# Reasons to Switch: The Interplay of Effort, Time, and Associative Learning

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**Jonathan Mendl**

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Erstgutachterin: Prof. Dr. Gesine Dreisbach

Zweitgutachter: Prof. Dr. Marco Steinhauser

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# CONTENT

# ABSTRACT

Cognitive control describes the key human ability to override dominant action tendencies to achieve current goals. In everyday life, this often requires flexible adjustments to changing demands and switching between different tasks. Effectively managing these instances of cognitive flexibility is perceived as effortful. Generally, people tend to avoid wasting effort. Therefore, economic approaches based on cost-benefit analyses may provide an explanation for the decision to switch voluntarily. Alternatively, the associative learning account of cognitive flexibility states that learned associations can automatically trigger flexibility. The present dissertation project aimed at investigating the evidence for both accounts to better understand the reasons to switch between tasks. First, we explored the economic role of the individual task-switching ability and introspection about this ability (Study 1). People who (think they) are better at switching, may be more inclined to do so. Second, we took a closer look at the costs of switching by disentangling the effort costs and temporal costs of a task switch (Study 2). People may avoid task switches simply because switching takes time. Last, we thoroughly tested the associative learning account of cognitive flexibility by associating simple task-irrelevant cues with cognitive flexibility (Study 3). The learned cues may automatically trigger the associated flexibility. In line with an economic perspective, the results of Study 1 and Study 2 showed that individuals take their ability and associated temporal costs into account when deciding to switch. Critically, in Study 3, associative learning using task-irrelevant cues did not reduce the general avoidance of task switches. Task-relevant experiences appear to be necessary to give us a valid reason to switch. In sum, the present studies provide a deeper insight into effort-based decision-making and associative learning. The decision to switch is strongly guided by economic cost-benefit analyses. Therefore, this project provides a promising foundation for future research by highlighting the link between cognitive effort and flexibility.

# PART I:

# INTRODUCTION

# Reasons to Switch

When reading an academic thesis (what you are doing right now), it is necessary to strongly focus on the task at hand. You should try to avoid distraction and put the thesis in the center of your attention. Still, sometimes you might *want* to or *need* to switch to a different task. You might want to check your email, grab a quick cup of coffee, or look up a citation that seems relevant to your work. In other cases, you may need to adapt to changes in the environment, for example when taking an important phone call, attending a scheduled meeting, or assisting a colleague in need. This form of flexible behavior requires cognitive effort. This is especially apparent when considering the most effortful and stressful workdays which typically involve juggling between several critical tasks at once. Moreover, scientists often use multitasking deliberately as a tool to induce stress in controlled environments (Becker et al., 2023; Wetherell & Carter, 2014). Given the evident costs of flexibility, it seems rather surprising that we still sometimes voluntarily switch to different tasks. Therefore, the present project aimed at exploring the circumstances that determine the willingness to switch despite the associated effort. In other words, we investigated the reasons to switch.

I begin by providing an overview of the overarching topic of cognitive control and one of its central functions, namely cognitive flexibility. Next, I explain how flexibility and especially the motivation to switch between different tasks can be measured. After introducing the concept of effort, I highlight the link between cognitive flexibility and cognitive effort by describing how effort-based economic decision-making processes may modulate flexibility. Last, I provide an alternative non-economic approach to modulate flexibility, i.e. the associative learning account of cognitive flexibility. The present studies explored the evidence for both approaches by investigating the economic account and the associative learning account of cognitive flexibility.

# Cognitive Control

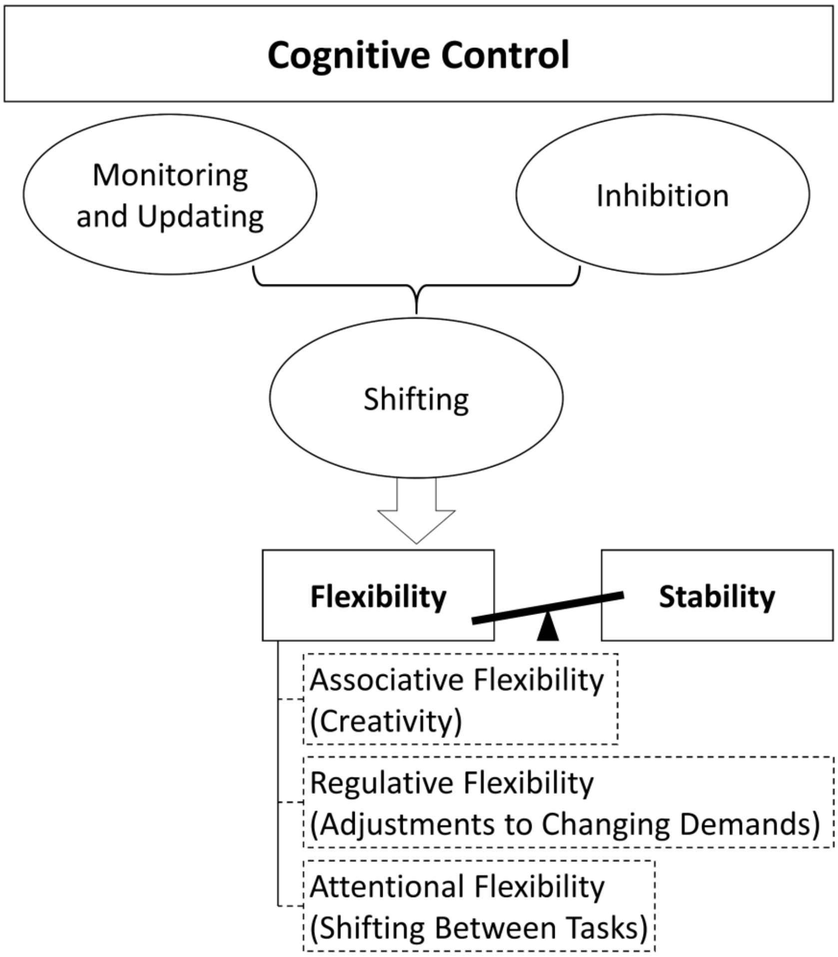
Switching between different tasks relies on the fundamental human ability of cognitive control. This ability is the cornerstone of adaptive behavior. It describes a set of distinct executive functions that enable individuals to regulate and direct their thoughts, actions, and emotions in line with their goals by overriding automatic action tendencies (see Braver, 2012; J. D. Cohen, 2017; Diamond, 2013). Cognitive control involves the ability to maintain and shield information, inhibit automatic or habitual responses, and flexibly adapt to changing circumstances. Executive functions are required for advanced behavioral patterns including self-regulation, decision-making, planning, and complex problem-solving. This allows us, for example, to maintain a healthy diet, resist temptations, solve difficult math problems, focus on writing a paper, and efficiently switch between a phone call and answering emails. Without cognitive control, human behavior would be characterized by impulsivity, automatic reactions, and a diminished ability to regulate actions. Individuals might primarily act on immediate desires without considering long-term consequences and show reduced intentional goal pursuit. Taken together, executive functions are essential for success in many areas of life (Diamond, 2013). Because cognitive control is the foundation of goal-directed behavior, it is crucial to understand its nuanced dynamics.

The concept of cognitive control stems from the distinction between controlled and automatic processing (J. D. Cohen, 2017). This distinction has been a pivotal topic in psychological research in the past 50 years (Posner & Snyder, 1975; Shiffrin & Schneider, 1977). Automatic processes represent fast, reflexive responses initiated without deliberate intent and often rooted in habits or triggered by immediate stimuli. In contrast, controlled processes are slower and involve intentional cognitive processes or planning. The former are characterized by efficiency and speed, while the latter demand cognitive resources (Schneider et al., 1982). However, the strict dichotomy between automatic and controlled processes was largely abandoned (Botvinick & Cohen, 2014). Supposedly automatic processes sometimes appear to require control and, conversely, controlled processes can occur rather automatically (Fabio et al., 2019; MacLeod & Dunbar, 1988). Hence, a dichotomous categorization is not appropriate giving rise to the idea of a continuum of automaticity (MacLeod, 1991). Furthermore, several studies showed that cognitive control can be triggered through lower-level learning processes (Chiu & Egner, 2017; Chiu et al., 2020; Crump & Logan, 2010; Dreisbach, 2006; Logan & Zbrodoff, 1979; Siqi-Liu & Egner, 2020; Surrey et al., 2017). For example, learned contexts can automatically influence control (for reviews, see Bugg, 2017; Bugg & Crump, 2012). These findings are the foundation of the associative learning account of cognitive control (Abrahamse et al., 2016). This account states that cognitive control is based on simple associative learning processes in contrast to the early dichotomy of automatic and controlled processing. Hence, other classifications of cognitive control were required.

To get a better understanding of the nuances of cognitive control, Miyake et al. (2000) categorized the concept into three distinct functions: monitoring and updating, inhibition, and shifting (see Figure 1). The monitoring and updating function centers around the active monitoring and modification of working memory contents (Lehto, 1996), for example when solving long math problems like 5 + 14 - 11 + 34. To solve the problem, you constantly have to update the result of the previous operation and maintain it in working memory. This function plays a key role in the maintenance of goal-relevant information. Inhibition, on the other hand, refers to the suppression of prepotent responses, showcasing the ability to resist interferences (Logan, 1994). For example, inhibition is required when resisting temptations to stay on a healthy diet. Lastly, the shifting function involves the ability to flexibly transition between mental sets or tasks (Monsell, 1996), for example when switching between a phone call and answering emails. This function demonstrates the adaptability of behavior in response to changing demands. Together, the three functions display the unity and diversity of cognitive control because they refer to separate processes that are also related to one another (Lehto et al., 2003; Miyake et al., 2000). The three-dimensional structure is widely used to study cognitive control (Diamond, 2013). The model was also modified by including unity as a common executive function factor that influences all three functions (Friedman & Miyake, 2017). Although the three-dimensional structure was not always confirmed in recent studies (Karr et al., 2018; Sambol et al., 2023), the framework still offers a comprehensive overview of the nuanced functions of cognitive control.

**Figure 1**

*Schematic Depiction of the Structure of Cognitive Control and Flexibility*




*Note*. Cognitive control can be categorized into three functions: monitoring and updating, inhibition, and shifting (Miyake et al., 2000). Shifting depends on the other two functions and represents the foundation of cognitive flexibility (Diamond, 2013). The antagonistic control modes of flexibility and stability form a balance (Dreisbach & Fröber, 2019; Goschke, 2013; Hommel, 2015; Musslick & Cohen, 2021). Flexibility can be divided into associative, regulative, and attentional flexibility (Sacharin, 2009). Attentional flexibility refers to the shifting between different tasks which was the central topic of the present project.

Of particular interest is the shifting function which depends on the effective interplay of inhibition and updating (Diamond, 2013). The shifting function raised a fundamental question about the dynamics of cognitive control. How can the cognitive system navigate the balance between staying focused on one task while inhibiting distractions on the one side, and updating information to shift to new tasks on the other side? In other words, how can we manage the balance between cognitive stability and flexibility? With this, we arrive at the first main topic of the present project, namely cognitive flexibility with its counterpart cognitive stability.

# The Balance of Flexibility and Stability

The necessary antagonistic nature of cognitive control modes has been the subject of several theoretical frameworks (Braver, 2012; Dreisbach & Fröber, 2019; Egner, 2014; Goschke, 2013; Hommel, 2015). The concept of control dilemmas by Goschke (2013) describes dichotomous challenges that cognitive control has to resolve. The dilemmas arise because adaptive behavior requires a balance between different control functions. The shielding-shifting dilemma describes the tension between the need to shield goals from competing responses and the need to flexibly shift goals in changing environments. The selection-monitoring dilemma highlights the challenge of focusing attention on goal-relevant information without completely suppressing task-irrelevant, but potentially important information. The anticipation-discounting dilemma refers to the balance between pursuing long-term goals and satisfying immediate needs. The exploitation-exploration dilemma involves a trade-off between selecting known rewarding actions and exploring new possibilities. Finally, the plasticity-stability dilemma explores balancing the updating of knowledge with the benefits of stable habits. In the face of these challenges, cognitive control must find a dynamic balance between conflicting demands (Goschke, 2013; Goschke & Bolte, 2014). At their core, many of these dilemmas describe a conflict between the concepts of stability and flexibility which was described in detail by Hommel (2015).

In the meta-control state model, Hommel (2015) defines cognitive control as a balance between stability and flexibility (see also Dreisbach & Fröber, 2019; Musslick & Cohen, 2021; see Figure 1). Both control modes bring potential advantages but also disadvantages. Stability allows focusing on goal-relevant information and filtering irrelevant information. This is particularly important when concentrating on a single task to shield task-relevant information. However, this persistence can lead to rigid behavior, meaning that alternative options are not considered even when they are potentially meaningful. This rigidity poses the danger of overlooking useful information. Conversely, adopting a flexible control mode allows switching between different goals and tasks. This is necessary to adapt behavior to the constantly changing demands of the environment. However, excessively high flexibility can be associated with increased distractibility and interferences regarding the cross-talk between different task representations. These complementary advantages and disadvantages show that there is not *one* optimal control mode (Dreisbach & Fröber, 2019). A dynamic balance between flexibility and stability is required to show appropriate behavior in distinct situations. Understanding this balance has been the goal of countless studies and is a key topic in cognitive control research.

Various influences on this balance between flexibility and stability have been summarized in extensive reviews (Dreisbach & Fröber, 2019; Hommel, 2015). As long-term factors, learning processes such as extended practice with video games (Cardoso-Leite et al., 2016; Colzato et al., 2010; Ryu et al., 2021; Strobach et al., 2012) and bilingualism (Bialystok & Craik, 2010; Ibrahim et al., 2013) can facilitate cognitive flexibility. In addition, several studies uncovered short-term effects that influence the moment-to-moment balance between flexibility and stability. One prominent short-term factor is positive affect which has been shown to shift the balance towards flexibility (e.g., Ashby et al., 1999; Dreisbach, 2006; Dreisbach & Goschke, 2004; Fröber & Dreisbach, 2012; van Wouwe et al., 2011). Reward when given as a gift (non-performance-contingent) has a similar effect on cognitive flexibility (Fröber & Dreisbach, 2014, 2016a; van Steenbergen et al., 2009). Conversely, unchanged performance-contingent reward typically leads to increased stability and reduced flexibility (Chiew & Braver, 2014; Fröber & Dreisbach, 2014, 2016a; Hefer & Dreisbach, 2016, 2017; Jurczyk et al., 2021). When using changing reward magnitudes, an increase in performance-contingent reward can also promote cognitive flexibility (Fröber & Dreisbach, 2016b, 2021; Fröber, Jurczyk, Mendl, & Dreisbach, 2021; Fröber et al., 2019; Fröber et al., 2020; Jurczyk et al., 2019; Shen & Chun, 2011). Last, according to the learning perspective of cognitive flexibility, lower-level learning processes can directly trigger associated control modes (for a review, see Braem & Egner, 2018).

In sum, this overview of potential long- and short-term influences on the flexibility-stability balance highlights the versatile nature of cognitive control. While the optimal balance depends on the situation, especially cognitive flexibility plays a critical role in overcoming the complex demands of modern life. Flexibility is essential for exploring innovative solutions, adjusting behavior to unexpected changes, and efficiently switching between different tasks. Consequently, extensive research has been conducted to elucidate the underlying mechanisms of this central ability. When studying the concept of cognitive flexibility, the first step is to select an effective method for measuring flexibility.

# Measures of Cognitive Flexibility

There are several different paradigms to measure flexibility. However, they often tap into distinct aspects of cognitive flexibility (Diamond, 2013; Dreisbach & Fröber, 2019; Hommel, 2015). Sacharin (2009) distinguished three components of flexibility: associative flexibility (creativity), regulative flexibility (adjustments to changing demands), and attentional flexibility (shifting between different tasks; see Figure 1).

Creativity describes a rather broad form of cognitive flexibility. It refers to the ability to generate novel ideas or solutions. To measure creativity multiple tasks can be used such as the verbal fluency tasks (Baldo et al., 2001), the alternate use task (Guilford, 1967), and the remote associates test (Mednick, 1968). In all these tasks, participants have to generate as many items to a given letter or category. Producing a larger number of items indicates a higher level of creativity.

The second component of flexibility, regulative flexibility, refers to adjustments to changing cognitive demands (Sacharin, 2009). There are multiple ways to measure this ability. The Wisconsin Card Sorting Test requires participants to sort cards according to changing criteria, thereby assessing the ability to adapt to changing sorting rules (Berg, 1948). Additionally, this ability can be investigated using conflict tasks like the Stroop task (Stroop, 1935), the Erikson Flanker task (Eriksen & Eriksen, 1974), and the Simon task (Simon & Wolf, 1963). In all these tasks, the congruency effect indicates the interference by the task-irrelevant feature. Adjustments to changing demands can be measured in the form of congruency sequence effects (how individuals adjust control in response to conflicts; Gratton et al., 1992), item-specific proportion congruency effects (how individuals adjust control in response to the frequency of conflict within certain stimuli; Jacoby et al., 2003), and context-specific proportion congruency effects (how individuals adjust control in response to the frequency of conflict within a certain context; Logan & Zbrodoff, 1979). Furthermore, cueing paradigms like the Posner cueing task (Posner, 1980) or the AX continuous performance task (Servan-Schreiber et al., 1996) measure flexible adaptation to rare events.

At its most basic level, cognitive flexibility refers to the shifting of attention between different features or tasks (Sacharin, 2009). This form of flexibility, demands the shifting of cognitive resources from one simple task to another which can be measured in the task-switching paradigm (Jersild, 1927). The task-switching paradigm with its various versions and adjustable parameters provides an invaluable tool for investigating and understanding the fundamental mechanisms underlying cognitive flexibility. For this reason, the studies of the present project used a variant of this paradigm to measure the reasons to switch.

## Standard Task Switching

There are a number of comprehensive reviews about the task-switching paradigm (Kiesel et al., 2010; Koch & Kiesel, 2022; Monsell, 2003; Vandierendonck et al., 2010). In all versions of this paradigm, participants have to perform two (or more) simple tasks. A task (or task set) can be defined as a set of rules, e.g., responding to odd numbers with the left key and to even numbers with the right key. Participants have to use these rules to react to the presented target stimulus by categorizing the stimulus and selecting the correct response (Kiesel et al., 2010). As a first approach to investigate task switching, two different kinds of blocks were used. In mixed blocks, the tasks switched on every trial.

In single-task blocks, only one task was presented (Allport et al., 1994; Jersild, 1927). Poorer performance in mixed blocks compared to single-task blocks was taken as evidence for a switching process required in the former. However, this approach confounds switch-specific costs with mixing costs. Mixing costs refer to the global costs associated with blocks with more than one task (even if the task is repeated within this list) compared to single-task blocks possibly due to overall increased working memory demand in mixed blocks (Koch et al., 2005; Rogers & Monsell, 1995). Due to this confound, most researchers prefer one of the other paradigms to investigate task switching (for an overview of different task-switching paradigms, see Table 1).

**Table 1**

*Overview of the Different Types of Task-Switching Paradigms*

| Choice type | Paradigm | Main characteristics | Main dependent measure |
|---|---|---|---|
| Forced choice | Block procedure | Mixed blocks with alternating tasks and single-task blocks | Switch cost = Ability to switch; Mixing cost |
| | Alternating-runs | Task alternating every *n* trials resulting in predictable task switches and repetitions | Switch cost |
| | Cued task switching | Switches and repetitions of cued tasks in random order | Switch cost |
| | Intermittent instruction | Cuing a task repetition or switch after a sequence of repetitions | Switch cost; Restart cost |
| Free choice | 100 % voluntary task switching | Voluntary choice between tasks often restricted by a randomness instruction | Voluntary Switch Rate (VSR) = Motivation to switch |
| | Double-registration procedure | Separate task choice and task execution | |
| | Task switching with preview | Only replacing stimulus of executed task (preview for potential switch) | |
| | Self-organized task switching | Voluntary choice between tasks biased by stimulus availability | |
| Mixture of free and forced choice | Hybrid task switching | Unrestricted voluntary choice between tasks intermixed with forced-choice trials | |

*Note.* Forced choice refers to paradigms in which the participant has to respond to a predetermined task. Free choice means that the participant can decide which task to perform.

In all other paradigms, the relevant task can either repeat or switch from one trial to the next (within the same block). To measure the ability to switch between tasks, the performance on task switches is compared to the performance on task repetitions. As a critical and robust finding, performance is typically worse on task switches (Koch & Kiesel, 2022). This performance difference between task switches and repetitions is called switch cost. Higher switch costs suggest lower cognitive flexibility. Depending on the type of task-switching paradigm used, different parameters can be modified to investigate distinct underlying mechanisms.

One early option is the alternating-runs paradigm (Rogers & Monsell, 1995). In this paradigm, the task (Task A or Task B) alternates after every *n* trials, e.g., after every two trials (A-A-B-B-A-A-B-B). Typically, participants get some visual guide to keep track of the current task. For example, the stimulus is presented in one of four quadrants of a circle in a clockwise order. If the stimulus is presented in one of the two left quadrants, task A has to be executed. If the stimulus is presented in one of the right quadrants, task B has to be executed (Monsell, 2003; Vandierendonck et al., 2010). In this method, the task order is fully predictable. This makes it difficult to study task preparation processes because researchers cannot control whether or when the participants start to prepare for the upcoming task. Hence, disentangling task preparation and decay processes is not possible with the alternating-runs paradigm.

In contrast, in the cued task-switching paradigm (Meiran, 1996), the task of the current trial is announced by a task cue which is presented prior to, or together with the stimulus. For example, the cue "odd/even" announces that a parity categorization task should be applied to the number stimulus, and "smaller/larger" (than 5) announces a magnitude categorization task (Kiesel et al., 2007). The advantage of this procedure is that the tasks can be presented in a random and unpredictable order. Therefore, task preparation processes can only be initiated after the presentation of the task cue. This makes it possible to examine preparation processes by varying the intervals between the task cue and the target stimulus (Meiran, 2000). A longer cue-stimulus interval typically results in reduced switch costs suggesting that some degree of task preparation (task-set reconfiguration; see Rogers & Monsell, 1995) is required when switching tasks. This preparation seems to be mostly task-specific, i.e., if only the transition (a task switch) can be prepared without knowledge of the specific task, the results showed only little influence of preparation time on the switch costs (Dreisbach et al., 2002; Koch, 2008). Additionally, studies investigating the decay of task set activation (task set inertia) indicated that a task switch also requires inhibition of the previous task set (Allport et al., 1994; Mayr & Keele, 2000). Taken together, the switch costs may reflect an activation advantage of task repetitions over task switches because task switches require activation of the to-be-executed task and inhibition of the previously executed task set (Allport & Wylie, 2000). These mechanisms fit nicely into the framework of the three cognitive control functions where the shifting function requires monitoring, updating, and inhibition (Diamond, 2013).

For completeness, there is also the intermittent instruction paradigm as a subtype of the cued task-switching paradigm (Monsell, 2003). Here, a sequence of trials of one task is occasionally interrupted by instructions to switch or repeat the task until the next instruction is given (Gopher et al., 2000). As an interesting finding apart from the switch costs, there are general restart costs: responses are slower on repetitions after an interrupting instruction compared to repetitions in any other position of the sequence.

In task-switching paradigms, the stimuli can either be bivalent or univalent. With bivalent stimuli, both task sets can be applied to a target stimulus (for example using a parity and a magnitude task for number stimuli). As a result, strong interference effects can emerge because one stimulus elicits either the same or a different response for the two tasks (Koch & Kiesel, 2022). Univalent stimuli, on the other hand, only relate to a single task (for example using a magnitude task for number stimuli and a letter categorization task as closer to A or Z in the alphabet for letter stimuli). In addition to reducing between-tasks interference, univalent stimuli have the advantage of not requiring task cues because the stimulus itself indicates the required task (e.g., Fröber & Dreisbach, 2016b).

Taken together, these standard versions of the task-switching paradigm offer versatile tools to investigate the switch costs as a measure of cognitive flexibility. Hence, the switch costs capture the shifting function of cognitive control. More precisely, lower switch costs indicate a better *ability* to flexibly switch between different tasks. Extending beyond this ability to switch tasks, cognitive control research has also explored the *motivation* to switch tasks (Dreisbach & Mendl, 2024). What drives people to switch between tasks? If we think about everyday life, in most situations, we can freely choose which task to engage in at a given moment and when to switch to another task. The voluntary task-switching paradigm (Arrington & Logan, 2004) serves as a direct tool to assess this voluntary choice behavior. By using this paradigm, we can investigate under what circumstances people decide to allocate cognitive flexibility in order to switch tasks (Dreisbach & Mendl, 2024). Exploring the motivation to switch taps into a crucial aspect of cognitive flexibility and illustrates the interplay of task-switching behavior and decision-making.

