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The surface structure and the deep structure of sequential control: What can we learn from task span switch costs?

Ulrich Mayr
University of Oregon, Eugene, Oregon

A large component of response time switch costs in the cued task-switching paradigm is linked to cue changes without task changes, suggesting costs might reflect passive priming rather than endogenous control. In contrast, the task span procedure requires subjects to guide task selection via sequences of memorized task cues and therefore may be better suited to reflect endogenous switch processes (Logan, 2004). The present experiments combined the task span procedure with a 2:1 mapping between cues and tasks, allowing separation of cue-switch costs from true task-switch costs. Replicating findings with the cued task-switching paradigm, results showed both substantial cue-switch costs and actual task-switch costs (Experiments 1 and 2) as well as sensitivity of cue-switch costs, but not of task-switch costs, to opportunity for preparation (Experiment 2). Apparently, simple action plans use "surface level" phonological or articulatory codes that contain no task information. These results suggest that the distinction between cue-related and task-related processes is critical no matter whether tasks are cued exogenously or endogenously.

Task-switch costs reflect the difference in response time (RT) or accuracy between trial-to-trial transitions with and without changes in task demands. The switch-cost contrast has been used with the hope that it provides useful information about how people can change task-relevant representations in response to changing internal or external demands (see, e.g., Logan, 2003; Mayr, 2003; Monsell, 2003); however, there is considerable disagreement about how to best assess and interpret task-switch costs.

One frequently used procedure, the *task-cuing paradigm*, has come under particular scrutiny. In this procedure, tasks are externally signaled on a trial-by-trial basis (e.g., through a written task label). It allows a high degree of experimental control over the time to prepare for the next task or over the decay of information from the task on the previous trial (Meiran, 1996). However, results reported in parallel by Logan and Bundesen (2003) and Mayr and Kliegl (2003) suggest that switch costs derived from this procedure reflect changes in task cues at least as much as they reflect changes in actual task-relevant representations (i.e., reconfigurations). These authors used a procedure in which two cues were mapped to each task, so that in addition to standard no-switch transitions (cue and task stay the same) and task-switch transitions (cue and task change), cue-switch transitions (cue changes but task stays the same) could also be implemented. Interestingly, RT cue-switch costs made up a substantial share of total costs, although there was an additional, robust "true" task-switch component (i.e., the difference between the cue-switch and the task-switch conditions). Logan and Bundesen interpreted the cue-switch cost in terms of passive cue priming and suggested that the remaining task-switch component also reflects priming on a more conceptual level (i.e., because cues associated with the same task have semantic or episodic associations). In contrast to Logan and Bundesen, Mayr and Kliegl suggested that the latter component does reflect "true" reconfiguration of an internal representation (Mayr, 2006; see also Monsell & Mizon, 2006). With regard to the cue-switch component, there is not much of a disagreement at first sight: What Logan and Bundesen refer to as a *cue-encoding effect*, Mayr and Kliegl called a *retrieval effect*, on the basis of the assumption that repetition of the same cue allows a retrieval shortcut. In the study of memory, it has long been acknowledged that there is no encoding without retrieval (and vice versa); on the level of processes, therefore, this is a mere difference in semantics.

Sometimes, however, semantics do matter—namely, when a particular concept comes with extra baggage that may or may not be appropriate for a particular situation. The term *encoding* is usually applied to the act of processing information presented externally, such as visual or auditory task cues. With this in mind, one might suggest that there is a simple way of getting rid of the cue-encoding switch component. All one would have to do to eliminate the contribution of cue-encoding processes is

U. Mayr, mayr@uoregon.edu

to design a task-switching situation that works without presentation of external task cues.

This was in fact one, though certainly not the only, motivation for designing the so-called *task span paradigm* as a procedure that ensures that switch costs actually reflect top-down control rather than cue priming (Logan, 2004). In this procedure, participants receive a short list of task names and are asked to apply these sequentially to successively appearing stimuli. A number of interesting questions can be addressed with this paradigm, such as to what degree serial-task memory and task-selection processes require overlapping resources. However, what we are concerned with here is the degree to which switch costs derived from this procedure can be safely interpreted in terms of actual task-switch costs.

The interpretation of task span switch costs as "true" switch costs is based on an important untested assumption—namely, that people actually code the "deep structure" of the sequence in terms of tasks' identities. Given an abstract sequence such as Task A–Task A–Task B, people might internally represent this as "Do Task A twice and then switch to Task B." Here, the actual task labels should not play a major role. For example, if Task A required attending to the shape of an object and Task B required attending to the color of an object, it should be irrelevant whether the task list contained the labels *shape–shape–color* or, alternatively, *shape–form–color*. Obviously, the latter list contains a switch in cue without a switch in task at Position 2, but if it is the actual task structure that matters, then this should not elicit a substantial cue-switch cost.

