Task Switching Versus Cue Switching: Using Transition Cuing to Disentangle Sequential Effects in Task-Switching Performance

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Recent methodological advances have allowed researchers to address confounds in the measurement of task-switch costs in task-switching performance by dissociating cue switching from task switching. For example, in the transition-cuing procedure, which involves presenting cues for task transitions rather than for tasks, cue transitions (cue switches and cue repetitions) and task transitions (task switches and task repetitions) can be examined in a complete factorial design. Transition cuing removes the confound between cue transitions and first-order task transitions, but it introduces a confound between cue transitions and longer task sequences. In the present study, transition cuing was studied with two cues per transition (*REPEAT* and *AGAIN* for task repetitions; *SWITCH* and *CHANGE* for task switches), enabling a partial deconfounding of cue transitions and task sequences. Two experiments revealed robust sequential effects, with higher order task transitions affecting performance when cue transitions were held constant and with cue transitions affecting performance when task sequences were held constant. Methodological and theoretical implications of these findings for research on task switching are discussed.

Keywords: task switching, switch cost, transition cuing, sequential effects, executive control

The task-switching paradigm has been used extensively to investigate executive control of cognition (Monsell, 2003). Task switches are typically slower and less accurate than task repetitions, and these *task-switch costs* are thought to reflect some aspect of executive control processing, with some researchers suggesting that task-switch costs in response time (RT) reflect the duration of an executive control process (e.g., Meiran, 1996; Monsell & Mizon, 2006; Rubinstein, Meyer, & Evans, 2001). However, if one is to make inferences about task-switch costs as indices of executive control, it is important to identify factors that affect how they are interpreted.

Many procedures have been used to observe task-switch costs, but perhaps the most popular is the *task-cuing procedure*, which involves presenting a cue that indicates which task to perform on a target. When one cue is assigned to each task and cues are presented randomly across trials, task-switch cost can be measured as the difference between cue–task switches and cue–task repetitions (Meiran, 1996). However, cue transitions and task transitions are confounded in this situation (i.e., cue repetitions [CRs] are always task repetitions; cue switches [CSs] are always task switches), raising the possibility that the measured task-switch costs reflect cue-transition effects rather than task-transition ef-

fects. This issue can be addressed by using two cues per task (e.g., A1 and A2 for Task A, B1 and B2 for Task B), allowing three types of trials: task repetitions involving CRs (e.g., A1, A1), task repetitions involving CSs (e.g., A1, A2), and task switches involving CSs (e.g., A1, B1).

Task-switch cost can be measured as the difference between the latter two trial types, for which the task transition differs but the cue transition remains unchanged. In this situation, task-switch costs have still been obtained, but they are often smaller than the differences between the two types of task repetitions (for which the cue transition changes), suggesting that earlier task-switch costs measured with one cue per task were contaminated with cue-transition effects (Logan & Bundesen, 2003; Mayr & Kliegl, 2003).

The use of two cues per task represents a methodological advance over previous work, but it has a limitation: Task transitions and cue transitions cannot be combined factorially because it is not possible to have a task switch involving a CR. One way to achieve a complete factorial design is to replace task cues with transition cues that indicate when to repeat and switch tasks. Forstmann, Brass, and Koch (in press; see also Forstmann, Brass, Koch, & von Cramon, 2005; Rushworth, Hadland, Paus, & Sipila, 2002) used this *transition-cuing procedure* by assigning an arbitrary shape cue to each transition: a triangle on its tip indicated a task repetition and a triangle on its base indicated a task switch. They found that CSs were slower than CRs for task repetitions (replicating what has been found with two cues per task) but that the opposite effect occurred for task switches.

Forstmann et al. (in press) argued that the cue-transition effect associated with task switches in their study was inconsistent with theories that attributed task-switch costs to priming of cue encoding (Logan & Bundesen, 2003; Schneider & Logan, 2005) because one would expect CRs to always be faster than CSs. They argued

that their data were more consistent with the idea that a cue–task association was formed after every trial and that this binding affected cue-based task retrieval on the next trial (Koch & Allport, 2006; Mayr & Kliegl, 2003) by facilitating or interfering with retrieval depending on the cue transition.

However, the results of Forstmann et al. (in press) come from a transition-cuing experiment involving only one cue per transition, which has its own limitation: Cue transitions are confounded with task sequences. For example, if the word *REPEAT* signals a task repetition and the word *SWITCH* signals a task switch, then cue transitions are associated with task sequences as follows:

1. Repeating a task-switch cue (e.g., *SWITCH*, *SWITCH*) produces an ABA sequence.

2. Switching from a task-repetition cue to a task-switch cue (e.g., *REPEAT*, *SWITCH*) produces a BBA sequence.

3. Switching from a task-switch cue to a task-repetition cue (e.g., *SWITCH*, *REPEAT*) produces a BAA sequence.

4. Repeating a task-repetition cue (e.g., *REPEAT*, *REPEAT*) produces an AAA sequence.

Performance may be affected by more than just immediate task transitions, possibly obscuring concomitant cue-transition effects. Indeed, a prominent finding in the literature is that ABA sequences are slower than other sequences (e.g., CBA), an effect attributed to persisting inhibition of Task A (Mayr & Keele, 2000). Run-position analyses of unpredictable task-switching data have indicated that ABA and BAA sequences are slower than BBA and AAA sequences, respectively (Monsell, Sumner, & Waters, 2003), and this finding is supported by recent analyses of higher order sequential effects in task switching (Brown, Reynolds, & Braver, in press; see also Gehring, Bryck, Jonides, Albin, & Badre, 2003). In recognition of these sequential effects, Forstmann et al. (in press) concluded that "we face the problem that the use of transition cues to dissociate the effects of cue switching and task switching results in confounds with differences in the associated task sequences. It is unclear how worrisome this confound really is, ranging from minimal to potentially rather severe."

