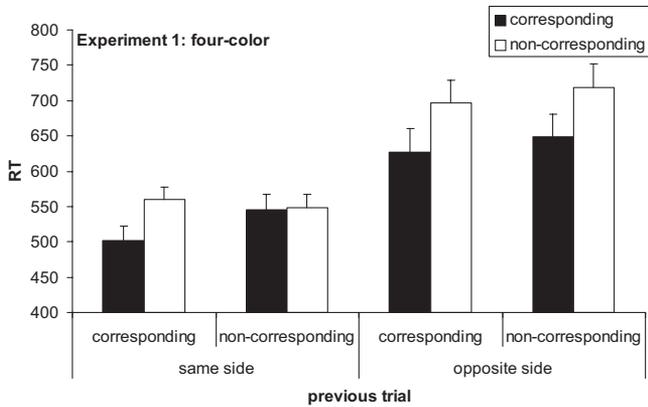
Figure 2. Average reaction times (RTs) for Experiment 1, four-color version. The left-hand side depicts same-side sequences, and the right-hand side depicts opposite-side sequences. Sequential modulations are restricted to the same-side sequences. Error bars indicate standard error of the mean.

Results: Two-Color Version

RTs. The first trials of each block, incorrect trials, and trials following incorrect trials were excluded from the RT analysis (13.3% of all trials). The remaining trials were entered into a three-way ANOVA with the following variables: side (same vs. opposite), correspondence on previous trial (corresponding vs. noncorresponding), and correspondence on current trial (corresponding vs. noncorresponding). There were significant main effects of side, $F(1, 11) = 31.20$, $p < .0005$, and correspondence on current trial, $F(1, 11) = 50.75$, $p < .0005$. RTs were faster when the stimulus side was the same as on the preceding trial than when the stimulus switched sides from one trial to the next (550 ms vs. 660 ms, respectively). RTs were also faster on corresponding trials than on noncorresponding trials (580 ms vs. 630 ms, respectively).

Table 1
Average Reaction Times and Error Rates in Experiment 1, Four-Color Version

Previous trial	Current trial		
	Corresponding	Noncorresponding	Simon effect
Reaction times (ms)			
Same side			
Corresponding	495	557	63
Noncorresponding	538	537	−1
Opposite side			
Corresponding	616	684	68
Noncorresponding	639	703	64
Error rates			
Same side			
Corresponding	−0.02	0.07	0.05
Noncorresponding	0.03	0.05	0.03
Opposite side			
Corresponding	0.04	0.11	0.08
Noncorresponding	0.04	0.12	0.08

At first pass, the results were highly similar to those for the four-color version. There were sequential modulations of the Simon effect, as indicated by the significant interaction of correspondence on previous trial and correspondence on current trial, $F(1, 11) = 22.63$, $p < .005$. This interaction was further modulated by the side variable, resulting in a significant three-way interaction, $F(1, 11) = 18.33$, $p < .005$. The pattern of the three-way interaction paralleled the pattern in the four-color version as indicated by the separate two-way ANOVAs with the variables of correspondence on previous trial and correspondence on current trial, conducted separately for same- and opposite-side sequences. There were significant sequential modulations for same-side sequences, $F(1, 11) = 38.23$, $p < .0005$, whereas sequential modulations were not significant for opposite-side sequences ($p > .27$), as shown in Figure 3.

There is a critical difference between the four-color and the two-color versions with respect to the opposite-side sequences. As noted earlier, in the four-color version, all trials of opposite-side sequences involved changes in both stimulus features and response features (i.e., they were complete-change sequences). In the two-color version, however, opposite-side sequences sometimes involved repetition of color, the task-relevant feature of the stimulus. These partial repetition trials might be masking an effect of control recruited by the conflict on the previous trial. Thus, we eliminated the color repetition trials from the opposite-side sequences and conducted a two-way ANOVA with the following variables: correspondence on previous trial and correspondence on current trial. The main effect of correspondence on current trial was significant, $F(1, 11) = 8.40$, $p < .05$. There was also a significant interaction between correspondence on previous trial and correspondence on current trial, $F(1, 11) = 11.13$, $p < .01$, indicating significant sequential modulations. Figure 4B shows the pattern: There was a Simon effect of 73 ms after corresponding trials (709 vs. 636 ms), whereas the Simon effect decreased to 16 ms after noncorresponding trials (660 vs. 643 ms). This interaction is a clear indication of sequential effects when the stimulus changes sides on consecutive trials.

To rule out the possibility that our elimination of stimulus repetitions led to a spurious sequential modulation, we carried out the following analysis in the four-color data. We eliminated those
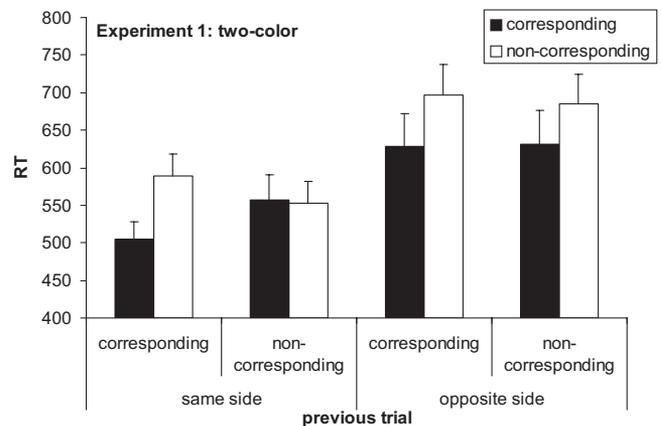

Figure 3. Average reaction times (RTs) for Experiment 1, two-color version. Error bars indicate standard error of the mean.
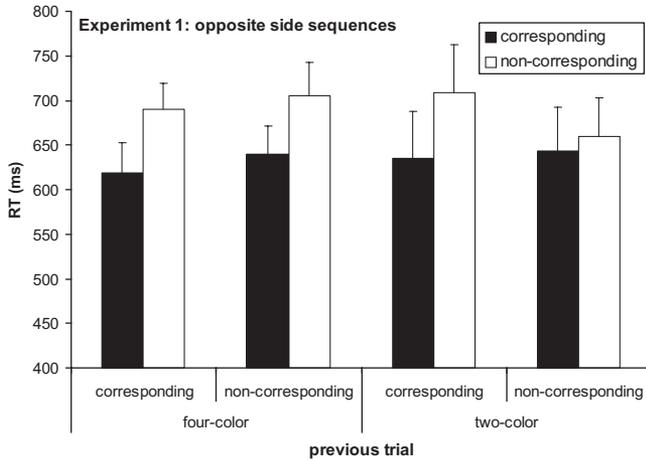
Figure 4. Sequential modulations for opposite-side sequences after color repetitions were eliminated. Note that these trials represent complete-change sequences. Error bars indicate standard error of the mean. RT = reaction time.

trials that corresponded to the eliminated color repetition trials in the two-color experiment. In other words, we eliminated relative response location repetitions from opposite-side sequences. This elimination left sequences with exactly the same composition of feature overlap as in the two-color experiment. If the sequential modulations we saw in the two-color data after eliminating color repetitions were indeed not due to control but rather were due to feature overlap, then we should find the same sequential modulations in the four-color data as well.

