

## **What Matters in the Cued Task-Switching Paradigm: Tasks or Cues?**

Ulrich Mayr

University of Oregon

Running head: Cue-specific versus task-specific switch costs

Ulrich Mayr  
Department of Psychology  
University of Oregon  
Eugene, OR 97403  
Telephone: 541 346 4959, Fax: -4911  
E-mail: [mayr@darkwing.uoregon.edu](mailto:mayr@darkwing.uoregon.edu)

## Abstract

Schneider and Logan (in press) recently showed that cue-switch and task-switch costs are sensitive to the relative probability of cue switches versus task switches. From this they concluded that task switch costs reflect priming of cue-cue transitions rather than actual task switching operations. However, because this design confounded probability of specific cue transitions with probability of task switches, the results could also reflect task-switch-level adjustments. The present experiment ( $N=80$ ) pits the critical prediction of the cue-priming account, namely that costs for high-probability cue-cue transitions are smaller than for low-probability cue-cue transitions, against the main prediction of the switch-probability account, namely that switch probability irrespective of specific cue-cue transitions determines switch costs. Whereas the cue-priming prediction was rejected a specific version of the probability account, namely that subjects are sensitive to the probability of a task switch given a cue switch, was fully confirmed. Thus, tasks are in fact the critical representational units that determine task-switch cost.

156 words

**What Matters in the Cued Task-Switching Paradigm: Tasks or Cues?**

Switch costs as assessed in the task-switching paradigm are typically interpreted as a reflection of the processing demands involved in changing task-specific cognitive configurations (for a recent review, see Monsell, 2003). Recently, however, for one variant of the task-switching paradigm, the so-called cueing paradigm, this interpretation of switch costs has been called into question. In the cueing paradigm, tasks vary randomly on a trial-by-trial basis and each task is indicated by a (usually) visual cue. The cueing paradigm allows tight control over when subjects can start preparing for an upcoming task and therefore has proven useful for examining how proactive control influences switch costs (e.g., Meiran, 1996). However, in two parallel papers, Logan and Bundesen (2003) and Mayr and Kliegl (2003) noted that in this paradigm task changes are completely confounded with cue changes and therefore introduced a new variant, which uses a 4:2 mapping between cues and tasks. This allows examining three types of transitions: no switch, cue switch without task switch, and task switch. The contrast between the no-switch and the cue-switch condition yields the costs associated with changing the cue (henceforth referred to as *cue-switch costs*). Both in Mayr and Kliegl (M&K) and Logan and Bundesen (L&B) these costs were surprisingly large suggesting that a substantial component of the traditional switch costs had nothing to do with changing cognitive configurations per se. Where the results of M&K and L&B differed was with respect to the second contrast, that between the cue-switch and the task-switch transitions (henceforth called *true task-switch cost*). Whereas this component was substantial in M&K, it was non-existent in L&B; in some of the succeeding papers by Logan and coworkers, true task switch costs were present, but only very small (e.g.,

Logan & Bundesen, 2006). Based on the finding of substantial task-switch costs, M&K concluded that there is true task switching going in the cueing paradigm, whereas L&B suggested that subjects form compounds between cues and stimuli that allow disambiguating response selection. According to this view, "switching" occurs between these compounds and not between the actual tasks. The question whether or not there is true task switching in the cued task switching paradigm is of considerable theoretical relevance. At stake is to what degree task sets are a mandatory, organizing principle in human action control.

One noteworthy difference between the procedure used by L&B and the one used by M&K was in terms of switch probabilities. In L&B, all cues and stimuli were selected randomly without constraints. With four cues mapped to two tasks, there are four different no-switch cue-cue transitions, four different cue-switch cue-cue transitions, but eight different possible task-switch cue-cue transitions (see Table 1 for a listing of all specific cue-transitions possible with a 4:2 cue-task mapping). Thus, random cue selection leads to a no-switch and a cue-switch probability of  $p=.25$  each, but a task-switch probability of  $p=.5$ . In contrast, M&K had used a probability of  $p=.33$  for each type of transition (see also Exp. 3 in Monsell & Mizon, in press). Why should this matter? A critical decision subjects may have to make on each trial of the cued task-switching paradigm is whether or not to abandon the current task set. It is possible that this decision is at least partly based on the switch probability in a given situation. We will refer to such a switch-probability-based adaptation of configuration activities simply as the "task-level adaptation" account (for a nearly identical suggestion, see Monsell & Mizon, in press). This type of adaptation might occur in two different ways. First,