The purpose of the present study was to address this confound so as to better understand sequential effects associated with cue transitions and task transitions. To this end, we conducted two experiments using the transition-cuing procedure, with two meaningful word cues assigned to each transition: A task repetition was signaled by *REPEAT* or *AGAIN*, and a task switch was signaled by *SWITCH* or *CHANGE*. The use of two cues per transition permits six different task sequences that span three consecutive trials and involve either a CS or a CR from Trial n − 1 to n: ABA–CS (e.g., *CHANGE*, *SWITCH*), ABA–CR (e.g., *SWITCH*, *SWITCH*), BBA–CS (e.g., *REPEAT*, *SWITCH*), BAA–CS (e.g., *SWITCH*, *REPEAT*), AAA–CS (e.g., *AGAIN*, *REPEAT*), and AAA–CR (e.g., *REPEAT*, *REPEAT*).

The two task sequences not possible in Forstmann et al.'s (in press) study are ABA–CS and AAA–CS, which are critical for assessing the confound between cue transition and task sequence in two ways. First, one can compare different task sequences that all involve CSs (ABA–CS, BBA–CS, BAA–CS, AAA–CS) to see how first- and second-order task transitions (from Trial n − 1 to n and from Trial n − 2 to n − 1, respectively) affect performance.[1] Second, one can compare different cue transitions associated with the same task sequence (ABA–CS vs. ABA–CR; AAA–CS vs. AAA–CR) to see if CRs are actually slower than CSs for task switches (i.e., ABA–CR > ABA–CS), which could have implications for theorizing about cue–task associations.

Besides the different task sequences, we also examined other variables that could affect performance. The cue–target interval (CTI) was manipulated to measure the time course functions associated with each task sequence, which are informative for interpreting cue-transition effects as evidence for priming of cue encoding (Logan & Bundesen, 2003; Schneider & Logan, 2005). To facilitate scoring of the manual responses for accuracy, we used a 1:1 response–key mapping in Experiment 1 (with tasks mapped to different hands), but to address the confound between task switching and hand switching that occurs with this mapping, we used a 2:1 response–key mapping in Experiment 2. Finally, to demonstrate that any sequential effects are not limited to specific tasks, we used digit and letter judgments in Experiments 1 and 2, respectively.

Method

Subjects

Fifty individuals (25 per experiment) from Vanderbilt University participated for course credit or monetary compensation.

Apparatus

Both experiments used E-Prime software (Psychology Software Tools, Pittsburgh, PA) operating on computers that registered input from keyboards and displayed output on monitors.

Cues, Tasks, and Stimuli

Subjects were cued to repeat or switch tasks across trials. The cues were the words *REPEAT*, *AGAIN*, *SWITCH*, and *CHANGE*. The cues *REPEAT* (33 mm × 5 mm) and *AGAIN* (28 mm × 5 mm) indicated that subjects were to perform the same task they performed on the preceding trial (i.e., repeat tasks). The cues *SWITCH* and *CHANGE* (each 33 mm × 5 mm) indicated that subjects were to perform the task they did not perform on the preceding trial (i.e., switch tasks). In Experiment 1, the subjects judged whether a digit was odd or even (parity task) or lower or higher than 5 (magnitude task). The targets were *1*, *2*, *3*, *4*, *6*, *7*, *8*, and *9* (each 6 mm × 7 mm). In Experiment 2, the subjects judged whether a letter was in the first or second half of the alphabet (half task) or was formed entirely of straight lines or had curved lines (form task). The

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[1] Throughout this article, specific transitions within a task sequence will be referenced as follows: A *first-order* transition is from Trial n − 1 to n, a *second-order* transition is from Trial n − 2 to n − 1, and a *third-order* transition is from Trial n − 3 to n − 2.

targets were *C, D, G, J, T, V, X,* and *Z* (each 6 mm × 7 mm), which were all response incongruent (i.e., the correct response differed between tasks) with the prescribed response–key mapping (see below). All stimuli were displayed in white on a black background and viewed at a distance of about 60 cm.

Procedure

Subjects completed the experiments in private testing rooms after providing informed consent. Instructions were presented on-screen and explained by the experimenter.

Each experiment was divided into blocks of 80 trials. Each block began with a screen indicating the previous task to be held in memory for interpreting the cue on the first trial. For example, in Experiment 1, if the parity task was designated as the previous task to start a block and the cue on the first trial was *SWITCH*, then the magnitude task had to be performed. Subsequent trials were based on subjects' memory of the task performed on the preceding trial, and the instructions emphasized the importance of continually updating memory for the previous task.

Each trial used a similar format. First, a cue was displayed in the center of the screen. After a variable CTI (0, 200, 400, 600, or 800 ms), a target appeared 17 mm below the cue. The cue and the target remained onscreen until the subject responded by pressing a key. In Experiment 1 (1:1 response–key mapping), subjects pressed *D* (for odd) or *F* (for even) with the middle and index fingers of the left hand, respectively, and *J* (for low) or *K* (for high) with the index and middle fingers of the right hand, respectively. In Experiment 2 (2:1 response–key mapping), subjects pressed *1* (for first half or straight form) or *2* (for second half or curved form) on the numeric keypad with the index and middle fingers of the right hand, respectively. After a response, the cue and the target were erased, and the next trial commenced after 500 ms. The response and associated RT were recorded for every trial. The cue, target, and CTI on each trial were randomly selected from a 4 (cue) × 8 (target) × 5 (CTI) block design.