The results revealed that there were no sequential modulations in the opposite-side sequences, even after going through the same elimination procedure as in the two-color experiment. The two-way interaction between previous correspondence and current correspondence was nonsignificant, $F(1, 11) < 1$. The Simon effect was 70 ms after corresponding trials (689 ms vs. 619 ms for noncorresponding and corresponding trials, respectively) and 66 ms after noncorresponding trials (705 ms vs. 639 ms, Figure 4A). Thus, the elimination procedure by itself does not seem to be the likely cause of the appearance of sequential modulations in the two-color experiment.

Errors. After excluding the first trials of each block, the error rate data were submitted to a three-way ANOVA with the following variables: side (same vs. opposite), correspondence on previous trial, and correspondence on current trial (corresponding vs. noncorresponding). There was a main effect only of side, $F(1, 11) = 18.79$, $p < .005$, with a lower rate of errors on same-side sequences (.05) than different-side sequences (.08). No other effect was significant, but the pattern of the error rates paralleled the RT pattern (Table 2).

Discussion

The results of Experiment 1 provide evidence for local recruitment of control by conflict. We manipulated task structure so that in one case (the four-color version), the task could be divided into two nonoverlapping subtasks, whereas in the other case (the two-color version), there was overlap between the two potential sub-

tasks, and they formed an integrated whole. In the four-color version, sequential modulations were present only within the same-side sequences, suggesting that the processes responsible for sequential modulations are operating locally within the specific subtask that occurred in the last trial.

Feature integration or conflict adaptation? Note that we did not have a means of dissociating control effects from feature-overlap effects in the same-side sequences in this task, and accepting local sequential modulations required assuming that control as well as bottom-up processes operated in those sequences. Multiple studies support the notion that this assumption is a reasonable one (Akçay & Hazeltine, 2007; Kerns et al., 2004; Notebaert et al., 2006; Notebaert & Verguts, in press). Nevertheless, the results of the four-color version are completely consistent with a feature-integration account, which would predict that with complete-change sequences (as in opposite-side sequences) there should be no sequential modulations. In the two-color version, on the other hand, there was evidence of sequential modulations in the opposite-side sequences, once we eliminated the color repetitions from those sequences. This result speaks against a feature-integration account, because these trials represent complete-change sequences. We also saw that the elimination of color repetitions is not the sole factor responsible for the sequential modulations in the two-color case, because elimination of the same trials in the four-color experiment did not result in any sequential modulations.

One explanation for this pattern of results is that in the four-color task, no control process is operating, whereas in the two-color case we see control effects. In other words, all of the sequential modulations in the four-color task are due to feature overlap. However, we do not think that this is likely because the two versions of the task were identical in all aspects (i.e., stimulus display, timing, etc.) except for the overlap of stimulus sets for the two subtasks. Therefore, we see no reason why control might operate in one case but not in the other.

Table 2

Average Reaction Times and Error Rates in Experiment 1, Two-Color Version

Previous trial	Current trial		
	Corresponding	Noncorresponding	Simon effect
Reaction times (ms)			
Same side			
Corresponding	504	589	85
Noncorresponding	557	553	−4
Opposite side			
Corresponding	628	696	68
Noncorresponding	632	684	52
Error rates			
Same side			
Corresponding	0.03	0.06	0.03
Noncorresponding	0.04	0.05	0.01
Opposite side			
Corresponding	0.06	0.10	0.04
Noncorresponding	0.07	0.10	0.04

Thus, the more parsimonious account is to hold that control is occurring in both versions of the task but that its expression is sensitive to task structure. That is, control is implemented locally. When the stimuli in the two halves of the display belong to the same task, the conflict in one half of the display affects performance in the other half. In contrast, when the stimuli in the two halves belong to different tasks, the effects of control are restricted to the side of the conflict. However, before accepting the role of information about task structure in the recruitment of control, we need to rule out another alternative account.

Task resetting. In both versions of the task, there was a significant cost in performance when the stimulus switched sides. This cost may reflect a task-switching process. It is likely that task switching engages executive control processes (perhaps similar to those engaged by conflict) and that the control settings are reset by the task-switching operation. That is, sequential effects might be dependent on task structure simply because having to switch between tasks is costly in terms of processing resources, and switching disrupts the control recruited by the conflict.

This alternative account can be assessed by looking at sequential modulations on the same side in sequences where the stimulus switches to the opposite-side from Trial $n - 2$ to Trial $n - 1$ and switches back again on Trial n. In such sequences, correspondence on Trial $n - 2$ should not lead to sequential modulations if the control recruited by the conflict is completely reset on opposite-side sequences. On the other hand, if control is recruited locally, then the correspondence on Trial $n - 2$ should affect the magnitude of the correspondence effect on Trial n, because the local effects resulting from Trial $n - 2$ should be unaffected by Trial $n - 1$ and therefore preserved on Trial n.

In the data from the four-color version, we isolated those trials and looked at sequential modulations of the Simon effect by the correspondence on the Trial $n - 2$.[2] The interaction of Trial $n - 2$ correspondence and correspondence on current trial did not approach significance in this subset of trials ($F < 1$), even though there was a numerical trend in the direction of a reduced Simon effect after noncorresponding trials (78 ms vs. 58 ms after corresponding vs. noncorresponding trials, respectively). Thus, there was a trend toward sequential modulations on the same-side sequences interleaved with a trial on the opposite side.

The fact that we did not find significant evidence for same-side sequences on Trial $n - 2$ with a side switch suggests that the resetting hypothesis is indeed a viable hypothesis with respect to the results of the four-color experiment. However, we cannot rule out locally recruited control on the basis of this null result, especially given the low power and nonsignificant trend in the predicted direction. In the analysis of Trial $n - 2$ sequential effects, we eliminated a large number of trials to isolate the relevant sequence of trials. This slicing of data left too few trials for some cells in the analysis (as few as 9 trials) and, therefore, might have been prone to variation and should be viewed with caution. This issue is revisited in Experiment 2.

Experiment 2

Experiment 2 had two aims: First, we asked whether the subtask-specific recruitment of control by conflict was caused by our use of subtasks that were lateralized to separate hemispheres. That is, the critical aspect of our task might not have been its

divisibility to subtasks per se, which would lead to separate task representations, but might have been the complete lateralization of stimulus and response sets, which led to the decoupling of the task representations. Therefore, we sought to replicate the four-color version of the first experiment within one hemisphere.