relatively high switch probabilities could lead subjects to generally prepare for the more difficult task-switch transitions by abandoning or suppressing tasks after each trial (or on a majority of trials). Whereas this would be advantageous if on a given trial a task change actually occurs, this strategy would lead to costs on trials where the task stays the same, no matter whether these involve a cue switch or not. The net result would be a reduction of switch costs, which would go along with an increase in both no-switch and cue-switch RTs. A second possibility is that subjects also take some cue-based information into account when deciding whether or not to abandon the current task set. Specifically, the result of a fast check of whether or not the cue has changed relative to the preceding trial might enter into the decision process. Thus, what counts here is the conditional probability of a task switch given a cue switch. Whereas in M&K this conditional probability was  $p=.5$ , it was  $p=.67$  in L&B. The higher conditional probability might bias subjects to generally initiate a task switch in case of a cue switch. This would reduce switch costs relative to cue-switch RTs, but it would not necessarily lead to an increase of no-switch RTs. In other words, if subjects operate on the basis of conditional probabilities, we might see a reduction of true task-switch costs and an increase of cue-switch costs as a function of an increase of task switch probability.

Monsell and Mizon (in press) and Schneider and Logan (in press) recently reported experiments in which both cue and task transition probabilities were manipulated. For example, in Schneider and Logan, the probability of no-switch, cue-switch, and task-switch transitions was varied across three different groups such that for each group one of the three transitions was  $p=.7$  and that of the other two was  $p=.15$ . Results showed that these probability manipulations did in fact have strong effects on the

pattern of cue-switch and task-switch costs. Specifically, true task-switch costs were very small when task-switch transitions occurred with high probability, cue-switch costs were very large when no-switch costs occurred with high probability (while true task-switch costs were substantial), and both types of costs were moderate when cue-switch transitions were highly probable. Generally, very similar results were also reported by Monsell and Mizon. While this overall pattern is consistent with the task-level adaptation account (and was also interpreted in this manner by Monsell & Mizon), Logan and Schneider proposed a very different interpretation. Extending the L&B cue-priming model, they suggested that subjects learn high-probability, *specific cue-cue* transitions. According to this view, task-switch costs are reduced in case of high-probability switch transitions, not because subjects adapt to frequent switching per se. Rather, reduced switch costs result from the fact that if task-switch probability is high, there is also a larger probability of those specific cue-cue transitions that go along with a task switch. For example, if cues "A" and "B" indicate task 1 and cues "C" and "D" indicate task 2, the cue-cue transitions A-C, A-D, C-A, C-B would have a higher probability than all other possible transitions. Of course, the same explanation also holds for the reduced cue-switch costs in case of frequent cue-switch trials or reduced no-switch RTs in case of frequent no-switch trials. Clearly, this account can explain the results of both Logan and Schneider and also of Monsell and Mizon just as well as the task-level adaptation account.

Obviously, the simple manipulation of task-switch probabilities (as used in Schneider & Logan, in press, and Monsell & Mizon, in press) confounds switch probability and the probability of specific cue-cue transitions and therefore does not

allow distinguishing between task-level adaptation and the cue-priming account. However, the two accounts would make differential predictions if we manage to manipulate the probability of specific cue transitions. The design used in the present study capitalizes on the earlier mentioned fact that with a 4:2 mapping between cues and tasks there are eight different cue-cue transitions for task switches, but only four for cue-switch and no-switch transitions. This allowed us to expose subjects to identical cue-cue transition frequencies for the no-switch, the cue-switch, and one half of the possible task switch transitions (henceforth referred to as task-switch-1 transitions). For the second half of the possible task-switch transitions (henceforth referred to as task-switch-2 transitions) we used a between-group manipulation where one group was exposed to a low probability, the other to a high probability of task-switch-2 transitions (see Table 1).