## Voluntary Task Switching

In the voluntary task-switching paradigm, participants can decide for themselves which task to perform on a given trial (Arrington & Logan, 2004; for a review, see Arrington et al., 2014; see Table 1). This freedom of choice is typically achieved by mapping the two tasks to separate hands and keys (e.g., using the left hand with the keys “d” and “f” for the magnitude task, and the right hand with the keys “j” and “k” for the parity task; Arrington & Logan, 2004). Therefore, the participant's task choice can be derived from the response hand. When inferring task choice, error trials are challenging because it is uncertain whether the confusion occurred between fingers (indicating the same task) or between hands (indicating different tasks). Since confusing response hands appeared to be less likely than confusing response keys (Arrington & Logan, 2004; Scheffers & Coles, 2000; Vandamme et al., 2010), task choice on error trials can still be coded according to the chosen hand. This way, all deliberate choices can be considered (Fröber & Dreisbach, 2016b).

Task selection can take place in different ways. To allow a choice, the stimulus must either be bivalent or two univalent stimuli must be presented simultaneously. Alternatively, in the double registration procedure, participants first have to indicate their task choice with one hand and execute the task with the other hand. This procedure is useful to disentangle task selection and task execution processes (Arrington & Logan, 2005; Fröber et al., 2019).

It is still possible to measure reliable switch costs in voluntary paradigms. Voluntary task switches lead to worse performance compared to voluntary repetitions. However, the switch costs are typically reduced compared to standard (cued) task switching (Arrington & Logan, 2005; Gollan et al., 2014). This may indicate that self-guided task preparation can facilitate performance on voluntary task switches. In addition to performance, the main purpose of the voluntary task-switching paradigm is to study choice processes (Arrington et al., 2014). First, by looking at task choice, the task bias in favor of one of the two tasks can be assessed. This can provide insight into decision processes. Second, the voluntary paradigm makes it possible to measure the frequency of the different task transitions (task repetition, task switch). Based on this frequency, the voluntary switch rate (VSR, or inversely, the repetition rate) can be calculated. The VSR indicates how often participants voluntarily switch between tasks, reflecting their motivation to switch (Dreisbach & Mendl, 2024).

Without any restriction, participants only rarely switch tasks resulting in a repetition bias (Kessler et al., 2009). Extremely low VSRs can make it difficult to study modulations of the VSR. Therefore, many studies use some kind of randomness instruction, asking participants to do both tasks about equally often but in a random order, sometimes adding that the choices should simulate the results of random coin tosses. These randomness instructions successfully reduce the repetition bias leading to an increased VSR. However, the resulting VSR is still considerably lower than true random choices (50 %) would produce (Arrington et al., 2014). Interestingly, when participants have to produce random sequences of two letters without task execution, they show a bias towards more switches (Arrington & Logan, 2004). This suggests that the repetition bias is tied to task performance and cannot be attributed to the participants' general inability to produce random sequences.

Apart from the randomness instruction, different variations of the voluntary task-switching paradigm produce reasonable VSRs without restricting participants' choices with global instructions. In the self-organized task-switching paradigm (Mittelstädt, Miller, & Kiesel, 2018) the motivation to switch is increased by manipulating the stimulus availability of the two tasks. More precisely, the stimulus of a task repetition is presented with a stimulus onset asynchrony (SOA) that increases with every consecutive repetition. Therefore, the stimulus of a task switch is available first leading to larger VSRs with increasing SOA. Interestingly, participants tend to switch tasks when the current SOA approximately matches the switch costs which may indicate a trade-off process to maximize performance (Mittelstädt, Miller, & Kiesel, 2018, 2019; Mittelstädt et al., 2021; Monno et al., 2021). This paradigm has been applied to uncover various effects on voluntary task choice, such as effects of perceptual processing demands (Mittelstädt et al., 2022) and effector-specific task mappings (Mittelstädt et al., 2023). Similarly, the task switching with preview paradigm (Reissland & Manzey, 2016) investigates choice behavior by only replacing the chosen stimulus thereby providing a preview for the potential switch stimulus between trials (Brüning et al., 2021).

Alternatively, the hybrid task-switching paradigm (Fröber & Dreisbach, 2016b) provides a tool to measure unrestricted voluntary switching (without manipulating stimulus availability). In this paradigm, voluntary (free-choice) trials are intermixed with single-task (forced-choice) trials. This procedure leads to larger VSRs within the free-choice trials. More specifically, it has been shown that a larger ratio of forced choices and a larger switch rate within these forced-choice trials resulted in increased VSRs (Fröber & Dreisbach, 2017). Dreisbach and Fröber (2019) argued that frequent forced choices and especially forced switches motivate participants to simultaneously maintain both tasks active in working memory instead of generally lowering the updating threshold in working memory. This claim is supported by findings that the switching-induced flexibility does not transfer to new tasks (Fröber, Jurczyk, & Dreisbach, 2021). In sum, the hybrid task-switching paradigm is a useful instrument to measure voluntary task choice without imposing a randomness instruction and without manipulating stimulus availability. In previous studies, this paradigm was employed to investigate numerous influences on voluntary behavior, for example, effects of changing reward magnitudes (Fröber & Dreisbach, 2016b, 2021; Fröber, Jurczyk, Mendl, & Dreisbach, 2021; Fröber et al., 2019; Fröber et al., 2020; Fröber et al., 2018; Jurczyk et al., 2019) and effects of the context (Fröber & Dreisbach, 2017; Fröber, Jurczyk, & Dreisbach, 2021). Notably, a critical advantage of this paradigm lies in the ability to separately measure the VSR on voluntary trials and the performance (including switch cost) on forced-choice trials. When using 100 % voluntary paradigms, the trial numbers of the design cells in the performance analysis (typically RT and error rate) depend on the participants' choice behavior which can lead to imprecise data and confounding the VSR with task switch training (see Introduction of Study 1).

Taken together, voluntary task-switching paradigms directly measure the motivation to switch (Dreisbach & Mendl, 2024). They capture cognitive flexibility as volitional switching between tasks. However, individuals generally appear to avoid this voluntary switching, as indicated by the repetition bias. This bias may reflect the human tendency to avoid the waste of effort (Hull, 1943). Here, we consider voluntary task-switching behavior in the context of effort avoidance and cost-benefit analyses. Therefore, the present project connected two largely separate areas of research, i.e. cognitive flexibility and cognitive effort. It is crucial to take cognitive effort into account when investigating cognitive flexibility. Therefore, I will first elaborate on theories about the concept of effort and later highlight the link between flexibility and effort.

# Cognitive Effort

### The Concept of Effort

Everyone is familiar with the feeling of effort. It typically feels unpleasant, difficult, and demanding (Kool & Botvinick, 2014; Kurzban, 2016; Westbrook & Braver, 2015). When considering effort, one may initially only think of physical activities such as engaging in sports (like weightlifting, running, or power yoga), doing manual labor, or climbing a mountain. However, effort is not limited to physical domains but also frequently arises in mental activities. Trying to find the best move in a chess game, solving complex math problems, programming new experiments, and developing a comprehensive structure for a dissertation thesis can all describe situations that feel effortful on a cognitive level.

Effort can be defined as the intensification of mental or physical activity (Eisenberger, 1992; Inzlicht et al., 2018). More specifically, effort refers to the factor standing between the objective task characteristics and the individual ability on the one side, and the resulting performance on the other side (Shenhav et al., 2017). The maximum achievable performance is based on the first two factors (task characteristics and ability). The actual performance depends on the invested effort. The task characteristics of a physical task may for example include the weight of an object to be lifted, the length of a running route, or the incline when climbing. In this context, the ability factor refers to the individual's strength, stamina, or climbing experience. Similarly, regarding cognitive tasks, the characteristics may include the time constraints in a chess game, the number of processing steps in a math problem, or the complexity of the experiment to be programmed. Here, the ability factor relates to chess expertise, proficiency in numerical cognition, and programming skills, respectively. In all these examples, the invested effort ultimately determines how well the tasks are completed. Several theories have been developed around the phenomenon of effort to better understand effort-based decision-making (Kool & Botvinick, 2018; Kool et al., 2017; Kurzban et al., 2013; Richter et al., 2016; Shenhav et al., 2013; Westbrook & Braver, 2015). Here, I will focus on the Motivational Intensity Theory and the Expected Value of Control model as two influential approaches on this topic. Both approaches aim to explain effort investment. The Motivational Intensity Theory describes the mobilization of effort in a given task as a function of task difficulty and success importance whereas the Expected Value of Control model focuses on the economic decision process to allocate cognitive control (Silvestrini et al., 2023).

The Motivational Intensity Theory (Brehm & Self, 1989) states that invested effort is based on a general resource conservation principle. Effort requires some form of resource that should not be wasted. Therefore, effort engagement needs to be sufficiently justified. Following this principle, the Motivational Intensity Theory makes specific predictions of how the effort in different types of task situations depends on task difficulty and/or success importance (for a review, see Richter et al., 2016). Success importance is defined as the extent to which the benefits associated with success outweigh the costs (so how positive success will be for you). For tasks with dichotomous outcomes and known difficulty, effort should depend directly on the task difficulty while success importance sets the potential maximum of effort. Increased task difficulty should lead to increased effort engagement until a certain threshold (determined by success importance) is reached. Beyond this threshold, no effort is invested because it does not seem worthwhile. Similarly, when the task appears impossible, no resources should be wasted. When the difficulty is not known, the sole relevant indicator for effort engagement is success importance. In this scenario, individuals may invest more effort than necessary, but not more than is justified by success importance. Last, when a wide range of outcomes are possible and individuals can choose the desired task difficulty themselves (e.g., by choosing how much to study for an exam with multiple possible passing grades), success importance directly predicts the appropriate task difficulty which in turn predicts invested effort (Richter et al., 2016). The predictions of this theory are in line with some related findings in cardiovascular (Richter et al., 2008; Richter & Gendolla, 2009; Wright et al., 1990) and hand-grip studies (Richter, 2015; Stanek & Richter, 2021). Other studies investigated additional factors such as ability, fatigue, and affect to explain effort investment extending the theory (Gendolla & Krüsken, 2001; Wright, 2014). Taken together, the Motivational Intensity Theory can explain effort engagement in certain tasks primarily based on task difficulty and success importance. In contrast, the Expected Value of Control model focuses particularly on the decision to allocate control in an economic decision-making framework based on cost-benefit analyses. Therefore, the Expected Value of Control model is especially relevant for the present project to explain the decision to switch.

The Expected Value of Control (EVC) model (Shenhav et al., 2013; Shenhav et al., 2021; Shenhav et al., 2017) proposes that the investment of cognitive effort depends on the associated payoff and the associated effort cost. Individuals choose the appropriate amount of cognitive control to maximize the expected value of control which reflects the trade-off between the utility and the cost. This idea is based on a general principle of reinforcement learning that actions are selected to maximize rewards (Sutton & Barto, 2018). In the EVC model, the costs refer to the effort of cognitive control. A stronger intensity of control exponentially incurs a larger amount of effort costs. At the same time, investing more control allows you to achieve a higher level of performance (up to a threshold, the maximum possible performance). The performance level, in turn, is related to a certain expected outcome defining the payoff of control, a function of the probability of success and the value of the outcome (Shenhav et al., 2013). The difference between the payoff and the effort cost constitutes the expected value of control. The maximum of this difference function indicates the optimal amount of control that individuals should apply (Shenhav et al., 2017). The expected value of control is additionally influenced by the three factors of control efficacy, performance efficacy, and outcomes. Control efficacy relates to how effectively increased control is converted into increased performance which depends on the individual's skills. Performance efficacy describes the relationship between better performance and the potential outcome. A reduced contingency between performance and outcome results in a reduced allocation of cognitive effort. Last, a generally larger potential outcome related to the reward magnitude justifies increased effort investment (Shenhav et al., 2021). Taken together, the EVC model offers a nuanced framework for the willingness to invest cognitive effort. The mechanisms proposed by the model are consistent with several studies that have applied value-based decision-making to cognitive control research.

## Applications of Cognitive Effort in Decision-Making Research

Kool et al. (2010) directly investigated the avoidance of cognitive effort costs. They introduced the demand selection task where participants had to repeatedly choose between two options (demand decks). After the choice, the stimulus was presented. Critically, the two options differed with respect to the required amount of cognitive control. The high-demand deck contained a higher frequency of task switches compared to the low-demand deck with mostly task repetitions. The results showed that participants overall avoided the high-demand deck (more frequent task switching) even after controlling for awareness of deck differences, error rates, and strategies to minimize the experiment duration. The same avoidance of effort was found when using different types of cognitive demand such as an AX continuous performance task and different types of math problems (Kool et al., 2010). Multiple studies replicated this general principle of cognitive effort avoidance (Dunn et al., 2019; Dunn & Risko, 2019; Schouppe et al., 2014). Next to the effort costs, other studies have also considered the associated payoff.

According to the EVC model cognitive effort costs and potential rewards are integrated to select the optimal amount of control. This integration was shown in the cognitive effort discounting paradigm by Westbrook et al. (2013). In their study, the participants first experienced different cognitive demand levels of the *n*-back task (Kirchner, 1958). In this task, participants have to respond when the current letter in a sequence of letters matches the letter from exactly *n* steps before. A higher *n* (e.g. *n* = 4 compared to *n* = 1) is associated with higher cognitive demand because more letters have to be maintained in working memory and constantly updated. After experiencing the different demand levels of this task, the participants had to make a number of sequential choices between the easy 1-back and the more difficult *n*-back levels. The more difficult levels were associated with a larger monetary reward compared to the easy level. The participants were instructed that one of their choices (a certain *n*-back level associated with a certain reward) would have to be completed afterward. Critically, the reward for the easy task was successively adjusted in a stair-wise fashion depending on the previous choice to reach a point of indifference between the two options. The results showed that participants systematically devalued potential rewards as the cognitive demand increased. In other words, they willingly gave up a substantial amount of reward in order to avoid higher cognitive demands (Westbrook et al., 2013; Westbrook et al., 2019). Thus, in line with the EVC model, this finding suggested that the cognitive effort cost and the potential rewards are integrated when deciding whether to invest cognitive control.

## Effort and Flexibility

Cognitive effort is aversive and the waste thereof tends to be avoided. At the same time, effort is crucial for successful task performance. Therefore, individuals should consider both the required effort and the potential payoff in order to maximize the expected value of control (Shenhav et al., 2021). Applying these ideas to voluntary task-switching behavior, individuals should consider the cognitive effort costs of switching (indicated by the switch costs) and the potential reward when deciding whether to repeat or switch tasks. Without reward or other influences on the expected value of control, participants typically show a large repetition bias and only rarely switch tasks (Kessler et al., 2009). They seem to avoid the effort costs of switching which results in a low willingness to switch. In everyday life, investing cognitive effort in the form of cognitive flexibility can be very useful and sometimes even necessary for success. Thus, the present project aimed at exploring factors that are part of the cost-benefit analysis of cognitive flexibility and thereby influence the willingness to switch tasks. The EVC model suggests that several potential factors may influence the decision to switch.

First, the expected reward can justify cognitive effort investment. This notion can be tested by manipulating the payoff of task switches. In the study by Braem (2017), one group of participants received a higher reward for cued task switches whereas the other group received a higher reward for cued repetitions. In a subsequent voluntary task-switching phase without reward, the participants in the first group (with more reward for task switches) voluntarily switched tasks more often. The associated reward value outweighed the effort costs of switching. In a similar vein, Braun and Arrington (2018) independently manipulated the reward for the current and the alternative task in a voluntary task-switching paradigm. The results showed that decreasing the reward of the just executed task or increasing the reward for the alternative task increases the proportion of voluntary task switches. Taken together, increasing the (associated) reward for task switches can offset the effort costs.

When deciding between two alternatives, individuals should not only consider the potential rewards but also the relative costs. In voluntary task-switching paradigms, participants effectively have two behavioral options: to repeat or to switch the task. Thus, apart from increasing the payoff of task switches, one can also make task repetitions less attractive to increase the willingness to switch. The self-organized task-switching paradigm successively reduced the stimulus availability of task repetitions to induce larger VSRs (Mittelstädt, Miller, & Kiesel, 2018). In this case, repetitions were less attractive because the participants had to wait for the repetition stimulus to appear whereas a task switch was possible right away. Similarly, pairing task repetitions with increasing physical effort resulted in increased voluntary switches (Langhanns et al., 2021). The physical effort costs of repetitions encouraged participants to invest the cognitive effort of a task switch. Taken together, the rewards and costs of both alternatives (i.e., switches and repetitions) appear to be considered when deciding whether to repeat or switch tasks.

In voluntary task-switching experiments, there is typically a large variability regarding the willingness to switch reflected in the VSR (Arrington et al., 2014). While most participants generally tend to avoid switches, some seem to be unfazed by the associated effort costs of a task switch. These interindividual differences suggest that the effort costs of a task switch may objectively and/or subjectively differ between participants. Dreisbach and Jurczyk (2022) showed that the individual performance difference between two tasks of unequal difficulty and (to some extent) the subjective effort costs, measured using the cognitive effort discounting procedure (Westbrook et al., 2013), influenced the VSR to the more difficult task. Participants who had less trouble performing the difficult task and who had lower subjective effort costs switched to the difficult task more often. In a similar fashion, the individual switch costs should guide the general decision to switch. Some participants may find it easier to apply the cognitive effort of switching tasks while others experience larger effort costs when switching. The switch costs are the main behavioral indicator of the effort costs of switching. It is difficult to pinpoint the exact role of the switch costs in the EVC model. Higher switch costs might indicate the additional cognitive resources required when switching and directly reflect the effort costs of switching. Alternatively, the switch costs might indicate the individual switching ability and therefore relate to the control efficacy (how efficiently the individual can translate control into performance). In either case, higher switch costs should result in increased effort costs and thereby a lower willingness to switch tasks. In line with this prediction, many studies found a negative relationship between the switch costs and the VSR (Mayr & Bell, 2006; Mittelstädt, Dignath, et al., 2018; Mittelstädt, Miller, & Kiesel, 2018, 2019). Lower switch costs (reflecting a higher switching ability or lower associated effort costs) resulted in larger VSRs. Participants appear to take their switch costs into account when deciding to switch tasks.

Vermeylen et al. (2019) provided supporting evidence for this perspective about the avoidance of switching. They showed that task switches are related to a more negative affect compared to task repetitions. In an affective priming procedure (Fazio, 2001), the transition cues (repetition, switch) of a previous cued task-switching phase were presented as primes before participants had to evaluate neutral targets (Chinese pictographs). The switch-prime led to more negative evaluations. This aversive nature of task switches may be the result of the associated effort costs. Furthermore, in an additional study, a more negative evaluation (potentially reflecting higher effort costs) was related to a lower willingness to switch tasks voluntarily (Vermeylen et al., 2022). This finding supports the notion that participants consider the aversive effort costs of switching when deciding to switch. Economic effort-based decisions may therefore influence cognitive flexibility. However, there are also automatic and non-economic factors that can trigger flexibility. Recently the associative learning account of cognitive control proposed that flexibility is grounded in simple associative learning processes (Braem & Egner, 2018).

# Associative Learning of Flexibility

The EVC model offers a comprehensive approach to explain the investment of cognitive flexibility based on decision-making processes. These processes may be described as controlled and economic. However, flexibility can also be triggered automatically. The associative learning account of cognitive control (Abrahamse et al., 2016; Braem & Egner, 2018) provides an alternative non-economic approach to how cognitive flexibility can be modulated. The account states that lower-level associative learning processes can directly trigger the associated control mode. In other words, individuals can learn where to be flexible and show appropriate behavior.

The flexibility-stability balance highlights that different situations require different levels of cognitive stability and flexibility (Hommel, 2015). For example, reading an academic paper typically requires high task focus and stability whereas answering numerous emails that have accumulated after a holiday requires dynamic and flexible behavior. Hence, certain contexts (reading a paper vs. answering numerous emails) are closely linked to particular control modes. To show adaptive behavior, the cognitive system needs to engage the appropriate control mode demanded by a given context. The associative learning account of cognitive control suggests that lower-level learning processes can directly trigger associated control modes (Abrahamse et al., 2016; Braem & Egner, 2018). When a control mode is repeatedly activated in a specific context, control mode representations are linked to the context in an associative network. Consequently, the control mode can be triggered directly by that context. Thus, learned associations may provide an alternative non-economic reason to switch tasks despite the associated costs.

Several studies showed how cognitive flexibility is influenced by the context. In a block with a higher frequency of (forced) task switches, participants show reduced switch costs (Dreisbach & Haider, 2006) and higher VSRs (Fröber & Dreisbach, 2017; Fröber, Jurczyk, & Dreisbach, 2021). A similar effect occurs when the frequency of switching is tied to the location of the stimulus (Crump & Logan, 2010; Leboe et al., 2008) or the target identity (Chiu & Egner, 2017; Chiu et al., 2020). According to the associative learning account, in these studies, the context automatically triggered the associated cognitive flexibility. Participants learned when to be flexible. Importantly, these modulations of cognitive flexibility do not reflect controlled economic decisions. Simple learning processes directly trigger flexibility.

The present project explored both accounts (economic perspective based on the EVC model and associative learning account of cognitive flexibility) to investigate the reasons to switch. Study 1 and Study 2 focused on economic decision-making during voluntary task choice. Study 3 explored associative learning of cognitive flexibility using task-irrelevant cues. In this way, we can examine the evidence for both approaches to better understand the decision to switch.

# The Present Studies

### Study 1: Introspection

**Study 1** focused on the influence of the introspective task-switching ability on voluntary choice behavior. Generally, introspection describes the metacognitive ability to observe one's own cognitive processes (Danziger, 1980). One critical purpose of introspection is to evaluate the own performance. This estimated performance, especially bad performance, may serve as a guide in decision-making to adjust behavior by either increasing control when it is necessary and justified, or choosing an alternative behavioral option. In most cognitive control tasks (e.g., Stroop task, Eriksen Flanker task, or task switching) conditions that require higher cognitive control are associated with worse performance. Therefore, previous research has investigated whether the *objectively* evident costs of control are reflected in *subjective* estimations of the own performance.

Closely related to task switching, a large body of research investigated introspection in dual-task settings. In the dual-task paradigm, participants have to complete two tasks in succession on each trial while the interval (SOA) between the tasks can be varied (Fischer & Janczyk, 2022; Pashler, 1994). Typically, shorter SOAs result in slower reactions to the second task. This is called the Psychological Refractory Period effect. The SOA manipulation provides a helpful tool for assessing the temporal dynamics of mental processes (Fischer & Janczyk, 2022). To capture introspection during dual-tasking, several studies used some form of a visual analogue scale (VAS). After each trial, participants had to subjectively estimate their reaction time (RT) to the second task on a presented scale for example by clicking on the scale with the cursor. Interestingly, introspective estimations did not accurately reflect the systematic variation of the RT of the second task as a function of the SOA (Bratzke & Bryce, 2016; Bratzke et al., 2014; Bryce & Bratzke, 2015, 2017; Corallo et al., 2008; Marti et al., 2010). Hence, it appears that the participants were not aware of the typical costs associated with dual-tasking.

Because the switch costs reflect similar performance costs when executing more than one task (with sequential, non-overlapping tasks as opposed to dual-tasking), the question arises whether the reduced performance following task switches can be captured by introspection. Multiple experiments showed that participants indeed reported slower RTs for task switches compared to repetitions (Bratzke & Bryce, 2019, 2022). However, the influence of preparation time on objective performance was not reflected in subjective estimations (Bratzke & Bryce, 2019). In sum, introspection about the switch costs appears to be possible. Still, some participants may over- or underestimate their switch costs. Therefore, in **Study 1**, we investigated whether the decision to switch is based on the objective or the introspective switch costs. If the motivation to switch depends on the associated costs according to economic cost-benefit analyses, the objective switch costs might either automatically influence the VSR or they might first be translated into subjective representations to guide behavior. Thus, the first study aimed at the underlying processes of how the individual switch costs can influence the motivation to switch.