A sequential representation based on the identity of tasks is certainly plausible and, in fact, has already been shown to underlie implicit representations of task sequences (Gotler, Meiran, & Tzelgov, 2003). However, it is also well known that people can efficiently code serial-order information through phonological or articulatory representations. In fact, there is evidence that articulatory suppression interferes with certain aspects of task selection (see, e.g., Baddeley, Chincotta, & Adlam, 2001; Bryck & Mayr, 2005). Thus, people's default strategy may be to represent even simple action plans using articulatory or phonological codes that contain no information about the underlying task structure. In this case, the change of an internal cue should lead to a retrieval cost, just as the change of an external cue does. Thus, what is at stake here is not only whether or not task span switch costs allow the assessment of actual task-switch costs. The broader question is: What is the representational format of simple, ad hoc action plans, and how does this format affect the fluent execution of such plans?

In addressing these questions, we combined the 2:1 cue–task mapping with the task span procedure. Participants performed short, three-trial task sequences on the basis of memorized short lists of task labels. Sequences contained no-switch, cue-switch, and task-switch transitions (e.g., *color–shape–shape* or *color–shape–form*). If participants represent these simple sequences in terms of verbal task labels, we should see both substantial cue-switch costs and substantial task-switch costs. Alternatively, if sequences are represented in terms of task identities, a change in cue should not matter, and we should see only task-switch costs.

EXPERIMENT 1

Method

Participants. Eighteen students from the University of Oregon participated in a 1-h session in exchange for course credit.

Task, Stimuli, and Design. Depending on the task, participants were supposed to discriminate either the color or the shape of an object. The object could be a circle, a square, or a triangle, all of about the same size (the side length of the square was 1 cm), and it could appear in green, blue, or red. The object was presented on a 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.

Participants performed the tasks in sequences of three trials each. Prior to each sequence, a sequence cue was presented that listed the labels of the three consecutive tasks. Each task could be indicated by two different labels (*color* or *hue* for the color task, *shape* or *form* for the shape task). Tasks were selected randomly with the constraint that each sequence had to consist of one task-switch transition and either one no-switch or one cue-switch transition. For example, both *color–hue–shape* and *color–color–shape* would be allowed sequences. The three task labels were arranged in a vertical list just above the stimulus frame for 2,000 msec, followed by only the stimulus frame for 1,000 msec. The first stimulus then followed, remaining visible until the response was made, followed by only the stimulus frame for 100 msec. The second and the third trials were presented in the same manner. In case of an error, the stimulus stayed on until the correct response was executed.

Procedure. There were three practice blocks with 15 three-trial sequences each. In the first two blocks, only one of the two tasks was presented per block (but with sequence cues that could contain both labels for each task). Order of introducing the two different tasks was counterbalanced. The third practice block involved both tasks. After that, five test blocks with 30 three-trial sequences were presented.

Results and Discussion

RTs larger than 3,000 msec were excluded from analysis, as were RTs from all sequences in which at least one error was made. We focused only on the second and third sequence positions, since only these could be categorized into no-switch, cue-switch, and task-switch transitions.

Figure 1 shows RTs across all transition types. As is evident, there was a large cue-switch cost [$M = 170$ msec, $SD = 136$; $t(17) = 5.32$, $p < .001$] and a smaller task-switch cost [$M = 79$ msec, $SD = 133$; $t(17) = 2.53$, $p < .05$]. There was a small and nonsignificant error cue-switch cost [$M = 0.6\%$, $SD = 3.4\%$; $t(17) = 0.78$, $p > .5$] and a significant task-switch cost [$M = 2.3\%$, $SD = 3.6\%$; $t(17) = 2.7$, $p < .05$]. The difference between the substantial RT cue-switch cost and the nonsignificant error cue-switch cost may seem surprising (but see Experiment 2); a change in cue should slow down retrieval of the next relevant task. However, given that this is not a particularly hard retrieval problem, it should not necessarily lead to a large increase in errors.