This manipulation allowed two independent, critical tests of the task-level adaptation versus the cue-priming account:

1) If the task-level adaptation account has any merit then the manipulation of task-switch-2 probability should affect task switch and possibly also cue switch costs for the other three transition types (i.e., no-switch vs. cue-switch vs. task-switch-1). Specifically, when probability of task-switch-2 transitions is high then also the unspecific switch probability increases. This in turn may induce subjects to either adapt to the generally increased switch probability or to the increased conditional probability of task switches given a cue switch. Thus, we predict here smaller task-switch costs (computed as the difference between cue-switch and task-switch-1 transitions) and possibly also larger cue-switch costs (computed as the difference between cue-switch and no-switch transitions) than when task-switch-2 transitions are frequent. Importantly, to examine task-switch

costs we will only look at task-switch-1 transitions. Thus, any effect on task-switch costs could not be explained in terms probabilities of specific of cue-cue transitions because these are identical across the two groups of subjects and the three transition types (no-switch, cue-switch, and task-switch-1 transitions).<sup>1</sup>

Second, the manipulation of task-switch-2 probabilities allows us to examine a straightforward prediction of cue-priming model, namely that frequent cue-cue transitions should be performed faster than infrequent cue-cue transitions. Thus, here we need to compare for both groups task-switch-1 and task-switch-2 transitions.

To further examine the characteristics of possible general adaptation effects, we also manipulated the cue-stimulus interval (CSI). On the one hand, general adaptation effects might affect processes that with sufficient time can be executed proactively, that is before the stimulus appears. In this case they would fully affect cue-switch or task-switch costs only in the short CSI condition, but would be absorbed into the preparation interval when the CSI is long. On the other hand, adaptation effects might affect the availability of the just-executed task in a manner that is less affected by proactive processes. For example, general adaptation effects might operate through the degree to which the just-executed task-set is inhibited. Task-set inhibition has been shown to exert effects that are relatively insensitive to preparatory activity (e.g., Mayr & Keele, 2000; Schuch & Koch, 2003).

## Method

### *Participants*

Eighty students of the University of Oregon participated in a 1-hour session exchange of course credits.

*Task and Stimuli and Design*

Depending on the task, participants were supposed to discriminate either the color or the shape of an object. The object could either be a circle, a square, or a triangle of about the same size (i. e., the side length of the square was 1 cm) and it could appear in green, blue, or red. The object was presented on black background within a white frame with a side length of 2.5 cm. The frame was visible throughout the entire block. Responses were entered with the index, middle, or ring finger of the preferred hand using the "1"-key, the "2"-key, and the "3"-key of the numerical keyboard. Circle and green were mapped onto the "1"-key, square and blue were mapped onto the "2"-key, and triangle and red were mapped onto the "3"-key. Stimuli remained on the screen until the correct response was entered. Thus, in case of an incorrect response, participants could continue only after pressing the correct response.

The interval between the response and the stimulus for the next trials was 1100 ms. The cue-stimulus interval (CSI) was either 100 ms or 1000 ms. The cue for the color task was either the word "color" or "dye". The cue for the shape task was either "shape" or "form".

With two cues per task, there are four possible cue transitions for no-switch and cue-switch trials and eight possible transitions for task-switch trials. The eight cue transitions for task-switch trials were assigned to two sets of four (Set 1: color-shape, dye-form, shape-dye, form-color; Set 2: color-form, dye-shape, shape-color, form-dye). Subjects were randomly assigned to two groups of subjects. In both groups, one of the two sets was presented with equal frequency as the no-switch and the cue-switch transitions condition transitions. In the high-switch-probability group the remaining set

of task-switch cue transitions occurred with high probability, in the low-switch-probability group it occurred with low probability. Table 1 presents the exact frequencies for the two groups.

### *Procedure and Design*

Block length was 90 trials. There were two practice blocks in which participants could become familiar with each of the two tasks and the cue-task associations. Order of introducing the two different tasks was counterbalanced. Following practice, eight test blocks were presented. Blocks alternated between short CSI and long CSI with CSI of the initial test block counterbalanced across subjects.

### Results and Discussion

Response times larger than 4000 ms were excluded from analysis (eliminating 1% of the RT distribution), as well as RTs from incorrect trials or from trials following incorrect trials.