Second, we wished to further assess the hypothesis that resetting was due to task switching. As noted earlier, the design of Experiment 1 did not allow for a powerful test of the Trial $n - 2$ sequential modulations, because of the small number of trials allowed into the analysis. In Experiment 2, only the four-color version was used, allowing us to gather more trials for the Trial $n - 2$ analysis and to conduct a more powerful analysis.

Methods

Participants. Twelve undergraduate students from the University of Iowa participated in the study in exchange for course credit. All were right-handed (self-report) and had normal or corrected-to-normal vision.

Apparatus and stimuli. The apparatus for presenting the stimuli and collecting the responses was the same as in Experiment 1. The stimuli were the same as in Experiment 1, with the following differences: The four white boxes were now arranged vertically in a column at the center of the screen. Two of the boxes were above and two were below the fixation cross. The distance between the fixation cross and the center of the nearer boxes was 5.1°, and the distance between the centers of the two boxes in a pair above or below the center was 5.4° (see Figure 1B). Blue and yellow squares always appeared in one of the two boxes that were above the fixation cross, and red and green squares always appeared in one of the boxes below the fixation cross. In this experiment, the term *side* refers to the two halves divided by the horizontal meridian.

Because the stimulus locations were vertically arranged in this experiment, the response keys were also vertically arranged, so that they would correspond with the possible stimulus locations. Participants responded with their left hand on half of the session and with their right hand on the other half, with the order counterbalanced across participants. Red, green, blue, and yellow mapped to the keys from bottom to the top in the order mentioned.

Design and procedure. Participants completed 10 blocks of 65 trials with each hand. The first block with each hand was considered practice and was not taken into analyses. Stimuli were drawn from pseudorandomized lists, made separately for each block to ensure each stimulus combination followed other stimulus combinations equally often, making the side switches unpredictable for the participant.

Each trial began with 500 ms of fixation display with the four boxes and a fixation cross visible on the display. After 500 ms, the stimulus was presented for 150 ms. The response interval was again 2,000 ms from the stimulus onset; however, the trial ended as soon as the response was given. Thus, in this experiment, the response-to-stimulus interval was fixed at 500 ms (the duration of the fixation cross at the start of each trial). If an incorrect response

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[2] We excluded every first and second trial that followed an incorrect trial in this analysis, because we were looking at sequential effects of a two-back trial.

was given, a feedback screen was displayed for 2,000 ms with the word *incorrect* at the center of the screen.

Results

RTs. We excluded first trials of each block, any trials with an incorrect response or response omission, and trials following these trials from RT analyses. The remaining trials were collapsed across hands and submitted to a three-way ANOVA with the following variables: side (same as vs. different from previous trial), correspondence on previous trial (corresponding vs. noncorresponding), and correspondence on current trial (corresponding vs. noncorresponding). The main effect of side was highly significant, $F(1, 11) = 90.37$, $p < .0005$, as was the main effect of correspondence on current trial, $F(1, 11) = 129.40$, $p < .0005$. The main effect of correspondence on previous trial approached significance, $F(1, 11) = 4.80$, $p < .06$. Furthermore, all of the interactions were significant. Side interacted both with correspondence on previous trial, $F(1, 11) = 8.98$, $p < .05$, and with correspondence on current trial, $F(1, 11) = 35.60$, $p < .0005$. The interaction between correspondence on previous trial and correspondence on current trial was also significant, $F(1, 11) = 39.23$, $p < .0005$. Critically, these variables produced a significant three-way interaction, $F(1, 11) = 8.04$, $p < .05$.

The pattern of the three-way interaction closely paralleled the three-way interaction in the four-color version of Experiment 1 (Figure 5). Separate two-way ANOVAs conducted on same-side sequences and previous-side sequences confirmed that correspondence on previous trial and current trial interacted only in same-side sequences, $F(1, 11) = 56.79$, $p < .0005$, but not in opposite-side sequences, $F(1, 11) = 1.80$, $p > .2$. Thus, sequential modulations were confined to the same-side sequences again.

Next, we looked at the Trial $n - 2$ sequential effects for those sequences where the stimulus switched sides from Trial $n - 2$ to Trial $n - 1$ and switched back to the same location as Trial $n - 2$ on Trial n. A two-way ANOVA with the variables of correspondence on Trial $n - 2$ and correspondence on current trial revealed a significant two-way interaction of the two variables, $F(1, 11) = 7.27$, $p < .05$ (see Figure 6). Thus, sequential modulations on the same side are still significant even after two trials that involved two side switches. This is strong evidence against the resetting
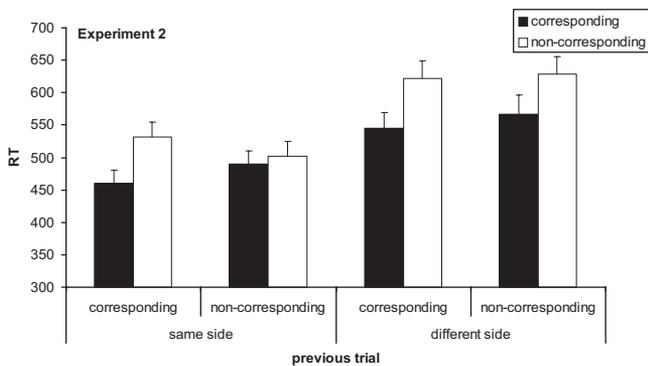

Figure 6. Correspondence effects on Trial n as a function of correspondence on Trial $n - 2$ for sequences where the stimulus switched from one side to the other and back from Trial $n - 2$ to Trial $n - 1$ to Trial n (Experiment 2). RT = reaction time.

hypothesis, because the resetting hypothesis would require a complete reset as a result of the two task switches.

Errors. The first trials of each block were eliminated from the error rates analyses. The remaining data were subjected to a three-way ANOVA with the following variables: side (same vs. opposite), correspondence on previous trial (corresponding vs. noncorresponding), and correspondence on current trial (corresponding vs. noncorresponding). All possible main effects and interactions were significant. Critically, there was a three-way interaction, $F(1, 11) = 11.00$, $p < .01$. The pattern of this interaction (see Table 3) closely paralleled the pattern for RT, indicating that the effects in RT were not due to a speed–accuracy trade-off.