Both experiments started with practice trials (1 block in Experiment 1; 2 blocks in Experiment 2) followed by 10 blocks of experimental trials. Blocks were separated by rest periods, and every two blocks represented one replication of the block design.

Results

Practice blocks, the first two trials of each block, and trials with RTs exceeding 3,500 ms (2.0% and 3.8% of trials in Experiments 1 and 2, respectively) were excluded from all analyses. Task sequences were classified post hoc from cue transitions. Mean error rate and RT (for correct trials) for Trial *n* of each task sequence are presented in Figure 1. For statistical analysis, these data were partitioned by CTI and submitted to 6 (task sequence) × 5 (CTI) repeated-measures analyses of variance (ANOVAs), which are summarized in Table 1.

Error Rate Analysis

Mean error rates were 2.2% (*SE* = 0.1%) and 1.6% (*SE* = 0.1%) in Experiments 1 and 2, respectively.[2] For Experiment 1, an ANOVA (see Table 1) revealed a main effect of task sequence,

reflecting a higher error rate for ABA–CR trials (*M* = 4.5%, *SE* = 0.5%) compared with all other trials (*M* = 1.8%, *SE* = 0.2%; see Figure 1). There was no main effect of CTI, but there was an interaction between task sequence and CTI reflecting a decrease in error rate for ABA–CR trials from 6.2% to 2.9% as CTI increased from 0 to 800 ms. For Experiment 2, an ANOVA (see Table 1) revealed a main effect of task sequence, reflecting a higher error rate for ABA–CR trials (*M* = 3.3%, *SE* = 0.4%) compared with all other trials (*M* = 1.3%, *SE* = 0.2%; see Figure 1). There was no main effect of CTI and no interaction between task sequence and CTI. An additional ANOVA with experiment as a between-subjects factor revealed no further effects.

RT Analysis

For both experiments, ANOVAs (see Table 1) revealed a main effect of task sequence. Pairwise comparisons with Fisher's least significant difference (α = .05), which was 41 and 59 ms for Experiments 1 and 2, respectively, indicated that mean RTs differed significantly across all task sequences such that ABA–CS > ABA–CR > BBA–CS > BAA–CS > AAA–CS > AAA–CR. As can be seen in Figure 1, for first-order task transitions (from Trial *n* − 1 to *n*), all task switches were slower than all task repetitions (ABA, BBA > BAA, AAA). However, these task-switch costs were affected by second-order task transitions (from Trial *n* − 2 to *n* − 1) such that RT for a given first-order task transition was longer when the second-order task transition was a task switch rather than a task repetition (ABA > BBA; BAA > AAA), even when all task sequences involved CSs (ABA–CS > BBA–CS; BAA–CS > AAA–CS). RT was also affected by first-order cue transitions (from Trial *n* − 1 to *n*) such that CSs were slower than CRs when the task

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[2] Given that a 1-back memory task is embedded within the transition-cuing procedure, it is important to acknowledge that performance could be affected by occasional memory lapses. To identify and account for deviations from the veridical task sequence arising from memory lapses, we scored accuracy by the following method. In Experiment 1, the 1:1 response–key mapping allows one to distinguish between errors that involve pressing the wrong key for the correct task and errors that involve pressing a key for the incorrect task. If three or more consecutive responses for the incorrect task occurred, we assumed that the subject had experienced a memory lapse and had switched to the complementary task sequence. The first two trials in the series of errors were excluded, and subsequent trials were scored as correct or incorrect according to the complementary task sequence. In Experiment 2, the 2:1 response–key mapping does not allow one to distinguish between different errors. However, all targets were chosen to be response incongruent with the prescribed response–key mapping, therefore a consecutive series of errors was assumed to reflect a switch to the complementary task sequence, and accuracy was scored according to the same method as in Experiment 1. Fortunately, memory lapses were rare—the mean numbers of deviations from the veridical task sequence were 2.3 and 2.8 in Experiments 1 and 2, respectively.

First-order task transition			TS		TR		

Figure 1. Mean response time (RT, in ms) and error rate (ER, as a percentage of incorrect responses) on Trial *n* as a function of first-order cue transition (CS = cue switch; CR = cue repetition) and first- and second-order task transitions (TS = task switch; TR = task repetition). Standard errors of the means appear in parentheses.

sequence was held constant (ABA–CS > ABA–CR; AAA–CS > AAA–CR).[3]

For both experiments, ANOVAs (see Table 1) also revealed a main effect of CTI, reflecting the typical decrease in RT with CTI, and an interaction between task sequence and CTI (see Figure 2). The interaction was assessed in two ways. First, we examined changes in the cue-transition effect across CTI, holding task sequence constant. Mean RTs were calculated for Trial *n* of ABA–CS, ABA–CR, AAA–CS, and AAA–CR sequences at each CTI. The data for the ABA and AAA sequences were submitted to separate ANOVAs, which revealed that the cue-transition effect did not vary with CTI for the ABA sequence in either experiment (both *p*s > .23) but did vary for the AAA sequence (both *p*s < .005), decreasing by 129 and 187 ms in Experiments 1 and 2, respectively, as CTI increased from 0 to 800 ms (see Figure 2). Second, we examined changes in the task-transition effect (i.e., task-switch cost) across CTI, holding cue transition constant. Mean RTs were calculated for task switches (averaging Trial *n* RTs for ABA–CS and BBA–CS sequences) and task repetitions (averaging Trial *n* RTs for BAA–CS and AAA–CS sequences) at each CTI. ANOVAs revealed that the task-transition effect varied with CTI in each experiment (both *p*s < .05). In Experiment 1, the interaction reflects a decrease in task-switch cost from 199 to 110 ms as CTI increased from 0 to 600 ms, followed by an increase to 212 ms at the CTI of 800 ms. In Experiment 2, the interaction reflects a monotonic decrease in task-switch cost from 238 to 163 ms as CTI increased from 0 to 800 ms.