### Study 2: Temporal Cost

**Study 2** continued to explore the economic cost-benefit analysis during voluntary task switching. Here, we took a closer look at the nature of the switch costs beyond effort. Switching tasks simply takes time. Time is costly. Similar to effort costs, when two behavioral options differ only in the time it takes to complete them, individuals tend to choose the faster option. Like effort, time should not be wasted. Thus, for decision-making, time may represent an additional cost factor (temporal cost) that influences the expected value of a particular action (similar to the expected value of control, Shenhav et al., 2021). Overall, these time costs can be considered jointly with other potential costs (especially effort costs) and benefits to guide behavior. This implies that time costs might be able to outweigh associated effort costs. A recent study showed that just like higher task difficulty (increased required effort), a longer duration of a cognitive task reduced its choice rate (Janczyk et al., 2022). Instead, participants were inclined to choose the alternative physical task (carrying a bucket with a variable weight). The temporal costs of the cognitive task could to some extent outweigh the physical effort of the alternative task. Taken together, temporal costs just like effort costs may guide decision-making.

In previous studies, the costs of switching were often interpreted in terms of effort costs (Dreisbach & Jurczyk, 2022; Kool et al., 2010). The switch costs indicate the effort required to switch tasks. However, it has been largely overlooked that switching also incurs temporal costs. A task switch takes longer to complete compared to a task repetition. This typical finding is defined as the RT switch costs (Kiesel et al., 2010; Koch & Kiesel, 2022; Monsell, 2003; Vandierendonck et al., 2010). In other words, the switch costs measure (at least) two facets: the effort costs and temporal costs of switching. So, instead of the associated effort, switches might simply be avoided due to the associated temporal costs.

This notion was already implied by studies using the self-organized task-switching paradigm where participants tended to switch when the waiting time for the repetition stimulus was similar to the switch costs (Mittelstädt, Miller, & Kiesel, 2018, 2019; Mittelstädt et al., 2021; Monno et al., 2021). However, this paradigm primarily measures the effects of stimulus availability and does not allow to disentangle temporal costs and effort costs. Therefore, **Study 2** systematically manipulated the interval following task switches between blocks to modulate the associated temporal costs of switching independent of the associated effort costs (and without biasing stimulus availability). Study 1 and Study 2 both examined the cost of switching (Study 1: objective and introspective switch costs; Study 2: temporal costs).

## Study 3: Associative Learning

In **Study 3**, we focused on associative learning processes and whether they can trigger cognitive flexibility as indicated by a higher willingness to switch tasks despite the associated costs. Study 3 tested the limits of this associative learning account of cognitive control (Abrahamse et al., 2016; Braem & Egner, 2018). Specifically, we investigated whether *task-irrelevant* cues that were previously associated with task repetitions or task switches would influence cognitive flexibility as measured with the VSR. According to the associative learning account, the control modes should become associated with the cues. In an associative network, the learned cues should automatically trigger the associated control mode. This would provide an alternative non-economic approach to modulate cognitive flexibility.

Together, the present project considered the role of effort-based decision-making and associative learning in the voluntary task-switching paradigm. The three studies examined how the objective or introspective switch costs (Study 1), associated temporal costs (Study 2), and lower-level associative learning (Study 3) influence voluntary choice behavior. The main goal was to advance our understanding of the reasons to switch.

# PART II:

# PEER-REVIEWED STUDIES

# Study 1:

# The Role of Objective and Introspective Switch Costs in Voluntary Task Choice

Jonathan Mendl & Gesine Dreisbach

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**Abstract**

Human beings are cognitive misers. One facet of this effort avoidance is the reluctance to voluntarily switch tasks when repeating the same task is allowed. Yet, participants voluntarily switch despite the resulting costs. This paradox might be resolved if the individual switching ability or sensitivity is considered. Here we investigate whether the voluntary switch rate (VSR) is governed by the objective or the subjective (introspective) switch costs. Three experiments were conducted utilizing voluntary task switching with forced and free task choices intermixed. In Experiments 1 and 3, objective switch costs were measured on forced tasks, and subjective switch costs were calculated from (introspectively) estimated reaction times in a separate phase. In Experiment 2, objective and subjective costs were measured in the same phase. In Experiments 2 and 3, we additionally manipulated the forced switch rate (FSR). Results show that objective and subjective switch costs were lower in blocks with higher FSRs. The objective switch costs predicted VSR in Experiments 1 and (partially) 3. The subjective switch costs predicted the VSR only in Experiment 3 (the lower the costs, the higher the VSR). Hence, the present study offers first insights under which circumstances introspection guides decision-making during voluntary task-switching.

*Keywords:* cognitive control, voluntary task switching, introspection, mental effort

## Public Significance Statement

In everyday life, investing effort is one key determinant of success. Yet, human beings tend to avoid effort. We show how invested effort (the voluntary switch rate) in our paradigm depends on the individual ability (the switch costs). And we also show that participants have introspection about these costs and provide the first evidence that these subjective costs may also guide effort investment.

## Introduction

In numerous instances, success relies heavily on the invested effort (Shenhav et al., 2017). For example, when studying for a test, preparing a presentation, or trying to become physically more active, higher effort makes goal attainment more likely. However, the decision to engage in effortful behavior is not made easily as effort itself feels aversive (Dreisbach & Fischer, 2012, 2015; Inzlicht et al., 2018; Kurzban, 2016; Kurzban et al., 2013). Hence, the law of least effort states that people tend to generally avoid effort (Hull, 1943). This principle applies to physical effort but also holds true for mental effort (Kool et al., 2010; Schouppe et al., 2014). Given the choice, human beings tend to prefer the cognitively less demanding option. However, task demands are not only a feature of the respective task but also depend on the individual ability, as the same activity can be differently strenuous for different people (Dreisbach & Jurczyk, 2022). This raises the question whether the seemingly irrational behavior of a voluntary task switch (with its associated costs) can in part be explained by the individual switching ability and/or the individual sensitivity to these costs. More precisely, here we aim to investigate to what extent the objective and/or subjectively experienced switch costs predict the voluntary choice to switch and to explore the sensitivity of introspection during task switching.

The prime paradigm to examine voluntary task choice is the voluntary task-switching paradigm (Arrington & Logan, 2004, 2005; for a review, see Arrington et al., 2014). Here, participants can select one of two or more tasks to perform on a given trial. As the most fundamental finding in task-switching studies, it has been shown that switching is linked to performance decrements (Dreisbach, 2012; Kiesel et al., 2010; Monsell, 2003; Vandierendonck et al., 2010) and is experienced as effortful and aversive (Vermeylen et al., 2019). Therefore, under complete freedom of choice, participants seem to consider these costs by exhibiting a strong repetition bias and only rarely choose to switch (Kessler et al., 2009). For that reason, in most voluntary task-switching studies, participants are given a global randomness instruction to perform each task equally often and in a random order to counteract this bias and obtain viable voluntary switch rates (VSR; see Arrington & Logan, 2004).[1] The present study aims at elucidating the potential connection between individual (objective and subjective) task performance and task choice.

Various previous studies could already demonstrate how economic considerations are reflected in voluntary task-switching behavior. Dreisbach and Jurczyk (2022) used two tasks of unequal difficulty and calculated the performance costs as the difference in reaction times (RTs) for the more demanding task and the easier task. The costs of performing the hard task negatively predicted the switch rate to the difficult task: With lower performance costs, switches to the difficult task were more likely. Apart from such performance costs between tasks of unequal difficulty, there is also evidence that switch costs themselves relate to the VSR. For example, Mayr and Bell (2006) revealed a reliable correlation between switch rates in a voluntary task-switching paradigm and the switch costs in the same paradigm. Furthermore, they could show that lower switch costs in a separate alternating runs paradigm (see Rogers & Monsell, 1995) were also associated with higher VSRs. Using a modified version of the voluntary paradigm with a preview for the non-selected task or a delay for the switch stimulus, Mittelstädt and colleagues repeatedly reported correlations between the VSR and the switch costs (Mittelstädt, Dignath, et al., 2018; Mittelstädt, Miller, & Kiesel, 2018, 2019). In sum, these findings indicate that there is a connection between task-switching performance and the VSR. People who have a harder time switching between different tasks, appear to do so less often on a voluntary basis, which may reflect economic considerations in voluntary task choice (Dreisbach & Jurczyk, 2022; Kool et al., 2010). One pending issue concerns the underlying processes: To what extent does introspection about the individual switch costs contribute to this relationship? In other words, to what degree are people aware of their switch costs, and do these introspective switch costs influence voluntary task choice?

The first question has already been answered by Bratzke and Bryce (2019). Using standard task-switching paradigms (alternating runs in Experiments 1a and 1b; cued task-switching in Experiment 2), they asked participants to estimate their RTs every other trial on a visual analogue scale. In all experiments, the switch costs were evident in the subjective measure. However, introspection about multitasking performance, in general, is not without its limits. Particularly, subjective RTs did not mirror all effects of increased preparation time. Likewise, in dual-task experiments, participants did not show full introspection about the effects of the stimulus onset asynchrony (Bratzke & Bryce, 2016; Bratzke & Janczyk, 2021; Bryce & Bratzke, 2014, 2015, 2017). Taken together, participants seem

[1] Depending on the instruction, in voluntary task switching paradigms with free task choices on every trial, the VSR is typically extremely low (Kessler et al., 2009, 4-13 %). In order to increase the VSR, therefore most studies use a “randomness instruction” that asks participants to try to choose each task equally often but in a random order as if “flipping-a-coin” (Arrington & Logan, 2004). And even with these instructions, participants hardly reach the expected 50% VSR.

to have introspection about task-switching costs to some extent. Therefore, participants may use this subjective information to guide their task choice. However, and as outlined above, this information might not be fully accurate. Some participants might feel like switching takes a lot longer than repeating tasks while the objective switch costs are in fact small. Conversely, others might not report subjective switch costs while objective RTs exhibit large switch costs. As the decision to switch seems to be influenced by the actual (objective) switch costs, the introspective switch costs might also add to this effect. Hence, we aim to investigate whether and how both measures, objective and subjective costs, predict the voluntary switch rate. Moreover, we also aim to further explore the sensitivity of introspection during task switching.

To that end, in three experiments, we used a hybrid task-switching (HTS) paradigm, where voluntary trials (free choice) and trials with only one specified task (forced choice, half switch, half repetition) were intermixed (Fröber & Dreisbach, 2016b, 2017). The main advantage thereof lies in the fact that no global randomness instruction needs to be given to acquire sufficient switch rates. Such instructions hinder the idea of free choice by giving participants an external global goal. In that way, the present study is the first to investigate the connection between switch costs and truly voluntary switch rates as all previous studies either applied these randomness instructions (Mayr & Bell, 2006; Mittelstädt, Dignath, et al., 2018) or incentivized switching by increasing the availability of the switch stimulus, while participants were told to respond as quickly as possible (Mittelstädt, Miller, & Kiesel, 2018, 2019). Apart from that, forced choices in the HTS paradigm allow to measure switch costs independently of the VSR and based on an equal trial number and practice. In contrast, when using a standard VTS paradigm with 100 % voluntary choices, switch costs must be calculated from these very voluntary choices, thereby confounding practice effects and VSR: Participants with a higher VSR might get more training in switching tasks and therefore, progressively show reduced switch costs. In the hybrid paradigm, this training effect can only play a subordinate role as the largest amount of practice happens in the forced-choice trials that are the same for every participant (see also Dreisbach & Jurczyk, 2022).

To measure introspective switch costs, in the first experiment, we added a second phase similar to the method introduced by Bratzke and Bryce (2019, Experiment 2). Participants gave estimates of their own RT on a visual analogue scale at the end of every second trial in a block of 100 % forced choices (half switch, half repeat). The main question of interest was whether objective switch costs from the HTS phase and/or subjective switch costs from the introspection phase would predict the VSR.

## Experiment 1

### Method

#### *Participants*

Using G*Power 3.1.9.7, for detecting an effect (t-test, fixed model, single regression coefficient) with a medium effect size ($f^2$=0.15), a power of 95 %, and a significance level of 5 % in a multiple regression with two predictors, at least 74 participants are needed (Faul et al., 2009). To ensure a high sensitivity in the case of potentially smaller effect sizes, we went for a larger sample size of 120 participants. Thus, 120 subjects took part in the experiment and gave informed consent prior to the experiment in accordance with the ethical standards of the national research committee and with the 1964 Helsinki declaration and its later amendments. The age ranged from 18 to 38 years (*M*=21.70 years, *SD*=3.43). Of all subjects, 95 were female (25 male) and 102 were right-handed (15 left-handed, 3 ambidextrous). Subjects received course credit for completing the experiment. Two participants had to be excluded, leading to a final sample of 118 (see data preprocessing for exclusion criteria).

#### *Apparatus and Stimuli*

The experiment was programmed using lab.js (Henninger et al., 2022) and data collection took place online via open lab (https://open-lab.online/; Shevchenko, 2022).[2] The hybrid procedure and the tasks and stimuli were adapted from Fröber and Dreisbach (2017). Participants had to respond to numbers (125, 132, 139, 146, 160, 167, 174, 181) and letters (B, D, F, H, S, U, W, Y). For the number task, the stimuli had to be categorized as smaller (left key) or larger (right key) than 153. In the letter task, participants had to decide whether a given letter is closer to A (left key) or closer to Z (right key) in the alphabet. One task was always presented above the center of the screen and the other task was always presented below the center of the screen. Responses were given by pressing the “g” and “h” keys with the index and middle finger of the left hand for the upper task and pressing “k” and “l” with the index and middle finger of the right hand for the lower task. The task-to-hand/position mapping was counterbalanced across participants. The stimuli were shown in black ink (Arial font, 32 pt.) on a white background. To estimate their RTs, participants had to click with the mouse cursor on the respective position on a visual analogue scale, labeled only at the extremes with “0 ms” and “1500 ms”. We selected this range to cover the typical RTs in the present task.

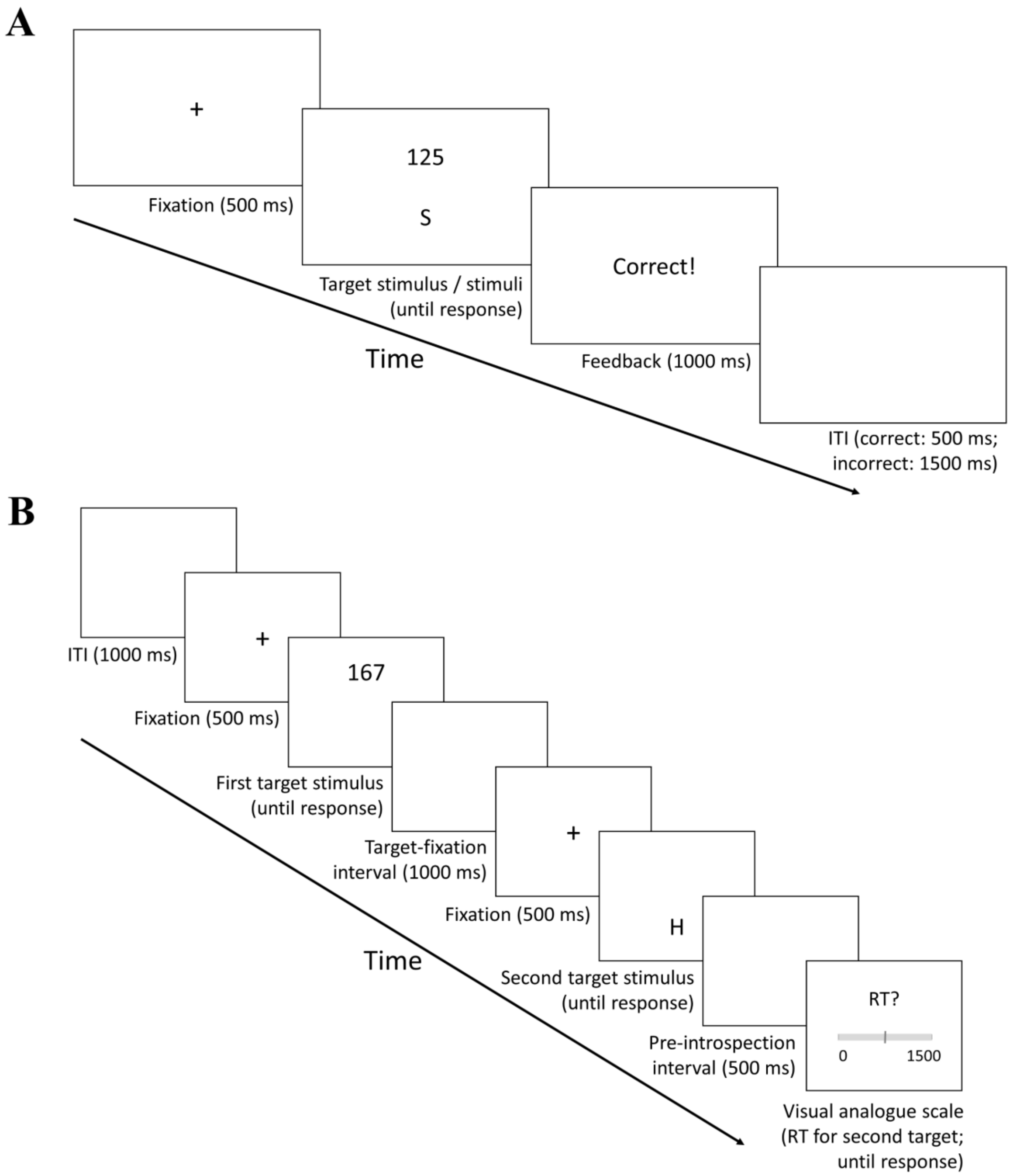
[2] The resulting mean VSR, RT, and error rate levels (23.03 %, 711 ms, and 7.62 %, respectively) were similar to previous lab studies with a similar paradigm (e.g., Fröber & Dreisbach, 2017, Experiment 1, 50 % forced choice condition with a mean VSR of 21.19 %, RT of around 700 ms, error rate around 4 %).

### *Procedure*

Practice trials and HTS trials consisted of a fixation cross in the center of the screen for 500 ms which was followed by the target stimulus (forced choice) or stimuli (free choice) until a response was given. After feedback in form of the German word for correct ("Richtig!") or wrong ("Fehler!") for 1000 ms, a blank screen for an intertrial interval (ITI) of 500 ms after correct and 1500 ms after erroneous responses occurred (see Figure 1, panel A). In the introspection phase, each trial consisted of two separate forced-choice targets. More specifically, a single trial started with an initial ITI (1000 ms), a fixation (500 ms), and the first target (until response). After a blank screen (1000 ms), another fixation (500 ms) and the second target appeared (until response). Last, after another blank screen (500 ms), the visual analogue scale for the estimation of the RT to the second target (until response; see Figure 1, panel B) was presented. In case of an error, feedback was displayed above the scale. It was shown whether the response to the first, the second, or to both targets was erroneous ("Fehler bei erstem Reiz!", "Fehler bei zweitem Reiz!").

**Figure 1**

*Schematic depiction of a single trial sequence in the hybrid task-switching phase (A) and the introspection phase (B) of Experiment 1*




The experiment began with separate practice blocks for each task (8 trials each), starting with the task, which was always presented above. After that, a short task-switching practice followed, where all stimuli of both tasks were shown in random order, while on a given trial only one stimulus was displayed (forced choice; 16 trials). Next, one of the two main phases followed: the HTS phase or the introspection phase. The order of these phases was counterbalanced across participants to be able to measure the potential impact of the order: Starting with the introspection phase might make participants more aware of their objective switch costs in the HTS phase. Starting with the HTS phase and the associated practice with the tasks might lead to an underestimation of the later following measure of the subjective switch costs. In the HTS phase, free-choice trials were introduced in a short voluntary task-switching practice (16 trials), where stimuli of both tasks were presented on each trial. Participants could freely decide which task to perform on a given trial. Subsequently, they were instructed that forced-choice and free-choice trials would appear intermixed in the next block and that they were completely free to choose one of the two tasks on free-choice trials. This experimental phase consisted of 256 trials (128 forced and free choices, respectively). The four possible choice sequences were roughly equally distributed. In the forced-choice trials, the switch rate was set to 50 % by choosing tasks on a trial-by-trial basis (such that the previous - potentially freely chosen trial - was taken into account to assure the 50% forced switch rate). The introspection phase started with a short practice phase, to make participants familiar with the visual analogue scale (8 trials with 2 targets each). Participants were told to estimate the response time to the second task in each trial by clicking on the respective position on the scale. For the next trial to start, subjects had to press the spacebar after being instructed to place the mouse cursor in the center of the screen. Finally, there were 3 test blocks with 64 forced-choice introspection trials each (2 tasks per trial). All 16 stimuli were presented equally often and in pseudorandomized order without direct stimulus repetitions and with an equal amount of task repetitions and switches. One session lasted approximately 50 minutes.

### *Design*

Error rates and RTs of forced-choice trials in the HTS phase, of the second target in the introspection phase, and introspective RTs were analyzed as a function of task transition (repetition vs. switch). The VSR was defined as the rate of switches in voluntary trials of the HTS phase. In the same phase, objective switch costs were calculated by subtracting mean RTs on forced repetitions from mean RTs on forced switches. Lastly, for the introspective switch costs, the same formula was applied using the estimated RTs. Objective switch costs from the HTS and introspective switch costs were entered as predictors for the VSR in a multiple regression. Raw data files associated with this article can be found online (http://doi.org/10.5283/epub.52586).

## Results

### *Data preprocessing*

For the analysis of error rates of the forced-choice trials in the HTS phase, the first trial (0.78 % of all trials) was excluded. Additionally, for the RT analysis, all error trials (7.56 %), post-error trials (5.56 %), trials with RTs faster than 100 or slower than 8000 ms (0.02 %), and trials with RTs more than three *SD*s above or below the individual cell mean (1.57 %) were excluded. In the introspection phase, for the analysis of error rates of the second target, we excluded the first trial (1.56 % of all trials).

Additionally, for the objective and subjective RT analyses in the introspection phase, trials with an error on the first target (9.53 %), or the second target (7.20 %), trials with RTs faster than 100 or slower than 8000 ms (0.03 %) and, finally, trials with RTs differing more than three *SD*s from the individual cell mean (3.24 %) were excluded. Furthermore, two participants had to be excluded, one due to extremely high overall mean error rates (39.84 %), and one due to extremely high error rates (42.06 %) and RTs (1494 ms) as inspected via boxplots (deviation of more than three interquartile ranges from the third quartile). To calculate the VSR, all voluntary trials of the HTS phase were used. We included erroneous trials to capture all possible intentional choices (Arrington & Logan, 2004). In error trials, the task was assigned according to the hand used to respond as participants are more likely to pick the wrong finger than the wrong hand (Scheffers & Coles, 2000).

### *Objective and subjective RTs*

The analyses revealed significant switch costs of 159 ms ($SD$ = 96) in the forced-choice trials of the HTS phase, $t(117) = 17.90$, $p < .001$, $d = 1.65$. Responses were faster on task repetitions ($M$ = 632 ms, $SD$ = 133) compared to switches ($M$ = 790 ms, $SD$ = 185). Similarly, in the introspection phase, the switch costs of the objective RTs ($M$ = 34 ms, $SD$ = 45) were significant, $t(117) = 8.15$, $p < .001$, $d$ = 0.75, with faster RTs on repetitions ($M$ = 600 ms, $SD$ = 104) compared to switches ($M$ = 634 ms, $SD$ = 119). Furthermore, the results yielded significant subjective switch costs ($M$ = 64 ms, $SD$ = 62), $t(117)$ = 11.11, $p < .001$, $d = 1.02$. Participants estimated their RTs faster on repetitions ($M$ = 585 ms, $SD$ = 178) compared to switches ($M$ = 649 ms, $SD$ = 177). Notably, these subjective switch costs were correlated with the individual objective switch costs of the introspection phase, $r = .368$, $p < .001$. In absolute terms, participants significantly overestimated their switch costs, $t(117) = 5.24$, $p < .001$, $d$ = 0.48. Last, objective forced-choice and voluntary switch costs in the HTS phase were significantly correlated ($r = .64$, $p < .001$), showing that the forced-choice measure is an appropriate proxy for the voluntary measure.