Discussion

Experiment 2 replicated the finding that when stimulus and response sets could be segregated into two nonoverlapping sets, or

Table 3
Average Reaction Times and Error Rates for Experiment 2

	Current trial		
Previous trial	Corresponding	Noncorresponding	Simon effect
	Reaction times (ms)		
Same side			
Corresponding	462	534	72
Noncorresponding	492	503	11
Opposite side			
Corresponding	546	622	76
Noncorresponding	568	627	60
	Error rates		
Same side			
Corresponding	0.02	0.10	0.08
Noncorresponding	0.04	0.06	0.02
Different side			
Corresponding	0.04	0.15	0.10
Noncorresponding	0.04	0.14	0.10

Figure 5. Average reaction times (RTs) for Experiment 2. The pattern closely resembles the pattern of Experiment 1, four-color. Error bars indicate standard error of the mean.
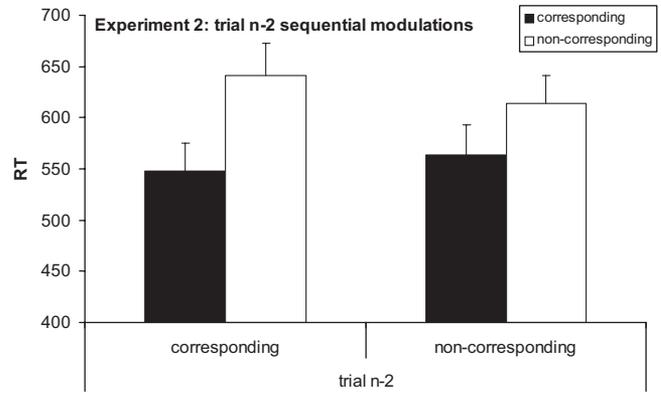
subtasks, there were no sequential modulations of correspondence effects across subtasks. We argue that this was due to the modulation of specific subtask representations that were the source of the conflict on the previous trial. These representations could be maintained separately within a single hemisphere.

Critically, Experiment 2 also provided evidence against the task-switch resetting account of the local sequential effects. If the control systems were reset every time there was a switch from one subtask to the other, then no sequential effects should have been observed after side-switch trials, even if control was exerted globally. However, the two accounts make distinct predictions relating to the effects of correspondence on Trial $n - 2$. According to the resetting hypotheses, correspondence on Trial $n - 2$ should have had no effect on the magnitude of correspondence effects on Trial n. In contrast, the local control hypothesis predicts that, after two consecutive switches, the correspondence of Trial $n - 2$ should affect the correspondence effect on Trial n, because local control settings should be preserved. This pattern was observed in both experiments, and in Experiment 2, which had greater power, the interaction between correspondence on Trial $n - 2$ and correspondence on Trial n was significant (see also Mayr et al., 2003). Thus, we favor the local control hypothesis over the resetting hypothesis. In the General Discussion section, we describe some possible means by which local control might be implemented.

Experiment 3

In the two-color version of Experiment 1, we demonstrated that when the stimulus sets overlapped for two separate response sets, sequential modulations were observed, indicating global recruitment of control. We now ask the complementary question: Would a common response set for two different stimulus sets lead to global sequential modulations? It might be that the task structure is determined exclusively by the stimulus sets, and as long as they are separable, there would be only local sequential modulations. On the other hand, task representations might take both stimulus and response sets into account, in which case we should see global sequential modulations when the response set is common across two stimulus sets.

To determine the nature of the task representation that affects the deployment of the control, we used a 4:2 stimulus–response mapping in this experiment. We used the same two pairs of colors as in the first two experiments (red–green and blue–yellow). The stimulus sets were again presented in distinct areas of the display, as in Experiment 1. Red and green were always presented above the fixation cross, and blue and yellow were always presented below (Figure 1C). However, one color from each pair mapped onto the same response, so that response sets were the same for both stimulus sets.

Methods

Participants. Participants were 13 students from the University of Iowa. All had normal or corrected-to-normal vision and were right-handed.

Apparatus and stimuli. The stimulus display consisted of four boxes that were the same size as in the previous experiments. The boxes were arranged so that their centers were at the corners of an imaginary rectangle that was centered in the screen and extended 13.0° horizontally and 8.0° vertically (see Figure 1C). There was a fixation cross at the center of the screen.

The colors that were used in this experiment were red, green, blue, and yellow. Red and green formed a stimulus pair, and they were always presented in one of the two upper boxes. Blue and yellow were always presented in one of the two lower boxes. There were two response keys. Red and blue mapped onto the left key, and green and yellow mapped onto the right key. The left key was pressed with the left index finger, and the right key was pressed with the right index finger.

Procedure and design. Participants went through 20 blocks of 65 trials. The first block was considered practice and was not taken into the analyses. The stimulus lists were pseudorandom lists that were generated to satisfy the rule that every stimulus combination followed every stimulus combination equally often.

Each trial started with 500 ms of fixation display with the fixation cross and the four boxes. Subsequently, the stimulus was presented for 150 ms in one of the four boxes. This was followed by 1,850 ms of fixation screen, which also defined the response interval. The response had to be given during this interval, and if the correct response was given, then the next trial commenced after the end of the response interval (i.e., the interstimulus interval was fixed at 2,500 ms). If an incorrect response was given or the participant did not respond during the given interval, feedback was given on the screen by presenting the word *incorrect* for 2,000 ms before starting the next trial.

Results

RTs. First trials of each block, incorrect trials, and trials following such trials were excluded from the analysis (11.3% trials). The remaining data were submitted to a three-way ANOVA with the following variables: stimulus set (same vs. different), correspondence on previous trial, and correspondence on current trial. The main effect of stimulus set was highly significant, $F(1, 12) = 47.88, p < .0005$, indicating faster overall RTs for same-stimulus-set sequences than for different-stimulus-set sequences (536 ms vs. 586 ms). The main effect of correspondence on previous trial also was significant, $F(1, 12) = 5.52, p < .05$. The main effect of correspondence on current trial approached significance, $F(1, 12) = 4.64, p < .053$.

The only significant two-way interaction was between correspondence on previous trial and correspondence on current trial, $F(1, 12) = 87.35, p < .0005$, indicating strong sequential modulations. The three-way interaction of stimulus set, correspondence on previous trial, and correspondence on current trial was also significant, $F(1, 12) = 6.47, p < .05$.

