

**Preparing a task is sufficient to generate a subsequent task-switch cost affecting task
performance**

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Author Note:

This work was supported by the Economic and Social Research Council [grant number ES/R005613/1] and was previously presented at a meeting of the Experimental Psychology Society (Bournemouth, UK, July 2019). For the purpose of open access, the authors have applied a Creative Commons Attribution (CC BY) licence to any Author Accepted Manuscript version arising. The data are available at <https://reshare.ukdataservice.ac.uk/854364/>. Analysis code and research materials are available at <https://osf.io/d3r8z/>. The preregistration document is available at <https://aspredicted.org/t4vx2.pdf>. We would like to thank Madhuri Thakur, Milena Gimadieva and Vasilena Voynikova for assistance with data collection.

Abstract

This study investigated the nature of switch costs after trials on which the cued task had been either only prepared (cue-only trials) or both prepared and performed (completed trials). Previous studies have found that task-switch costs occur following cue-only trials, demonstrating that preparing – without performing – a task is sufficient to produce a subsequent switch cost. However, it is not clear whether switch costs after these different types of trial reflect an impact of task switching upon task preparation or task performance on the current trial. The present study examined this question using a double-registration procedure with both cue-only and completed trials. Participants responded to both task-cue and target stimuli. In cue responses, a cost of switching task cues (cue-switch cost) but not of switching tasks (task-switch cost) followed both cue-only and completed trials. In target responses, a task-switch cost but no cue-switch cost followed both cue-only trials and completed trials, and this task-switch cost was larger following completed than cue-only trials. The presence of the task-switch cost in target responses following cue-only trials indicates a specific impact of previous preparation upon task performance, and the increased size of this cost following completed trials indicates an additional impact of previous performance. Together, these results suggest that both task preparation and task performance contribute to the subsequent task-switch cost affecting task performance.

Keywords: cognitive control, double-registration procedure, preparation, performance, task switching

Preparing a task is sufficient to generate a subsequent task-switch cost affecting task performance

What is the difference between thought and action, in terms of their effects on subsequent behaviour? It seems clear that current behaviour depends on past action, but do past intentions to perform a task also affect current behaviour even when they were not in fact performed? If they do, do they do so in the same way that past action influences current behaviour? To address this question, the present study examined the influence of task preparation and task performance on the subsequent preparation and performance of a task.

The influence of a prior task on current task performance has been investigated using the cued task-switching paradigm (e.g., Meiran, 1996). In a standard version of the paradigm, participants are asked to perform two different tasks on different trials (e.g., judging the colour or the shape of a target stimulus). A task cue (such as the name of the task; e.g., COLOUR) is presented at the beginning of each trial to indicate which of the two tasks should be performed on that trial. Responses are faster and more accurate when the task that is performed on the current trial is the same task as was performed on the immediately preceding trial (*repeat trials*) than when it is a different task (*switch trials*). This difference is termed the *switch cost* (see reviews by Kiesel et al., 2010; Monsell, 2003; and Vandierendonck, Liefoghe, and Verbruggen, 2010). Switch costs indicate that some aspect of the previous task must persist on the current trial.

A number of models of task switching have been put forward to explain the switch cost and how it behaves. These models vary with respect to the emphasis they put on aspects of *task preparation* versus *task performance* in generating and overcoming switch costs as well as on the type of mechanism on which they rely. A number of models rely on priming-based processes, either at the stage of task preparation or of task performance. A model that

emphasises the role of preparation-related priming is that of Altmann and Gray (2008). In this model, switch costs in RT occur because the meaning of the task cue can be interpreted more quickly when the task repeats than when it switches – in other words, the interpretation of a cue in terms of a specific task benefits from having been recently primed. Another priming-based model by Schneider and Logan (2005) generates the RT switch cost because associations between cues for the same task allow cue-encoding to proceed more quickly when the task repeats than when it switches. The model of Allport and Wylie (1999) also features priming, but at the level of the task-sets required for performance rather than of preparation-specific effects related to task cues. According to this model, performance of the currently required task requires resolution of a competition between tasks that operate on the same target stimuli, which is accomplished by both facilitation of the required task and inhibition of the alternative task. Such facilitation and inhibition persist into the next trial in the form of positive and negative priming of the two task-sets, affecting the speed of performance and resulting in a switch cost. While Allport and Wylie posit a role for task preparation in reducing an existing switch cost, it is not clear that they would necessarily expect the effects of preparation to persist to the following trial; thus, we assume that in their model, the persisting effects of priming would be primarily, or entirely, driven by task performance. Allport and Wylie (2000) have since incorporated into their model a mechanism whereby the retrieval of previous trial instances allows faster processing when targets, responses and tasks are repeated than when targets re-occur in the context of a different task; clearly, this version of the model requires previous performance and also would affect current performance (rather than preparation).

A different class of model from that based on priming is that within which stage-like control processes reconfigure the task-set before it is used on trials involving a task switch. Two-stage models (e.g., Mayr & Kliegl, 2003; Meiran, 2000; Rogers & Monsell, 1995;

Rubinstein, Meyer, & Evans, 2001) state that one component of reconfiguration can be driven endogenously prior to target onset and another component can only be driven exogenously following target onset. Hence, these models involve both preparation- and performance-driven aspects of task-switching. De Jong (2000) suggested that there might be just one component of reconfiguration, and that it does not always occur in advance of target onset: according to this *failure-to-engage* hypothesis, reconfiguring the task set in advance of target onset is optional, and reconfiguration that did not occur before target onset is postponed until after target onset. These reconfiguration-based models, then, propose that task-switching is driven partly during preparation and partly during performance.

A recent model returns to the idea of priming, but at the level of features (such as a specific stimulus or response), rather than at the level of an entire task-set. Schmidt and Liefoghe (2016; see also Schmidt, Liefoghe, & De Houwer, 2020) made the case that what is usually measured as the switch cost is composed largely of biases caused by systematic differences between task-repeat and task-switch trials in terms of re-occurrence of trial features. For instance, the benefit of processing of an identical cue from one trial to the next can only contribute to improved performance on task-repeat trials, not on task-switch trials. Similarly, when selecting the same response from one trial to the next, performance can benefit from repetition of that response's meaning (e.g. "odd" in an odd/even judgement task) only on task-repeat trials, and not on task-switch trials where the same response will have a different meaning (e.g. "lower than 5"). Schmidt and Liefoghe recommended that researchers control for these feature-level factors if they wish to measure the actual cost of switching tasks. They found that when they excluded all such feature-repetitions during analysis, a small *remaining switch cost* was found. They concluded that whereas the majority of the switch cost as usually measured was the result of bias from feature-level effects, this remaining cost might yet be shown to represent the occurrence of processes at the level of

task sets, such as task-set priming or reconfiguration. However, they speculated that it could alternatively represent some further lower-level effect not yet controlled for. One such possibility is *inter-cue priming* (already proposed as part of their model by Schneider & Logan, 2005), where different cues for a single task could prime each other via shared meanings or associations. Another possibility speculatively suggested by Schmidt and Liefoghe is *passive conceptual response priming*, where a single task cue might prime all of the response meanings (e.g. “odd” and “even”) of a single task. With respect to our research question regarding the roles of preparation and performance in task switching, many of the feature-based effects identified by Schmidt and Liefoghe would both be driven by and impact upon task performance since they relate to repetition of features of target stimuli and responses (as described above, re. the retrieval mechanism of Allport & Wylie, 2000); others, such as cue-repetition and inter-cue priming, relate solely to task preparation. It seems likely that the passive conceptual response-priming effect, however, would involve both stages, potentially being driven by preparation on the preceding trial yet affecting performance on the current trial.

If we are to be able to discriminate between these theoretical ideas in terms of how accurately they describe the differential contributions of preparation and performance in task-switching, we will need to collect behavioural data using methods that distinguish preparation from performance, preferably both in terms of which of these stages lead to the *generation* of a subsequent switch cost and which are *affected by* the switch cost. The *cue-only* trial method (Lenartowicz, Yeung, & Cohen, 2011) allows the contribution of task preparation to the generation of a subsequent switch cost to be assessed separately from the contribution of task performance. The *double-registration* method (Arrington, Logan, & Schneider, 2007) allows a switch cost that affects task preparation to be assessed separately from a switch cost that affects task performance. These methods will next be described in turn, before we

describe how we combined them in the current study to tackle our central research question regarding the comparative nature of switch costs following preparation versus performance.