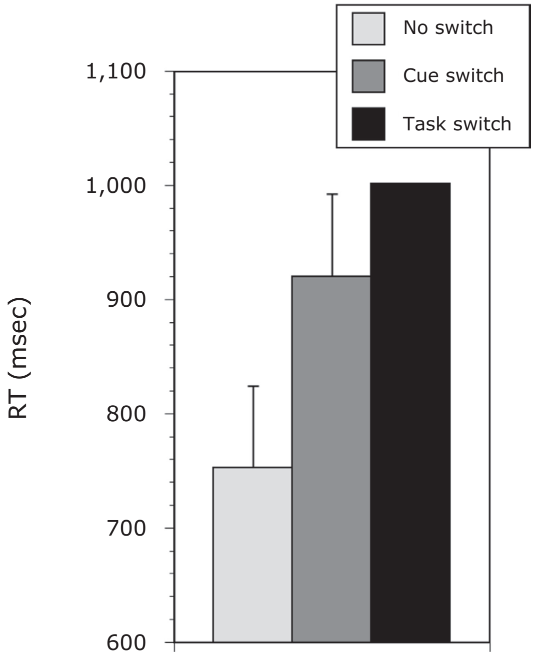

Figure 1. Mean response times (RTs) as a function of trial-to-trial transition type in Experiment 1. Error bars reflect 95% confidence intervals, computed separately for the cue-switch and the task-switch contrasts.

One possibility is that it takes time for people to realize the distinction between the sequence of task labels and the deep structure of the sequence of actual tasks. When looking at the final two blocks, however, we found the same basic pattern of costs as that for the entire session (cue switch, $M = 178$ msec, $SD = 182$; task switch, $M = 75$ msec, $SD = 136$).

To avoid sequences with no switches at all and sequences with two switches (which might elicit additional inhibitory effects; Mayr & Keele, 2000), each of our sequences contained exactly one switch. A potential problem with this design is that after the first transition, the type of the second transition is completely predictable. However, when adding the sequence-position factor (Position 2 vs. Position 3), there was no reliable interaction with the cue-switch factor [$F(1,17) = 2.32$, $p > .14$] or the task-switch factor [$F(1,17) = 0.15$, $p > .7$].

EXPERIMENT 2

The finding of substantial cue-switch costs is consistent with the notion that sequential control uses superficial codes that do not contain information about the actual tasks. However, the finding of additional actual task-switch costs may suggest that sequential control also uses deeper level information, at least to some degree. Alternatively, as suggested by Mayr and Kliegl (2003; see also Arrington, Logan, & Schneider, 2007), cue-switch and task-switch costs represent different aspects of task control. Cue-switch costs reflect cue-driven retrieval to access the next task set, whereas "true" task-switch costs reflect a change of the actual reconfiguration that usually (but not always; see Monsell & Mizon, 2006) occurs after the stimulus has been presented. Consistent with this claim, Mayr and Kliegl (2003) showed that cue-switch costs were reduced with sufficient opportunity for preparation (i.e., a long cue–stimulus interval). In contrast, task-switch costs were unaffected by the cue–stimulus interval, presumably because they reflect shaping of an attentional setting to the specific, stimulus-elicited conflict, which may be difficult to achieve before the stimulus is presented. In order to assess the degree to which a similar pattern of preparation-dependent cue-switch costs and preparation-independent task-switch costs also holds for the task span procedure, we manipulated the response–stimulus interval (RSI) in Experiment 2.

Method

Participants. Twenty-eight students from the University of Oregon participated in a 1-h session in exchange for course credit.

Tasks, Stimuli, Design, and Procedure. This experiment was identical to Experiment 1, except that the RSI between successive stimulus displays varied randomly between 100 and 1,000 msec from trial to trial—that is, within sequences.

Results and Discussion

Data were treated in the same way as in Experiment 1. Figure 2 shows RTs across no-switch, cue-switch, and task-switch transitions for the two different RSIs. As is evident, both cue-switch and task-switch costs were substantial for the short RSI. For the longer RSI, the cue-switch cost, but not the task-switch cost, was reduced. An ANOVA with the cue-switch contrast as one factor and RSI as the second factor confirmed main effects for the cue-switch contrast [$F(1,27) = 48.86$, $p < .01$] and for the RSI factor [$F(1,27) = 49.49$, $p < .01$] and that cue-switch costs were reliably reduced as a function of RSI