To better understand this three-way interaction, we divided the data into two halves, depending on whether the stimulus on Trial n belonged to the same set as or a different set from the stimulus on Trial $n - 1$, and we conducted separate two-way ANOVAs with the variables of correspondence on previous trial and correspondence on current trial. For the same-set trials, there were significant sequential modulations (i.e., two-way interaction), $F(1, 12) = 86.25, p < .0005$. This was also true for the different-set trials, $F(1, 12) = 33.41, p < .0005$ (Figure 7). Given that we found significant sequential modulations for both same- and different-set trials, it is somewhat puzzling that we also found a three-way interaction in the preceding ANOVA. Apparently, the Simon ef-
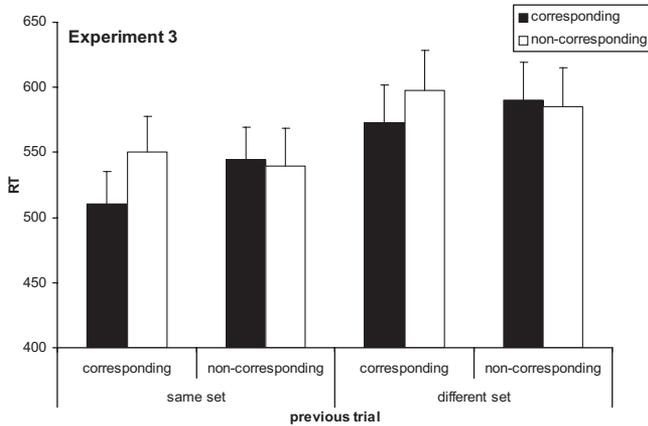
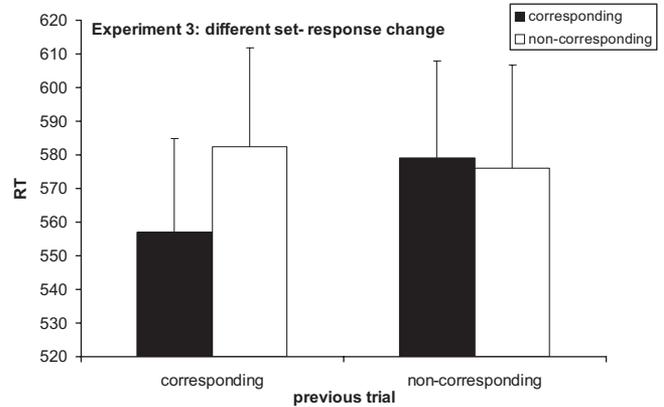*Figure 7.* Average reaction times (RTs) for Experiment 3. Error bars indicate standard error of the mean.

Figure 8. Sequential modulations for opposite-side sequences after response repetitions were eliminated. These trials were complete-change sequences, and sequential modulations could not be attributed to priming. Error bars indicate standard error of the mean. RT = reaction time.

fect on different-set trials when the preceding trial was a corresponding trial (39 ms) was reliably smaller than the Simon effect on same-set trials following a corresponding trial (26 ms). Nevertheless, the important point is that the sequential modulations are robust for both types of sequences.

Note that, unlike in the previous experiments, the 4:2 stimulus–response mapping task allowed for response repetitions in different set sequences. Under such conditions, it is possible that sequential modulations stemmed from response priming effects. Note, however, that in different-set sequences, response repetitions always went with a stimulus change (i.e., they were all partial repetitions). Moreover, response repetitions were equally frequent for all correspondence sequences. This makes it unlikely that response repetitions were responsible for across set sequential modulations.

Nonetheless, we tested the role of response repetitions in producing the pattern of sequential effects empirically. All trials with response repetitions were excluded from the different-set sequences, and the data were submitted to a two-way ANOVA with correspondence on previous trial and correspondence on current trial as variables. The results yielded clear sequential modulations of the Simon effect in the form of a significant two-way interaction, $F(1, 12) = 13.73$, $p < .005$. The Simon effect was 25 ms for trials following a corresponding trial—significant according to a one-tailed, paired-samples t test, $t(12) = 2.94$, $p < .02$—and was -3 ms for trials following noncorresponding trials, $t < 1$ (Figure 8). We therefore conclude that sequential modulations were not confined to same-stimulus-set sequences, and they cannot be explained solely by response priming. Because eliminating response repetitions left only complete change of stimulus location and response in the different set sequences, the sequential modulations are not likely to be explained solely by the feature-integration account.

Error rates. The first trials of each block were left out of the error rates analyses. The data (Table 4) were entered into a three-way ANOVA with the following variables: stimulus set (same vs. different), correspondence on previous trial, and correspondence on current trial. Paralleling the RT data, the main effects of stimulus set, $F(1, 12) = 33.42$, $p < .0005$, correspondence on previous trial, $F(1, 12) = 7.61$, $p < .02$, and correspondence on current trial, $F(1, 12) = 4.70$, $p < .052$, were significant. Stimulus

set interacted significantly with correspondence on current trial, $F(1, 12) = 6,17$, $p < .05$. The interaction of correspondence on previous trial with correspondence on current trial approached significance, $F(1, 12) = 3.83$, $p = .074$, whereas the three-way interaction was not significant ($F < 1$).

Discussion

The results of Experiment 3 demonstrate that a shared response set is sufficient for the transfer of control at least in our task, even when the two tasks use nonoverlapping stimulus sets. Sequential modulations were significant in different-stimulus-set sequences even after eliminating response repetitions. Therefore, we can again rule out priming as the sole account of sequential modulations in different-side sequences. The fact that sequential modulations can be seen across nonoverlapping stimulus sets when they have the response set in common indicates that the task represen-

Table 4

Average Reaction Times and Error Rates in Experiment 3

	Current trial		
Previous trial	Corresponding	Noncorresponding	Simon effect
	Reaction times (ms)		
Same set			
Corresponding	511	550	39
Noncorresponding	545	539	−6
Different set			
Corresponding	572	598	26
Noncorresponding	590	585	−5
	Error rates		
Same set			
Corresponding	0.02	0.03	0.01
Noncorresponding	0.04	0.04	0.00
Different set			
Corresponding	0.05	0.08	0.03
Noncorresponding	0.06	0.08	0.02

tations that affect the deployment of control are sensitive to properties of both stimulus and response sets. This idea is consistent with the notion of prefrontal cortex representations that encode for abstract task rules (Wallis, Anderson, & Miller, 2001), but do not participate directly in the processing of stimulus or response.

Experiment 4

In the first three experiments, we found evidence that sequential modulations are sensitive to task structure. We have argued that these effects are best explained by the local recruitment of control processes, which are deployed depending on the task structure. One major caveat with these experiments, however, is the fact that we were not able to dissociate control processes from other bottom-up processes (e.g., feature integration) in the same-side/set sequences in the earlier experiments. This limitation is endemic to two-choice tasks, such as those used in Experiments 1–3.

There are reasons to assume that the patterns consistent with local control do not stem from feature-integration effects. Numerous studies have repeatedly found that control processes, as well as some kind of bottom-up processes, operate in parallel to lead to sequential effects (Akçay & Hazeltine, 2007; Kerns et al., 2004; Notebaert et al., 2006; Notebaert & Verguts, 2007; Ullsperger et al., 2005; Wühr, 2005). Therefore, it seems reasonable to assume that control processes do operate in our task as well. Nevertheless, in Experiment 4, we sought direct evidence for local control that was not attributable to feature-overlap effects. To do this, we used a four-choice task with letter stimuli and a three-choice task with color stimuli. The stimulus display was arranged so that for one task, the stimuli and responses were arranged vertically (letter task), and for the other task, the stimuli and responses were arranged horizontally (color task). These differences between tasks were introduced to make the tasks as distinct as possible and also to prevent any hypothetical association between relative stimulus locations across two tasks.