In the standard procedure, the cued task is both prepared and performed on every trial, so a switch cost measured on the current trial could result either from having prepared the cued task on the preceding trial or from having performed it. Lenartowicz et al. (2011) distinguished these two possible sources of the switch cost by converting a proportion of trials into *cue-only* trials, on which only a task cue but no target stimulus was presented; on such trials, the cued task could be prepared but not performed. On the remaining trials (which we term *completed* trials), the task cue was followed by a target requiring a task-appropriate response, so that the cued task was both prepared and performed. A substantial switch cost followed cue-only trials when the current trial had a short cue-target interval (Lenartowicz et al., 2011; see also Swainson, Martin, & Prosser, 2017; Swainson, Prosser, Karavasilev, & Romanczuk, 2021). This shows that preparation in the absence of performance has a persisting effect upon subsequent behaviour and that, contrary to what had previously seemed to be the case (e.g., Los & Van der Burg, 2010; Schuch & Koch, 2003; Verbruggen, Liefoghe, & Vandierendonck, 2006), no element of task performance such as response selection is required to drive a subsequent switch cost. It is yet to be determined whether the switch cost produced by preparation is the same type of cost as that produced by performance, and that question is the focus of the current study.

In the *double-registration* procedure (Arrington & Logan, 2005; Arrington, Logan, & Schneider, 2007), participants respond first to the task cue (*cue response*) to identify which task is to be performed on the trial, and then to the target stimulus (*target response*) to perform the cued task. Hence, separate switch costs can be obtained relating to cue processing (task preparation) and target processing (task performance). Using this procedure (with only completed trials), Arrington et al. found that cue responses exhibited a cost of

switching task cues (*cue-switch cost*) but no cost of switching tasks (while controlling for cue-switching: *task-switch cost*). Importantly, target responses exhibited a task-switch cost but not a cue-switch cost, on the basis of which result the authors concluded that the double-registration procedure had enabled them to demonstrate what they called a “true” task-switch cost: i.e., a cost of switching tasks that affected target processing (measured at target response) without also capturing any remaining costs related to cue processing. Arrington et al. were not able to determine conclusively from their data whether this task-switch cost in target responses represented the interfering effect of task-priming, the need for exogenous task-set reconfiguration, or the need for endogenous task-set reconfiguration. It is also worth noting that from the perspective of Schmidt and colleagues’ recent work (2016; 2020), Arrington et al.’s true task-switch cost might have included some feature-based biases because target items and responses could repeat across trials.

The Present Study

The present study combined a number of design features from studies described above to allow us to address our central research question. First, it included cue-only trials, such that we would be able to isolate switch costs caused by preparation alone on the preceding trial and compare their nature with that of switch costs that followed both preparation and performance on the preceding trial. Second, it used a double-registration design, such that we would be able to measure switch costs separately at the task-preparation and task-performance stages of the current trial. Combining the cue-only and double-registration techniques would allow us to detect whether preparation and performance produce switch costs that differentially affect preparation and performance on the subsequent trial. Third, it allowed us to prevent from occurring or to exclude from analysis (as recommended by Schmidt & Liefooghe, 2016) the repetition of identical trial features, such that we would be able to define costs of switching at the level of task, unconfounded as far as possible by item-

level confounds. We note here that when we refer to isolating the effects of preparation in the current study, we include all processes involved in cue-only trials (and the pre-target stage of completed trials) as being part of that preparation. Hence, the selection and execution of a cue response constituted part of the preparation stage of trials in this study, but they do not do so in most task-switching studies. We will return to this issue in the Discussion section. Importantly, cue-only trials still included no element of task performance at all because no target was presented, and no target response was therefore selected or executed, on those trials. Hence, the separation between task preparation and task performance was retained here.

We tested three preregistered hypotheses (see below, in Transparency and Openness), all specifically regarding the task-switch cost.

Hypothesis 1: Preparation will affect subsequent preparation.

The first hypothesis states: *There will be a larger task-switch cost in cue responses following cue-only trials than following completed trials.* A task-switch cost that affects cue-RTs would represent a cost of switching tasks that impacts upon the speed of task identification – an aspect of task preparation – on the current trial. According to the model of Altmann and Gray (2008), the task-switch cost in RTs is due to a priming effect affecting the speed of interpretation of the task cue in terms of the task it signals, and therefore this cost should presumably affect cue-RTs. In Schneider and Logan's (2005) model, the slower responses on switch versus repeat trials (in both cases, with a switch of cue) stems from the benefit of associated cues priming each other more than non-associated cues do. Similarly, Schmidt and Liefoghe (2016) suggested that the remaining switch cost they observed even after excluding feature repetitions might stem from such inter-cue priming. Priming that stems from task preparation on the preceding trial that causes speeding of preparation for the

same task on the current trial should cause a task-switch cost that affects cue responses, and should occur following both cue-only and completed trials.

Arrington et al. (2007), on the basis of Schneider and Logan's (2005) model, had also predicted that semantic and associative priming between cues for the same task (e.g., *parity* and *odd-even*) should benefit task identification on task-repeat trials to produce a task-switch cost in cue-RTs, but in fact they found no such cost (at least, within the experiments that they considered to have successfully isolated cue processing and target processing to cue-RTs and target-RTs respectively: Expts. 2, 3b & 4b). Nevertheless, we anticipated that we might see this type of cost in our study because of the likely stronger semantic relatedness between the two cues of each task, both naming the relevant stimulus attribute (*COLOUR* and *HUE*; *SHAPE* and *FORM*), than was available between the name and mapping cues used by Arrington et al. We predicted that if a task-switch cost was indeed seen in cue-RTs in the present study, this cost should be larger following cue-only trials than following completed trials because in our design cue-only trials would be substantially shorter in duration than completed trials, a difference that we have found previously to be associated with larger switch costs following cue-only than completed trials (Swainson et al., 2021).

Hypothesis 2: Preparation will affect subsequent performance.

The second hypothesis states: *There might be a task-switch cost present in target responses following cue-only trials, even if cue-switch costs (switching cues versus repeating cues on task-repeat trials) are isolated in our data such that they do not affect target-responses.* This hypothesis tested the possibility that the occurrence of task preparation alone (i.e., on a cue-only trial) would be sufficient to cause what Arrington et al. (2007) referred to as a “true” cost of switching tasks on the next trial – i.e., a cost of switching tasks that affects task performance (i.e., target responses) and is uncontaminated by cue-processing costs (i.e., in the absence of a cue-switching cost in target responses).

Several task-switching models posit that task preparation leads directly to a change in readiness to perform a task via endogenous reconfiguration of the task set (e.g., Mayr & Kliegl, 2003; Meiran, 2000; Rogers & Monsell, 1995; Rubinstein et al., 2001) and therefore it seems plausible that endogenous reconfiguration occurring on cue-only trials might drive a task-switch cost seen in the target responses of a subsequent completed trial as long as that cost was not able to be entirely overcome prior to target onset on that subsequent trial. Arrington et al. (2007) speculated that the “true” task-switch costs they found following completed trials might constitute the time taken by a process of endogenous task-set reconfiguration that was not completed on every trial prior to target onset, as in the failure-to-engage hypothesis of de Jong (2000). Arrington et al. could not rule out either exogenous reconfiguration or task-priming as potential alternative explanations for this cost. However, if we were to observe such a cost following cue-only trials in the current study, we should at least be able to rule out exogenous reconfiguration on the preceding trial as the source of the switch cost because it can presumably only be driven by performance (which does not take place on cue-only trials). Task-set priming (Allport & Wylie, 1999) would also appear to be unlikely as a source of such a cost, since it would presumably be primarily (or solely) driven by performance. However, it seems plausible that passive conceptual response priming – speculatively suggested by Schmidt and Liefoghe (2016) to be a possible source of the switch cost that remains despite exclusion of feature-based priming effects – could provide an additional way for preparation to affect subsequent performance (as per Hypothesis 2) if priming of a task’s conceptual responses by a cue can persist from one trial to the next.

Hypothesis 3: Performance will affect subsequent performance.

The third hypothesis states: *There will be a larger task-switch cost in target responses following completed trials than following cue-only trials.* The majority of task-switching models predict that the task-switch cost reflects either a slowing of, or a delay to, task performance and would therefore predict that a task-switch cost should be seen in target-RTs. In addition, a number of these models (e.g., Allport & Wylie, 1999; Rogers & Monsell, 1995; de Jong, 2000) posit a special role for performance in the process of effecting a task switch on the current trial. Assuming that the effects of that task switch persist into the next trial, it seems reasonable to assume that the same models would also predict that performance would be involved also in establishing the conditions for a subsequent task-switch cost (seen in target responses). This would have the effect of either producing a cost, or adding to any cost already established by preparation on the preceding trial.

In the model of Allport and Wylie (1999), the difficulty of performing a task under conditions of between-task conflict is resolved by positive and negative task priming that persist, causing a switch cost affecting subsequent task performance. Hence, task-set priming should cause a task-switch cost that would affect target-RTs especially (and potentially only) after completed trials. The retrieval-based mechanism that Allport and Wylie developed subsequently (e.g., Allport & Wylie, 2000) would also be driven by performance and would impact upon performance, since it centres around the binding of specific target stimuli, responses, and tasks. Two-stage reconfiguration models (e.g., Mayr & Kliegl, 2003; Meiran, 2000; Rogers & Monsell, 1995; Rubinstein, Meyer, & Evans, 2001) seem likely to predict larger switch costs following completed trials than following cue-only trials because cue-only trials would only involve endogenous reconfiguration whereas completed trials would involve both endogenous and exogenous reconfiguration. According to de Jong's failure-to-engage model, reconfiguration will sometimes fail to occur before a target is presented.