### *Error rates*

The error rate analysis of the HTS phase brought up significant switch costs of 3.32 % ($SD$ = 6.37), $t(117) = 5.66$, $p < .001$, $d = 0.52$. Participants made fewer errors on task repetitions ($M$ = 5.96 %, $SD$ = 5.57) compared to switches ($M$ = 9.28 %, $SD$ = 8.26). The same pattern emerged for the error rates in the introspection phase, with significant switch costs of 0.72 % ($SD$ = 3.73), $t(117) = 2.09$, $p = .039$, $d$ = 0.19. Again, error rates were smaller on repetitions ($M$ = 7.98 %, $SD$ = 5.98) compared to switches ($M$ = 8.70 %, $SD$ = 6.21).

### *Multiple regression*

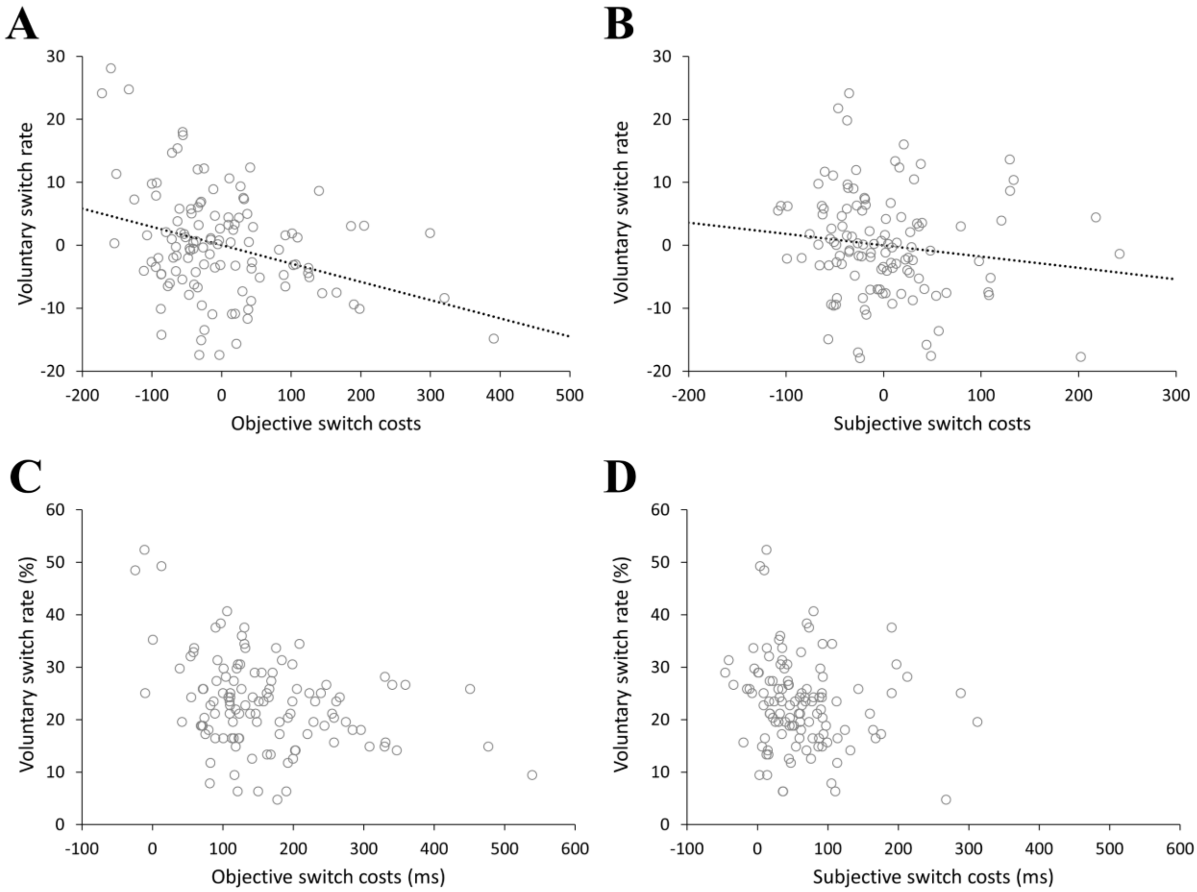
For our main hypothesis, we conducted a multiple linear regression with the objective switch costs of the HTS phase and subjective switch costs of the introspection phase as predictors for the VSR. The regression equation was significant, $F(2, 115) = 8.78$, $p < .001$. The $R^2$ of .13 (adjusted $R^2$ = .12) indicated a medium effect size according to J. Cohen (1988). In the model, the VSR was equal to 28.81 - .029 (objective switch costs) - .018 (subjective switch costs). However, the analysis showed that only the objective costs were a significant predictor, $\beta = -0.32$, $t(115) = -3.69$, $p < .001$, whereas the subjective switch costs were not, $\beta = -0.13$, $t(115) = -1.45$, $p = .149$. The lower the objective switch costs, the higher the VSR. The partial regression plots and scatter plots are shown in Figure 2.[3]

The order analysis (HTS vs. introspection first) only brought up practice effects: Participants had lower objective RTs in HTS when introspection came first and had lower objective RTS in the introspection phase when HTS came first. Subjective switch costs were not affected by the order. We can therefore exclude that metacognitive awareness was influenced by task-switching practice (cf. Koriat, 1997). Separate regression analysis for both groups brought up qualitatively the same results (with only a marginal significant overall model in the HTS first group, which, however, may be due to power issues when including only half of the participants; see Supplemental Materials for details).

[3] Excluding 8 cases with a Cook's *D* larger than $4/n$ lead to the same statistical pattern of results.

**Figure 2**

*Partial regression plots (normalized: A, B) and scatter plots (C, D) of the VSR as a function of the objective switch costs (A, C) and the VSR as a function of the subjective switch costs (B, D) of Experiment 1.*




*Note.* The dotted lines represent the corresponding regression lines of the multiple regression.

## Discussion

The results of the first experiment replicated the findings by Bratzke and Bryce (2019), showing that participants were subjectively aware of their switch costs. In fact, participants' switch cost estimations were related to their actual switch costs, as indicated by the significant correlation between the objective and subjective switch costs during the introspection phase. Therefore, participants might use this introspection as a guiding factor for task choice. However, results from the multiple linear regression analysis using the objective switch costs of the HTS phase and the subjective switch costs as predictors for the VSR only partially support this rationale. The overall regression model shows that both predictors together explain a moderate amount (11-13 %) of variance. But only the objective switch costs had a significant beta weight. Participants with higher objective switch costs chose to switch tasks less often. The subjective switch costs did not further add to this effect.

Our reasoning was that participants who think that they do not produce much costs when switching tasks might also be those who switch more often voluntarily. The results of Experiment 1 gave no clear indication that this is the case. However, given that the objective switch costs in the HTS phase at least moderately explain the VSR, the question remains whether participants use this information. Maybe, measuring the subjective switch costs in a separate introspection phase precluded the assumed association. Given that the actual forced-choice switch costs of the two phases were much lower in the introspection phase (34 ms vs. 159 ms in the HTS phase) the respective subjective switch costs might not be a good estimate for the assumed subjective switch costs during the HTS phase.[4] To get closer to the actual (task) decision process, we therefore decided to measure the introspective RTs during the HTS phase in Experiment 2. This way, the subjective switch costs, next to the objective switch costs, should be a stronger predictor of the switching behavior. Additionally, we aimed to further investigate the sensitivity of participants' introspection about their task-switching performance. Previous studies have shown that the switch costs and the VSR are modulated by the current task demands. In the context of frequent task switches, participants exhibit reduced switch costs and tend to switch tasks more often (Crump & Logan, 2010; Dreisbach & Haider, 2006; Fröber & Dreisbach, 2017; Fröber, Jurczyk, & Dreisbach, 2021; Siqi-Liu & Egner, 2020). Here, we will use this well-documented switch frequency effect to investigate whether participants will not only be sensitive to the existence of switch costs but also be sensitive to the size of the switch costs depending on the forced switch rate between blocks within the HTS phase. If so, we should find not only smaller objective but also smaller introspective switch costs in blocks with a higher switch rate as compared to blocks with a lower switch rate.

## Experiment 2

### Method

#### *Participants*

Using the same rationale as in Experiment 1, we decided to go for a larger sample than the 74 participants indicated by the power analysis. Therefore, another 100 subjects were recruited to take part in the second experiment and gave informed consent in accordance with the ethical standards of the national research committee and with the 1964 Helsinki declaration and its later amendments. They were between 18 and 52 years old ($M$ = 24.67 years, $SD$ = 4.37). Of all participants, 77 were female (22 male, 1 diverse) and 90 were right-handed (9 left-handed, 1 ambidextrous). Subjects received course credit or 8 € for completing the experiment. We excluded one participant, leading to a final

[4] This notion is supported by the exploratory finding that the objective switch costs of the introspection phase do not significantly predict the voluntary switch rate in the HTS phase, $F(1, 116) = 2.29$, $p = .133$, $R^2 = .02$, adjusted $R^2 = .01$.

sample of 99 (see data preprocessing for exclusion criteria). No participant had taken part in Experiment 1.

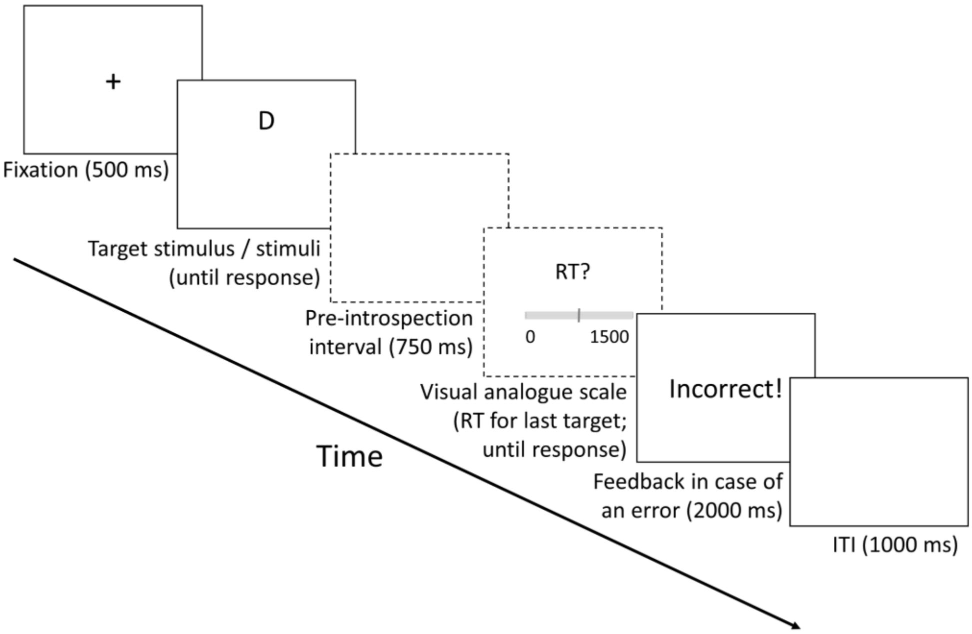
### ***Apparatus, stimuli, procedure, and design***

The method was similar to the first experiment.[5] The same tasks and stimuli were used. After short practice blocks for each task (8 trials), forced-choice task switching (16 trials), and voluntary task switching (16 trials) with the same trial structure as in Experiment 1, participants were instructed to secretly estimate their RTs on each trial because, on some trials, they would be asked to give their estimation on a visual analogue scale. The subjective RTs were collected after each second forced-choice trial. There were four test blocks of 256 trials each consisting of 50% forced-choice and 50% free-choice trials. The forced switch rate in these forced-choice trials was alternating between blocks with either 75 % switches (and 25 % repetitions) or 25 % switches (and 75 % repetitions). The forced switch rate (FSR) of the first test block was counterbalanced across participants. A single trial consisted of a fixation cross (500 ms), the target stimulus or the target stimuli (until response), feedback only in case of an error (2000 ms), and an ITI (1000 ms; see Figure 3). Additionally, after each second forced-choice trial, a pre-introspection interval (750 ms) and the visual analogue scale (until response) like Experiment 1 were shown. If there was an error in the current introspection trial, the feedback was shown above the scale and the feedback screen was skipped.

[5] Again, the resulting RT, and error rate levels (657 ms and 8.38 %, respectively) were comparable to previous lab studies with a similar paradigm. The mean VSR of 14.30 % appears to be smaller than in previous studies. This might be the case due to the intermixed introspection.

**Figure 3**

*Schematic depiction of a single trial sequence in Experiment 2. The screens with dotted lines were only presented after each second forced-choice trial.*




RTs of the forced-choice trials and introspective RTs were analyzed as a function of task transition (repetition vs. switch). The VSR, objective switch costs, and subjective switch costs were calculated the same way as in Experiment 1. Again, the VSR was used as the dependent variable in a multiple linear regression with objective switch costs and subjective switch costs as predictors. The context effect of frequent forced task switching on the objective and subjective RTs was investigated by utilizing a 2 (FSR: 25 %, 75 %) x 2 (transition: repetition, switch) repeated measures design. Similarly, the VSR was analyzed as a function of FSR. Raw data files associated with this article can be found online (http://doi.org/10.5283/epub.52586).

**Results**

***Data preprocessing***

We first excluded the first trial after each introspection trial in order to reduce the influence of restart costs which would result in an underestimation of the objective switch costs (e.g. Allport & Wylie, 2000; 25 % of all trials). For the VSR analysis, similar to Experiment 1, we used all remaining free-choice trials. For the objective and subjective RT analyses, we only used trials where introspective RT estimates were given. We further excluded error trials (8.67 % of all introspection trials), trials following an error (7.28 %), trials with an RT faster than 100 or slower than 8000 ms (0.06 %), and, last, trials with RTs differing more than three *SD*s from the individual cell mean (1.93 %). One participant had to be excluded due to extremely high overall mean error rates (31.45 %) and RTs (1434 ms) as inspected via boxplots (deviation of more than three interquartile ranges from the third quartile).

### *Objective and subjective RTs*

The results again show significant objective switch costs of 184 ms (*SD* = 79), $t(98) = 23.27$, $p < .001$, $d = 2.34$, with faster RTs on task repetitions (*M* = 565 ms, *SD* = 88) compared to switches (*M* = 749 ms, *SD* = 138). Additionally, there were significant introspective switch costs of 69 ms (*SD* = 55), $t(98) = 12.39$, $p < .001$, $d = 1.25$. Participants estimated that they responded faster on repetitions (*M* = 629 ms, *SD* = 201) compared to switches (*M* = 698 ms, *SD* = 197). In contrast to the findings of Experiment 1, there was no significant correlation between objective and subjective switch costs, $r = -.09$, $p = .369$. In absolute terms, participants significantly underestimated their switch costs, $t(98) = 11.45$, $p < .001$, $d = 1.15$. As in Experiment 1, the objective forced-choice and voluntary switch costs were significantly correlated ($r = .59$, $p < .001$).

### *Error rates*

The analysis of the error rates revealed significant switch costs of 4.16 % (*SD* = 5.60), $t(98) = 7.39$, $p < .001$, $d = 0.74$. There were fewer errors on task repetitions (*M* = 6.30 %, *SD* = 3.70) compared to task switches (*M* = 10.46 %, *SD* = 6.50).

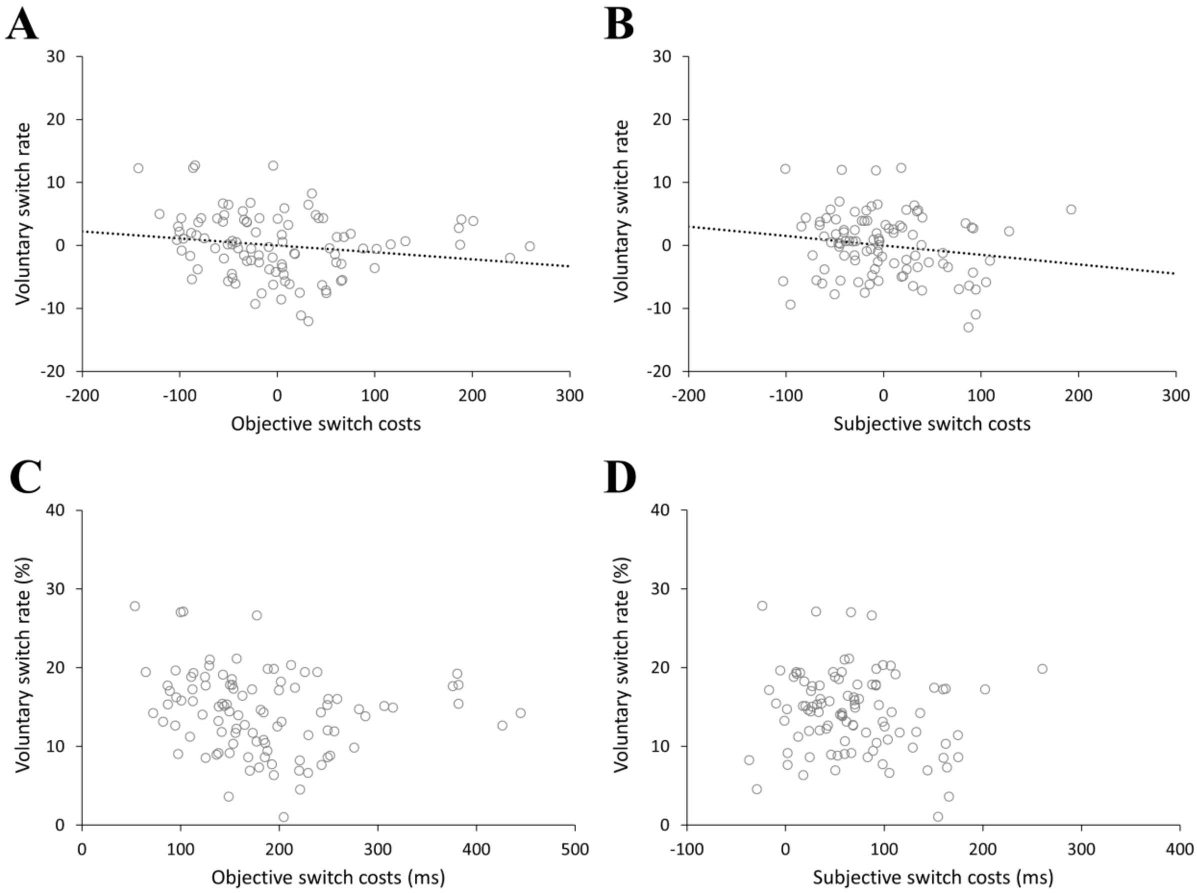
### *Multiple regression*

In the multiple regression analysis with objective and subjective switch costs as predictors for the VSR, the regression equation failed to reach significance, $F(2, 96) = 2.70$, $p = .072$, $R^2 = .05$, adjusted $R^2 = .03$, indicating a small effect size according to J. Cohen (1988). Neither the objective switch costs, $\beta = -0.18$, $t(96) = -1.77$, $p = .080$, nor the subjective switch costs were significant predictors, $\beta = -0.17$, $t(96) = -1.66$, $p = .099$. The partial regression plots and scatter plots are shown in Figure 4.[6]

[6] Conducting separate multiple linear regression analyses for the 25 % and the 75 % FSR conditions or excluding 9 cases with a Cook's *D* larger than 4/*n* lead to the same statistical pattern of results.

**Figure 4**

*Partial regression plots (normalized: A, B) and scatter plots (C, D) of the VSR as a function of the objective switch costs (A, C) and the VSR as a function of the subjective switch costs (B, D) of Experiment 2.*




*Note.* The dotted lines represent the corresponding regression lines of the multiple regression.

### *Effects of the forced switch rate*

The 2 x 2 ANOVA of the objective RTs with the independent variables FSR (25 %, 75 %) and transition (repetition, switch) revealed a significant main effect of transition, $F(1, 98) = 491.86$, $p < .001$, $\eta_p^2 = .83$, which again indicates overall objective switch costs. More importantly, the interaction between FSR and transition was significant, $F(1, 98) = 44.01$, $p < .001$, $\eta_p^2 = .31$. As can be seen in Figure 5 (panel A), there were larger switch costs with an FSR of 25 % ($M$ = 224 ms, $SD$ = 122) compared to an FSR of 75 % ($M$ = 153 ms, $SD$ = 71). The smaller switch costs in the 75 % FSR condition were driven by slowed RTs on repetitions, $t(98) = 4.62$, $p < .001$, $d = 0.46$, and faster RTs on switches, $t(98) = 5.04$, $p < .001$, $d = 0.51$, compared to the 25 % FSR condition. The main effect FSR did not reach significance, $F(1, 98) = 2.65$, $p = .106$, $\eta_p^2 = .03$.

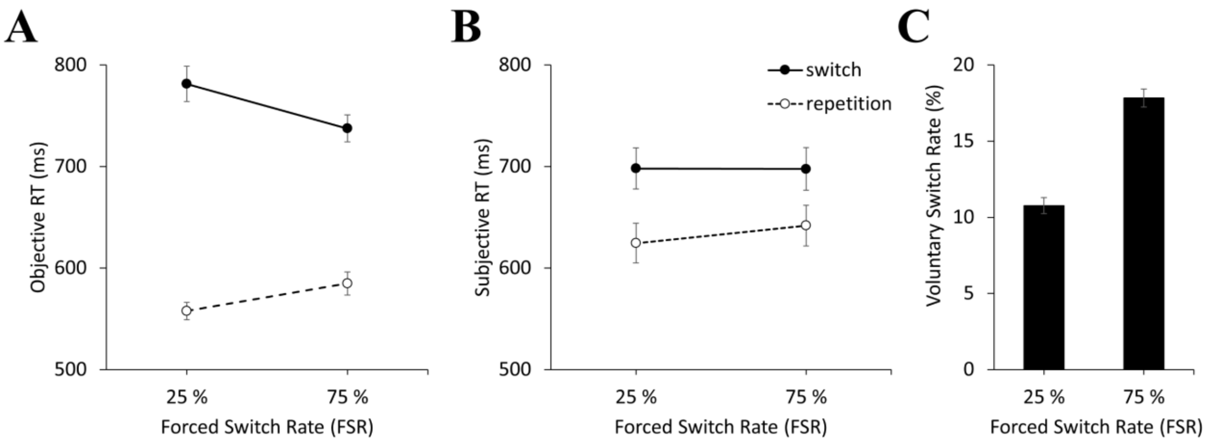
The corresponding 2 x 2 ANOVA of the subjective RTs brought up a significant the main effect of transition, $F(1, 98) = 136.85$, $p < .001$, $\eta_p^2 = .58$, which indicates overall subjective switch costs. Additionally, the analysis revealed a significant interaction between FSR and transition, $F(1, 98) = 13.42$, $p < .001$, $\eta_p^2 = .12$, with larger subjective switch costs in the 25 % FSR condition (*M* = 73 ms, *SD* = 59) as compared to the 75 % FSR condition (*M* = 56 ms, *SD* = 49; see Figure 5, panel B). The difference between the 25% and the 75 % FSR was only driven by slowed RTs on repetitions, $t(98) = 3.13$, $p = .002$, $d = 0.31$, whereas task switches did not differ between FSR conditions, $t(98) = 0.08$, $p = .940$, $d = 0.01$. Last, there was no significant main effect FSR, $F(1, 98) = 2.82$, $p = .096$, $\eta_p^2 = .03$.

A similar 2 x 2 ANOVA was conducted for the error rates. The analysis revealed a significant effect of transition, $F(1, 98) = 50.37$, $p < .001$, $\eta_p^2 = .34$, suggesting overall switch costs. The effect of the FSR and the interaction did not reach significance (all *F*s < 1.43, all *p*s > .235).

Finally, also the VSR was significantly modulated by the FSR, $t(98) = 15.43$, $p < .001$, $d = 1.55$. Participants switched tasks more often in blocks with a 75 % forced switches (*M* = 17.84 %, *SD* = 5.82) as compared to blocks with a 25 % FSR (*M* = 10.77 %, *SD* = 5.28; see Figure 5, panel C).

**Figure 5**

*Objective (panel A) and subjective (panel B) RTs as a function of FSR (25 %, 75 %) and transition (repetition, switch). VSR (panel C) as a function of FSR in Experiment 2.*




*Note.* Error bars represent ±1 standard error of the mean.