Methods

Participants. Twelve undergraduate students from the University of Iowa participated in the experiment in exchange for course credit.

Apparatus and stimuli. The stimulus display consisted of four white boxes arranged in a column on the left side of the fixation cross and three white boxes in a row on the right side of the fixation cross (see Figure 1D). The stimuli for the letter task were the letters *O, S, T,* and *X* presented in white and mapped to the response keys in that order from top to bottom. The stimuli for the color task were green, yellow, and red mapped to keys in this order from left to right. The stimulus–response mapping was introduced to the participants explicitly as one letter task and one color task in the instructions. Other details of the apparatus were the same as in previous experiments.

Design and procedure. We wanted to keep the relative frequencies of each task equal, so half of the trials were the letter task, and half were the color task. At the same time, we equated the frequency of each stimulus combination within each task. To satisfy these constraints, stimulus lists with 288 trials were generated offline. The stimulus for a given trial was selected at random (without replacement) from the list of 288, and every 3 blocks, this

list was reset. Participants went through 12 blocks of 96 trials. The first block was considered practice and was not included in any analyses.

Results

RTs. Incorrect trials, trials following an incorrect response (10.1%), and the first trials of each block were eliminated from the RT analyses. Preliminary analyses showed that task (color vs. letter) did not interact with any of the other variables (all $ps > .2$), so we have collapsed the data across tasks to gain in statistical power.

The data were submitted to a three-way ANOVA with the following variables: side (same vs. opposite), correspondence on previous trial, and correspondence on current trial. The main effects of side, $F(1, 11) = 125.39$, $p < .0001$, correspondence on previous trial, $F(1, 11) = 12.40$, $p < .01$, and correspondence on current trial, $F(1, 11) = 113.57$, $p < .0005$, were significant. The interaction between correspondence on previous trial and correspondence on current trial was also significant, $F(1, 11) = 5.12$, $p < .05$. The only other significant interaction was between side and correspondence on previous trial, $F(1, 11) = 7.56$, $p < .05$. The three-way interaction was not significant, $F(1, 11) = 2.54$, $p = .14$ (see Table 5 and Figure 9).

Next, we eliminated all repetition of stimulus location and response from the data, leaving only complete-change trials, and we entered the data into a three-way ANOVA with the same variables described earlier (note that opposite-side sequences already contained only complete-change trials). All the main effects and interaction effects that were significant for the overall data remained significant. More important, the three-way interaction among side, correspondence on the current trial, and correspondence on the previous trial was also significant, $F(1, 11) = 5.39$, $p < .05$. Separate two-way ANOVAs for the same-side and opposite-side sequences revealed a significant two-way interaction between correspondence on current trial and correspondence on previous trial for same-side sequences, $F(1, 11) = 10.75$, $p < .01$,

Table 5

Average Reaction Times and Error Rates in Experiment 4

| Previous trial	Current trial		
	Corresponding	Noncorresponding	Simon effect
	Reaction times (ms)		
Same set			
Corresponding	551	640	90
Noncorresponding	596	645	48
Different set			
Corresponding	685	760	76
Noncorresponding	686	752	65
	Error rates		
Same set			
Corresponding	0.03	0.06	0.03
Noncorresponding	0.03	0.05	0.02
Different set			
Corresponding	0.05	0.07	0.02
Noncorresponding	0.04	0.06	0.02
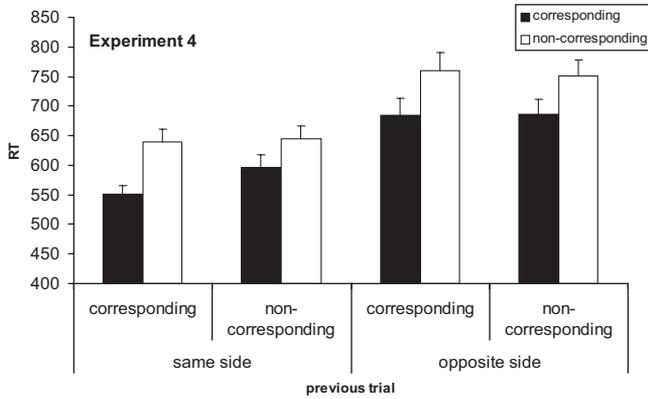
Figure 9. Average reaction times for Experiment 4. Note that opposite-side sequences were complete-change sequences. Error bars indicate standard error of the mean. RT = reaction time.

but not for opposite-side sequences, $F(1, 11) < 1$ (see Figure 10). The pattern of data showed strong sequential modulations on same-side sequences; the Simon effect was 100 ms after corresponding trials and 43 ms after noncorresponding trials. Sequential effects were absent in opposite-side sequences, with the Simon effect 76 ms after corresponding trials and 65 ms after noncorresponding trials (see Table 5). Because these trials did not include any repetition of any kind, we conclude that sequential modulations on the same-side sequences were due to control, which was recruited locally.

Error rates. A three-way ANOVA on the error rates with the variables of side (same vs. opposite), correspondence on previous trial, and correspondence on current trial yielded a significant effect of side, $F(1, 11) = 18.33$, $p < .002$, a significant effect of correspondence on current trial, $F(1, 11) = 17.13$, $p < .005$, and a two-way interaction between side and previous correspondence, $F(1, 11) = 5.85$, $p < .05$. No other effects were significant. The pattern of the data, however, paralleled the pattern for RT (Table 5).

Discussion

In Experiment 4, we have shown that control can be recruited locally while eliminating the possibility that these results stem from feature-integration effects. These results are in line with the results of Experiments 1 and 2, where we also had found evidence for local recruitment of control. However, in those experiments, we did not have any means of dissociating control from feature overlap in the same-side sequences. Therefore, the results of the current experiment close a critical gap by ruling out the idea that all local sequential effects are solely due to feature overlap.

General Discussion

We investigated whether control recruited by conflict would be sensitive to task structure. In Experiment 1, we used two slightly different versions of a Simon task. In the four-color version, the task could be parsed into two nonoverlapping subtasks, and we found no sequential modulations across the two subtasks. Thus, control did not appear to be recruited for the other subtask. In contrast, in the two-color version, the stimulus set was composed of the same colors for the two response sets. In this case, the sequential modulations were significant across subtasks, indicating that control was recruited for both response sets. In Experiment 2, we replicated the findings from the four-color task with stimulus and response sets that were restricted to a single cerebral hemisphere. As in Experiment 1, there were local sequential modulations only when the task could be parsed into two nonoverlapping subtasks. The results of Experiment 3 showed that having a common response set for two nonoverlapping stimulus sets is sufficient to obtain sequential modulations across the two different stimulus sets. Last, in Experiment 4, we showed local control in complete-change trials. These results suggest that control processes that are recruited by conflict are sensitive to task structure and that the task structure is based on representations that take into account both stimulus and response properties.