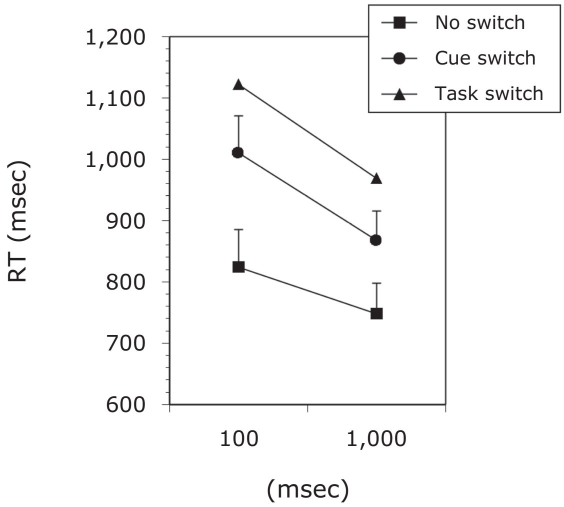

Figure 2. Mean response times (RTs) as a function of trial-to-trial transition type and response–stimulus interval in Experiment 2. Error bars reflect 95% confidence intervals, computed separately for the cue-switch and the task-switch contrasts.

[$F(1,27) = 4.23$, $p < .05$]. The corresponding ANOVA for the task-switch contrast revealed a task-switch effect [$F(1,27) = 21.82$, $p < .01$] and an RSI effect [$F(1,27) = 53.15$, $p < .01$] but no reliable interaction between the two factors [$F(1,27) < 0.2$, $p > .6$].

There was a significant error cue-switch cost [$M = 1.8\%$, $SD = 2.9\%$; $F(1,27) = 9.73$, $p < .01$] and an almost significant task-switch cost [$M = 1.4\%$, $SD = 3.9\%$; $F(1,27) = 3.72$, $p < .07$]. Neither of these effects interacted with RSI [both Fs$(1,28) < 1.6$, ps $> .2$].

Again, we also checked to what degree the cost pattern was robust across both sequence positions. When including sequence position in the ANOVA, there was, if anything, a slight tendency for cue-switch costs to be larger for Position 3 than for Position 2 [$F(1,27) = 2.28$, $p = .14$]. However, the two critical sequence $\times$ RSI $\times$ cue switch (or task switch) interactions were far from reliable [$F(1,27) < 1$].

The finding that with increasing RSI, cue-switch costs, but not task-switch costs, were reduced is consistent with the claim that cue-driven retrieval is reflected in cue-switch costs and also with a number of earlier results with the cued task-switching paradigm (see, e.g., Mayr, 2006; Mayr & Kliegl, 2003). In a direct comparison of the cued-switching and the task span procedures, Logan (2007) showed that the latter produced overall larger switch costs as a result of the need to coordinate between plan-level and stimulus-level processes in the task span paradigm. It may be more difficult to prepare for these "extra" processes, leaving an overall larger residual cost (for a related result, see Koch, 2003).

GENERAL DISCUSSION

Logan's (2004) task span procedure, in which participants perform short sequences of memorized tasks, was married with the 2:1 cue–task mapping design that has proven informative in the context of the cued task-switching paradigm. The result was very clear: As in the cued task-switching paradigm, most of the total RT switch costs in the task span procedure are due to changes in memorized task cues rather than to changes in actual tasks. Also, just as in the cued task-switching paradigm, cue-switch costs, but not task-switch costs, proved sensitive to the opportunity for preparation. In short, when it comes to control of task sets, internal memorized cues behave much like externally presented cues.

It may be tempting to interpret cue-switch costs and actual task-switch costs as two additive components that combine to make up the total switch costs in the standard paradigm. However, increasing the number of cues may also increase the retrieval burden (see, e.g., Mayr & Kliegl, 2003). In fact, findings by Altmann (2006) and results from a control experiment[1] suggest that total switch costs are somewhat larger with two cues per task than with one cue per task (but see also Schneider & Logan, in press). More generally, precise absolute and relative cost estimates may depend on a variety of factors, such as the number of cues per task (Altmann, 2006), cue transparency (Logan & Schneider, 2006), or relative frequencies of cue and task changes (Mayr, 2006). However, the main point of the 2:1 paradigm is that the qualitative pattern of both substantial cue-switch costs and substantial task-switch costs allows for distinguishing between cue-related and task-related processes. In light of both several successful attempts at functionally dissociating between cue-switch and task-switch costs (see, e.g., Jost, Mayr, & Rösler, 2008; Mayr & Kliegl, 2003) and the relatively subtle differences in total costs between the 2:1 and the 1:1 mappings found with our paradigm, there is little reason to doubt the validity of this approach.