An additional ANOVA on these data with experiment as a between-subjects factor revealed that RT was longer in Experiment 2 (*M* = 1,201 ms, *SE* = 13 ms) than in Experiment 1 (*M* = 1,066 ms, *SE* = 11 ms), $F(1, 48) = 8.13$, $MS_e = 839,218.23$, $p < .01$, $\eta_p^2 = .14$, and there was an interaction between experiment and CTI, $F(4, 192) = 7.82$, $MS_e = 17,471.57$, $p < .001$, $\eta_p^2 = .14$, such that the CTI effect was smaller in Experiment 1 than in Experiment 2. However, no other effects were significant, indicating that differences in response–key mappings and tasks did not affect the differences among task sequences.[4]

To determine the extent to which performance was modulated by sequential effects, we also examined the RT data for four-task sequences (e.g., BABA), excluding the first three trials of each block and focusing on only those task sequences with both first- and second-order CSs (from Trial *n* – 1 to *n* and from Trial *n* – 2

to *n* – 1, respectively). Mean RTs for Trial *n* of each of the eight possible four-task sequences are presented in Figure 3. These data were submitted to a 2 (first-order task transition) × 2 (second-order task transition) × 2 (third-order task transition) ANOVA, which is summarized in Table 2. Consistent with earlier analyses, there were significant main effects of first- and second-order task transitions. However, there was also a significant main effect of third-order task transition (from Trial *n* – 3 to *n* – 2; see Table 2) such that third-order task switches were 44 and 57 ms slower than third-order task repetitions in Experiments 1 and 2, respectively. The significant three-way interaction in Experiment 2 reflects the slight reversal of RTs for third-order task switches and task repetitions for BABA and AABA sequences (see Figure 3).

The preceding analysis indicates that RT becomes progressively longer with every task switch in a task sequence. Put another way, RT becomes longer as task-switch frequency increases (for related findings, see Schneider & Logan, 2006a, 2006b). To examine this angle more directly, we recoded the data from the preceding

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[3] Given that ABA–CS trials were slower but more accurate than ABA–CR trials, the RT comparison could be compromised by a speed–accuracy tradeoff. To investigate this possibility, we calculated mean RTs separately for error and correct responses for the ABA–CS and ABA–CR task sequences, collapsing across CTI and excluding subjects who did not make any errors for one or both sequences (6 and 8 subjects in Experiments 1 and 2, respectively). If there was a speed–accuracy tradeoff, then one might expect error responses to be significantly faster than correct responses. However, separate 2 (task sequence) × 2 (response type) ANOVAs revealed that the difference between response types was not significant in either experiment, Experiment 1: $F(1, 18) = 3.32$, $MS_e = 111,418.81$, $p = .09$; Experiment 2: $F(1, 16) = 2.89$, $MS_e = 163,969.84$, $p = .11$, and that there was no interaction between task sequence and response type, Experiment 1: $F(1, 18) = 1.27$, $MS_e = 90,236.32$, $p = .27$; Experiment 2: $F(1, 16) = 1.10$, $MS_e = 70,264.15$, $p = .31$, suggesting that the RT comparison is not compromised.

[4] The lack of an interaction between experiment and task sequence is interesting in light of the finding that task-switch costs tend to be larger with 2:1 response–key mappings than with 1:1 response–key mappings (Meiran, 2000). However, our data were numerically consistent with this finding: Task-switch cost was larger with the 2:1 response–key mapping in Experiment 2 (342 ms) than with the 1:1 response–key mapping in Experiment 1 (292 ms), although it is important to note that the experiments differed in overall RT.

Table 1

Summary Table for the ANOVAs Conducted on Mean Response Time and Error Rate as a Function of Task Sequence and Cue–Target Interval (CTI)

Effect	df	Experiment 1						Experiment 2					
		Response time			Error rate			Response time			Error rate		
		F	MS_e	η_p^2	F	MS_e	η_p^2	F	MS_e	η_p^2	F	MS_e	η_p^2
Task sequence (T)	5, 120	163.15**	26,777.65	.87	12.37**	13.32	.34	103.05**	55,200.83	.81	7.51**	12.71	.24
CTI (C)	4, 96	272.06**	17,725.25	.92	1.74	11.33	.07	421.08**	17,217.89	.95	0.47	8.00	.02
T × C	20, 480	4.14**	11,744.51	.15	1.71*	11.08	.07	5.32**	13,522.62	.18	1.09	8.59	.04