Figure 1 shows cue-switch and task-switch costs as a function of probability of task-switch-2 transitions. Two separate 2 x 2 x 2 ANOVAs were conducted, one contrasting no-switch and cue-switch transitions, the other cue-switch and task-switch transitions. Both analyses also included the Group factor (High vs. Low Probability Task-Switch-2 transitions) and the CSI factor. As can be seen, irrespective of probability of Task-Switch-2 transitions, there were substantial cue-switch costs,  $F(1,78)=58.36$ ,  $p<.001$ , which became smaller for the long CSI condition,  $F(1,78)=10.36$ ,  $p<.01$ . There were also substantial true task-switch costs in all conditions,  $F(1,78)=152.96$ ,  $p<.01$ , which were stable across the CSI manipulation,  $F(1,78)=.53$ ,  $p>.4$ . This pattern of CSI-

dependent cue-switch and CSI-independent true task-switch costs replicates the results reported by Mayr and Kliegl (2003; but see Monsell & Mizon, 2004).

For the critical question regarding general adaptation effects we need to look at the effect of the probability of Task-Switch-2 transitions on the remaining transitions. As apparent from Figure 1, cue-switch costs were larger when task-switch-2 transitions were frequent whereas task-switch costs were larger when task-switch-2 transitions were rare. Both the interaction between group and the cue-switch factor,  $F(1,78)=4.15, p<.05$ , and the interaction between group and the task-switch factor were reliable,  $F(1,78)=8.52, p<.01$ . With regard to errors, there were no effects that counteracted RT results. True task-switch costs were smaller for frequent than for the rare task-switch-2 transitions,  $F(1,78)=5.05, p<.05$ . No comparable effect was observed for cue-switch costs,  $F(1,78)=0.0$ . Overall, the prediction that switch probability modulates costs independently of specific cue transitions was fully confirmed.

Next we turn to the critical prediction of the cue-priming account that can be tested in our design, namely that high-probability cue-cue transitions (i.e., task-switch-1 for the task-switch-2 rare group and task-switch-2 for the task-switch-2 frequent group) are processed faster than low-probability cue-cue transitions. Table 2 contains the relevant results in form of difference scores (i.e., "frequency effects"). As apparent, three of the four RT scores were slightly negative (indicating faster processing for the low-probability transitions) and three of the four error scores were slightly negative (indicating more accurate processing of low-probability transitions). Neither of the two positive scores,  $t(39)>.24, p>.8$ , nor any of the negative scores were reliable,  $t(39)>1.7, p>.09$ . Possibly, specific cue-transition learning effects can be found only later in

practice. However, an analysis that focused on the second half of the test blocks again revealed no trace of cue-specific learning. Thus, the prediction that probability of specific cue-cue transitions modulates switch costs could not be confirmed in the present data set.

### Conclusion

The results presented here suggest that subjects modulate their switching behavior as a function of switch probability. More precisely, the joint increase of cue-switch costs and decrease of task-switch costs as a function of switch probability suggests that subjects were responding to the conditional probability of a task switch given a cue switch. It seems plausible that people are able to conduct a fast same-different judgment regarding whether or not the cue has changed compared to the preceding trial. In case of a cue change, and if switch probability is high, subjects may be more inclined to abandon the current task set. Abandoning the last relevant task should result in additional costs on cue-switch trials (because then the abandoned task would have to be reactivated).

While we did find clear evidence for general probability effects that could not be explained in terms of specific cue-priming effects, we found no positive evidence for learning of specific cue-cue transitions as suggested by Schneider and Logan (in press). Of course, we cannot rule out that such learning may have played a role in the studies by Logan and Schneider (in press) or Monsell and Mizon (in press). Manipulations of the probabilities of different task-level transition categories were larger in these studies (i.e., 15% versus 70%) than the specific cue-cue manipulations we achieved here (40% versus 20% and 10% versus 30%, see Table 1). There is also no theoretical reason why task-level adaptations and learning of cue-cue transitions could not both exist and maybe

become apparent under different boundary conditions. However, our results constitute a clear case where both true task-switch costs and a probability-based modulation of such costs are present, but cannot be explained in terms of specific cue transitions. Also, our manipulations are of about the same size as those implemented across recent studies using the 4:2 cue-task paradigm (e.g., see Table 1). At the very least this casts doubt on the ability of the cue-based priming model to explain the entire range of cued task-switching effects in terms of cue-based priming effects. Our results are, however, compatible with Monsell and Mizon's account of probability modulations of switch costs. An additional advantage of the adaptation account is that it can readily explain the absence of switch costs in the Logan and Bundesen study (which had used a conditional switch probability of .67) compared to the finding of substantial switch costs in Mayr and Kliegl (which had used a conditional switch probability of .5).