Because no target is ever presented on cue-only trials, it can be assumed that reconfiguration would simply not occur at all if it failed to occur during preparation on a cue-only trial, presumably leaving the active task-set reflecting the task that had been performed most recently and hence leading to a reduced subsequent task-switch cost.

A number of the trial features whose repetition contributes to exaggerated switch costs according to the model of Schmidt and Liefoghe (2016) relate to the target or response; they could in principle, therefore, produce or increase a switch cost measured at target responses following completed trials. We deliberately excluded from our design immediate repetitions of target features, but responses were allowed to repeat; this factor could contribute to the task-switch cost at target responses, as suggested by Schmidt and Liefoghe, causing that cost to be larger following completed trials than following cue-only trials.

Method

Participants

In total, 50 participants were recruited on a voluntary basis or in exchange for course credit. Ten of the 40 participants initially tested were excluded from the analysis according to the preregistered criteria (see Results), and another 10 were recruited to replace them. The final set of analysable datasets were from 40 participants¹ (27 females and 13 males; age 18-50 years, mean 26 years). This study was approved by the ethics committee of the School of Psychology, University of Aberdeen.

Apparatus and Stimuli

¹ A post-exclusion sample size of 40 participants was chosen a priori and would enable us to detect effect sizes of $d_z = 0.4$ with 80% power with a one-sided test.

The apparatus consisted of a personal computer running E-Prime 2.0 (Psychology Software Tools) and a 20" 60-Hz monitor. Target stimuli were coloured shapes (approximately 2.5 cm wide). There were four shapes (circle, star, triangle, and square) and four colours (red, blue, yellow, and green), shown centrally on a black background. Task-cues were white, in bold Courier New font, size 28: "COLOUR" and "HUE" (colour task); "SHAPE" and "FORM" (shape task). Target-responses were selected at random on each trial. The same target feature was not allowed to repeat on two consecutive completed trials (including across intervening cue-only trials).

Responses were registered with RB-740 Cedrus button-boxes (Cedrus Corporation, 2003). Participants pressed four buttons, which were arranged horizontally, with index and middle fingers of both hands. Left-hand fingers executed cue-responses (middle = colour task, and index = shape task). Right-hand fingers executed target-responses: two shapes and two colours were assigned to each of the two fingers. Circles and stars were assigned to one finger, and triangles and squares were assigned to the other finger; similarly, red and green were assigned to one finger, and blue and yellow were assigned to the other finger. Thus, there were four possible combinations of the shape and colour pairs to the two fingers, which were counterbalanced across participants.

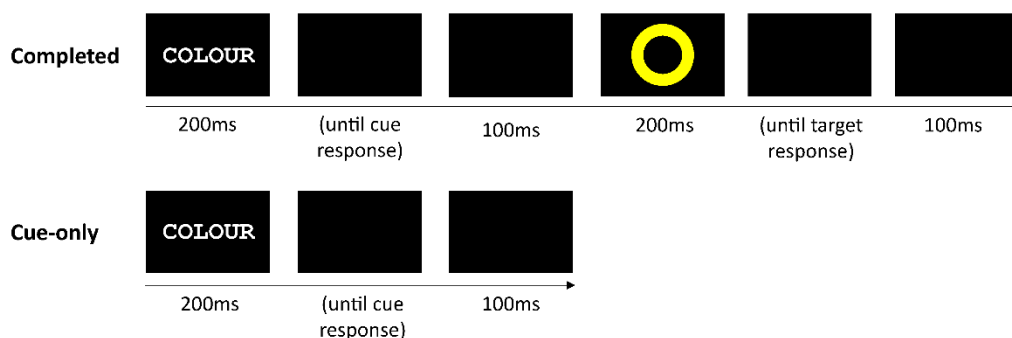
Procedure

Up to four participants were tested concurrently in 40-minute sessions, separated by barrier screens so that they could not see each other's monitors. All participants being tested concurrently began their sessions at the same time and were given instructions on-screen, so they could work at their own pace. Trials were either *completed* or *cue-only* trials (see Figure 1). All trials started with a task-cue presented for 200ms, which was replaced by a blank screen until the cue-response was made, followed by a 100ms interval. Cue-only trials

terminated at that point, to be followed by onset of the cue for the next trial. If the cue response was incorrect, the message “WRONG RESPONSE” was shown in magenta for 500ms, followed by a 500ms blank screen. If the cue response was incorrect on a completed trial, the trial terminated at that point without the target being shown. On completed trials with a correct cue response, a target stimulus was shown for 200ms after the blank screen following the cue-response. The target was replaced by a blank screen until the target response was made (up to a maximum of 60 seconds), followed by a blank screen for 100ms. If a target response was incorrect, the message “WRONG RESPONSE” was shown in magenta for 500ms, followed by a 500ms blank screen; otherwise, the next trial then began with the onset of its task cue. Cue-RT was the interval between the onset of a task cue and a button-press response, and target-RT was the interval between the onset of a target and a button-press response. Please note that the cue-target intervals, target-cue intervals and cue-cue intervals (following cue-only trials) were not controlled in this design, since each was dependent upon the execution of a (cue- or target-) response.

Figure 1

Timing of Trial Events in Completed and Cue-Only Trials



Participants first performed four blocks of practice trials. The first practice block (20 trials) included only completed trials of the colour task, with cue-repeat and task-repeat trials;

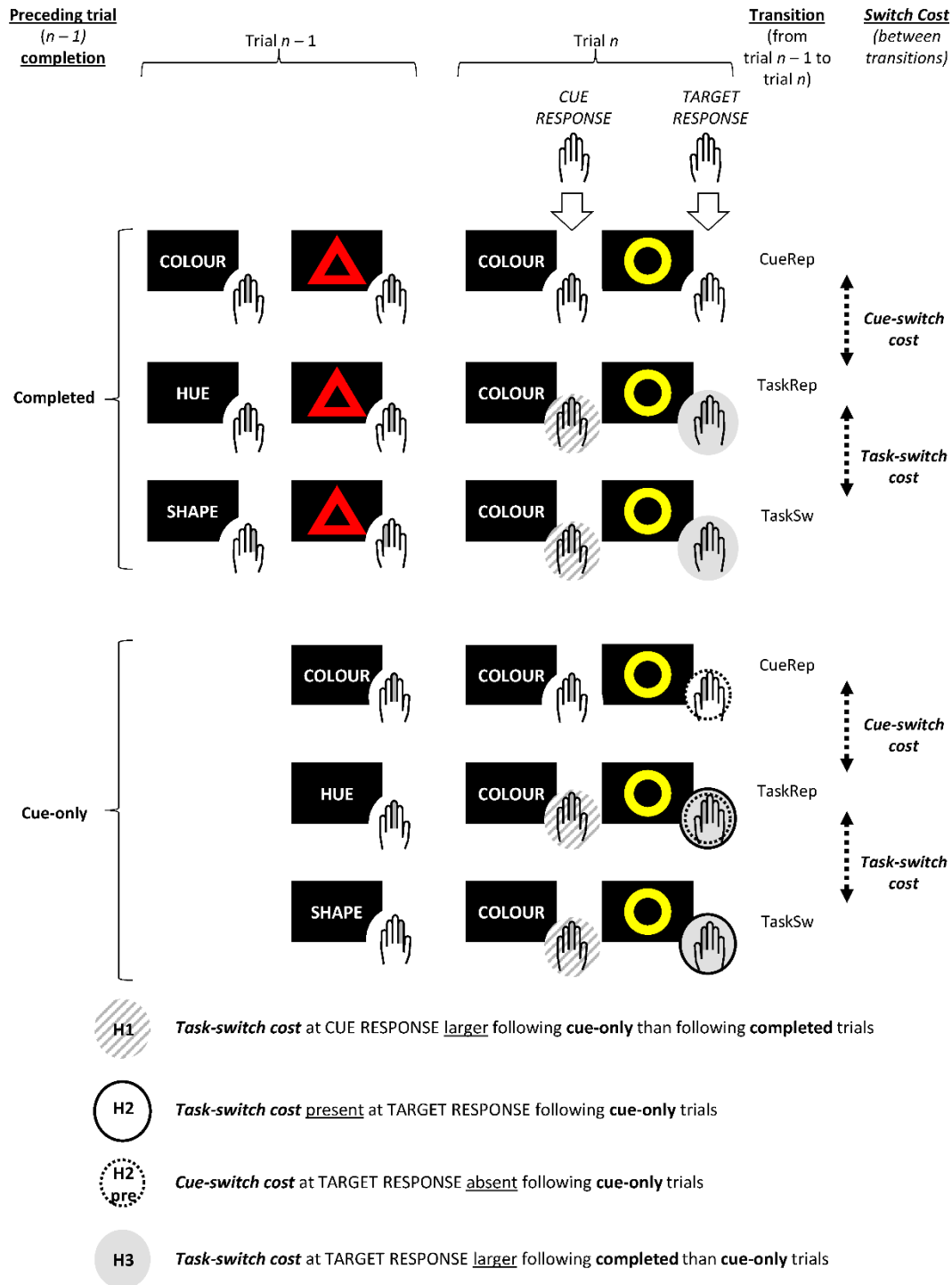
the second practice block (20 trials) included only completed trials of the shape task, with cue-repeat and task-repeat trials; and the third practice block (20 trials) included only completed trials, with a mixture of colour and shape tasks, and with cue-repeat, task-repeat and task-switch trials. Cue-only trials were included in the fourth practice block (41 trials), which had the structure of an experimental block. In this block, the first trial was always a completed trial; 12 of the remaining 40 trials were cue-only trials (30%) and 28 were completed trials. Cue-only trials were always followed by a completed trial. Participants were allowed to repeat the practice blocks if they wished to; otherwise, they proceeded to complete 15 experimental blocks of 41 trials each, which were the same as the fourth practice block. Participants were told to respond to every target using the most recently cued task, and they were asked to respond quickly while aiming to make fewer than seven errors per block.