Note. ANOVAs = analyses of variance. * $p < .05$. ** $p < .001$.
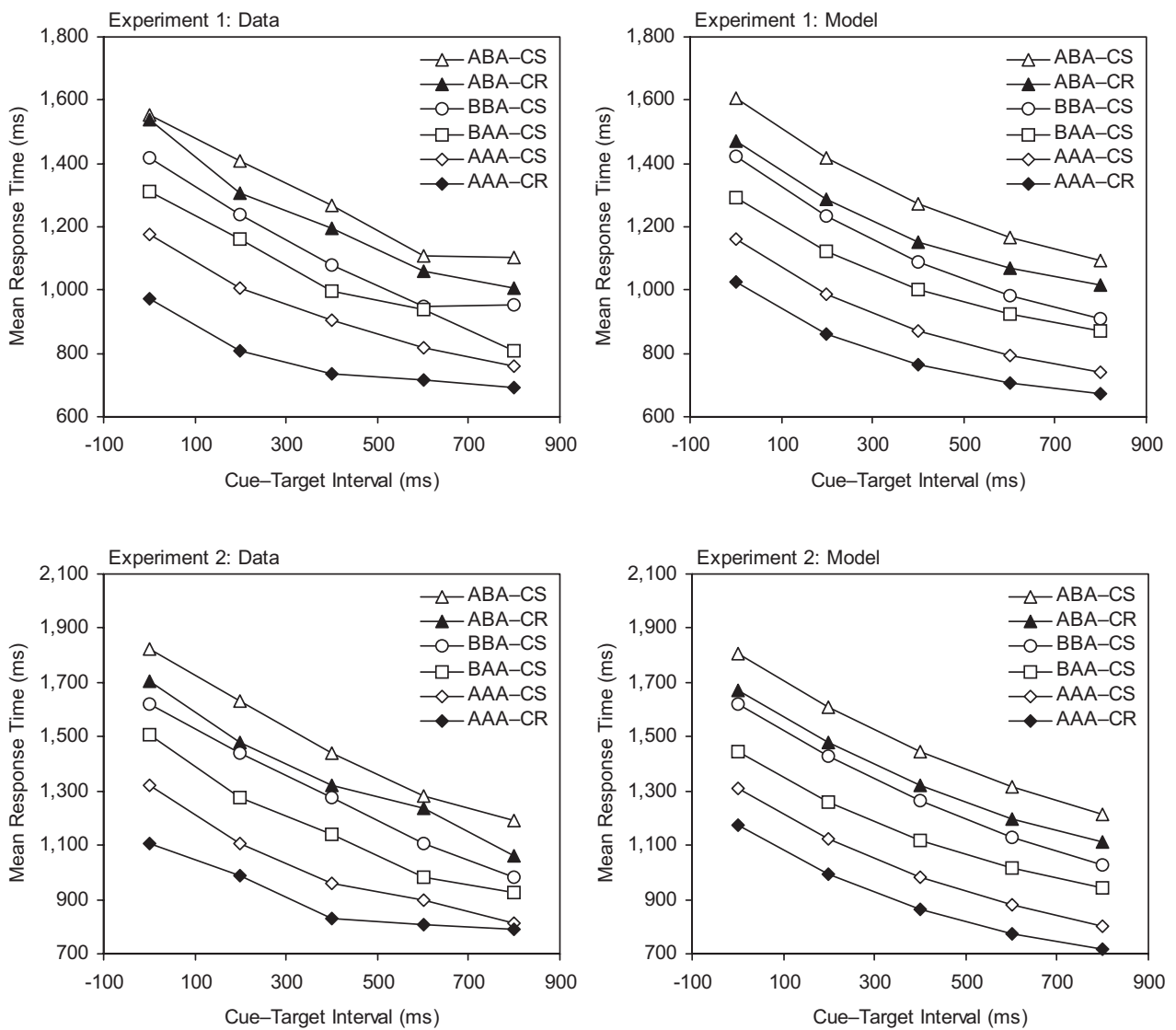

Figure 2. Mean response time on Trial *n* as a function of task sequence and cue–target interval. Symbols with the same shape denote the same task sequence (ABA, BBA, BAA, or AAA). Unfilled and filled symbols denote first-order cue switches (CS) and cue repetitions (CR), respectively. Observed data and model predictions appear in the left and right panels, respectively.
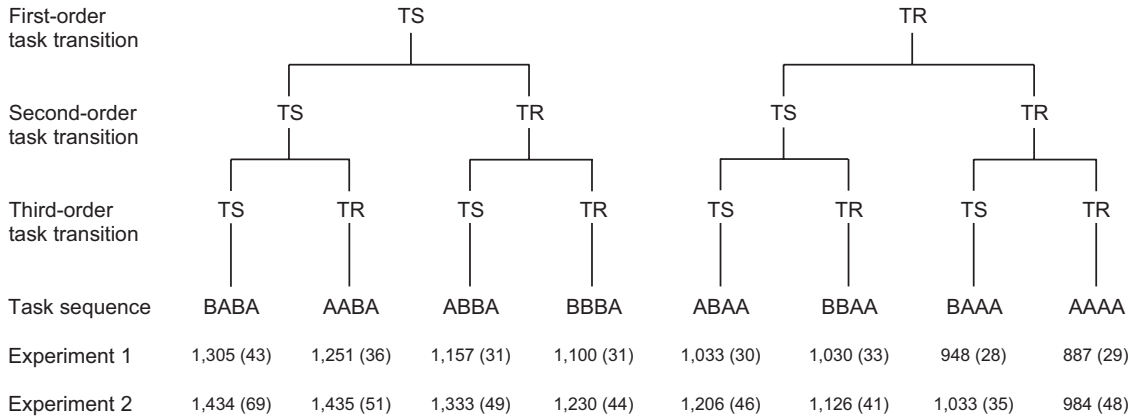
First-order task transition	TS				TR			
Second-order task transition	TS		TR		TS		TR	
Third-order task transition	TS	TR	TS	TR	TS	TR	TS	TR
Task sequence	BABA	AABA	ABBA	BBBA	ABAA	BBAA	BAAA	AAAA
Experiment 1	1,305 (43)	1,251 (36)	1,157 (31)	1,100 (31)	1,033 (30)	1,030 (33)	948 (28)	887 (29)
Experiment 2	1,434 (69)	1,435 (51)	1,333 (49)	1,230 (44)	1,206 (46)	1,126 (41)	1,033 (35)	984 (48)

Figure 3. Mean response time (in ms) on Trial *n* as a function of first-, second-, and third-order task transitions (TS = task switch; TR = task repetition). Standard errors of the means appear in parentheses.

analysis in terms of the number of task switches in each task sequence (0, 1, 2, or 3). Mean RTs as a function of frequency are plotted in Figure 4; these data were submitted to one-way ANOVAs, with frequency as the factor. A linear contrast on the effect of frequency was highly significant for each experiment— Experiment 1: $F(1, 72) = 783.12$, $MS_e = 3,649.11$, $p < .001$, $\eta_p^2 = .92$; Experiment 2: $F(1, 72) = 384.16$, $MS_e = 9,293.25$, $p < .001$, $\eta_p^2 = .84$—and the data in Figure 4 are well-represented by linear functions with slopes of 151 and 169 ms for Experiments 1 and 2, respectively.