Sequential modulations are robust across a range of tasks. Simon (Stürmer et al., 2002), Stroop (Kerns et al., 2004), and flanker
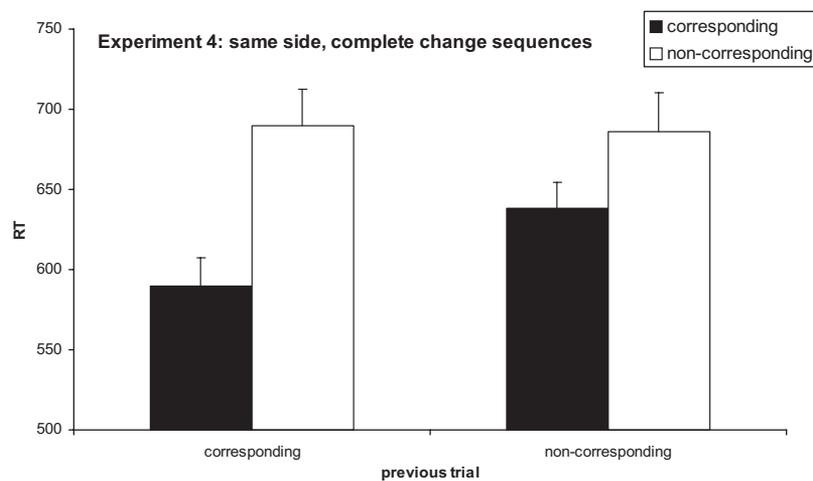

Figure 10. Sequential modulations on the same-side sequences after eliminating all but complete-change trials in Experiment 4. Error bars indicate standard error of the mean. RT = reaction time.

(Gratton et al., 1992) tasks all show reduced compatibility effects on trials following incompatible trials compared with trials following compatible trials. Given this range, one might suppose that the phenomenon was driven by strategic factors—participants might simply engage in more effortful processing in relation to the perceived difficulty of the task. However, the results from the present experiments demonstrate that the effects of conflict are not generic; conflict induces changes in task operations that are task specific.

Control or Feature Overlap?

The present experiments were designed within the framework of the conflict-monitoring account of sequential modulations (Botvinick et al., 2001; Kerns et al., 2004; MacDonald et al., 2000). This account holds that automatic response activation from the irrelevant stimulus information leads to response conflict on noncorresponding trials, which is detected by a conflict-monitoring process, which, in turn, signals control processes to alter task operations appropriately on the next trial.

The present study, like previous studies (Akçay & Hazeltine, 2007; Kerns et al., 2004; Notebaert & Verguts, in press; Ullsperger et al., 2005), suggests that sequential modulations are unlikely to be solely due to some kind of feature-overlap effects. We report three instances where complete-change sequences produced significant sequential modulations. The first case was in the two-color condition of Experiment 1, in which opposite-side sequences yielded significant sequential modulations after eliminating color repetition trials. Similarly, in Experiment 3, sequential modulations were significant for different-stimulus-set sequences when the response repetitions were eliminated. Furthermore, in Experiment 4, we showed local sequential modulations in complete chance sequences, ruling out a bottom-up account for same-side sequences as well. These findings are in line with the studies reviewed earlier.

It is interesting to note, despite the multitude of studies (see earlier discussion), that a recent study (Wendt, Kluwe, & Peters, 2006) claimed to have found support for feature-overlap effects as the sole source of sequential modulation. In this study, Wendt et al. (2006) delayed the response with various manipulations to abolish an overall Simon effect and presumably conflict. Despite this, they observed sequential modulations in the data, with no Simon effect after corresponding trials and a reversed effect after noncorresponding trials. We think that the authors were correct in attributing this to feature-overlap effects (although not necessarily to feature-integration effects) rather than control effects. However, Wendt et al. did not consider the possibility that in cases where conflict did occur, control as well as feature-overlap effects might have operated to cause the pattern of sequential modulations, as demonstrated by earlier studies (Akçay & Hazeltine, 2007; Notebaert et al., 2006). Thus, their findings would be completely in line with a conflict-monitoring account, because conflict monitoring does not preclude feature-overlap effects.

Is Control Recruited by Conflict?

Our experiments can also be viewed in the context of the question of whether conflict itself is a sufficient factor in recruiting control. Researchers have reported conditions in which conflict does not increase control. Using electromyographic activation of the incorrect response, a commonly used measure of response conflict, Burle et al. (2005) looked at trials with incorrect response activation in a two-choice Simon task. Because incorrect response activation can occur on corresponding trials as well as noncorresponding trials, they were able to dissociate conflict from correspondence. They found that after noncorresponding trials with incorrect response activation, there was a decrease in Simon effect. However, after corresponding trials with incorrect response activation, there was no decrease in Simon effect. Thus, the decrease seemed to be caused not by the incorrect response activation, but rather by the correspondence of the stimulus and response locations.

A similar finding was reported by Kunde (2003) in a prime–probe correspondence paradigm. He found that primes with very short durations, which escape conscious perception, do not lead to sequential modulations, whereas they do cause a correspondence effect that can be seen in RT. This finding suggests that to recruit control, there has to be awareness of what caused the conflict (in this case the prime). In other words, for conflict to recruit control, perhaps there has to be a perceivable source of conflict in the stimulus.

This argument can be applied to Burle et al.'s (2005) finding as well. On noncorresponding trials, there was a mismatch between the stimulus location and response location, which could potentially be perceived as leading to the response conflict. On the other hand, on corresponding trials, there was presumably nothing in the stimulus that could be perceived as potentially leading to conflict. In this case, one of the necessary conditions for recruiting control was lacking, and control was not recruited.

In summary, the findings of Kunde's (2003) study and Burle et al.'s (2005) study do not require abandoning the concept of conflict as a factor in cognitive control. Rather, they underscore other factors that come into play in the recruitment of control. Conflict, although possibly a valuable signal to recruit control, is itself not sufficient, and the recruitment of control depends on other factors. In this perspective, the present study suggests that conflict fails to recruit control for other tasks, as seen in the local sequential modulations. This finding suggests that recruitment of control, therefore, may not depend only on awareness of the source of conflict, but the source of conflict (i.e., the task where conflict occurred) might also directly determine the extent of control.