## Discussion

The aim of the second experiment was again to investigate the role of subjective and objective switch costs in voluntary task choice. By measuring subjective switch costs within the HTS paradigm, we tried to strengthen the link between the subjective measure and the VSR. As in Experiment 1, we found that participants were sensitive to their switch costs, thereby confirming previous findings in the literature (Bratzke & Bryce, 2019). Furthermore, we could extend these findings by showing that participants are not only sensitive to the general switch costs but even to the forced switch frequency in each context: Participants correctly estimated lower switch costs in the 75 % FSR condition with frequent forced task switches and higher switch costs in the 25% FSR condition, respectively. However, in contrast to Experiment 1 and our hypothesis, there was no correlation between the objective and subjective switch costs and neither one of these measures predicted the VSR. One possible reason for the difference compared to Experiment 1 might be that participants were overwhelmed by the dual-task situation because subjective RT estimations were intermixed in an HTS paradigm. Therefore, participants' usage of objective or subjective performance for decision-making was mitigated.

The discrepant results of Experiments 1 and 2 cannot be interpreted unequivocally. Because we changed two aspects between experiments by collecting subjective RTs during the HTS phase and, additionally, manipulating the forced switch frequency, the reason for the inconsistent findings is not entirely clear. Because we lean towards the hypothesis that the concurrent collection of introspective RTs interfered with the decision process, we decided to separate both processes again. Hence, we conducted Experiment 3 where we measured subjective RTs again in a separate phase while modulating the FSR in the HTS phase. If the use of only one phase in Experiment 1 was the sole reason for the differing results, we should find objective switch costs to be a significant predictor for the VSR in Experiment 3.

## Experiment 3

### Method

#### *Participants*

Another 100 participants took part in Experiment 3 and gave informed consent in accordance with the ethical standards of the national research committee and with the 1964 Helsinki declaration and its later amendments. The age ranged from 18 to 48 years (M = 24.89 years, SD = 5.31). 82 participants were female (18 male) and 94 were right-handed (5 left-handed, 1 ambidextrous). As compensation, they received course credit or 8 € for completing the experiment. One participant had to be excluded, leading to a final sample of 99 (see data preprocessing for exclusion criteria). The online registration procedure made sure that no participant had taken part in the first two experiments.

#### *Apparatus, stimuli, procedure, and design*

The method was similar to Experiment 1.[7] We used two separate phases (in counterbalanced order; for analyses of the order see Supplemental Materials). The trial structure and the entire introspection phase were the same as in Experiment 1. The HTS phase consisted of four test blocks

[7] As in Experiment 1, the resulting VSR (21.81 %), RT (709 ms), and error rate levels (7.67 %) were comparable to previous lab studies with a similar paradigm.

with 128 trials each (50 % free choice and 50 % forced choice) alternating between blocks with either a 75 % FSR or a 25 % FSR. The FSR of the first test block was counterbalanced across participants.

RTs of the forced-choice trials and introspective RTs were analyzed as a function of task transition (repetition vs. switch). The VSR, objective switch costs, and subjective switch costs were calculated the same way as in Experiment 1. To investigate the main question, the VSR was used as the dependent variable in a multiple linear regression with objective and subjective switch costs as predictors. To investigate the effect of the forced switch frequency, the forced-choice RTs of the HTS phase were additionally analyzed in a 2 (FSR: 25 %, 75 %) x 2 (transition: repetition vs. switch) design. Raw data files associated with this article can be found online (http://doi.org/10.5283/epub.52586).

**Results**

***Data preprocessing***

The preprocessing steps were the same as in Experiment 1. Before analyzing the error rates of the forced-choice trials in the HTS phase, we excluded the first trial (1.56 % of all trials). Additionally, for the RT analysis, error trials (8.18 %), post-error trials (5.64 %), trials with RTs faster than 100 ms or slower than 8000 ms (0.04 %), and, last, trials with RTs more than three SDs above or below the individual cell mean (1.73 %) were excluded. For the VSR analysis, we again used all remaining free-choice trials. In the introspection phase, before analyzing the error rates of the second target, we excluded the first trial (1.56 % of all trials). In addition, for the objective and subjective RT analyses in the introspection phase, trials with an error on the first (10.37 %) or the second target (7.67 %), trials with RTs faster than 100 or slower than 8000 ms (0.05 %) and, finally, trials with RTs differing more than three SDs from the individual cell mean (3.37 %) were excluded. One participant had to be excluded prior to the final analyses due to extremely high overall mean error rates (73.29 %; deviation of more than three interquartile ranges from the third quartile).

***Objective and subjective RTs***

The results revealed significant objective switch costs of 170 ms ($SD$ = 128) in the HTS phase, $t(98) = 13.19$, $p < .001$, $d = 1.33$ (repetitions: $M$ = 624 ms, $SD$ = 106; switches: $M$ = 794 ms, $SD$ = 207).and significant objective switch costs of 113 ms ($SD$ = 84) in the introspection phase, $t(98) = 13.49$, $p < .001$, $d = 1.36$ (repetitions: $M$ = 566 ms, $SD$ = 84; switches: $M$ = 679 ms, $SD$ = 147). Furthermore, subjective switch costs were also significant ($M$ = 60 ms, $SD$ = 67), $t(98) = 8.88$, $p < .001$, $d = 0.89$ (repetitions: $M$ = 549 ms, $SD$ = 194; switches: $M$ = 609 ms, $SD$ = 208).[8] As in Experiment 1, there was a significant correlation between objective and subjective switch costs in the introspection phase, $r = .320$, $p = .001$. In absolute terms, participants significantly underestimated their objective switch costs, $t(98) = 5.92$,

[8] Again, the order of phases had no significant effect on the subjective RTs, which is why we can exclude that metacognitive awareness was influenced by task switching practice (cf. Koriat, 1997)

$p < .001$, $d = 0.60$. Objective forced-choice and voluntary switch costs in the HTS phase were significantly correlated ($r = .61$, $p < .001$).

### Error rates

The analysis of error rates brought up significant switch costs of 3.93 % ($SD = 5.07$) in the HTS phase, $t(98) = 7.72$, $p < .001$, $d = 0.78$ (repetitions: $M = 5.70$ %, $SD = 4.21$; switches: $M = 9.64$ %, $SD = 6.54$). There were no significant switch costs in the introspective phase, $t(98) = 0.15$, $p = .881$, $d = 0.02$ (repetitions: $M = 9.47$ %, $SD = 6.94$; switches: $M = 9.53$ %, $SD = 6.94$).

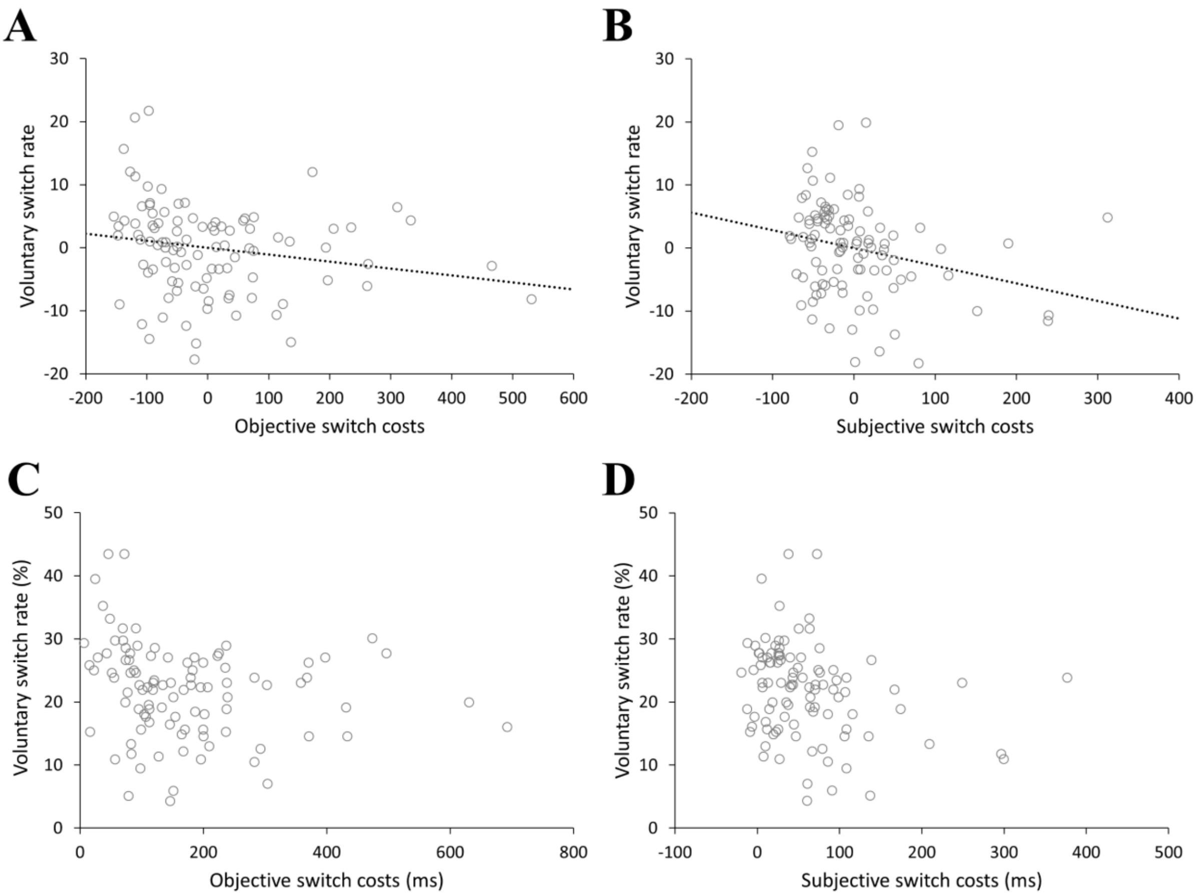
### Multiple regression

The multiple regression with objective and subjective switch costs as predictors for the VSR was significant, $F(2, 96) = 5.39$, $p = .006$, $R^2 = .10$, adjusted $R^2 = .08$, indicating a small effect size according to J. Cohen (1988). In the model, the VSR was equal to 25.33 - .011 (objective switch costs) - .028 (subjective switch costs). This time, only the subjective switch costs were a significant predictor for the VSR, $\beta = -0.25$, $t(96) = -2.61$ $p = .010$, whereas the objective costs were not, $\beta = -0.19$, $t(96) = -1.91$, $p = .059$. The partial regression plots and scatter plots are shown in Figure 6.[9]

[9] When excluding 6 cases with a Cook's $D$ larger than $4/n$, both predictors showed significant contribution to the overall model (all $t$s > -2.80, all $p$s < .007).

**Figure 6**

*Partial regression plots (normalized: A, B) and scatter plots (C, D) of the VSR as a function of the objective switch costs (A, C) and the VSR as a function of the subjective switch costs (B, D) of Experiment 3.*

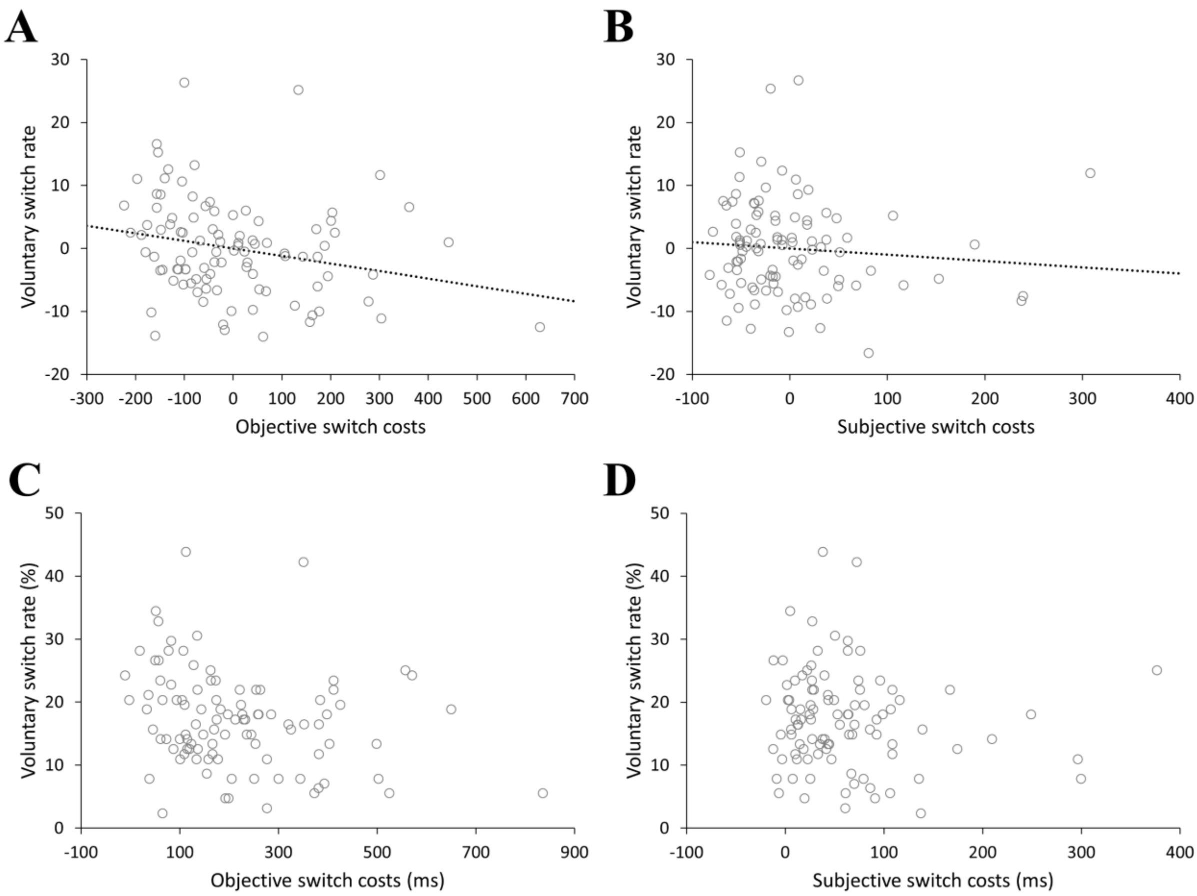



*Note.* The dotted lines represent the corresponding regression lines of the multiple regression.

Given that the objective switch costs and VSR both heavily rely on the specific FSR in a given block, we also conducted multiple regressions for each FSR condition, separately (as was also done for Experiment 2, see Footnote 6). Note that this analysis was not planned in advance but seems necessary in retrospect because the subjective costs might be more representative of one of the two conditions. We used the VSR of the respective FSR condition of the HTS phase as the dependent variable. The objective switch costs of the same FSR condition and the subjective switch costs of the introspection phase (without any FSR manipulation, 50 % FSR) served as predictors. In the 25 % FSR condition, the overall multiple regression model reached significance, $F(2, 96) = 3.32$, $p = .040$, $R^2 = .07$, adjusted $R^2 = .05$, indicating a small effect size according to J. Cohen (1988). In the model, the VSR was equal to 20.36 - .012 (objective switch costs) - .010 (subjective switch costs). Here, only the objective costs were a significant predictor for the VSR, $\beta = -0.23$, $t(96) = -2.36$ $p = .021$, whereas the subjective switch costs were not, β = -0.09, $t(96) = -0.91$, $p = .364$. The partial regression plots and scatter plots are depicted in Figure 7. In the 75 % FSR condition, this pattern was reversed. The overall model reached significance, $F(2, 96) = 8.03$, $p < .001$, $R^2 = .14$, adjusted $R^2 = .13$, indicating a medium effect size according to J. Cohen (1988). The VSR was equal to 30.34 - .010 (objective switch costs) - .043 (subjective switch costs). Only the subjective switch costs significantly predicted the VSR of the 75 % FSR condition, β = -0.34, $t(96) = -3.58$, $p < .001$, whereas the objective costs did not, β = -0.13, $t(96) = -1.41$, $p = .163$. The partial regression plots and scatter plots are shown in Figure 8.[10]

**Figure 7**

*Partial regression plots (normalized: A, B) and scatter plots (C, D) of the VSR as a function of the objective switch costs (A, C) and the VSR as a function of the subjective switch costs (B, D) of only the 25 % FSR condition of Experiment 3.*

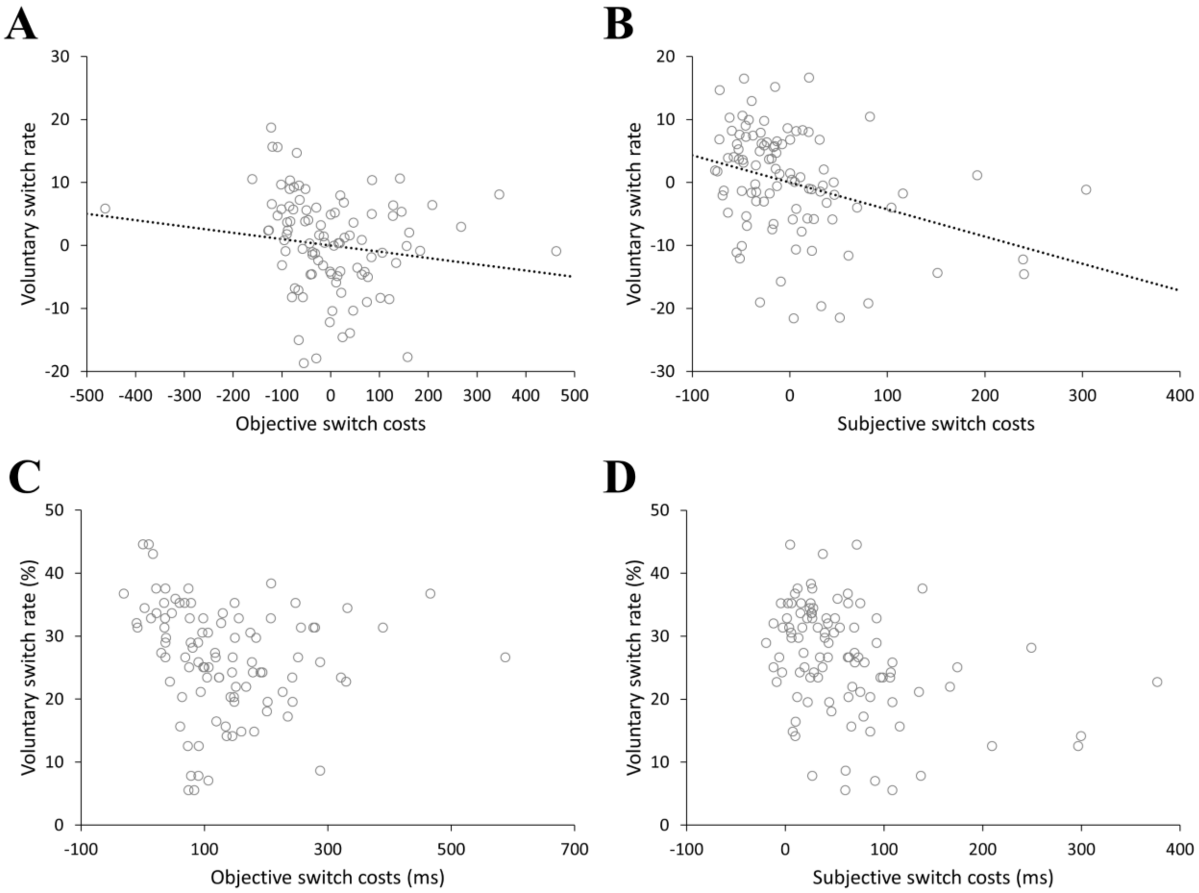



*Note.* The dotted lines represent the corresponding regression lines of the multiple regression.

[10] The same statistical patterns emerge when excluding all cases with a Cook's *D* larger than 4/*n* ( 6 cases in the 25 % FSR condition, 4 cases in the 75 % FSR condition).

**Figure 8**

*Partial regression plots (normalized: A, B) and scatter plots (C, D) of the VSR as a function of the objective switch costs (A, C) and the VSR as a function of the subjective switch costs (B, D) of only the 75 % FSR condition of Experiment 3.*




*Note.* The dotted lines represent the corresponding regression lines of the multiple regression.

### *Effects of the forced switch rate*

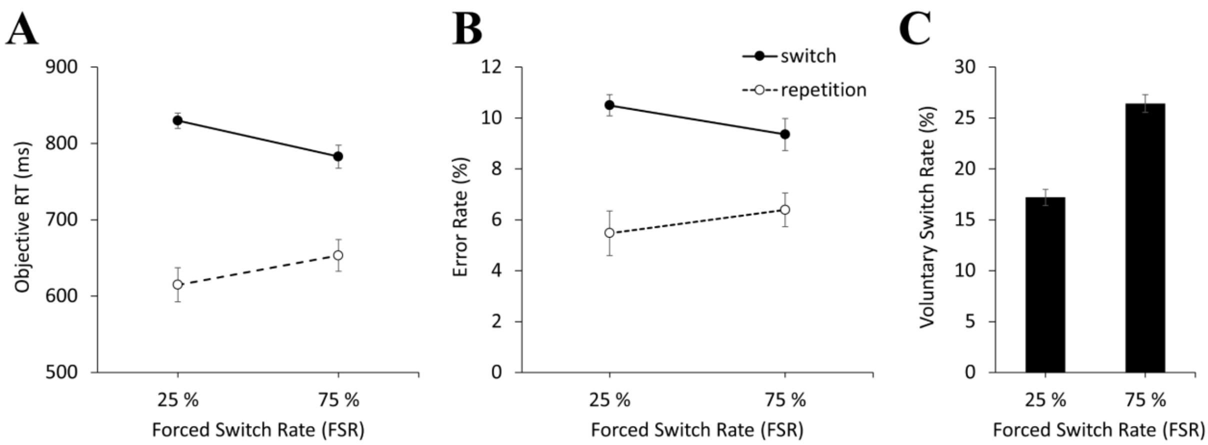
The 2 x 2 ANOVA of the RTs in the HTS phase with the independent variables FSR (25 %, 75 %) and transition (repetition, switch) resulted in a significant main effect of transition, $F(1, 98) = 198.86$, $p < .001$, $\eta_p^2 = .67$, again indicating overall significant switch costs. Furthermore, the interaction between FSR and transition was significant, $F(1, 98) = 50.72$, $p < .001$, $\eta_p^2 = .34$. Switch costs were larger with an FSR of 25 % ($M$ = 215 ms, $SD$ = 154) compared to an FSR of 75 % ($M$ = 130 ms, $SD$ = 114; see Figure 9, panel A). The main effect FSR did not reach significance, $F(1, 98) = 0.28$, $p = .601$, $\eta_p^2 < .01$.

In the 2 x 2 ANOVA of the error rates, there was a significant main effect of transition, $F(1, 98) = 53.59$, $p < .001$, $\eta_p^2 = .35$, and a significant interaction between FSR and transition $F(1, 98) = 4.96$, $p = .028$, $\eta_p^2 = .05$. There were larger switch costs with an FSR of 25 % ($M$ = 5.02 %, $SD$ = 7.88) compared to an FSR of 75 % (*M* = 2.97 %, *SD* = 6.26; see Figure 9, panel B). There was no significant main effect of the FSR, $F(1, 98) = 0.08$, $p = .777$, $\eta_p^2 < .01$.

Last the VSR was significantly modulated by the switch frequency, $t(98) = 13.43$, $p < .001$, $d = 1.35$. Participants switched tasks more often voluntarily in blocks with a 75 % FSR (*M* = 26.42 %, *SD* = 8.62) compared to a 25 % FSR (*M* = 17.20 %, *SD* = 7.77; see Figure 9, panel C).

**Figure 9**

*Objective RTs (panel A) and error rates (panel B) as a function of FSR (25 %, 75 %) and transition (repetition, switch). VSR (panel C) as a function of FSR in Experiment 3.*




*Note.* Error bars represent ±1 standard error of the mean.