Discussion

The use of two cues per transition in the transition-cuing procedure allowed us to identify the following sequential effects in task-switching performance:

1. For first-order task transitions (from Trial $n − 1$ to n), regardless of the cue transition and higher order task transitions, task switches were slower than task repetitions (see Figure 1).

2. For a given task sequence (holding first- and second-order task transitions constant), mean RT was longer when the first-order cue transition was a CS rather than a CR (see Figure 1).

3. For a given first-order task transition (even when holding cue transition constant), mean RT became progressively longer with higher order task transitions that were task switches rather than task repetitions (see Figure 3). Put another way, mean RT became longer as task-switch frequency within a four-task sequence increased (see Figure 4).

The first effect reflects the standard task-switch cost; the second and third effects demonstrate how task-switch cost can be separately modulated by cue transitions and preceding task transitions. These sequential effects have important methodological and theoretical implications for research on task switching.

Methodological Implications

Our use of two cues per transition in the transition-cuing procedure represents a methodological advance over previous work because it let us partially deconfound cue transitions and task sequences. This allows us to demonstrate that cue-transition effects can contribute to task-switch costs if cue transitions are con-

Table 2

Summary Table for the ANOVAs Conducted on Mean Response Time as a Function of First-, Second-, and Third-Order Task Transitions

Effect	df	Experiment 1			Experiment 2		
		F	MS_e	η_p^2	F	MS_e	η_p^2
First-order task transition (F)	1, 24	219.38**	11,954.26	.90	95.46**	38,278.48	.80
Second-order task transition (S)	1, 24	105.55**	8,219.20	.81	53.28**	22,669.02	.69
Third-order task transition (T)	1, 24	20.87**	4,565.89	.47	12.00*	13,908.08	.33
F × S	1, 24	1.70	9,240.70	.07	0.03	14,119.99	.00
F × T	1, 24	0.85	8,498.70	.03	0.15	13,956.57	.01
S × T	1, 24	1.06	10,669.13	.04	1.05	15,733.80	.04
F × S × T	1, 24	1.38	6,881.32	.05	5.37*	10,726.46	.18

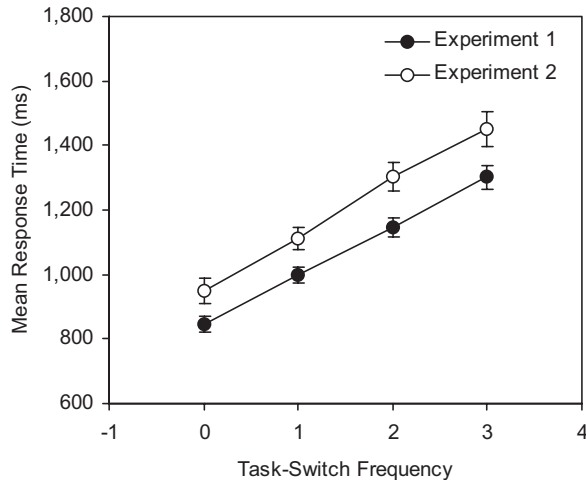*Note.* ANOVAs = analyses of variance. * $p < .05$. ** $p < .001$.

Figure 4. Mean response time on Trial *n* as a function of task-switch frequency within a four-task sequence. Error bars represent standard errors of the means.

founded with first- and second-order task transitions and that, even when the cue transition remains unchanged, second- and third-order task transitions can contribute to task-switch costs calculated from first-order task transitions.

For example, if task-switch cost is calculated from task sequences involving only CSs (ABA–CS, BBA–CS, BAA–CS, AAA–CS), it is 253 ms (averaged across Experiments 1 and 2). However, if task sequences involving CRs (ABA–CR, AAA–CR) are added in, the task-switch cost increases to 317 ms, partially reflecting the contribution of cue-transition effects. Another example is the calculation of cue-transition effects. If they are calculated from task sequences that would occur with only one cue per transition (ABA–CR, BBA–CS, BAA–CS, AAA–CR), then the cue-transition effect is 260 ms for task repetitions and –85 ms for task switches, replicating the opposing effects found by Forstmann et al. (in press). However, if cue-transition effects are calculated from the same task sequence (ABA–CS and ABA–CR; AAA–CS and AAA–CR), then the cue-transition effect is 132 ms for task repetitions and 90 ms for task switches, indicating that confounding cue transitions with task sequences can radically alter a pattern of data, which we view as "rather severe" (Forstmann et al., in press).

Theoretical Implications

Our findings are relevant for assessing ideas about cue–task associations involved in cue-based task retrieval (Forstmann et al., in press; Mayr & Kliegl, 2003) and priming of cue encoding (Logan & Bundesen, 2003; Schneider & Logan, 2005) and for theorizing about additional mechanisms that may underlie sequential effects in task-switching performance.