The task (colour or shape) for the first trial of each block was selected at random. On 50% of the subsequent trials, the task was the same as that in the preceding trial; of these, half were *cue-repeat* trials, where the task cue repeated (e.g., COLOUR→COLOUR) and the other half were *task-repeat* trials, where the task cue switched (e.g., HUE→COLOUR). The remaining 50% of trials were *task-switch* trials, where both task-cue and task switched (e.g., FORM→COLOUR). The difference in performance between cue-repeat trials and task-repeat trials is the *cue-switch cost* (task-repeat – cue-repeat), whereas the difference in performance between task-repeat trials and task-switch trials is the *task-switch cost* (task-switch – task-repeat). The total numbers of trials per participant available for analysis prior to trial exclusions (see below) were as follows. Of completed trials following cue-only trials, there were 45 cue-repeat, 45 task-repeat, and 90 task-switch trials. Of completed trials following completed trials, there were 60 cue-repeat, 60 task-repeat, and 120 task-switch trials. After each experimental block, participants were shown their average target-RT and

the number of errors made in that block. Figure 2 illustrates the design with six example trial pairs (trial $n - 1$ and trial n) to show the three types of between-trial transition (cue-repeat, task-repeat, task-switch), and how the two types of switch cost (cue-switch cost, task-switch cost) are calculated by comparing performance between specific types of transition, following the two types of preceding trial completion (completed, cue-only).

Figure 2

Experimental Design, Showing Types of Transition and Switch Cost Following Each Type of Preceding Trial Completion, and Hypotheses 1-3



Note. Shaded index or middle finger indicate the response that might be required in these example trials. H = Hypothesis; pre = precondition

Transparency and openness

We report all data exclusions, all manipulations, and all measures in the study. The preregistration document is available at <https://aspredicted.org/t4vx2.pdf>²; this includes the planned sample size, details of data processing (including exclusions) and how the three main hypotheses would be tested, all of which were adhered to (while additional, non-preregistered, analyses were also carried out, as detailed below). Data were processed and analysed in R version 3.6.0 (R Core Team, 2019) using RStudio version 2022.2.2.485 (RStudio Team, 2022) with the following packages: tidyverse (Wickham, 2017), BayesFactor (Morey & Rouder, 2018), afex (Singmann, Bolker, Westfall, & Aust, 2019), ggpubr (Kassambara, 2018) and cowplot (Wilke, 2019). The data are available at <https://reshare.ukdataservice.ac.uk/854364/>. Analysis code and research materials are available at <https://osf.io/d3r8z/>.

Results

Ten participants were excluded (one participant for having target-response accuracy below 70%; nine further participants for having more than 10% target-RTs outside the range 200ms-3000ms) and replaced by new participants.

Mean cue-RT and target-RT were computed for each participant for completed trials on which the corresponding responses were correct. The first trial of each block was never analysed. For the analyses of cue-RT and cue-response accuracy, additional trials were excluded if cue-RT was less than 200ms or greater than 3000ms or if the cue response or the target response was incorrect on the preceding trial (mean percentage additional trials

² Please note that for ease of exposition, the three hypotheses are listed here in a different order than in the preregistration. Because the layout of the preregistration form assumed a single hypothesis, only Hypothesis 3 is listed under Question 2, with Hypotheses 1 and 2 being listed under Question 8.

excluded: 10.4% cue-RT; 11.3% cue-accuracy³). For the analyses of target-RT and target-response accuracy, additional trials were excluded if the cue-RT or the target-RT was less than 200ms or greater than 3000ms, if the cue response was incorrect on the current trial, or if the cue response or the target response was incorrect on the preceding trial (mean percentage additional trials excluded: 11.8% target-RT; 17.6% target-accuracy).

Figures 3 and 4 summarise the data relevant to the main analyses. In order to aid comparison of the statistical results with the data as shown in the figures, effects of transition are calculated as *switch costs* (where negative values indicate a relative benefit of switching either cue or task) and effects of preceding trial completion (PTC) on switch costs are calculated as *switch-cost differences*, which indicate the degree to which the cost on trials preceded by completed trials was larger than that on trials preceded by cue-only trials (i.e., [completed – cue-only]; negative values indicate a larger switch cost for trials preceded by cue-only trials than for trials preceded by completed trials).

Analysis strategy

As well as the preregistered hypothesis tests, 2x2 repeated-measures analyses of variance (ANOVAs; not preregistered) were carried out to provide a more comprehensive view of the effects present in the data. Separate repeated-measures ANOVAs were run on the RT and percentage error data from cue responses and target responses. The first set of ANOVAs focused on the effect of switching cues and included the factors preceding trial completion (cue-only, completed) and cue transition (cue-repeat, task-repeat). The second set of ANOVAs focused on the effect of switching tasks and included the factors preceding trial completion (cue-only, completed) and task transition (task-repeat, task-switch). Simple main effects analyses, run to follow up significant interactions, used two-tailed paired *t* tests. An

³ The reason for these values not being identical is that trials where the relevant (cue or target) response was incorrect had already been excluded from the RT analyses, but not from the accuracy analyses.

alpha of .05 was applied to the ANOVAs and their follow-up tests. These analyses were run using the R package afex (Singmann, Bolker, Westfall, & Aust, 2019).

The results of the preregistered hypothesis tests are reported within the relevant section. Since all of these hypotheses specified a direction, they were tested using one-tailed t tests, with an alpha of .05. We have aimed to avoid repeating reported results as far as possible; therefore, reporting of one-tailed results refers back to the two-tailed equivalent where applicable. We also report the Bayes Factor (BF_{10} ; not preregistered) of the one-tailed Bayesian t test equivalent for each of the preregistered hypothesis tests and follow-up analyses, calculated using the BayesFactor package for R (Morey & Rouder, 2018). The Bayes Factor (BF_{10}) shows the relative strength of evidence for the hypothesis tested, relative to the null. Rouder, Speckman, Sun, Morey, and Iverson (2009) cite Jeffreys' guidelines for interpretation, with a BF_{10} above 3 constituting some evidence, above 10 strong evidence, and above 30 very strong evidence in favour of the tested hypothesis; correspondingly, a BF_{10} below 0.33 would constitute some evidence, below 0.10 strong evidence, and below 0.033 very strong evidence in favour of the null.

Cue responses: cue-transition (cue-repeat vs. task-repeat; see Figure 3)

In the cue-RT data, there was a significant overall cost of switching versus repeating cues, $F(1,39) = 62.34$, $MSE = 9,317$, $p < .001$, $\eta_p^2 = 0.62$, and cue-RTs were significantly shorter following cue-only than completed trials, $F(1,39) = 13.33$, $MSE = 7,302$, $p < .001$, $\eta_p^2 = 0.25$. In addition, cue transition interacted significantly with preceding trial completion, $F(1,39) = 16.72$, $MSE = 2,785$, $p < .001$, $\eta_p^2 = 0.30$. The cost of cue switching following cue-only trials (mean 155ms), $t(39) = 7.45$, $p_{(two-tailed)} < .001$, $d_z = 1.18$ was larger than that following completed trials (mean 86ms), $t(39) = 6.54$, $p_{(two-tailed)} < .001$, $d_z = 1.03$.

In the cue-response error data, there was a significant overall cost of switching cues, $F(1,39) = 31.64$, $MSE = 17.07$, $p < .001$, $\eta_p^2 = 0.45$, and significantly more errors were made following cue-only than completed trials, $F(1,39) = 7.91$, $MSE = 10.56$, $p = .008$, $\eta_p^2 = 0.17$. The interaction between cue transition and preceding trial completion was not significant, $F(1,39) = 2.78$, $MSE = 20.48$, $p = .10$, $\eta_p^2 = 0.07$.