Other Evidence for Local Sequential Modulations

Multiple studies have investigated the issue of the scope of control. Kiesel et al. (2006) used a parity task with a magnitude task that had digits as stimuli. They found that when participants switched from one task to another across consecutive trials, sequential modulations were abolished, a pattern we have replicated in Experiments 1, 2, and 4. It is interesting to note that task switches in Kiesel et al.'s study involved the same stimulus and response sets—conditions that typically create large task-switching costs. On the surface, this might seem incongruent with the argument that sharing stimulus and/or response sets is critical to integrate to subtasks. However, we do not argue that sharing stimulus–response sets is the only critical factor. A task involves not only stimulus and response sets, but mappings between them that the cognitive system has to represent. If these mappings are

inconsistent across tasks (as they are for a subset of stimuli in the parity/magnitude tasks), then the system may not be able to represent the two tasks as an integrated whole. The sharing of stimulus–response sets may be an important factor in how the tasks are represented. Consistent stimulus–response mappings for the two tasks—that is, no stimuli are classified one way in one set and a different way in the other set—may be necessary for the integration of the two tasks so that adaptive control in one task transfers to the other. Note that all of our experiments used tasks with consistent mappings in this sense; the stimulus–response mappings were identical across two subtasks as in Experiment 1, two-color version, and were equally arbitrary in the rest of the experiments.

Another recent study addressing the question of the scope of control used two Stroop-like tasks, a name Stroop task and a number Stroop task (Fernandez-Duque & Knight, in press). In their first experiment, the authors found evidence for local control. However, after eliminating complete repetition trials (in which the exact stimulus combination occurred in the $n - 2$ trial), they found that sequential modulations were abolished even within task and thus concluded that there was no control effect in this experiment. As Notebaert et al. (2006) suggested, however, eliminating complete repetitions is not the cleanest way of dissociating control from a bottom-up account. Several studies (e.g., Akçay & Hazeltine, 2007; Kerns et al., 2004), including the present one, have shown sequential modulations of control by looking only at complete-change trials, where no repetition of any sort occurs. Thus, it is not clear whether the local sequential modulations in Fernandez-Duque and Knight's (in press) study were due to priming as they claimed or were due to control. As discussed earlier, a priming account seems implausible for local control in the current experiments.

Of importance, Fernandez-Duque and Knight (in press) identified yet another factor in determining the scope of control, namely the role of voluntary control. When these researchers gave participants a predictive cue about the congruency of the stimulus in one task (their Experiments 3 and 4), they found that the Stroop effect on the other task diminished significantly. This suggests that when control is recruited explicitly by voluntary processes (as opposed to automatically by conflict on the preceding trial), it can be recruited globally.

Scope of Conflict With Respect to Different Types of Conflict

In our study, we had only one type of conflict in all the experiments, namely, conflict between the irrelevant location of the stimulus and the location of the correct response (Simon conflict). This was because our goal was to examine the effects of task structure while holding the source of the conflict constant. Other studies have looked at a related but nevertheless different question (Kunde & Wühr, 2006; Wendt et al., 2006): Does type of conflict determine the scope of control recruited by it? There are at least two possibilities. Conflict of one type might not lead to recruitment of control processes that decrease conflict of another type. This can be called local control; however, in this case, it is local with respect to the conflict type rather than the structure of the task. The other possibility is that control might be recruited globally within a task, so that no matter what stimulus event (e.g.,

irrelevant primes or irrelevant stimulus location information) caused conflict on the preceding trial, in the next trial, processing is less prone to any kind of irrelevant information. Note that the local–global distinction made here is, in principle, orthogonal to the task-based local–global distinction we have made in this article: Within a task, one can have different types of conflict, and one might find local control based on the type of conflict. Alternatively, across tasks one can have the same kind of conflict (e.g., current experiments), and one might find local control (as we and other researchers have found).

Studies examining the scope of control with respect to different kinds of conflicts within a task have produced mixed results so far. On the one hand, Kunde and Wühr (2006) have found that conflict recruited control across two types of conflict (prime–target correspondence and the Simon effect). On the other hand, Wendt et al. (2006) found that sequential modulations were seen within only one conflict type and not across different types of conflict (e.g., Simon-type conflict did not result in sequential modulations in the flanker effect).

More research is needed in answering the question of the scope of control with respect to different types of conflict. Future research should take into account several factors. One critical factor is the nature of the information that is causing the conflict and hence the control processes recruited to deal with it. For instance, in Kunde and Wühr's (2006) experiment, which found global control across prime–target correspondence and the Simon effect, the nature of the irrelevant information might have been similar across the two forms of conflict. Both activated an irrelevant response location through spatial information. Therefore, even though the peripheral stimulus events were different, the type of conflict within the system might have had the same effect in such a task.

The Functional Architecture of Control

Along with the other studies examining the scope of control, the current study has implications for the functional architecture of control processes. Our results indicate that control is recruited by conflict depending on flexible task representations even when the same type of conflict is seen across different tasks. This suggests that control processes do not just modulate processing modules associated with different sources of irrelevant information. Rather, the system may assign processing modules flexibly so that each task is governed by more or less independent sets of processing modules. There is little understanding of how the brain parses tasks into separate representations. One possibility is that the brain may encode irrelevant information in continuous representations (as opposed to discrete nodes representing locations, as in traditional connectionist modules; cf. Erlhagen & Schöner, 2002). Then the division between tasks can result naturally from the continuous nature of these representations (e.g., closer locations could be grouped as one task, whereas more distant locations could be represented in different tasks). Such a hypothesis allows testable predictions about the effects of spatial layout of stimuli and responses. Note, however, that parsing based on spatial layout of the tasks is not likely to be the cause of differences between the four-color and two-color versions in Experiment 1, because these were identical with respect to the spatial layout of the stimuli.

Evidence suggests that parsing can also be based on the relevant information (Notebaert & Verguts, in press).

However the parsing of the tasks is done, the next question is, how is the conflict-monitoring module sensitive to the locus of conflict in different task representations? There are several possibilities. One possibility is that the conflict-monitoring module not only evaluates the level of conflict but also registers the source of the conflict. In other words, conflict might not be signaling just the need for control but also the locus in the system, where something had gone wrong. This would potentially increase the amount of processing required by the conflict monitor. Another possibility is that the conflict-monitoring module allocates resources to each task separately so that, in effect, each task has a mini-conflict-module.

Alternatively, the amount of modulation each task gets might be weighted by the total activation of that task, so that the more active task benefits more from control effects. This would result in stronger sequential modulations in the same task sequences, because the last task encountered would presumably be more active than the alternative task. Such a model would also satisfy the current spirit of theorizing about the central executive, because in this scheme no additional smart module needs to be assigned to finding out either where the conflict is or where to deploy control.

In summary, the present findings emphasize that the notion of a task seems to play a critical role in behavior. As shown in our research and in that of others (e.g., Kiesel et al., 2006), the task determines the scope of the operation of the control processes. These findings underscore the need to develop more a rigorous understanding of how the brain represents a task, a need that is echoed in the dual-task and task-switching literatures.

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