Recall that Forstmann et al. (in press) used one cue per transition and found that CSs were slower than CRs for task repetitions (BAA–CS > AAA–CR) but that CRs were slower than CSs for task switches (ABA–CR > BBA–CS). They explained the first effect by suggesting that cue-based retrieval of Task A on Trial *n* of the BAA–CS sequence suffered interference from Task A being

associated with a different cue on Trial *n* – 1. The second effect can be explained by assuming that cue-based retrieval of Task A on Trial *n* of the ABA–CR sequence suffered interference from the same cue being associated with Task B on Trial *n* – 1.

The idea of cue–task associations can account for our observation that AAA–CS trials were slower than AAA–CR trials, but it is incompatible with our finding that ABA–CS trials were slower than ABA–CR trials. The different cues on Trials *n* – 1 and *n* of the ABA–CS sequence would be associated with different tasks, whereas the same cue on Trials *n* – 1 and *n* of the ABA–CR sequence would be associated with different tasks, therefore one would expect ABA–CR trials to be slower than ABA–CS trials, contrary to what we found.

In contrast, both findings are compatible with the idea of priming of cue encoding such that CRs are faster than CSs because of repetition priming in short-term memory (Logan & Bundesen, 2003; Schneider & Logan, 2005). However, this conclusion must be qualified by two findings. First, although the difference between AAA–CS and AAA–CR trials decreased with CTI (as predicted by models of the priming effect, in which the contribution of cue encoding to RT decreases with CTI; see Logan & Bundesen, 2003), the difference between ABA–CS and ABA–CR trials did not decrease with CTI (see Figure 2). This null effect may indicate that cue-transition effects can be modulated by processes beyond cue encoding, but it is also possible that our experiments lacked sufficient power to detect an interaction involving ABA sequences (which were associated with the longest and most variable RTs; see Figure 1). Second, priming of cue encoding does not explain the differences between task sequences involving CSs (ABA–CS > BBA–CS > BAA–CS > AAA–CS). Even if one assumes that cues assigned to the same transition can prime each other (e.g., *CHANGE* primes *SWITCH*; Logan & Schneider, 2006b; Schneider & Logan, 2005), one would expect BBA–CS trials (e.g., *REPEAT*, *SWITCH*) to be slower than ABA–CS trials (e.g., *CHANGE*, *SWITCH*), which is contrary to what we found. Consequently, priming of cue encoding—by itself—does not suffice as a complete explanation of our data, but we contend that it should be part of any complete explanation. We believe that cue encoding must be supplemented by additional mechanisms to account for our sequential effects. What might those mechanisms be?

One possible mechanism is *mediator retrieval*. In Schneider and Logan's (2005) model of task-switching performance, response selection occurs by means of a compound retrieval cue strategy: The cue and the target are used as joint retrieval cues to select a response from memory (see also Logan & Bundesen, 2003, 2004). We have argued that when cues lack meaningful associations with unique tasks (as is the case with transition cues), mediators such as task names must be retrieved for the strategy to be successful (e.g., Logan & Schneider, 2006a). For example, the cue *SWITCH* is not associated with a specific task, therefore using it in conjunction with a target does not allow for selection of a unique response. However, combining the meaning of the word *switch* with the knowledge of the task performed on the preceding trial (e.g., the magnitude task) allows one to retrieve a task-name mediator (e.g., parity) that can be used with a target (e.g., 9) to select a unique response (e.g., odd). Task switching may require retrieval of a different mediator—a process that could be construed as a form of goal shifting (Rubinstein et al., 2001). Consequently, mediator

retrieval (and possibly response selection) may take longer on task-switch trials, resulting in task-switch costs (i.e., ABA, BBA > BAA, AAA).

To demonstrate that mediator retrieval is useful in accounting for our data and to obtain estimates of cue-encoding and mediator-retrieval effects that are separate from other differences among task sequences, we modeled the data in Figure 2 with the following equation (taken from Logan & Bundesen, 2003):

$$RT = RT_{Base} + \exp[-CTI/\mu_C] \cdot (\mu_C + \mu_M)$$

$$+ \frac{1/\mu_C}{1/\mu_C - 1/\mu_M} \cdot (\exp[-CTI/\mu_M] - \exp[-CTI/\mu_C]) \cdot \mu_M$$

The equation is based on the assumption that mean RT can be expressed as a function of three parameters: μ_C (mean cue-encoding time), μ_M (mean mediator-retrieval time), and RT_{Base} (residual processing time). Cue-encoding time reflects the time to encode a perceptual representation of the cue, which we assume differs for CRs (μ_{CR}) and CSs (μ_{CS}) because of priming from residual activation of the relevant cue in short-term memory on cue-repetition trials (Logan & Bundesen, 2003; Schneider & Logan, 2005). Mediator-retrieval time reflects the time to retrieve a mediator (or instantiate a task goal), which we assume differs for task repetitions (i.e., mediator repetitions; μ_{MR}) and task switches (i.e., mediator switches; μ_{MS}) because of priming from residual activation of the relevant mediator in short-term memory on task-repetition trials. Cue-encoding and mediator-retrieval times are assumed to be exponentially distributed such that their contributions to RT decrease as CTI increases. Residual processing time reflects the time to encode a target and to select and execute a response, which we assume differs across task sequences ($RT_{Base-ABA}$, $RT_{Base-BBA}$, $RT_{Base-BAA}$, $RT_{Base-AAA}$), possibly because of other mechanisms that are discussed below.

We fit this eight-parameter model to the 30 RT data points in each experiment, using the Solver routine in Microsoft Excel to minimize the root-mean-squared deviation (RMSD) between observed and predicted values (the Pearson product-moment correlation, r, was also computed), with the RT_{Base} values constrained such that $RT_{Base-ABA} \geq RT_{Base-BBA} \geq RT_{Base-BAA} \geq RT_{Base-AAA}$. Model predictions are presented in Figure 2, and the best-fitting parameter values (see Table 3) yielded RMSD = 32 ms and r = .991 for Experiment 1 and RMSD = 31 ms and r = .994 for Experiment 2.