## Discussion

The third experiment aimed at unraveling the discrepant results between Experiment 1 and 2 by using two phases as in Experiment 1 but also manipulating the forced switch frequency in the HTS phase as in Experiment 2. The results replicate the finding of subjective switch costs indicating that participants are sensitive to their switch costs (Bratzke & Bryce, 2019). Moreover, the influence of the FSR on the switch costs and the VSR was as expected: A higher forced switch frequency led to reduced switch costs and an increased VSR (Fröber & Dreisbach, 2017; Fröber, Jurczyk, & Dreisbach, 2021). In contrast to Experiments 1 and 2, the subjective switch costs were a significant predictor of the VSR whereas the objective switch costs were not. However, a closer look at the data revealed a more nuanced picture: when only looking at the 25 % FSR blocks in the HTS phase, the objective switch costs still predicted the VSR (as in Experiment 1) but the subjective switch costs did not. Conversely, when only looking at the 75 % FSR blocks in the HTS phase, the subjective switch costs were a reliable predictor but the objective switch costs were not. This pattern may reflect the fact that switch costs in a high FSR 75 % context reflect mutual interference due to two tasks being concurrently held active in working memory (Dreisbach & Fröber, 2019; Fröber, Jurczyk, & Dreisbach, 2021). And as such, these costs may not be ascribed to switching per se and therefore not be used as a guidance to switch. By contrast, objective switch costs in a low or medium FSR context (25 % in Experiment 3, 50 % in Experiment 1) reflect “actual switch costs” in terms of persisting activation of the previous and reconfiguration of the new task. Our results suggest that only these latter (“actual”) switch costs may guide the decision to switch.

Interestingly, the subjective switch costs showed the reversed pattern and only predicted the VSR in the FSR 75 % blocks. One admittedly speculative explanation could be that subjective switch costs are influenced by the participants’ individual effort avoidance: Effort avoidant participants presumably tend to report higher introspective switch costs. And this might in part explain why these introspective costs are a better predictor in a more effortful context of frequent switches, namely because effort avoidant participants switch less especially in an already effortful context. Also, a lower switch willingness of effort avoidant participants is easier to detect in a context where less effort avoidant participants tend to switch more often (i.e., in a context of frequent switches). Conversely, introspective switch costs are not a good predictor in a context where the VSR is already very low in general (as is the case in the low switch frequency blocks).

An alternative explanation for the pattern of the subjective switch costs might be grounded in the specific condition characteristics. The introspection phase resembled the FSR 75% blocks more than the FSR 25% blocks. Both, the forced-choice ratio and the forced switch rate have been shown to positively influence cognitive flexibility (Fröber & Dreisbach, 2017). Therefore, in Experiment 3, the introspection phase with a 50 % FSR and 100 % forced choice might be most similar to the condition with a 75 % FSR and 50 % forced-choice ratio in the HTS phase. This also becomes evident in the numerically comparable switch costs in these two conditions (75 % FSR in HTS phase: $M$ = 130 ms, $SD$ = 114; introspection phase: $M$ = 113 ms, $SD$ =84). Hence, only under these similar circumstances, that is, when the subjective costs are measured in a context that resembles the context in which the VSR is measured most, do we find the subjective costs to significantly predict the VSR. With a medium (Experiment 1, 50 %) or low FSR (Experiment 3, 25 % FSR condition) in the HTS phase, the context of the measured subjective switch costs may have been too dissimilar.

## General Discussion

The present study investigated the role of introspection in decision-making during voluntary task switching. We argued that people may be guided by either their objective switching ability and/or their introspective switching ability when deciding to switch tasks. If you are better at switching tasks and if it feels easier to switch tasks, you should do so more often. Therefore, we measured introspective switch costs based on RT estimations by the participant, objective switch costs, and the VSR. In the first and third experiments, subjective RTs were collected in a separate phase, and in the second experiment all three measures were obtained during the same HTS phase with forced and free choices, and RT estimations intermixed. The results show that (1) people are sensitive to their switch costs, (2) objective switch costs predict the VSR (but only in Experiment 1 and in the 25 % FSR condition of Experiment 3), (3) the subjective switch costs only influence voluntary task choice in Experiment 3 (especially in the 75 % FSR condition), and (4) introspection even reaches as far as to mirror context effects of the switch proportion on the switch costs.

The notion that objective switch costs predict the VSR converges with previous studies indicating a connection between task-switching performance and switch rate (Dreisbach & Jurczyk, 2022; Mayr & Bell, 2006; Mittelstädt, Dignath, et al., 2018; Mittelstädt, Miller, & Kiesel, 2018, 2019). However, in contrast to former studies, we measured the objective switch costs on intermixed forced choices using the hybrid task-switching paradigm and not on the voluntary choices proper. This at least reduces the influence of higher practice (and therefore smaller switch costs) as a confounding factor. Why objective switch costs did not predict VSR in the second experiment and overall in the third experiment, may stem from the specific procedure used there: in Experiment 2, participants were instructed to think about their RT on *each* trial. This may have imposed a dual-task situation (monitoring RTs during forced and voluntary task switching) which made the objective switch costs a less pure measure of the actual switch costs, thereby precluding its association with the VSR. That too much introspection can be harmful has also been shown for higher-level decision-making processes: On the level of complex information processing (e.g. curriculum planning) high introspection can be harmful and lead to focusing on less relevant information (Tordesillas & Chaiken, 1999; Wilson & Schooler, 1991). That means the higher task demands due to the concurrent and ongoing introspection might have distracted participants such that they were no longer able to use their performance as a guide for decision-making. Without this strong introspection focus, in Experiment 1, the choice behavior was predicted by the participants' switch cost performance. Another factor that seems to modulate the relationship between objective switch costs and the VSR is the manipulation of the forced switch frequency. Without the dual-tasking problem, in the third experiment, the objective costs only predicted choice behavior with a low FSR. A low FSR leads to larger switch costs, which makes switching more effortful and might make adaptations in choice behavior according to the performance more appropriate. In contrast, in blocks with a higher FSR and lower switch costs, objective costs did not predict the VSR. As previous studies have repeatedly reported the relationship between switch costs and switch rates (Mayr & Bell, 2006; Mittelstädt, Dignath, et al., 2018; Mittelstädt, Miller, & Kiesel, 2018, 2019), the present study is the first to unravel specific conditions that enhance or diminish this connection, namely the FSR.

One might also argue that the causal relationship between the switch costs and the VSR is the other way around, namely that choice behavior modulates the performance: When participants switch tasks more often voluntarily, they become better at switching tasks. As noted earlier, by using the hybrid paradigm, we can largely rule out this explanation as in half of the trials participants are forced to switch or repeat tasks and, therefore, to a great extent have similar practice with task switching (see Dreisbach & Jurczyk, 2022).

The general finding of subjective switch costs and their correlation with the objective switch costs in Experiments 1 and 3 is in line with the study by Bratzke and Bryce (2019). This shows again that participants' estimations represent their relative task-switching performance, while overall over- (Experiment 1) or underestimating (Experiment 3) their switch costs in absolute terms. It is important to note that Bratzke and Bryce (2019) used an alternating runs paradigm and a cued task-switching paradigm in their experiments. In both versions, the tasks were predictable (either by the order in the alternating runs paradigm or by the task cue). In contrast, in the present study, the task was not predictable and only with stimulus presentation, the current task was revealed. Furthermore, in the second experiment, the introspective estimates were given within a hybrid task-switching paradigm. So, in two ways the present results extend the findings of Bratzke and Bryce (2019): even with unpredictable tasks and with intermixed voluntary trials, participants are still able to subjectively report switch costs.

The subjective switch costs only modulated the VSR in Experiment 3. More importantly, this effect was mainly found in blocks with a high forced switch frequency in the HTS phase. One possible explanation for this discrepancy between experiments and FSR conditions might lie in the specific condition characteristics. As stated above in the discussion of Experiment 3, this might be due to the fact that the subjective switch costs measured in a block with 100% forced choices and 50% forced switches might be a better reflection of the objective costs in an HTS block with a higher switch frequency on forced choices. Taken together, this suggests that participants indeed utilize subjectively estimated switch costs to guide decision-making. Our findings also show that measuring the subjective costs and their association with voluntary task switching is rather complex: It cannot be done during the same phase (as results of Experiment 2 show) and may only be suited to predict the VSR in a context of higher flexibility as in the FSR 75% blocks in Experiment 3.

Task switching is effortful and typically avoided (Kool et al., 2010). The present study provides additional insight into this effort avoidance process. Participants are able to subjectively report their task-switching costs to some extent and, under certain circumstances engage in or disengage from investing effort. As in the present study, effort avoidance is typically based on the objective switch costs: The higher the costs, the lower the willingness to switch voluntarily. More generally, in a cost-benefit analysis higher costs lead to lower effort investment. This is in line with recent theories on effort-based decision-making (Kool & Botvinick, 2018; Richter et al., 2016; Westbrook & Braver, 2015) and a recent study suggesting that participants adjust their behavior in task switching according to the utility of effort (Otto et al., 2022). Extending this basic idea, the present findings suggest that introspection about the costs might in part be influenced by individual effort avoidance and thereby indirectly exert its influence on the decision to switch. Taken together, the results presented here add to the growing literature addressing the question of how differential costs modulate the decision for effort investment in (voluntary) task switching): First of all, objective switch costs are taken into account, as has been shown repeatedly and confirmed here (Mayr & Bell, 2006; Mittelstädt, Dignath, et al., 2018; Mittelstädt et al., 2019). Second, as reported recently, performance costs (measured as the difference between two tasks of unequal difficulty) also modulate the decision to switch (Dreisbach & Jurczyk, 2022). In that former study, first (marginal) evidence was also provided for the contribution of subjective effort costs: participants with higher subjective effort costs, as measured via the effort discounting paradigm (Westbrook et al., 2013) tended to switch less. And last but not least, introspective switch costs – presumably biased by individual effort avoidance - exert their impact on the decision to switch, which however can only be measured in a context of an overall higher (forced and therefore also voluntary) switch frequency. Furthermore, recent research by Otto et al. (2022) shows that effort investment is dynamically adjusted to its experienced utility, a finding which complements and extends assumptions from the classic motivational intensity theory (see also Brehm & Self, 1989; Richter et al., 2016).

Another important finding of the present study is that introspective RTs can clearly capture the switch frequency effect on the switch costs. In a context of frequent task switches, participants switch tasks more often voluntarily (Fröber & Dreisbach, 2017; Fröber, Jurczyk, & Dreisbach, 2021) and show reduced switch costs (Crump & Logan, 2010; Dreisbach & Haider, 2006; Siqi-Liu & Egner, 2020). The present results replicated both findings and extended the latter to subjective RTs. Previous studies using dual-task experiments had shown that introspection has several limits (Bratzke & Bryce, 2016, 2019; Bratzke & Janczyk, 2021; Bryce & Bratzke, 2014, 2015, 2017). Typically, objectively large effects of the stimulus onset asynchrony are not reflected in the subjective estimations. In the study by Bratzke and Bryce (2019), participants were sensitive to the task transition while the beneficial effects of increased preparation time were not correctly indicated. It seems that not every piece of information is captured by introspection. Therefore, the subjective sensitivity to rather subtle RT differences due to varied switch frequencies in the present experiment is even more remarkable. This shows how fine-grained introspection can function. Dreisbach and Fröber (2019) propose that the switch frequency effect stems from the fact that both tasks remain active in working memory when participants expect frequent forced task switches. With both tasks active, the activation difference between both tasks shrinks, such that participants respond faster on task switches and slower on repetitions. This pattern of reduction in switch costs with a high proportion of task switches is evident for the objective RTs. Notably, results from the subjective switch costs suggest that participants only register the slowed RTs on repetitions, but are insensitive to the faster RTs on task switches. This adds to the picture of introspection in multitasking situations being partly possible but far from perfect.

## Conclusion

In sum, the present study showed that objective switch costs are a reliable predictor of the VSR even when the potential confound of practice effects is minimized. Moreover, participants are subjectively aware of their switch costs and even sensitive to the switch frequency in a given context. Notably, the present study is the first to show that subjective switch costs predict the VSR. However, this latter relationship is rather subtle and depends on the context in which these measurements are taken. To conclude, our results show that objective and subjective switch costs both exert an influence on the decision to switch. The specific constraints under which this influence shows await further investigations.

# Study 2:

# Is Task Switching Avoided to Save Effort or Time? Shorter Intertrial Durations Following Task Switches Increase the Willingness to Switch Tasks

Jonathan Mendl & Gesine Dreisbach

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**Abstract**

Human decision-making is often described in terms of economic cost-benefit analyses. In voluntary task switching, the typical avoidance of task switches (repetition bias) has been primarily explained by effort costs. The present study investigated whether temporal costs independent of effort also guide the decision to switch. In two preregistered experiments ($N_{E1}$=86; $N_{E2}$=85), we used a hybrid task-switching paradigm with a mixture of predetermined (forced-choice) and voluntary (free-choice) trials. The duration of the intertrial interval *after* a switch was manipulated between blocks to be either always longer or always shorter than after a repetition. The results showed increased voluntary switch rates in blocks with a shorter interval following switches whereas the performance was not affected. Moreover, the effect was still evident in Experiment 2, where the interval was manipulated only after *forced-choice* task switches. This suggests that the temporal costs associated with switching contribute to the switch avoidance.

*Keywords*: cognitive flexibility, voluntary task switching, temporal costs

## Introduction

In every waking moment, humans can choose from a large variety of behavioral options and it is a central goal of psychological science to uncover the relevant determinants of such choices. Most accounts describe choice behavior as a result of economic cost-benefit analyses where the potential rewards of a choice alternative are compared to its potential costs (Kool & Botvinick, 2018; Kurzban et al., 2013; Shenhav et al., 2021). The most prominent factor adding to the costs is the associated effort (see Silvestrini et al., 2023). Here we define effort as the intensification of physical or mental activity (Inzlicht et al., 2018; Silvestrini & Gendolla, 2019) which is related to reactivity in the cardiovascular system(Brehm & Self, 1989; Gendolla et al., 2012; Richter et al., 2016). The waste of effort is typically avoided and humans tend to choose the least effortful option instead (but see Inzlicht et al., 2018; Wu et al., 2021). However, what has been largely overlooked is that the more effortful option is often the more time-consuming one (e.g. solving complex math problems compared to easy ones is more effortful but also takes more time). Given that time itself is a resource that should not be wasted (e.g., Kurzban et al., 2013), the question, therefore, arises whether individuals avoid wasting the temporal costs, i.e. the time consumed by a behavioral option, or the associated effort costs. In the present study, we aim to investigate the influence of temporal cost on voluntary choice behavior.

Choice behavior can be measured using the voluntary task-switching paradigm (Arrington & Logan, 2004, 2005; for a review, see Arrington et al., 2014). Here, two tasks are presented on a given trial and the participants are free to choose which task to perform. Typically, participants repeat tasks more often than they switch between them. This so-called repetition bias is reflected in the voluntary switch rate (VSR) computed as the percentage of how often the task is switched voluntarily. A widely held view is that participants avoid switching because switching is costly.

Several (standard) task-switching studies showed that performance (RT and error rate) is generally worse on task switches compared to task repetitions (Kiesel et al., 2010; Monsell, 2003; Vandierendonck et al., 2010). Critically, there is ample evidence that these switch costs are taken into account when deciding to switch tasks: Participants with larger switch costs switch tasks less often voluntarily (Mayr & Bell, 2006; Mendl & Dreisbach, 2022; Mittelstädt, Dignath, et al., 2018; Mittelstädt, Miller, & Kiesel, 2018, 2019). Additionally, there is first evidence that even introspective switch costs based on participants' RT estimations guide voluntary switching (Mendl & Dreisbach, 2022). That is, the switch costs appear to be integrated to regulate voluntary task-switching behavior. Notably, task switches not only take longer but also require additional processes (Kiesel et al., 2010; Monsell, 2003; Vandierendonck et al., 2010) thus demanding higher effort investment compared to repetitions. It follows that switching is costly in terms of both time and effort such that the question arises whether participants avoid switches to save effort or time[11]. In fact, most previous research on decision-making in voluntary task choice has been focused on the effort aspect of switch costs (see Dreisbach & Jurczyk, 2022) whereas the role of the mere passage of time itself remains unclear. Therefore, the present study investigated the effort-independent temporal costs of task switches by manipulating the duration following task switches to produce contexts where switching is consistently associated with either larger or smaller temporal costs.

The relevance of the temporal costs for voluntary task choice has already been implied by studies using the self-organized task-switching paradigm (Mittelstädt, Miller, & Kiesel, 2018, 2019, 2022; Mittelstädt et al., 2021; Monno et al., 2021). There, the repetition stimulus was presented shortly after the switch stimulus and the stimulus onset asynchrony (SOA) was successively increased with each repetition. It turned out that participants switched tasks when the SOA approached the RT switch costs. This may suggest that participants jointly consider the temporal costs of a task switch and the manipulated waiting time in their decision to switch (Mittelstädt et al., 2021). By manipulating the time prior to the repetition stimulus, this paradigm effectively captures effects of stimulus availability on task choice. That is, it shows that participants would trade effort against waiting time. However, it does not allow for the direct dissociation of temporal costs and effort costs (without biasing choice through stimulus availability).

In the present study, we, therefore, systematically manipulated the intertrial interval (ITI) *after* task switches between blocks to alter the temporal costs associated with switching whereas the associated effort remained untouched by the manipulation. In blocks with a short duration after task switches, the participants should learn that the associated temporal costs of switching are reduced which in turn should increase the willingness to switch tasks in this context. Conversely, in blocks with a longer duration following task switches, the associated temporal costs of switching are increased and participants should switch less often. This would be first evidence that the temporal costs independent of the associated effort–costs (and independent of stimulus availability effects) play a role in the decision to switch. We conducted two experiments using a hybrid task-switching paradigm (Fröber & Dreisbach, 2017; Fröber, Jurczyk, & Dreisbach, 2021) where unrestricted free choices (participants decide which task to perform) are mixed with forced choices (predetermined task). In Experiment 1, the ITI was manipulated after both, free and forced switches, in Experiment 2, the ITI was only manipulated after forced switches. We expected a higher VSR in blocks with a shorter ITI after task switches compared to blocks with a longer ITI after switches.

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[11] We are aware that effort and time are not entirely independent because the waste of time may also have effort-related implications later on. This dependency between effort and time, however, cannot be resolved because any activity (and the absence thereof) entails opportunity costs. However, it still makes sense to investigate whether the mere time costs (even if effort-related later on) have an impact independent of the associated effort itself.

## Experiment 1

### Method

#### *Participants*

Experiment 1 was preregistered (https://aspredicted.org/hm4xx.pdf). Data collection and analysis were performed in accordance with the preregistration protocol. A power analysis performed using G*Power 3.1.9.7 revealed that at least 45 participants are needed to detect a medium effect ($d$=0.5) in a one-tailed paired t-test with a power of .95 (referring to the key analysis of the VSR). To detect even smaller effect sizes, we preregistered and collected a larger sample of 88 participants. Two participants had to be excluded (for exclusion criteria, see Data Preprocessing) resulting in a final sample of $N$=86. The mean age was 21.52 years ($SD$=2.69) ranging from 18 to 33 years. Sixty-nine participants were female (16 male; 1 N/A) and 79 were right-handed (6 left-handed; 1 ambidextrous). All participants provided informed consent in accordance with the ethical standards of the national research committee and with the 1964 Helsinki declaration and its later amendments. The sample was not restricted to students but consisted mainly of psychology students (45) who received course credit for their participation. No other form of compensation was given.

#### *Apparatus and Stimuli*

The online experiment was programmed in lab.js (Henninger et al., 2022) and hosted on the platform Open Lab (Shevchenko, 2022). The stimuli were adopted from the study of Fröber and Dreisbach (2017). In the number task, participants had to categorize numbers (125, 132, 139, 146; 160, 167, 174, 181) as smaller or larger than 153. In the letter task, letters (B, D, F, H; S, U, W, Y) had to be categorized as closer to A or Z in the alphabet. Stimuli were presented in black ink (5.33% of the screen height in the default sans-serif font) on a white background. One task was always presented above the fixation cross while the other task was presented below the fixation cross (shifted by 12.50% of the screen height in the respective direction). Participants always had to respond to the upper task with their left hand according to an intuitive mapping (Dehaene et al., 1993), e.g., whether the number was smaller (G key; left middle finger) or larger (H key; left index finger) than 153, and to the lower task with their right hand, e.g., whether the letter was closer to A (K key; right index finger) or closer to Z (L key; right middle finger). The mapping between task and location (related to response hand and keys) was counterbalanced across participants.

#### *Procedure*

The experiment began with a short practice phase where both tasks were first introduced in separate blocks (8 trials each). Next, participants were familiarized with standard task switching between the two tasks (forced-choice: only one stimulus presented per trial; 16 trials) followed by a voluntary task-switching practice (free-choice: both stimuli presented in each trial; 16 trials). Throughout the experiment, no randomness instruction (e.g., performing both tasks equally often in a random order like a coin flip) was used, therefore, giving participants the unrestricted free choice between tasks. In all practice blocks, a single trial started with a fixation cross in the center of the screen (500 ms), followed by the target stimulus or stimuli until a response was made. Only in the practice phase, feedback was provided for 1000 ms in the form of the German words for correct ("Richtig!") and error ("Fehler!"). The intertrial interval (ITI) consisted of a blank screen for 1000 ms.

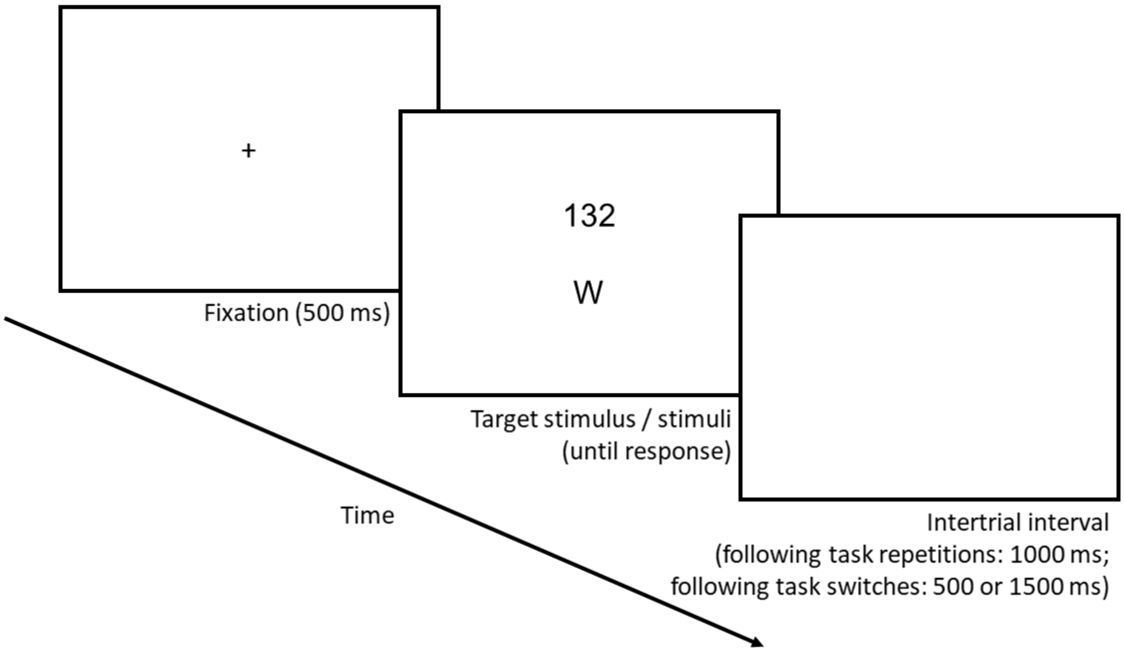
In the experimental phase, a hybrid task-switching paradigm (Fröber & Dreisbach, 2017) was employed with forced-choice and free-choice trials intermixed (50:50). Within the forced-choice trials, the switch rate was set to 50%. The first trial was always a forced-choice trial. The ITI after forced- and free-choice task switches was manipulated between blocks. There were eight experimental blocks (80 trials each) alternating between a short interval following task switches (500 ms) and a long interval following task switches (1500 ms). The interval after task repetitions always lasted 1000 ms. The duration condition in the first block was counterbalanced across participants. To increase the experienced association between the task switch and the subsequent ITI, no error feedback was provided after each trial. Only after each block, participants were informed about their error rate in the previous block, and the task rules were repeated[12]. A single trial started with a fixation cross (500 ms) followed by the stimulus or stimuli (until response) and the ITI (1000 ms after repetitions, 500 or 1500 ms after task switches; see Figure 1).

After the experimental phase, participants were told that the time interval between trials was sometimes longer or shorter. They were asked whether they noticed anything about the interval. Of the final sample of $N$=86, 23 participants generally noticed that the time of the interval differed. Only one participant noticed a relationship between the interval and the task transition. None of the participants were able to report the specific manipulation where the interval after task switches was shorter in some blocks and longer in others. Finally, participants were asked to describe the entire experiment as detailed as possible. This last question referred to an unrelated research question and will not be discussed here. One session lasted approximately 40 minutes.