The finding that task transitions rather than cue-cue transitions seem to matter in terms of adaptation effects is also broadly consistent with a number of results that suggest a functional dissociation between cue-switch and task-switch costs. For example, cue-switch costs seem more sensitive to practice and CSI manipulations than task-switch costs (e.g., Mayr & Kliegl, 2003; but see Monsell and Mizon for findings suggesting that also task-switch costs can be affected by CSI), whereas effects of task-set inhibition affect the task-switch component (Mayr & 2003). Koch (2001) reported results suggesting implicit learning of a sequence of tasks. Following up on this result, Gotler, Meiran, and Tzelgov (2003) used a 2:1 mapping between cues and tasks and showed that the implicit learning occurred on the task and not on the cue level, again pointing to tasks rather than cues as the critical representational entity in cued task-switching

situations. Finally, recent results using EEG (Jost, Mayr, & Rösler, in preparation) and fMRI (Brass & von Cramon, 2002; Bryck & Mayr, 2004) also suggest a neural-level dissociation between cue-switch and task-switch costs.

Along with the other recent evidence regarding a dissociation between cue and task-switch costs, the present data are compatible with the view that subjects step through the trials of a cued-task-switching paradigm by selecting among competing, abstract task sets. Likely, task-set reconfiguration and maintenance are both costly processes.

Therefore it is not too surprising to find that people adjust their decision making about when to abandon task sets to the probability structure of a given task-selection situation. An important question for further research is what exactly it means to strategically abandon a task set in case of a high frequency of task-switch transitions. One interesting possibility is that in case of a high switch probability, subjects may be more inclined to inhibit the just-executed task set (Mayr & Keele, 2000) when cues change across consecutive trials. As a consequence it would be more difficult to reactivate a just-suppressed task in case of a cue-switch trial, leading to larger cue-switch and smaller task-switch costs (for a related finding, see Mayr & Bell, in press). Interestingly, just as typically found for task-set inhibition effects (e.g., Mayr & Keele, 2000; Schuch & Koch, 2003), the general-adaptation effects we obtained in this study were not affected by the CSI manipulation. Of course the possibility that general-adaptation effects are instantiated via task-set inhibition is based on the assumption that inhibition is sensitive to the general selection context and thus under some strategic control. Interestingly, Philip and Koch (in press) recently reported evidence that supported this assumption.

They found that task-set inhibition was much larger under conditions of high switch probability than under conditions of low switch probability.

## References

- Brass, M., von Cramon, D. Y. (2002). The role of frontal cortex in task preparation. *Cerebral Cortex*, *12*, 908-914.
- Bryck, R. & Mayr, U. (March, 2004). *Dissociating cue-switching from task-switching via fMRI*. Poster presented at the Cognitive Neuroscience Society Meeting, San Francisco, USA.
- Koch, I. (2001). Automatic and intentional activation of task sets. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *27*, 1474-86.
- Gotler, A., Meiran, N., & Tzelgov, J. (2003). Nonintentional task set activation: Evidence from implicit task-sequence learning. *Psychonomic Bulletin & Review*, *10*, 890-896.
- Jost, K., Mayr, U., & Rösler, F. (2006). *Is task switching nothing but cue priming? Evidence from ERPs*. Manuscript in preparation.
- Logan, G. D., & Bundesen, C. (2003). Clever homunculus: Is there an endogenous act of control in the explicit task-cuing procedure? *Journal of Experimental Psychology: Human Perception and Performance*, *29*, 575-599.
- Logan, G. D., & Bundesen, C. (2004). Very clever homunculus: Compound stimulus strategies for the explicit task-cuing procedure. *Psychonomic Bulletin & Review*, *11*, 832-840.
- Mayr, U. & Bell, T. (in press). On how to be unpredictable: Evidence from the voluntary task-switching paradigm. *Psychological Science*.
- Mayr, U., & Keele, S. W. (2000). Changing internal constraints on action: The role of backward inhibition. *Journal of Experimental Psychology: General*, *129*, 4-26.