The present results have methodological and theoretical implications. From a methodological perspective, the issue regarding the best way of assessing switch costs is back in court. By the criterion established in the context of the cued task-switching procedure (i.e., substantial cue-switch costs), the simple switch contrast in the task span procedure is just as ambiguous as it is in the cued task-switching paradigm. Apparently, whatever the paradigm, measures must be taken to distinguish between cue-related and task-related costs. Moreover, the results also extend the question of the degree to which switch costs reflect cue-related processes to paradigms that have thus far been deemed "safe." For example, like the task span procedure, the alternate-runs procedure (Rogers & Monsell, 1995) and the voluntary task-switching procedure (Arrington & Logan, 2004) also require participants to cue themselves to different tasks. There is little reason to believe that the internal cues in these procedures would behave differently than they do in the task span procedure. This still leaves many interesting issues that can be addressed using variants of the task span or the voluntary task-switching procedure (see, e.g., Arrington & Logan, 2004; Mayr, 2009; Mayr & Bell, 2006; Schneider & Logan, 2006). However, care needs to be taken when interpreting switch costs as reflecting processes that operate on the level of task-relevant attentional configurations, rather than on the level of external or internal cues.

On a theoretical level, the present results allow conclusions about the representational format of simple ad hoc action plans. Apparently, such plans use largely surface-level codes with no information about the "deep structure" of the cognitive configurations that need to be established in a particular sequence. This is largely consistent with the hypothesis put forward by Baddeley et al. (2001; see also, Bryck & Mayr, 2005; Emerson & Miyake, 2003; Vygotsky & Luria, 1994) that the articulatory loop is utilized to establish control over a sequence of tasks.

The fact that superficial representations are used even for short task sequences may seem oddly inefficient, since it apparently keeps participants from realizing and benefiting from even obvious "deep-level" events such as task repetitions. As suggested by Bryck and Mayr (2005; see also Mayr, 2003), however, this strategy of outsourcing the sequencing function to the phonological or articulatory loop may prevent interference between plan-level and task-level processes. Such interference should occur if the same representational space was used to guide processing on the basis of current task rules while also trying to maintain rules that become relevant in the near future.

One remaining open question is what the boundary conditions for the use of superficial representations of action sequences are. For example, although cue-level representations seem to be relevant for ad hoc plans, one might speculate that they become less important with extensive practice. In fact, there is evidence that implicitly learned representations of task sequences are based on task-level rather than on cue-level information (Gotler et al., 2003).

Another open question is where exactly the cue-switch cost in the task span procedure has its origin. One possibility is that it occurs at retrieval and simply reflects a retrieval shortcut or an internal, cue-based priming effect when task labels repeat within a sequence. However, it is also possible that this effect originates earlier—namely, during encoding of the task sequence. Lien and Ruthruff (2004) showed that subtle gestalt features guide organization of a repeating task sequence into distinct chunks, resulting in substantial between-chunk costs, even when tasks repeat. Within-sequence cue repetitions may establish such chunk boundaries. That said, it is not clear that chunking played a major role in the present paradigm, given that people are typically able to use sequences of three tasks without breaking them into smaller chunks (see, e.g., Mayr, 2009). At this point, therefore, a retrieval-based account of the cue-switch cost seems more plausible.

In conclusion, the present results extend the finding that to a substantial degree, task-switch costs reflect changes in cues, rather than changes in tasks per se, to the task span procedure and probably even to related task-switching paradigms. These results have implications for the adequate assessment and interpretation of switch costs, and they strongly suggest that even simple ad hoc action plans rely to a large degree on superficial articulatory or phonological codes.

AUTHOR NOTE

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NOTE

1. The 2:1 mapping paradigm has been criticized because it may introduce an "additional layer of processing" (Altmann, 2006) that is not part of standard switch costs. The argument that cue-switch costs reflect an "extra" recoding process that needs to occur before actual task switching begins also implies that the actual task-switch costs from the 2:1 paradigm should *not* contain these recoding processes and therefore should be of approximately the same size as switch costs from the standard 1:1 paradigm (in which these extra processes supposedly do not occur). We ran a control experiment ($N = 24$) that was identical to Experiment 2 in all aspects except that it used a 1:1 mapping with different possible cue combinations counterbalanced across participants. We found that short-RSI and long-RSI switch costs ($M =$ 252 and 153 msec, respectively) were a bit smaller than in Experiment 2 ($M = 298$ and 219 msec, respectively) but also much larger than the actual task-switch costs from that experiment ($M = 112$ and 101 msec). The preparation effect on switch costs (99 msec) was of a magnitude similar to that in Experiment 2 [$F(1,23) = 14.43$, $p < .01$]. Thus, rather than producing a qualitative change in processing, the 2:1 mapping probably increases a retrieval or recoding burden that already exists in the 1:1 mapping but which, in that paradigm, cannot be distinguished from the actual task-switch costs.

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