[12] Depending on the magnitude of the error rate, participants were told to continue as before (with an error rate <10%), to avoid errors (with an error rate ≥10% and <20%), to pay more attention because many errors were made (with an error rate ≥20% and <30%), and to read the task rules again because too many errors were made (with an error rate ≥30%).

**Figure 1**

*Schematic Depiction of a Single (Free-Choice) Trial Sequence in Experiment 1.*




*Note.* The intertrial interval following task switches was manipulated between blocks and lasted either 500 or 1500 ms within an entire block.

### *Design*

The VSR (in %) was analyzed as a function of the within-subjects factor duration condition (short duration after task switches, long duration after task switches). For the mean RT in forced-choice trials (in ms) a 2 (duration condition) x 2 (transition: task repetition, task switch) repeated-measures design was applied. A significant modulation of the switch costs (and thereby potentially the associated effort) could limit conclusions about the mere temporal costs. For completeness, we also report the mean error rate in forced-choice trials (in %) applying the same design as for the RT. In the Supplemental Material, we provide additional violin plots containing individual data points, descriptive statistics for RT and error rates, and exploratory analyses of the performance (RT and error rate) in voluntary trials. All raw data files associated with this article are available online under the following link: http://doi.org/10.5283/epub.58125. Additional study materials of all experiments (lab.js experiment files, SPSS analysis scripts) will be shared upon request.

## Results

### *Data Preprocessing*

All free-choice trials were used to calculate the VSR. Because the first trial was always a forced-choice trial, no free-choice trials had to be excluded. The task choice was derived from the response hand. Error trials were included to capture any attempts of deliberate task switching (Arrington & Logan, 2004). On error trials, the chosen task was also derived from the response hand because responding with the wrong hand is less likely than responding with the wrong finger (Scheffers & Coles, 2000).

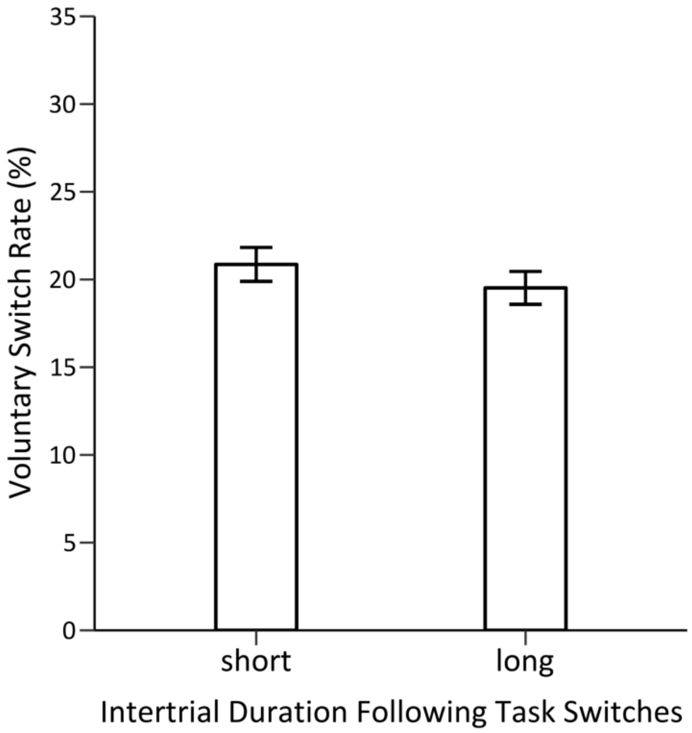
For the forced-choice error rate analysis, the first trial (2.50% of all forced-choice trials) was excluded. For the RT analysis, we additionally excluded error trials (4.61%), trials following errors (3.48%), RTs faster than 100 or slower than 8000 ms (0.03%), and RTs deviating more than 3 standard deviations from the individual cell mean (1.71%). In the Supplemental Material, we reported exploratory analyses of the performance (RT and error rate) in voluntary trials. Two participants had to be excluded prior to the final analyses due to extreme overall error rates (20.47%; 23.28%) more than 3 interquartile ranges above the third quartile. No participant displayed extreme RTs according to the same criterion.

***VSR***

The paired-sample t-test of the VSR was significant $t(85)=2.81$, $p=.003$, $d=0.30$. Participants switched tasks more often in blocks with a short duration after task switches ($M=20.86\%$, $SD=8.95$) compared to blocks with a long duration after task switches ($M=19.52\%$, $SD=8.69$; see Figure 2).

**Figure 2**

*Mean Voluntary Switch Rate (VSR, in %) as a Function of Duration Condition (Short Duration After Task Switches=500 ms, Long Duration After Task Switches=1500 ms) in Experiment 1.*




*Note.* Error bars represent ± one standard error of the mean.

### *RT*

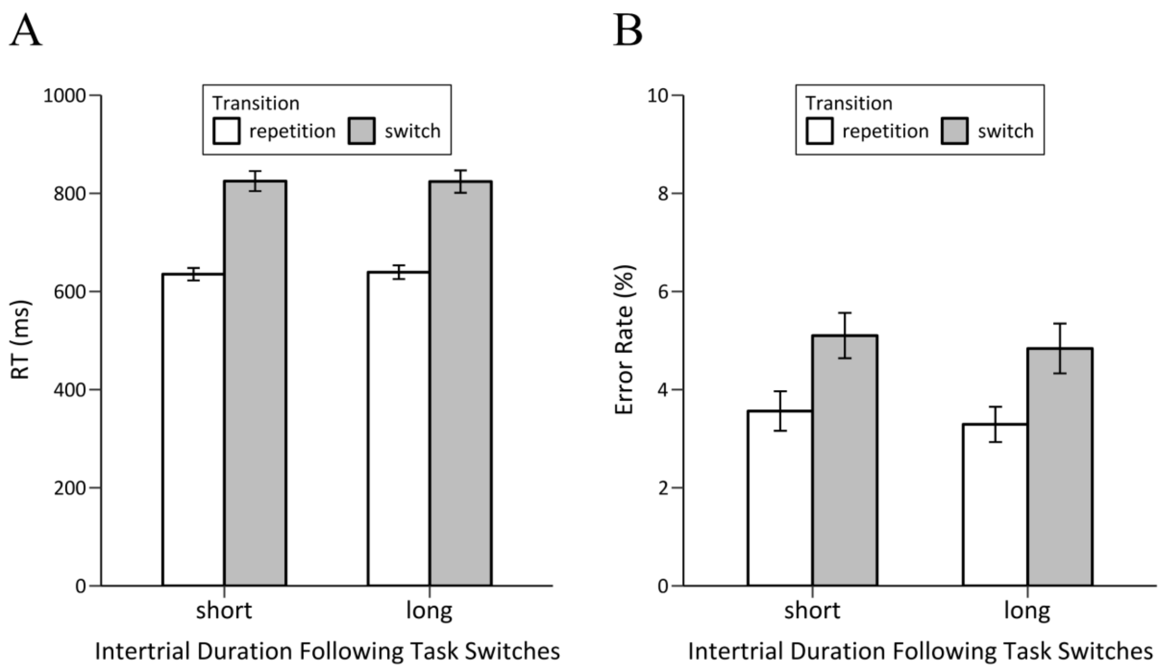
The 2 (duration condition) x 2 (transition) repeated-measures ANOVA of the RT revealed a significant main effect of transition $F(1, 85)=274.99$, $p<.001$, $\eta_p^2=.76$. Participants responded faster on repetitions ($M$=637 ms, $SD$=120) compared to task switches ($M$=825 ms, $SD$=196) indicating the typical switch costs. The main effect of the duration condition and the interaction were not significant (all $F$s<0.27, all $p$s>.602; see Figure 3, Panel A).

### *Error Rate*

For completeness, we additionally report a 2 (duration condition) x 2 (transition) repeated-measures ANOVA of the error rates which was not included in the preregistration. There was a significant main effect of transition $F(1, 85)=32.26$, $p<.001$, $\eta_p^2=.28$. Participants made less errors on task repetitions ($M$=3.43%, $SD$=3.28) than on task switches ($M$=4.97%, $SD$=4.23). The main effect of duration condition and the interaction were not significant (all $F$s<1.66, all $p$s>.201; see Figure 3, Panel B).

**Figure 3**

*Mean Reaction Time (RT, in ms; Panel A) and Error Rates (in %; Panel B) as a Function of Duration Condition (Short Duration After Task Switches=500 ms, Long Duration After Task Switches=1500 ms) and Transition (Task Repetition, Task Switch) in Experiment 1.*




*Note.* Error bars represent ± one standard error of the mean.

## Discussion

The results of Experiment 1 showed that participants voluntarily switched tasks more often in those blocks where the ITI following switches was shorter. The intertrial manipulation did not appear to modulate the RTs, error rates, or the overall evident switch costs. The findings can be taken as evidence that the temporal cost of task switches (independent from the associated effort) is a factor of its own that modulates the decision to switch. Even though the effect is small in absolute terms, it is still remarkable because our manipulation worked against the usually observed finding that a generally *longer* ITI is associated with higher VSRs due to longer preparation time and passive decay of task set activation (Arrington & Logan, 2004, 2005; Liefooghe et al., 2009; Yeung, 2010). However, in the present study, the condition with longer durations following task switches resulted in lower VSRs compared to the condition with shorter ITIs following task switches. Thus, the temporal costs of a task switch relative to a task repetition appeared to outweigh the typical overall ITI effect. Critically, because the switch costs were unaffected by the ITI manipulation, we can exclude that the observed effect originated from the actual switch costs. This means that the effort costs associated with switching which are partially reflected in the switch costs, presumably remained comparable across ITI conditions.

Note that we manipulated the ITI to alter the temporal costs that become associated with switching in a given context (block). These associated temporal costs should be integrated with other costs (e.g., effort cost) and benefits (Richter et al., 2016; Shenhav et al., 2021), thereby biasing the willingness to switch. However, because in Experiment 1, the ITI was manipulated after both forced and free choices, the VSR effect may also reflect overt strategic behavior. Participants may have switched more often to reduce the waiting time (the ITI) for the next trial in blocks with shorter intervals after task switches (although the post-experiment questionnaire where no participant noticed the exact manipulation speaks against this notion). Based on Experiment 1, it is therefore unclear whether the temporal costs associated with (forced) task switching in a given context can influence decision-making even when this behavior is no longer profitable. Therefore, in Experiment 2, we manipulated the ITI only after *forced* choices. This way, participants could no longer reduce the overall time spent on the experiment by choosing to switch. Still finding higher VSRs in blocks with shorter ITIs after forced switches would therefore provide direct evidence that the temporal costs associated with switching in a given context influence the decision to switch.

## Experiment 2

In Experiment 2, we manipulated the ITI after task switches only in forced-choice trials and kept the ITI after free choices constant for both task switches and repetitions. To ensure a comparable experience with the ITI manipulation of Experiment 1, we additionally increased the proportion of task switches in forced-choice trials. If participants learn to associate the temporal costs of the ITI with switching (or repeating), they should again switch tasks more often voluntarily with a short ITI after forced switches, even if they cannot strategically save time by doing so.

### Method

#### *Participants*

Experiment 2 was preregistered (https://aspredicted.org/q4bd7.pdf). As in Experiment 1, a sample of 88 participants was preregistered and collected. Three participants had to be excluded (for exclusion criteria see Data Preprocessing) resulting in a final sample of $N$=85. The mean age was 23.00 years ($SD$=4.62; range: 18-46). Sixty-seven participants were female (18 male) and 74 were right-handed (11 left-handed). All participants gave informed consent. The sample was not restricted to students but consisted mainly of psychology students (41) who received course credit for their participation. No other form of compensation was given.

#### *Apparatus, Procedure, and Design*

The procedure of Experiment 2 was very similar to that of Experiment 1. The same tasks and stimuli were used in a hybrid task-switching paradigm with forced-choice and free-choice trials intermixed (50:50). The main difference was that the ITI following task switches was manipulated only in forced-choice trials. Again, eight blocks with 80 trials each alternated between a short interval after forced switches (500 ms) and a long interval after forced switches (1500 ms). The ITI after free choice (for both task switches and repetitions) and after forced repetitions was set to 1000 ms. The second difference was that the switch rate within the forced-choice trials was increased to 75% in order to produce a similar frequency of ITI manipulations after task switches as in Experiment 1 to ensure learning. After the experiment, participants were asked if they noticed anything about the time interval between trials. In the final sample of *N*=85, 40 participants generally noticed that the time of the interval differed. Two participants noticed a relationship between the interval and the task transition. None of the participants were able to report the specific manipulation where the interval after forced task switches was shorter in some blocks and longer in others. The analysis design remained the same as in Experiment 1. In the Supplemental Material, we report additional violin plots containing individual data points, descriptive statistics of the RT and error rates, exploratory analyses of the performance (RT and error rate) in voluntary trials, and an exploratory analysis of the VSR when excluding voluntary trials directly after forced choice.

**Results**

***Data Preprocessing***

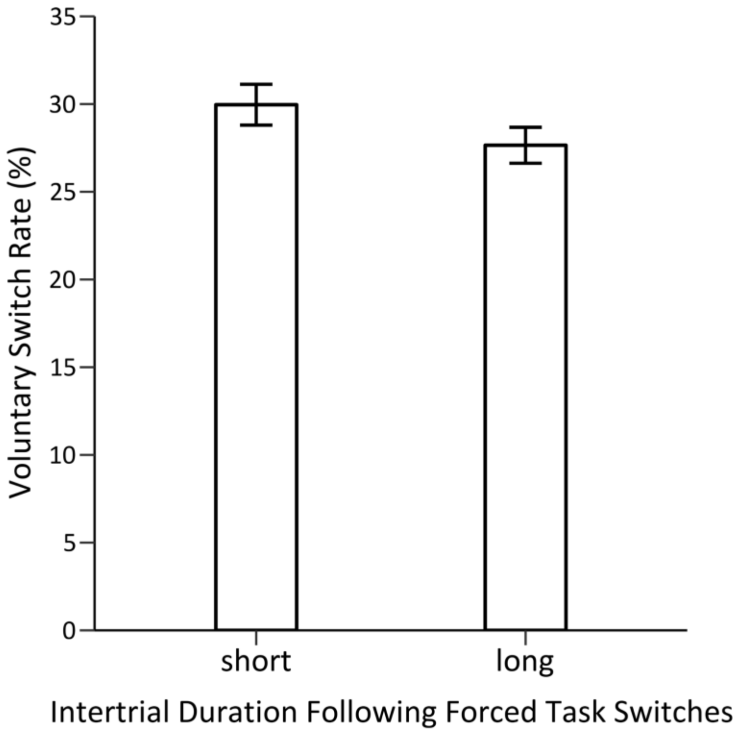
The VSR was calculated as in Experiment 1. Prior to the forced-choice error rate analysis, we excluded the first trial of each block (2.50% of all forced-choice trials). For the RT analysis, we additionally excluded error trials (3.86%), trials following errors (2.69%), RTs faster than 100 or slower than 8000 ms (0.02%), and RTs deviating more than 3 standard deviations from the individual cell mean (1.72%). Three participants had to be excluded before the final analyses due to extreme overall error rates (12.81%) or RTs (1495 ms, 1681 ms) more than 3 interquartile ranges above the third quartile.

***VSR***

The paired sample t-test of the VSR per duration condition was significant, $t(84)=3.47$, $p<.001$, $d=0.38$. Participants switched tasks more often in blocks with a short duration after forced task switches ($M$=29.96%, $SD$=10.71) than in blocks with a long duration after forced task switches ($M$=27.65%, $SD$=9.44; see Figure 4). Additionally, when excluding voluntary trials directly after forced choice trials to avoid potential influences of the preceding ITI on the VSR, the effect was still significant ($p$ = .030; see Supplemental Material).

**Figure 4**

*Mean Voluntary Switch Rate (VSR, in %) as a Function of Duration Condition (Short Duration After Forced Task Switches=500 ms, Long Duration After Forced Task Switches=1500 ms) in Experiment 2.*




*Note.* Error bars represent ± one standard error of the mean.

### *RT*

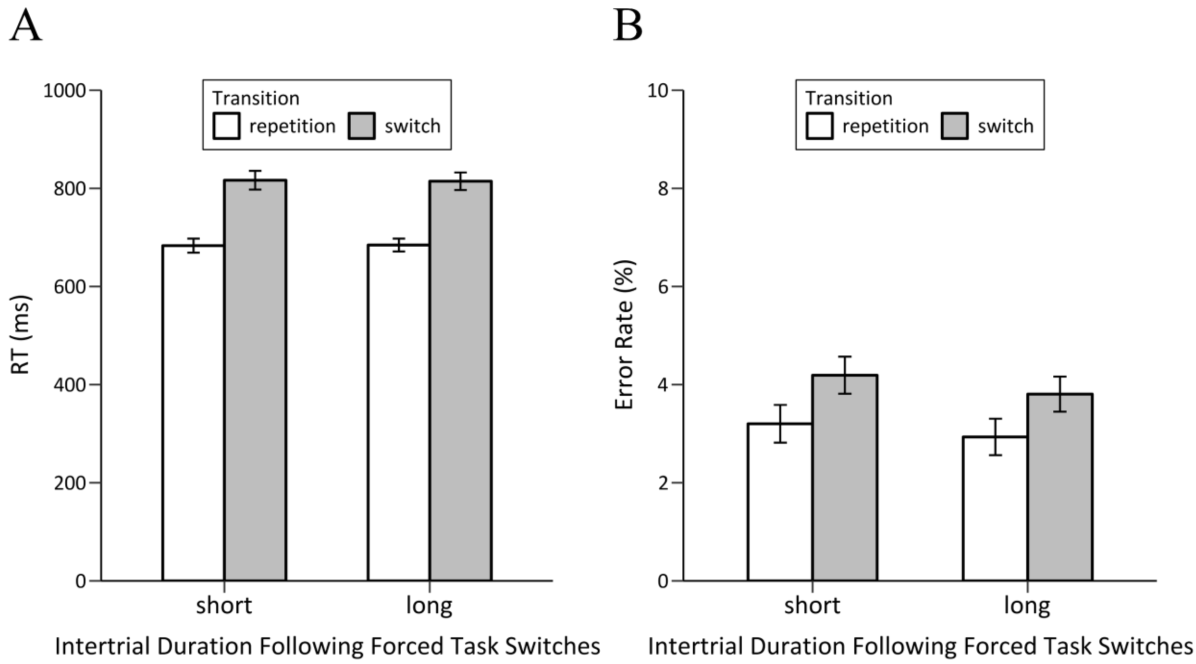
The 2 (duration condition) x 2 (transition) repeated-measures ANOVA of the RT showed the significant main effect of transition $F(1, 84)=254.81$, $p<.001$, $\eta_p^2=.75$. Reaction times were faster for task repetitions ($M$=684 ms, $SD$=123) than for task switches ($M$=816 ms, $SD$=168). The main effect of the duration condition and the interaction were not significant (all $F$s<0.15, all $p$s>.696; see Figure 5, Panel A).

### *Error Rate*

The 2 (duration condition) x 2 (transition) repeated-measures ANOVA of the error rates revealed a significant main effect of transition $F(1, 84)=11.44$ $p=.001$, $\eta_p^2=.12$. Participants made fewer errors on task repetitions ($M$=3.07%, $SD$=2.85) than on task switches ($M$=4.00%, $SD$=3.18). The main effect of duration condition and the interaction were not significant (all $F$s<1.59, all $p$s>.210; see Figure 5, Panel B).

**Figure 5**

*Mean Reaction Time (RT, in ms; Panel A) and Error Rates (in %; Panel B) as a Function of Duration Condition (Short Duration After Forced Task Switches=500 ms, Long Duration After Forced Task Switches=1500 ms) and Transition (Task Repetition, Task Switch) in Experiment 2.*




*Note.* Error bars represent ± one standard error of the mean.

## Discussion

The results of Experiment 2 mirrored those of Experiment 1. [13] Participants switched tasks more often in blocks with a shorter ITI after forced task switches. The RTs, error rates, and switch costs were again not affected by the ITI modulation. Because the ITI remained constant after free choices, it was no longer profitable to switch tasks more often in blocks with a shorter ITI after forced task switches. This suggests that the temporal costs associated with switching in a given block influenced the decision to switch.

## General Discussion

The present study investigated whether task switches are avoided because of their associated temporal costs. In a hybrid task-switching paradigm, we manipulated the ITI following task switches (Experiment 1: forced and free; Experiment 2: only forced) between blocks to modulate the associated

[13] Please note that the increased switch rate within the forced-choice trials in Experiment 2 resulted in descriptively reduced switch costs and increased VSRs compared to Experiment 1. It is well-documented that the higher the forced switch rate, the smaller the switch costs and the higher the VSR (see Fröber & Dreisbach, 2017).

temporal costs independent of the effort costs associated with switching. Consistent with the hypothesis, the results of both experiments showed that participants were more inclined to switch tasks in blocks with a shorter duration following task switches. Taken together, the associated temporal costs influence the decision to switch and are a relevant factor for the typical avoidance of task switches.

The present results are in line with an economic perspective on task choice behavior (Kool & Botvinick, 2018; Shenhav et al., 2021). Investing cognitive control in the form of a task switch requires sufficient justification because cognitive control incurs aversive effort costs (Silvestrini & Gendolla, 2019). To some extent, these associated effort costs are implied by the switch costs which may explain why participants with higher switch costs tend to switch tasks less often (Mayr & Bell, 2006; Mendl & Dreisbach, 2022; Mittelstädt, Dignath, et al., 2018; Mittelstädt, Miller, & Kiesel, 2018, 2019). However, in addition to effort costs, switching also incurs temporal costs as a task switch takes longer than a task repetition. Consequently, in the present study, we uncovered that the associated temporal costs independent of effort can shift the cost-benefit analysis either in favor of switches or repetitions. As predicted, participants switched tasks more often in blocks where task switches were associated with (relatively) lower temporal costs. This finding is consistent with previous studies showing that the duration of a given action represents a cost factor in decision-making (Janczyk et al., 2022; Potts et al., 2018).

It is important to note that in both experiments the effect of the ITI manipulation on the VSR (Experiment 1: VSR difference of 1.34%, $d$=0.30; Experiment 2: VSR difference of 2.31%, $d$=0.38) was small (J. Cohen, 1988). Considering the average ITI duration per condition, our timing manipulation worked against the general effect that longer ITIs result in increased VSRs due to longer preparation time and passive decay of task set activation (Arrington & Logan, 2004, 2005; Liefooghe et al., 2009; Yeung, 2010). Presumably, if the present finding reflects the sum of the two effects (relative temporal costs vs average ITI), the actual effect of the temporal costs should be somewhat larger. A future study may manipulate both, the time following task repetitions and the time following task switches inversely, to get the pure effect of the associated temporal costs without influences of the general ITI effect.

Additionally, it could be argued that the manipulated time difference of the ITI after task switches relative to repetitions (±500 ms) was too small. A larger time difference between repetitions and switches resulting in larger temporal costs could potentially have a stronger effect on the decision to switch. Future studies are needed to test whether and how the VSR scales with the magnitude of the time difference. However, the used time difference of the ITI following switches relative to repetitions (±500 ms) was already much longer than the evident temporal costs of a task switch (Experiment 1: 188 ms; Experiment 2: 132 ms). If the temporal costs would be the main influence on the motivation to switch tasks, the resulting VSR with a short interval after task switches should have been higher than 50% to indicate an actual preference for task switches. Because participants still preferred repetitions, there appear to be additional factors that drive the avoidance of switching. Here, the prominent effort costs of a task switch come to mind (Dreisbach & Jurczyk, 2022; Kool et al., 2010). In other words, even when the temporal costs of a task switch are outweighed by the shorter ITI following switches, participants are overall still reluctant to engage in the effort of switching. In conclusion, the present findings suggest that both temporal costs and effort costs contribute to the avoidance of switching.