Cue responses: task-transition (task-repeat vs. task-switch; see Figure 3)

In the cue-RT data, the main effect of task transition was not significant, $F(1,39) = 0.45$, $MSE = 6,355$, $p = .51$, $\eta_p^2 = 0.01$, and neither was that of preceding trial completion, $F(1,39) = 3.58$, $MSE = 7,953$, $p = .07$, $\eta_p^2 = 0.08$. The interaction between these two effects did not reach significance, $F(1,39) = 3.88$, $MSE = 1,359$, $p = 0.056$, $\eta_p^2 = 0.090$.

In the cue-response error data, the main effect of task transition did not reach significance, $F(1,39) = 3.95$, $MSE = 14.49$, $p = .054$, $\eta_p^2 = 0.09$, but the main effect of preceding trial completion was significant, $F(1,39) = 9.35$, $MSE = 11.68$, $p = .004$, $\eta_p^2 = 0.19$, with more errors being made following cue-only trials than following completed trials. The interaction between these effects was not significant, $F(1,39) = 2.50$, $MSE = 15.50$, $p = .12$, $\eta_p^2 = 0.06$.

Test of Hypothesis 1. Hypothesis 1 predicted a larger task-switch cost (task-switch – task-repeat) in cue responses following cue-only trials than following completed trials (and therefore a significantly negative task-switch cost difference [completed – cue-only]). No such difference was found. In fact, the task-switch cost difference was positive (and therefore lay in the opposite direction from that tested for) for both cue-RTs (mean task-switch cost difference = 23 ms), $t(39) = 1.97$, $p_{(\text{one-tailed})} = .97$, $d_z = 0.31$, $BF_{10} = 0.06$) and cue-response errors (mean task-switch cost difference = 1.97%), $t(39) = 1.58$, $p_{(\text{one-tailed})} = .94$, $d_z = 0.25$, $BF_{10} = 0.07$. Hypothesis 1 was therefore not supported.

Target responses: cue-transition (cue-repeat vs. task-repeat; see Figure 4)

In the target-RT data, neither of the main effects was significant: cue transition, $F(1,39) = 2.10$, $MSE = 4,829$, $p = .16$, $\eta_p^2 = 0.05$; preceding trial completion, $F(1,39) = 2.08$, $MSE = 9,812$, $p = .16$, $\eta_p^2 = 0.05$. However, the interaction between these factors was significant, $F(1,39) = 4.48$, $MSE = 6,803$, $p = .041$, $\eta_p^2 = 0.10$. There was a significant benefit (rather than a cost) of switching cues following cue-only trials (a mean switch benefit of 44 ms), $t(39) = 2.54$, $p_{(two-tailed)} = .015$, $d_z = 0.40$, but no significant effect of switching cues following completed trials, $t(39) = 0.69$, $p_{(two-tailed)} = .50$, $d_z = 0.11$ (a numerical mean cost of 12 ms).

In the target-response error data, neither main effect was significant: cue transition, $F(1,39) = 0.07$, $MSE = 14.79$, $p = .79$, $\eta_p^2 = 0.002$; preceding trial completion, $F(1,39) = 1.76$, $MSE = 18.69$, $p = 0.19$, $\eta_p^2 = 0.04$. The interaction between these factors was also not significant, $F(1,39) = 0.02$, $MSE = 21.12$, $p = .89$, $\eta_p^2 < .001$.

Test of precondition for Hypothesis 2: The precondition for Hypothesis 2 required the absence of a significant cost of switching cues in target responses following cue-only trials. This precondition was met in both target-RTs, where the negative cost of -44 ms reflected a benefit of switching cues, $p_{(one-tailed)} = .99$ (also reported above with a non-directional two-tailed test), $BF_{10} = 0.053$, and in target-response errors (mean cost = 0.27%), $t(39) = 0.27$, $p_{(one-tailed)} = 0.40$, $d_z = 0.04$, $BF_{10} = 0.21$. (Note that Hypothesis 2 itself concerns target responses and is reported below.)

Target responses: task-transition (task-repeat vs. task-switch; see Figure 4)

In the RT data, there was a significant overall cost of switching tasks, $F(1,39) = 70.13$, $MSE = 9,693$, $p < .001$, $\eta_p^2 = 0.64$, and RTs following completed trials were significantly slower than those following cue-only trials, $F(1,39) = 10.68$, $MSE = 8,154$, $p =$

.002, $\eta_p^2 = 0.21$. There was also a significant interaction between these two factors, $F(1,39) = 11.71$, $MSE = 5,932$, $p = .001$, $\eta_p^2 = 0.23$, with the task-switch cost following cue-only trials ($M = 89$ ms), $t(39) = 5.00$, $p_{(two-tailed)} < .001$, $d_z = 0.79$, being significantly smaller than that following completed trials ($M = 172$ ms), $t(39) = 7.96$, $p_{(two-tailed)} < .001$, $d_z = 1.26$.

In the percentage error data, there was overall a significant cost of switching tasks, $F(1,39) = 6.32$, $MSE = 20.06$, $p = 0.02$, $\eta_p^2 = 0.14$. The main effect of preceding trial completion was not significant, $F(1,39) = 1.24$, $MSE = 10.76$, $p = .27$, $\eta_p^2 = 0.03$, and neither was the interaction between the two factors, $F(1,39) = 0.55$, $MSE = 13.77$, $p = .46$, $\eta_p^2 = 0.01$.

Test of Hypothesis 2. Hypothesis 2 required there to be a significant cost of switching tasks at target responses where there had only been task preparation and not performance on the preceding trial. The task-switch cost in target-RTs following cue-only trials was significant in target-RTs (mean cost = 89 ms), $p_{(one-tailed)} < .001$ (also reported above with a non-directional two-tailed test), $BF_{10} = 3,306$, while it did not reach significance in target-response errors (mean cost = 1.34%), $t(39) = 1.68$, $p_{(one-tailed)} = 0.051$, $d_z = 0.27$, $BF_{10} = 1.16$. Therefore, Hypothesis 2 was supported.

We wished to exclude the possibility that this task-switch cost in target-RTs following cue-only trials, apparently driven by preparation on the immediately preceding cue-only trial, could have been driven by earlier performance of the same task. Hence, we conducted an additional test (not preregistered) for a task-switch cost that only included sequences of trials where the transition from trial $n - 2$ to trial n opposed the transition from trial $n - 1$ to trial n (i.e., ABA vs. BAA⁴). A persisting influence from the task performed on (completed) trial n

⁴ The notation ABA represents the sequence of tasks A (completed), then B (cue-only), then A (completed), with the final A referring to the current trial. In this sequence, the current trial involves a switch from the task cued on the immediately preceding trial but a repetition of the task from the trial before that. Repetition of cues was disallowed within the sequences analysed, so the precise comparison was between A_2BA_1 and BA_2A_1 sequences (where subscripts denote cues, such that A_1 and A_2 represent the same task with different cues).

– 2 would be expected to produce a benefit for ABA trial sequences relative to BAA sequences, whereas a persisting influence of the task prepared on (cue-only) trial $n - 1$ should produce a cost for the same comparison. A one-tailed paired t test⁵ showed that target responses on ABA sequences were significantly slower (mean difference = 84 ms) than those on BAA sequences, $t(39) = 3.28$, $p_{(\text{one-tailed})} = .001$; $d_z = 0.52$, $\text{BF}_{10} = 30.42$. This result confirms that the source of the task-switch cost observed following cue-only trials was the cue-only trials themselves, rather than the completed trials that preceded them.

Test of Hypothesis 3. Hypothesis 3 required the task-switch cost at target responses to be bigger when the preceding trial's task had been performed than when it had only been prepared. The task-switch cost in target-responses was larger following completed trials than following cue-only trials in target-RTs (mean task-switch cost difference [completed – cue-only] = 83 ms), $t(39) = 3.42$, $p_{(\text{one-tailed})} < .001$, $d_z = 0.54$, $\text{BF}_{10} = 43.2$; the task-switch cost difference in target-response errors was numerically positive but not significantly so (mean difference = 0.87%), $t(39) = 0.74$, $p_{(\text{one-tailed})} = .23$, $d_z = 0.12$, $\text{BF}_{10} = 0.34$. Therefore, Hypothesis 3 was supported.