Mayr, U., & Kliegl, R. (2003). Differential effects of cue changes and task changes on task-set selection costs. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 29, 362-372.

Meiran, N. (1996). Reconfiguration of processing mode prior to task performance. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 22, 1423-1442.

Monsell, S., & Mizon, G.A., (in press). Can the task-cuing paradigm measure an “endogenous” task-set reconfiguration process? *Journal of Experimental Psychology: Human Perception, and Performance*.

Monsell, S. (2003). Task switching. *Trends in Cognitive Sciences*, 7, 134-140.

Philip, A.M., & Koch, I. (in press). Task inhibition and task repetition in task switching. *European Journal of Cognitive Psychology*.

Schneider, D.W., & Logan, G. D. (2005). Modeling task switching without switching tasks: A short-term priming account of explicitly cued performance. *Journal of Experimental Psychology: General*, 134, 343-367.

Schneider, D.W., & Logan, G.D. (in press). Transition frequency alters “switch costs:” Priming of cue encoding in explicitly cued task switching performance. *Psychonomic Bulletin and Review*.

Footnotes

<sup>1</sup> It is worth pointing out that the manipulation of task-switch-2 transitions inversely and uniformly modulates the probability of all remaining transition types (see Table 1). This may compromise the ability to directly compare overall RTs and error scores across the two between-subject conditions. However because the relative frequencies of the three remaining transition types is not affected, this does not compromise the ability to interpret modulations of cue-switch costs and actual switch costs.

Table 1. Design of the current experiment with specific cue transitions, probabilities of transition types, and conditional probabilities of task switches given cue switches for the two between-subject groups (high versus low probability Task-Switch-2 transitions). For comparison, corresponding transition probabilities used by Mayr & Kliegl (2003, M&K) and Logan & Bundesen (2003, L&B) are shown.

Transition Type	Specific Cue-Cue Transitions	Transition Probabilities			
		Task-Switch-2		M&K	L&B
Switch Probability		High	Low		
No-Switch	A-A, B-B, C-C, D-D	.2	.3	.33	.25
Cue-Switch	A-B, B-A, C-D, D-C	.2	.3	.33	.25
Task-Switch-1	A-C, B-D, C-B, D-A	.2	.3	.33	.25
Task-Switch-2	A-D, B-C, C-A, D-B	.4	.1	0	.25
P(Task Switch/Cue Switch)		.75	.57	.5	.66

Note. Specific transitions for Task-Switch-1 and Task-Switch-2 for the current experiment were counterbalanced across subjects. Switch probability (high versus low) was manipulated between subjects. Cues A and B are associated with task 1; cues C and D are associated with task 2. In the actual experiment reported here, semantic task labels were used.

Table 2. Mean RTs and Error scores (SD) as a function of between-subject group (high versus low Task-Switch-2 probability), CSI, and all relevant transition types. In addition, frequency effects, the RT and error difference scores yielded by subtracting high-probability task-switch transitions from low-probability task-switch transitions within the two between subject-groups, are shown.

Task-Switch-2 Probability	RT		Errors	
	High	Low	High	Low
CSI=100 ms				
No-Switch	1039 (178)	971 (216)	4.2 (3.5)	5.8 (4.9)
Cue-Switch	1136 (228)	1041 (225)	3.8 (3.5)	4.8 (5.4)
Task-Switch-1	1208 (220)	1148 (244)	7.3 (4.8)	9.8 (7.3)
Task-Switch-2	1210 (226)	1153 (275)	7.8 (5.6)	9.4 (7.9)
Frequency Effect	-2 (63)	5 (121)	-.5 (4.0)	-.4 (4.8)
CSI=1000 ms				
No-Switch	829 (173)	812 (204)	5.1 (3.8)	6.1 (6.0)
Cue-Switch	893 (182)	836 (217)	3.8 (3.4)	5.4 (4.8)
Task-Switch-1	966 (191)	962 (240)	7.0 (4.7)	10.6 (7.5)
Task-Switch-2	978 (192)	938 (254)	7.7 (5.0)	10.8 (1.0)
Frequency Effect	-12 (84)	-24 (90)	-.7 (3.7)	.1 (5.6)

Figure Captions

Figure 1. Cue-switch and task-switch costs as a function of high versus low probability of task-switch-2 transitions. Error bars represent 95% between-subject confidence intervals.