For both model fits, μ_{CR} was less than μ_{CS} (see Table 3), resulting in repeated cue-encoding benefits of 135 and 136 ms in Experiments 1 and 2, respectively. The similarity between these values is consistent with what one might expect for experiments involving the same cues. For both model fits, μ_{MR} was less than μ_{MS} (see Table 3), resulting in repeated mediator-retrieval benefits (or alternatively, mediator-switch costs) of 131 and 175 ms in Experiments 1 and 2, respectively. The difference between these values is consistent with what one might expect for experiments involving different tasks that presumably require different mediators that may vary in accessibility. Finally, both model fits yielded a similar pattern of RT_{Base} values, with those for Experiment 2 only differing from those for Experiment 1 by about 12 ms.

To evaluate the necessity of having a mechanism such as mediator retrieval in the model, we compared the fits of the eight-parameter model with those of a nested six-parameter model

Table 3
Best-Fitting Parameter Values (in ms) From the Model Fits

Parameter	Experiment	
	1	2
μ_{CR}	373	474
μ_{CS}	508	610
μ_{MR}	27	90
μ_{MS}	158	265
$RT_{Base-ABA}$	941	931
$RT_{Base-BBA}$	759	748
$RT_{Base-BAA}$	759	748
$RT_{Base-AAA}$	627	611

Note. μ_{CR} = mean cue-encoding time for cue repetitions; μ_{CS} = mean cue-encoding time for cue switches; μ_{MR} = mean mediator-retrieval time for mediator repetitions; μ_{MS} = mean mediator-retrieval time for mediator switches; RT_{Base} = residual processing time for each task sequence (ABA, BBA, BAA, AAA).

(Model 2 from Logan & Bundesen, 2003) that lacked mediator retrieval (i.e., μ_{MR} and μ_{MS} were zero). The six-parameter model fit significantly worse than the eight-parameter model for both Experiment 1, $F(2, 22) = 4.16$, $p < .05$, and Experiment 2, $F(2, 22) = 10.21$, $p < .01$, indicating that a model with cue encoding and mediator retrieval fits the data better than a model with cue encoding only.

The outcome of this modeling exercise is important in two respects. First, it indicates that the theoretical framework underlying our past modeling efforts (e.g., Logan & Bundesen, 2003; Schneider & Logan, 2005) is not restricted to priming of cue encoding but is rich enough to accommodate other effects. Second, it indicates how one can formally disentangle sequential effects associated with cue encoding, mediator retrieval, and other processes to obtain estimates of their relative contributions to transition-cued performance.

Of course, cue encoding and mediator retrieval are not the only mechanisms that can produce sequential effects. As noted earlier, task-set inhibition has been proposed to account for the longer RTs associated with ABA sequences (Mayr & Keele, 2000). Given that task-set inhibition seems to be related to response selection (e.g., Schuch & Koch, 2003), it may be possible to incorporate its effects into RT_{Base} in our model, thereby providing justification for the use of different RT_{Base} values for different task sequences. However, the effects of task-set inhibition across longer task sequences (e.g., BABA) remain unclear, and, in light of the task-switch frequency effect we observed, it is important to note that putative task-set inhibition effects are modulated by the presence or absence of immediate task repetitions (Philipp & Koch, 2006).

Related to task-set inhibition, another mechanism for producing sequential effects is endogenous task-set reconfiguration (Monsell & Mizon, 2006), constrained by subjective expectancies regarding task switches (see Soetens, Boer, & Hueting, 1985). For example, Monsell et al. (2003) attributed the gradual decline in RT across a run of task repetitions in unpredictable task-cuing experiments to "expectation-based modulation of endogenous control input" (p. 336), such that any bias for the repeated task set is attenuated by the expectation of a task switch, at least until an asymptotic level of task-set readiness is achieved. This mechanism can account for

sequential effects such as BBAA > BAAA > AAAA, but, as noted by Monsell et al. (2003, p. 340), such interactions between task-switch expectancy and task performance have yet to be captured by formal models of task-set reconfiguration.

However, a computational model of task switching recently developed by Brown et al. (in press) has been shown to account for higher order sequential effects such as those observed in the present study (e.g., ABA > BBA > BAA > AAA). The key mechanism in the model that allows it to capture such effects is a "change detector" within a conflict-control loop that monitors for changes in task sets and responses across trials. When the change detector detects conflict arising from coactivation of alternative task sets (due to activation of the previous task set on task-switch trials), it forces response slowing that persists across subsequent trials, even if those trials are task repetitions. The result is that model RT becomes progressively longer on subsequent trials with every preceding task switch, consistent with the pattern of observed RTs in their study and in ours.

One limitation of Brown et al.'s (in press) model is that it does not produce cue-transition effects because once a cue is removed, the activity of cue-input units in the model is clamped to zero. As noted above, we contend that priming of cue encoding should be part of any complete explanation of our data; instantiating such a mechanism in Brown et al.'s model (perhaps by allowing cue-input-unit activity to persist across trials, subject to decay) may enable it to model our full data set, though this possibility remains to be explored.

Conclusion

The main conclusion to be drawn from the present study is that researchers must be cognizant of sequential effects beyond the immediately preceding trial if task-switch costs are to be used to make inferences about executive control. Identifying and resolving the limitations of different methodologies is a crucial component of the inference process, and attempting to model the various mechanisms underlying different sequential effects will undoubtedly contribute to theory development in task switching.

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