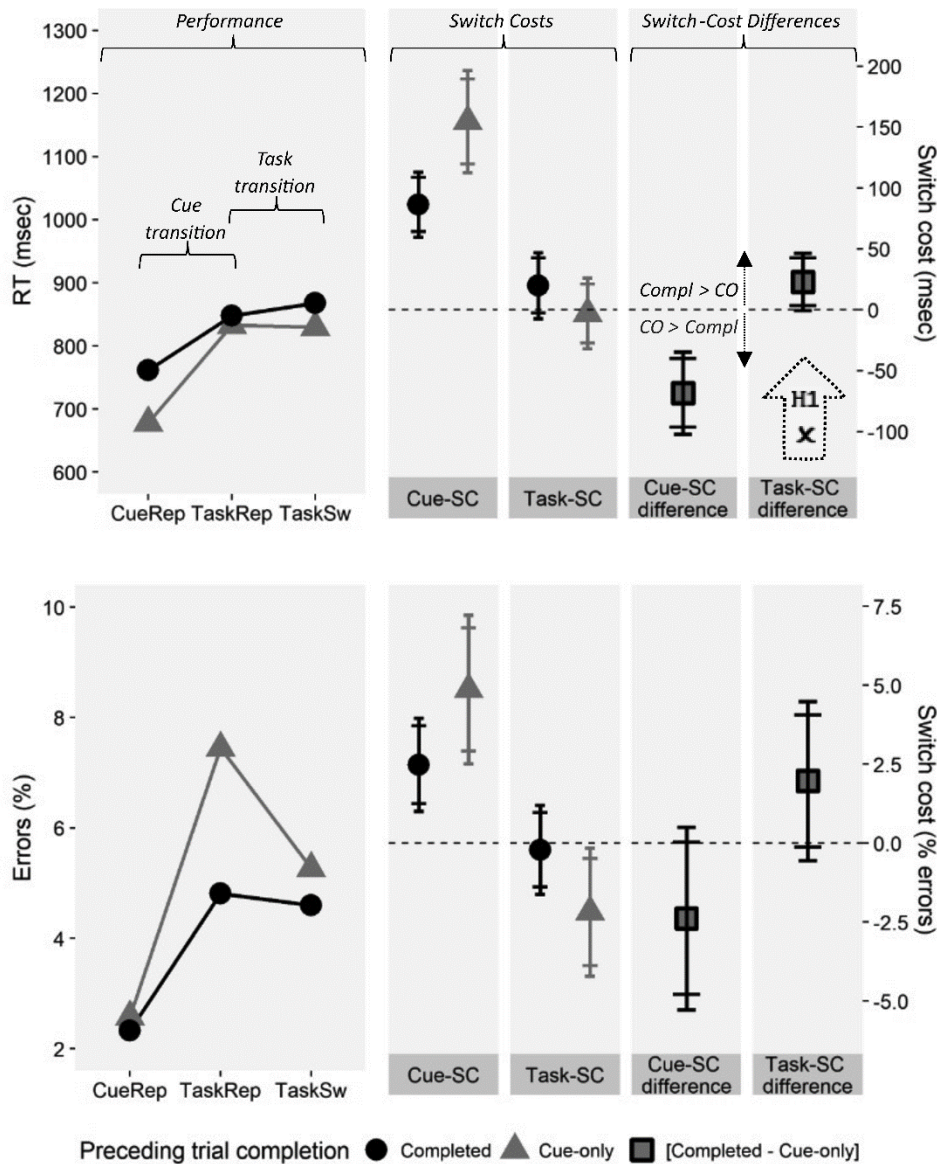
We followed up the test of Hypothesis 3 with an additional investigation (not preregistered) of whether the increased task-switch cost in target-RTs following completed trials had been due to the inclusion of trials on which the previous response repeated, since an increased switch cost on response-repetition trials is one of the feature-integration biases that Schmidt and Liefoghe (2016; see also Schmidt et al., 2020) found contributed to exaggerated switch costs. These data are shown in Figure 5. (Please note that cue-repeat

⁵ The number of trials presented to participants within each of these three-trial sequences had not been controlled by the experimental programme and therefore was not perfectly balanced for individual participants. The number of trials analysed for individuals for A_2BA_1 sequences was on average 18 (range 10 – 28) and for BA_2A_1 sequences was on average 18 (range 8 – 23).

data are shown in Figure 5 for completeness only; they are not included in the analyses reported here, which solely relate to the task-switch cost. Similarly, error data are shown in Figure 5 but are not analysed here since the error data had not significantly supported Hypothesis 3.) One-tailed t tests showed that the task-switch cost on response-repetition trials was not significantly larger than that on response-switch trials (in fact, on average it was 15 ms smaller), $t(39) = -0.49$, $p_{(\text{one-tailed})} = .69$, $d_z = -0.08$, $\text{BF}_{10} = 0.12$, and that the cost on response-switch sequences was larger than that following cue-only trials (mean difference = 92ms, $t(39) = 3.46$, $p_{(\text{one-tailed})} < .001$, $d_z = 0.55$, $\text{BF}_{10} = 47.19$). Thus, the increased task-switch cost at target responses following task performance reported above was not due to inflation of the task-switch cost when responses repeated.

Figure 3

Cue Responses: Performance, Switch Costs and Switch-Cost Differences in RTs and Errors

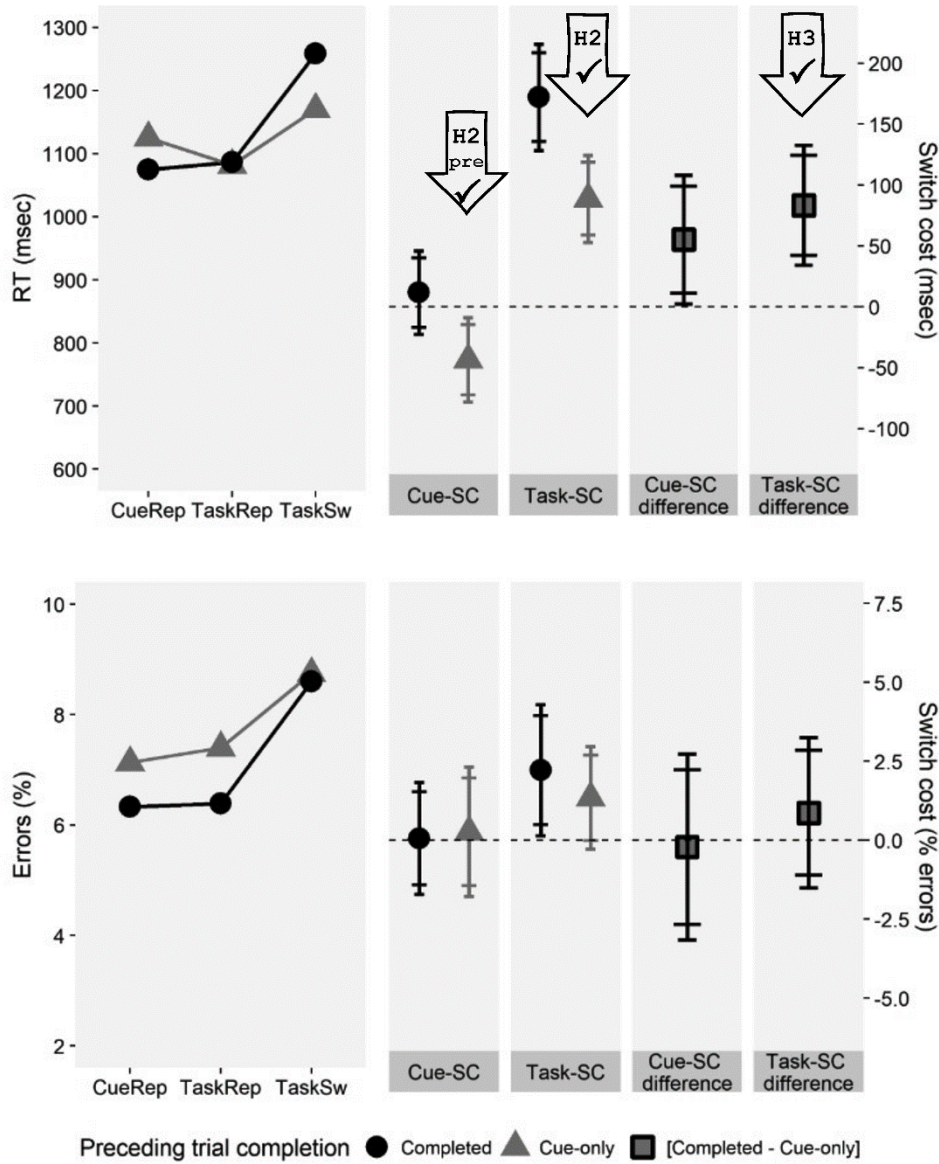


Note. Top row: Left panel shows mean cue-RTs on cue-repeat (CueRep), task-repeat (TaskRep) and task-switch (TaskSw) trials. Other panels show mean cue-switch costs (Cue-SC; [TaskRep – CueRep]) and task-switch costs (Task-SC; [TaskSw – TaskRep]) for cue-RTs, as well as the differences in switch costs between preceding-trial-completion conditions (Cue-SC difference and Task-SC difference, [completed – cue-only]). Bottom row: As top row, but for mean % error scores. Narrow, outer error bars show 95% confidence intervals (CIs); where both limits fall on the same side of the dashed zero line, the two-tailed t test for a switch cost (or benefit), or switch-cost difference, is significant ($p < .05$). Wide, inner error bars show 90% CIs; where the

lower limit is above the dashed zero line, the one-tailed t test for a positive switch cost, or a positive [completed – cue-only] SC difference score, is significant ($p < .05$); the upper limit being below the dashed line would indicate a significant negative [completed – cue-only] SC difference score (as was tested for by Hypothesis 1). Arrow indicates effect relevant for Hypothesis 1, which was not supported (✘).

Figure 4

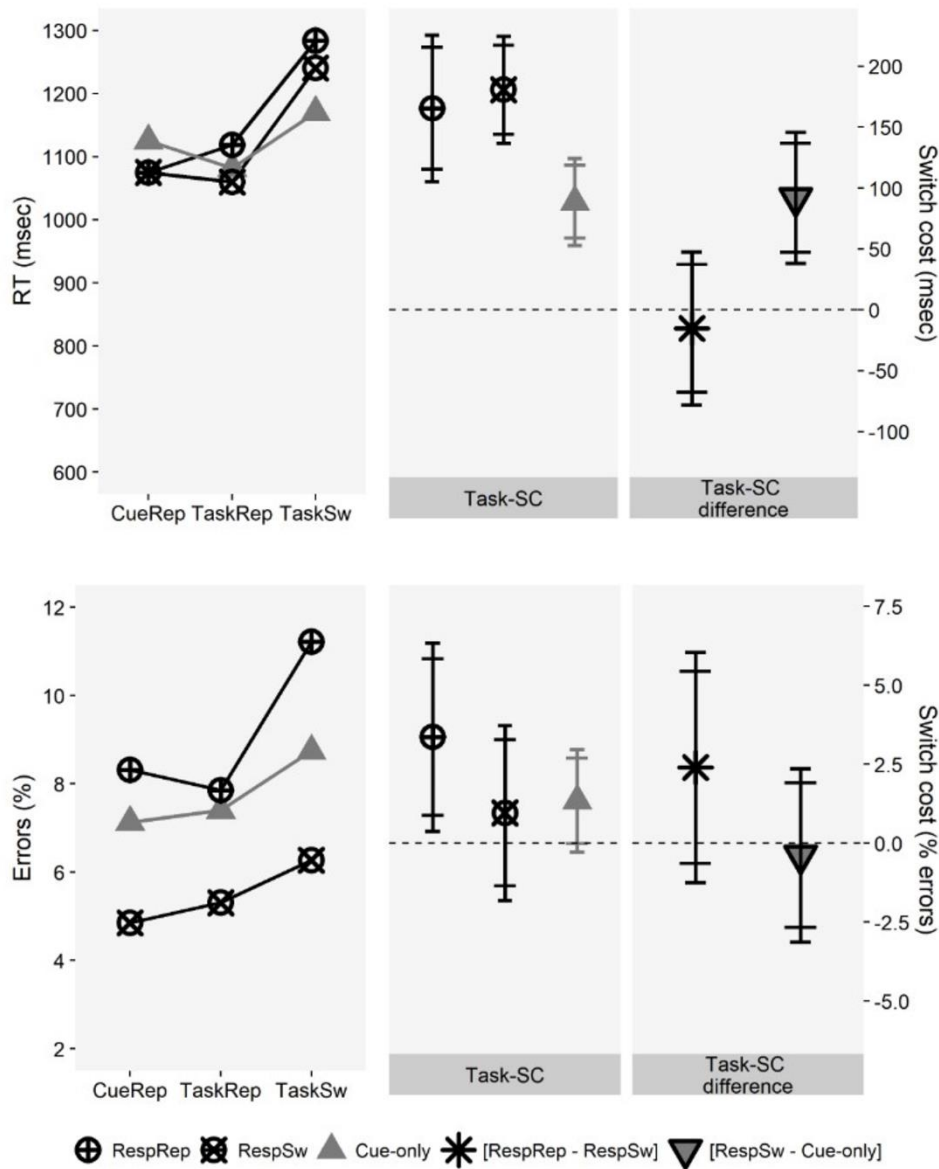
Target Responses: Performance, Switch Costs and Switch-Cost Differences in RTs and Errors



Note. Plotting conventions are as in Figure 3 except that target-RTs and % errors on target responses are shown here. Arrows indicate effects relevant for Hypothesis (or precondition) 2 or 3, which were supported (✓).

Figure 5

Target Responses: Performance, Switch Costs and Switch-Cost Differences in RTs and Errors, Differentiating Response-Repetition from Response-Switch Trials



Note. Plotting conventions are as in Figure 4, except that response-repetition (RespRep) and response-switch (RespSw) trials (where the preceding trial was completed) are shown separately here. Cue-only = trials following cue-only trials. Task-switch cost difference (Task-SC difference) scores show the effect of response repetition versus response switching ($[\text{RespRep} - \text{RespSw}]$) and the effect of performance on the preceding trial while excluding any effect of response repetition ($[\text{RespSw} - \text{Cue-only}]$).

Discussion

Previous research had suggested that response-selection might be necessary to drive a subsequent cost of switching between tasks (e.g., Los & Van den Burg, 2010; Schuch & Koch, 2003; Verbruggen et al., 2006). It has since become clear that task preparation alone is in fact sufficient to establish a subsequent switch cost (Lenartowicz et al., 2011; see also Swainson et al., 2017, 2021), but it was not known whether this cost was of the same nature as that following task performance. Here, we investigated the possibility that preparation and performance on the preceding trial might be differentially responsible for switch costs that impact upon the cue-processing versus the target-processing stages of the current trial. In fact, the results revealed a similar pattern of switch costs, regardless of whether the preceding trial involved only task preparation (cue-only trials) or both task preparation and task performance (completed trials): in both cases, cue-switch costs only occurred in cue responses and task-switch costs only occurred in target responses (although see below regarding caution when interpreting the presence/absence of costs at cue responses relative to those at target responses). In the remainder of this Discussion, we will first recap the main findings from our analyses, then highlight some interpretation-related issues resulting from the experimental design used in this study, and finally discuss the relevance of these results to task-switching models and theory.

Hypothesis 1 tested whether an element of the task-switch cost affecting preparation on the current trial would be larger following cue-only trials (because of their shorter duration) than following completed trials. The results did not support this hypothesis. In fact, we found no significant task-switch costs at cue responses following either type of trial completion (cue-only or completed) in this study. Arrington et al. (2007) had found no significant task-switch cost at cue responses either. We had reasoned that that the strong overlap in meaning between the two cues used for each of our two tasks might speed task

identification on task-repeat relative to task-switch trials, but no such effect was observed in the present experiment.

The pattern of switch costs found in this study – i.e., cue-switch costs in cue responses and task-switch costs in target responses – in fact mirrored what Arrington et al. (2007) had found previously using the double-registration procedure, with all trials being completed in that previous study. Of particular importance, we replicated those authors' finding that task-switch costs occurred in the absence of cue-switch costs in target responses following completed trials, and we also showed that the same type of cost occurred following cue-only trials, this latter result supporting Hypothesis 2. These results demonstrate that switching tasks impacted upon performance of the current task, uncontaminated by any spill-over of costs related to cue-processing, following trials on which only task preparation had taken place as well as following trials that had involved both task preparation and task performance. The additional analysis of target-RTs following cue-only trials showed that the task-switch cost was still present when the task on trial $n - 2$ differed from that on trial $n - 1$, consistent with the task-switch cost following cue-only trials having been driven by preparation on the immediately preceding cue-only trial rather than by performance on the earlier completed trial. Therefore, we conclude that task preparation alone is sufficient to produce subsequent task-switch costs affecting task performance.

Hypothesis 3 tested whether a task-switch cost in target responses would be larger following completed trials than following cue-only trials, and the results supported this hypothesis. An additional analysis, testing whether the increased task-switch cost in target-RTs following completed (versus cue-only) trials might be solely due to a bias caused by inclusion of response-repetition trials in the analysis, suggested that there was no such bias underlying the effect: repetition (versus switching) of the response did not increase the size of the task-switch cost following completed trials, and the task-switch cost following

completed trials was significantly larger than that following cue-only trials even when no response-repetitions were included in the analysis. Hence, performing the prepared task added to the size of the subsequent task-switch cost affecting target responses in a way that could not be attributed to a bias resulting from the repetition of specific stimulus or response features between consecutive trials.

The ANOVA-based analysis generated two unanticipated effects. The first of these was that the cue-switch cost seen in cue-RTs was larger following cue-only trials than following completed trials. Cue-repetition trials involve repeating a cue response to an identical stimulus, whereas task-repeat trials involve repeating a cue response to a different stimulus. The benefit of the identical over the same-response transition was studied by Pashler & Baylis (1991), who found that it decreased greatly when measured across intervening trials (rather than on consecutive trials). In our design, the target stimulus and target response intervened between the preceding and current cue when the preceding trial was a completed trial, but not when it was a cue-only trial. Hence, it seems likely that the decrease in the cue-switch cost (or cue-repetition benefit) at cue responses when the preceding trial was completed rather than cue-only is likely to have been due to the presence of these intervening events.

The second unanticipated effect, possibly related to the first, was a significant cue-switching benefit in target-RTs following cue-only trials that was not present following completed trials. This might have been due to the fact that the immediate repetition of an identical cue stimulus following cue-only trials was associated with especially short RTs for cue responses (see Figure 3; cue-repeat trials following cue-only trials). The short cue-RTs on cue-repeat trials might have meant that there was less time available for task preparation prior to target onset than there was on task-repeat trials, with the consequence that target-RTs were longer on cue-repeat than on task-repeat trials. Alternatively, participants might have

taken additional time to re-engage effort in order to process a target stimulus following the relatively undemanding repeated cue, or simply in order to slow down the overall pace of events.

Before moving on, we wish to point out two issues with respect to our experimental design that might limit the ways in which the resulting data should be interpreted. First, while adding cue responses to the cue-only task-switching method developed by Lenartowicz et al. (2011) has allowed us to investigate the nature of the switch cost following task preparation in a new way, it will necessarily also have changed the nature of what we are regarding as “preparation” to some extent, so that this now includes the selection and execution of a cue response. This change might conceivably alter the impact of preparation on subsequent processing. For instance, one might now consider responding to the cue and responding to the target as two separate types of “task” in themselves, such that processing every target includes the need to switch from the *cue task* and processing every cue will involve either a switch from the *target task* or a repetition (following cue-only trials) of the cue task. Arrington et al. (2007) considered whether this issue might have affected target responses in their study. They accepted that there might be a new element of switching involved with this method, but since their mean target RTs were no longer than in previous single-registration studies, they concluded that there was likely to have been no substantial effect of any such switch from cue to target responses. The target-RTs in the current study are actually substantially longer than we have seen in previous studies (i.e., approx. 1170ms here, compared with approx. 790ms on trials in Expt. 2 of Swainson et al. 2017, and approx. 870 ms in Expts. 1 and 2 in Swainson et al., 2021⁶). These increased RTs might indicate the

⁶ In each case, values are averaged across task-repeat and task-switch trials on completed trials following completed trials, and rounded to the nearest 10ms. The values from previous studies are taken from Swainson et al., 2017 (Table1, Expt. 2, preparation interval = 1000ms) and from Swainson et al., 2021 (Table 1, Expts. 1 & 2, preparation interval = 1000, 2400ms).

additional need to switch from responding to the cue to responding to the target, although the need to remember two additional task-to-cue-response mappings might also have increased RTs overall. A switch between cue and target “tasks” will have occurred on all target responses and any effect of that switch might be additive with the effects of switching between the colour and shape tasks, or conceivably these different types of switch might interact in ways that cannot be determined from these data. The cue-response data, in contrast, will include a mixture of switches and repeats with respect to the preceding response type; it is possible that these transitions might have had effects, and/or interactions with other effects, in ways that we did not predict and have not yet examined in this dataset.

A second issue is that we measured cue-switch and task-switch costs at both cue and target responses in order that we could compare the costs following cue-only trials with the costs following completed trials in each case, not to compare effects occurring at cue responses directly with those at target responses. In fact, the costs seen at cue responses may not be directly comparable with the costs seen at target responses because of a structural dissimilarity in the task design. Thus, cue stimuli could repeat, and they did so on 25% of trials. A cue response could therefore repeat between trials either together with a repetition or with a change in cue stimulus. In contrast, target stimuli could never repeat, and neither could their constituent features (e.g., the colour red). A target response could therefore only repeat between trials together with a change in target stimulus. It is quite feasible that this difference between the contexts in which cue responses and target responses were made could affect the switch costs measured via those responses.

Relevance of these results for task-switching theory

The key characteristics of the task-switch costs that we detected in this study were as follows. First, they affected target responses but not cue responses; second, they existed following task preparation in the absence of task performance (i.e., following cue-only trials;

Hypothesis 2); third, they were increased when task performance as well as task preparation had occurred on the preceding trial (i.e., following completed versus cue-only trials; Hypothesis 3). We will now consider the extent to which this pattern of results is consistent with different theoretical viewpoints.

While a cost entirely driven by preparation-related processes should be present following both cue-only and completed trials, models in which cue-processing on the preceding trial primes cue-processing on the current trial (Altmann & Gray, 2008; Schneider & Logan, 2005) do not seem to explain our finding that the task-switch cost following both cue-only and completed trials affects target responses rather than cue responses. We note that while we did not detect any task-switch cost in cue responses in our current study, we have detected one since in a study where there were no cue-repeat trials (Swainson, Prosser, & Yamaguchi, unpublished data), so it might be that cue-related task-priming is evident under some conditions and not others. Nevertheless, it seems clear that part of the total switch cost must result from processes other than priming of cue interpretation. Interestingly, this result seems to rule out inter-cue priming (Schneider & Logan, 2005) as, at least, the sole source of the remaining switch cost that Schmidt and Liefoghe (2016) found still to be present after excluding other feature-repetition effects, since inter-cue priming should affect the speed of cue responses rather than target responses.

We suggest that two mechanisms from the task-switching literature correspond well to the finding of a task-switch cost in target responses following cue-only (as well as completed) trials. The first of these mechanisms is endogenous reconfiguration. At first glance, this might seem implausible, because one might expect that any cost that could be established during the preparation interval of the preceding trial (i.e., on cue-only trials) should also be able to be overcome during the equivalent interval (prior to target onset) on the current trial. However, it would not have to be the case that such a cost could not be

overcome for a subsequent cost to be seen, only that it was not always overcome – in other words, endogenous reconfiguration would presumably only have to fail to take place prior to target onset (as per de Jong, 2000) on a proportion of completed trials following cue-only trials for a measurable a task-switch cost in target responses to be observed. It is also possible that it is more difficult to use the pre-target interval to reconfigure the task set following a cue-only trial than during the cue-only trial itself, because cue-only trials are relatively rare and therefore participants might not be as ready to switch tasks immediately following a cue-only trial as they would be on the cue-only trial itself (which would always have followed a completed trial; Swainson et al., 2021); thus, it might be that the effects of endogenous preparation that occurred on a cue-only trial could not necessarily be undone by endogenous preparation occurring prior to target onset on the subsequent completed trial.

A second, rather more speculative, mechanism that seems to fit well with the presence of a task-switch cost driven by preparation on the preceding trial and affecting performance on the current trial is that of passive conceptual response priming. This idea was referred to by Schmidt and Liefoghe (2016) as a possible way in which priming related to trial features might yet cause a task-switch cost despite the exclusion of exact feature repetitions. With respect to the current design, the task cue (e.g., COLOUR) presented on the preceding (cue-only or completed) trial would prime the conceptual responses (red, blue, yellow, green) associated with it (/its associated task); if such priming persisted between trials then it might allow speeded selection of the same conceptual responses (in comparison to those of the other task) on the current trial. Hence, in principle this seems to be a way in which priming might allow task preparation to affect subsequent task performance.

Finally, the increase in task-switch costs seen in target responses following completed relative to cue-only trials is consistent with a number of mechanisms, including: task-set priming (Allport & Wylie, 1999); the exogenous reconfiguration component of two-stage

reconfiguration models (Mayr & Kliegl, 2003; Meiran, 2000; Rogers & Monsell, 1995; Rubinstein et al., 2001); and de Jong's (2000) failure-to-engage model, whereby reconfiguration would be more likely to have taken place on preceding completed trials than on preceding cue-only trials. In all these cases, performance of the task on the preceding trial would be expected to have shifted the state of task-set readiness further towards that preceding trial's task more than preparation alone would, consistent with there being a greater subsequent task-switch cost in target-RTs following completed than cue-only trials.

Conclusion

In this double-registration cued task-switching paradigm, the same switch-cost elements were found to be present following trials that only involved task preparation (cue-only trials) as following trials that involved both task preparation and task performance (completed trials). Cue-switch costs affected cue responses (reflecting task identification) and task-switch costs affected target responses (reflecting task performance) following both types of trial. Notably, the task-switch cost affecting task performance was distinct from any cue-processing costs, and was therefore akin to the "true" switch cost of Arrington et al. (2007). The presence of this cost following cue-only trials is consistent with the need for endogenous task-set reconfiguration, especially as according to the failure-to-engage model of de Jong (1999), as well as (more speculatively) with a mechanism of passive conceptual response priming (Schmidt & Liefoghe, 2016) whereby cue-related processing on the preceding trial might prime performance-related processing on the current trial. The task-switch cost in target-responses was larger following completed trials than following cue-only trials, consistent with a number of models including task-set priming, two-stage task-set reconfiguration, and failure to engage. Together, these results suggest that both prior preparation and prior performance contribute to the task-switch cost affecting current task performance.

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