Another implication relates to the volatile nature of the present experimental procedure. The duration following task switches alternated each block while the blocks were rather short (80 trials). We chose this method to reduce the influence of time-on-task effects on the VSR. Still, participants were able to adjust to the rapidly changing context. As a result, the associated time costs of a task switch in the current block influenced choice behavior. In contrast, previous studies showed that the learning of transition associations in the task-switching paradigm can take very long and requires multiple days of training (Xu et al., 2023; for a review see Braem et al., 2024). The temporal costs may be a particularly important feature for the participants allowing them to learn the association with task switches more quickly. This implies that task-relevant features are learned more easily or are more likely to influence behavior (see Hommel, 2022; Mendl et al., 2024).

It is possible, that by manipulating the ITI following task switches between blocks, we modulated the introspective switch costs accordingly. While introspection about the costs in the dual-tasking paradigm is rather inaccurate (Bratzke & Bryce, 2016; Bryce & Bratzke, 2017), participants' estimations correctly display introspective switch costs (Bratzke & Bryce, 2019; Mendl & Dreisbach, 2022). It is not entirely clear whether this introspection plays a role in voluntary task choice or whether it is an epiphenomenon. Previous research indicated that, under specific circumstances, introspective switch costs are related to the VSR (Mendl & Dreisbach, 2022). In the present study, the ITI manipulation did not influence the objective switch costs but it may have influenced the introspective switch costs and thereby modulated the motivation to switch.

A potential mechanism behind the present effect of the temporal costs may be operant conditioning. If we assume that time is precious in general, the current ITI manipulation might have been perceived as reward or punishment. In blocks with a shorter duration following (forced) task switches participants may have felt rewarded for switching, and in the other blocks with longer durations following switches participants may therefore have felt punished. Therefore, through operant conditioning (Thorndike, 1911), participants show rewarded behavior more often in free choice. In this regard, the present findings are in line with the findings by Braem (2017), who rewarded participants either for switching or repeating and then found higher/lower VSRs in a transfer phase without reward. Generally, operant conditioning and reinforcement learning are closely related to economic choice behavior where expected rewards and costs guide behavior. In economic perspectives, temporal costs are especially relevant because they directly relate to the reward rate (reward per time; see Held et al., 2024; Leng et al., 2021). Understanding the reward aspect of temporal costs and the relationship between operant conditioning and economic accounts may be a critical endeavor for future research.

Regarding the question of why longer durations are perceived as punishment, a recent study showed that participants often prefer to do something, even something effortful, over doing nothing (Wu et al., 2021). Hence, doing nothing for a certain amount of time is perceived as aversive, sometimes even more aversive than effort. The higher switch rates in our paradigm revealed a similar preference for effort over waiting time even though we have no indication that participants were aware of this manipulation. Participants are more inclined to go for an effortful task switch when the alternative of repeating tasks is associated with a longer duration. The aversiveness of longer durations may be explained by the concept of opportunity costs (Kurzban et al., 2013). A longer time spent on one activity represents a missed opportunity to allocate that time toward a more beneficial alternative. Therefore, doing nothing for a certain time incurs an opportunity cost equal to the potential reward of alternative actions. This rationale can elucidate how the economic avoidance of temporal costs of switching emerges.

To conclude, the present study showed that in blocks with shorter intervals following task switches participants were motivated to switch tasks more often voluntarily. This provides direct evidence that task switching is not only avoided due to the associated effort cost but also due to the associated temporal costs. It follows that effort and time are both part of the equation in the cost-benefit analysis of voluntary task choice.

# Study 3:

# Flexibility by Association? No Evidence for an Influence of Cue-Transition Associations on Voluntary Task Switching

Jonathan Mendl, Kerstin Fröber, & Gesine Dreisbach

Mendl, J., Fröber, K., & Dreisbach, G. (2024). Flexibility by association? No evidence for an influence of cue-transition associations on voluntary task switching. *Journal of Experimental Psychology: Human Perception and Performance, 50*(3), 313–328.



**Abstract**

Some situations require cognitive flexibility, whereas others call for cognitive stability. Recent theories posit lower-level associative learning processes as the basis of contextual control. The present study incorporates six experiments to investigate whether cognitive flexibility can be triggered by task-irrelevant color cues in the task-switching paradigm. In a first learning phase, the cue colors were repeatedly paired with certain task transitions (repetition, switch) without explicit instruction. In the following test phase, voluntary trials were intermixed (where participants can freely choose the task) to measure the voluntary switch rate in response to the color cues. For Experiment 2a, cue size and duration were increased, and the learning phase was extended. Additionally, in Experiment 2b, the second half of the test phase consisted of 100% free choices. Experiment 3 contained catch trials to ensure cue processing. In Experiment 4, two tasks of unequal difficulty were used. Experiments 1-4 provided evidence for the null hypothesis indicating no effect of the transition association on the voluntary switch rate (all $BF_{10} < 0.265$). The control Experiment 5 ruled out that the null effect was due to the insensitivity of the paradigm. Therefore, flexibility by association appears to be harder to achieve than recent accounts suggest.

Keywords: cognitive control, voluntary task switching, associative learning

## Public Significance Statement

Cognitive flexibility and especially the ability to quickly switch between different tasks is an important requirement in everyday life, be it at work, at school, or during leisure time. Here, we investigate whether such cognitive flexibility can be enhanced by basic associative learning processes, as has been recently suggested. Our results from six experiments, however, dampen the enthusiasm as they provide no evidence that cognitive flexibility can be easily triggered by cues that had been paired with flexible behavior in the past. Instead, our results suggest that humans need more informative cues to enhance flexibility.

## Introduction

Many situations in everyday life require cognitive flexibility. For example, when answering various emails, we frequently need to switch from one topic to the next in order to complete the task effectively. Other situations like reading a book or a research article require cognitive stability: We need to focus on the task at hand and block out all possible distractions. People are typically able to adjust to the different demands of these two scenarios. This raises the question of how the cognitive system knows when to use a more flexible or more stable control mode. Recent theories suggest associative learning as the central mechanism that guides cognitive control (Abrahamse et al., 2016; Braem & Egner, 2018). The present study investigated whether cognitive flexibility can be triggered by association. More precisely, we investigate whether a (task-irrelevant) cue that is associated with flexibility in a learning phase would trigger flexibility in a subsequent test phase.

Cognitive control is often described as a dynamic balance between flexibility and stability (Dreisbach & Fröber, 2019; Goschke, 2013; Hommel, 2015; Musslick & Cohen, 2021). Cognitive flexibility facilitates switching between different goals and adapting to changing task demands, but also increases distractibility. On the other hand, cognitive stability is associated with increased task focus and goal shielding, which is accompanied by increased rigidity, where goal-relevant opportunities in the environment may be overlooked (Dreisbach & Wenke, 2011). Because both control modes have complementary advantages and disadvantages, effective behavior depends on the right balance between them. In classical theories, cognitive control is guided in a top-down manner (Diamond, 2013) for example by a supervisory attentional system (Norman & Shallice, 1986). These theories make a clear distinction between automatic and controlled processes, and cognitive control has the purpose of counteracting automatic processes. In contrast, recent theories argue that cognitive control can be triggered by low-level learning mechanisms in a bottom-up fashion (Abrahamse et al., 2016; Braem & Egner, 2018; Bugg & Egner, 2021). When a certain control state is repeatedly activated in a particular context (referring to aspects of the stimulus and/or of the response, or contextual, task-irrelevant features), reciprocal associations are formed. In an associative network, control state representations are bound to the context and can subsequently be triggered by these contextual features (for reviews, see Bugg, 2017; Bugg & Crump, 2012).

A perfect tool for investigating the balance between flexibility and stability is the task-switching paradigm (for reviews, see Kiesel et al., 2010; Monsell, 2003; Vandierendonck et al., 2010). Participants are required to perform two or more simple cognitive tasks. From one trial to the next, the given task can either be repeated or switched. Typically, responses to task switches are slower and more error-prone than responses to task repetitions (switch costs). This finding is often explained by task-set reconfiguration processes when switching tasks (Rogers & Monsell, 1995) or interference from the previous task set (Allport et al., 1994). Regarding the flexibility-stability balance, higher switch costs are interpreted as a sign of lower cognitive flexibility and vice versa (Dreisbach & Fröber, 2019). In this context, the voluntary task-switching paradigm provides an additional measure of the current control mode, the voluntary switch rate (VSR; Arrington & Logan, 2004, for a review, see Arrington et al., 2014). Here, participants can choose for themselves which task to perform on a given trial. The VSR indicates how often participants voluntarily switch tasks. Higher VSRs are taken as another index of increased cognitive flexibility.

The associative learning account of cognitive control posits that bottom-up processes form the basis of contextual control (Abrahamse et al., 2016; Braem & Egner, 2018; Bugg & Crump, 2012). Voluntary task-switching studies demonstrated such lower-level influences on cognitive flexibility. Mayr and Bell (2006) showed that a switch of the target stimulus made a voluntary task switch more likely, i.e., increased the VSR. Another study found similar influences of stimulus switches on the VSR even when an unattended feature of the stimulus changed (Yeung, 2010). Demanet et al. (2010) showed that the effect of stimulus switches on the VSR is more pronounced under cognitive load and that even irrelevant shape switches increase the VSR. In addition, switches of the stimulus location lead to increased VSRs (Arrington & Weaver, 2015; Mittelstädt, Miller, & Kiesel, 2018). In summary, cognitive flexibility seems to be easily triggered by simple bottom-up factors, namely changes of the stimulus or its location.

Beyond these more general lower-level influences on cognitive flexibility, the associative learning account suggests that a context can become associated with a particular control mode and later directly trigger the appropriate mode. Such *contextual* modulations of flexibility have been demonstrated in previous studies. In a context (block) of mostly task switches, participants adopted a more flexible control mode which led to reduced switch costs (Dreisbach & Haider, 2006) and a higher VSR (Fröber & Dreisbach, 2017; Fröber, Jurczyk, & Dreisbach, 2021). Similar to this list-wide switch effect, the task-switch probability can also be tied to the target location. At locations associated with a higher switch probability, the switch costs were smaller (Crump & Logan, 2010; Leboe et al., 2008). Furthermore, the switch probability can also be manipulated between items. For items associated with a higher switch probability, the switch costs are reduced (Chiu & Egner, 2017; Leboe et al., 2008) and VSRs are increased (Chiu et al., 2020). According to the associative learning account, in all these studies, the context (block, location, stimulus) influences behavior by activating the corresponding control mode.

Another direct way to trigger flexibility in a bottom-up fashion is reinforcement learning via operant conditioning. Braem (2017) rewarded either task repetitions or switches in a cued task-switching phase with high or low reward, respectively: One group received a higher reward for task switches, while the other group received a higher reward for task repetitions. In the following voluntary phase, participants who had been rewarded more for task switches showed a higher VSR than those in the other group. This finding was taken as the first evidence that flexibility can be shaped by operant conditioning.

To sum up, previous research has demonstrated contextual and bottom-up influences on the VSR. Cognitive flexibility can be triggered by features associated with task switches. And even subtle changes in stimulus features can bias cognitive flexibility. However, other studies have failed to show such bottom-up influences on flexible task choice (Arrington & Logan, 2005; Fröber & Dreisbach, 2016b; Jurczyk et al., 2021). For example, Jurczyk et al. presented task-irrelevant (meaningless) color cues before each trial in a voluntary task-switching paradigm and did *not* find any differences in VSR between cue repetitions and cue switches. This is in sharp contrast to the findings of Demanet et al. (2010), where task-irrelevant shape changes led to an increased VSR. In their study, the shape was presented simultaneously with the target and may therefore have been perceived as more task-relevant (or related to the current target). Taken together, previous studies about modulations of cognitive flexibility using task-irrelevant cues are inconclusive. Here, we will take a slightly different approach and ask whether otherwise meaningless task-irrelevant cues would modulate flexibility after these cues have been (implicitly) associated with either task switches or task repetitions. Note that these cues are useful but not necessary cues (Sudevan & Taylor, 1987). That means they are not task-relevant (because participants can perform the task without any knowledge or usage of the cues), but they are potentially useful for task performance.

### Overview of the Experiments

The present study consisted of six experiments (1, 2a, 2b, 3, 4, 5). The first five experiments began with a learning phase in which task-irrelevant color cues were repeatedly paired with a certain task transition (repeat or switch, respectively). In the subsequent test phase, the task was either predetermined (forced choice) or chosen by the participant (free choice). This way, we can investigate how the color cues associated with certain transitions would influence the VSR. Table 1 provides an overview of the general method of the experiments. Please note that because of the hybrid paradigm in the test phase, we did not have to restrict free choice by giving a typical randomness instruction which has been implemented in previous studies to obtain reasonable (sufficient) VSRs (Arrington & Logan, 2004). Only in the 100% free choice transfer phase of Experiment 2b, a randomness instruction was used. We expected to find facilitated performance with predictable transition cues in the learning phase. In the voluntary trials of the test phase, consistent with the associative learning account of cognitive control, we predicted a greater VSR in response to the cue associated with task switches compared to the cue associated with task repetitions.

**Table 1**

*Overview of the General Method of All Experiments and Further Manipulations Specific to Each Experiment*

| Experiment | Learning phase | Test phase: Mixed phase | Test phase: Transfer phase |
|---|---|---|---|
| General method (same across all experiments, except for Experiment 5) | • Forced-choice task switching<br>• Cue colors (blue, green) predict task transition | • Hybrid task switching (50% free choice, 50% forced choice)<br>• Color cues presented in forced choice (100% validity) and free choice | • Color cues only presented in free choice |
| 1 | • 100% validity of color cues<br>• Nonpredictive white cues (33.33% of trials) | | • Hybrid task switching<br>• White cues in forced choice |
| 2a | • 80% validity of color cues<br>• No white cues | | • Hybrid task switching<br>• White cues in forced choice |
| 2b | • 80% validity of color cues<br>• No white cues | | • Voluntary task switching (100% free choice)<br>• Only color cues |
| 3 | • 100% validity of color cues<br>• No white cues<br>• Interspersed catch trials | • Interspersed catch trials | • Hybrid task switching<br>• White cues in forced choice<br>• Interspersed catch trials |
| 4 | • 100% validity of color cues<br>• Nonpredictive white cues (33.33% of trials)<br>• Task switching between easy and difficult task | • Task switching between easy and difficult task | • Hybrid task switching<br>• White cues in forced choice<br>• Task switching between easy and difficult task |
| 5 | • Color cues (blue, green) predict *task identity* (not transition)<br>• 100% validity of the cues<br>• No white cues | | • Hybrid task switching<br>• White cues in forced choice |

## Experiment 1

### Method

#### *Participants*

A power analysis performed with G*Power 3.1.9.7 showed that at least 34 participants are needed to detect an effect of the cue in the repeated-measures ANOVA of the test phase with a medium effect size ($f$ = .25)[14] and a power of .80 (correlation among repeated measures set to .5). To detect even smaller effect sizes, we collected a larger sample of 50 participants.[15] The mean age was 26.04 years ($SD$ = 3.74; range 19-37). Of all participants, 33 were female (17 male), and 47 were right-handed (two left-handed, one ambidextrous). Prior to the experiment, informed consent was provided by all participants in accordance with the ethical standards of the national research committee and with the 1964 Helsinki declaration and its later amendments. Students of the University of Regensburg received course credit for their participation. One subject had to be excluded (see Data Preprocessing for exclusion criteria), resulting in a final sample of 49 participants.

#### *Transparency and Openness*

Experiment 1 was not preregistered. The preregistration status of all other experiments is indicated directly at the beginning of the respective method section. Data of Experiments 1 and 4 were collected in the year 2021, and Experiments 2a, 2b, and 3 were collected in 2022. Data of Experiment 5 was collected in 2023. All raw data files associated with this article can be found online under the following link: https://doi.org/10.5283/epub.55151. Additional study materials of all experiments (lab.js experiment files, SPSS analysis scripts) will be made available upon request.

#### *Apparatus and Stimuli*

The experiment was programmed using lab.js (Henninger et al., 2022) and hosted online for data collection via Open Lab (Shevchenko, 2022). The stimuli were adopted from Fröber and Dreisbach (2017) and consisted of eight numbers (125, 132, 139, 146, 160, 167, 174, 181) and eight letters (B, D, F, H, S, U, W, Y). In the number task, participants had to categorize the numbers as smaller (left key) or larger (right key) than 153. In the letter task, participants had to categorize the letters as closer to A (left key) or Z (right key) in the alphabet. One of the two tasks was always presented above the center of the screen and responses had to be made by pressing the “G” or “H” key with the index or middle finger of the left hand. The other task was always presented below the center of the screen and

[14] Previous studies, using the probability of task switches within the current block to induce cognitive flexibility (Fröber & Dreisbach, 2017; Fröber et al., 2021) reported medium to large effect sizes on the VSR. Due to the novelty of the current approach, we based the power analysis on a medium effect size.

[15] We used this sample size in all experiments except for Experiment 4. As the data collection of Experiment 4 took place in a practical seminar, the sample size was increased to 60 subjects (4 seminar participants had to collect 15 subjects each).

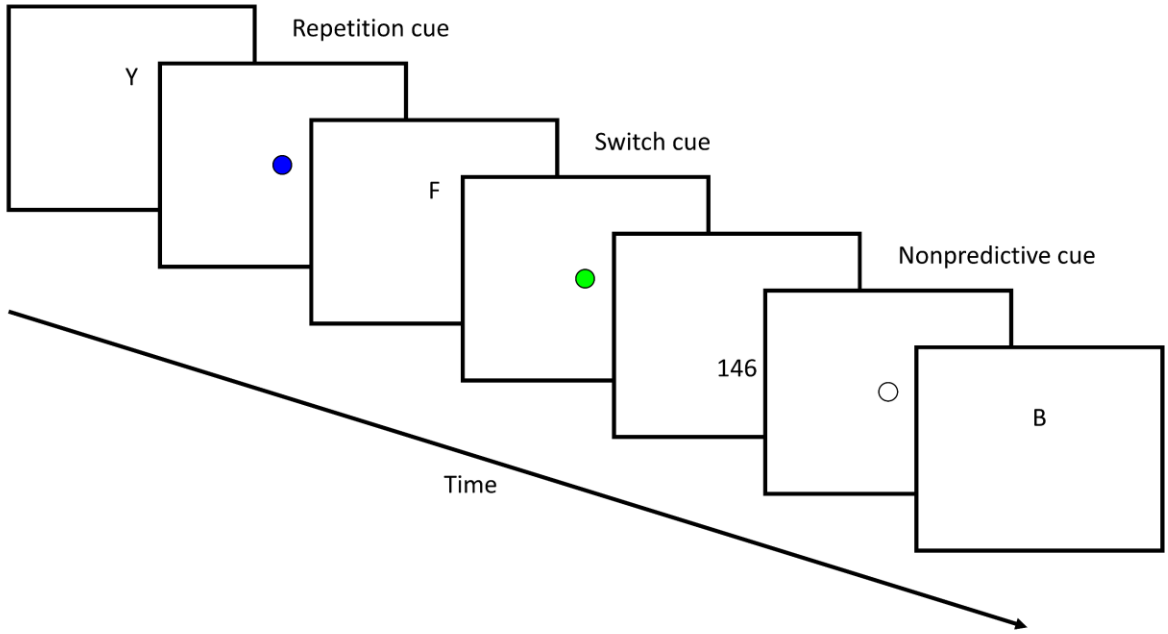
participants had to respond by pressing the “K” or “L” key with the index or middle finger of the right hand. The task-to-hand/position assignment remained the same during each session but was counterbalanced across participants. The stimuli were presented in black ink (default sans-serif font, 5.33% of the screen height) on a white background. All 16 stimuli appeared roughly equally often in pseudorandomized order with no direct stimulus repetitions. Circles (blue: RGB values 0, 0, 255; green: RGB values 0, 255, 0; white: RGB values 0, 0, 0) with a black frame and a diameter of 9.17% of the screen height were used as cues. One color always predicted task repetitions, whereas the other color always predicted task switches (color-transition association counterbalanced). The white cue always served as a neutral cue and had no predictive value.

***Procedure***

The experiment started with ten Ishihara plates to check participants for color blindness because colored cues were crucial for the main manipulation (Ishihara, 1918). Next, participants had to complete a short practice phase. First, the two tasks were introduced in two separate practice blocks (eight trials each). This was followed by a task-switching practice (16 trials) where the tasks repeated and switched in random order. By presenting only one stimulus per trial, the current task was predetermined (forced choice). Last, participants were introduced to voluntary task switching (16 trials). Here, stimuli from both tasks were presented on the screen simultaneously and participants could freely decide which task to perform on a given trial. Next, in the learning phase, participants completed two blocks of 96 forced-choice trials each. Here, each stimulus was preceded by a cue. Green or blue cues were perfectly associated with a certain task transition (repetition, switch), whereas white cues were nonpredictive (see Figure 1). The three cues were presented equally often, resulting in approximately equal numbers of task repetitions and switches. The first trial always started with a white cue because no task transition can be predicted here.

**Figure 1**

*Schematic Depiction of the Three Different Cues in the Learning Phase of Experiment 1*



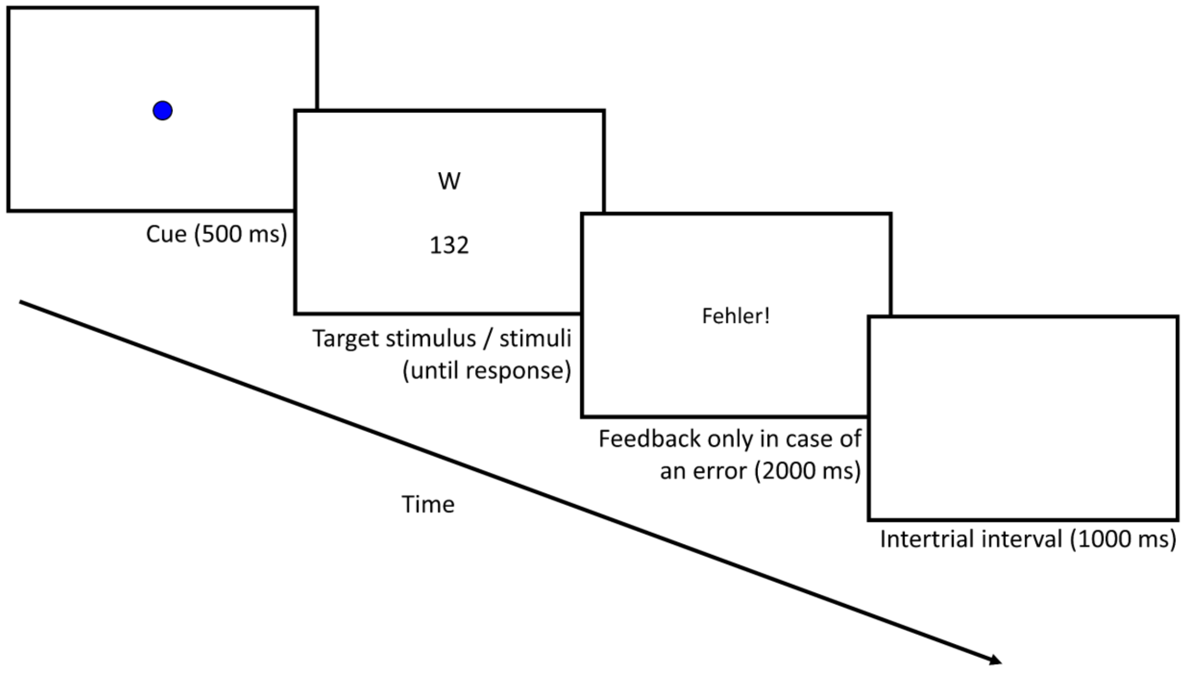

*Note.* One color always predicted task repetitions and the other color task switches (association counterbalanced across participants). After white, nonpredictive cues, both transition types followed equally often.

The subsequent test phase was divided into a mixed phase and a transfer phase, both using the hybrid task-switching paradigm with intermixed forced- and free-choice trials. In the free-choice trials, participants were free to choose which task to perform without any restriction in the form of a randomness instruction. In the forced-choice trials, the task switched in roughly 50 % of the trials. In the mixed phase (two blocks, 128 trials each), the cues were always colored (no white cues) and still predicted the following task transition in the forced-choice trials. Only the first trial, which was set to be a forced-choice trial, was preceded by a white cue. In free-choice trials, both cue colors were presented equally often. Because in this mixed phase, strategic usage of the cues to prepare for the upcoming task transition would still be adaptive half of the time (namely in the forced-choice trials), we also implemented the following transfer phase (two blocks, 128 trials each), where the colored cues were only presented before free-choice trials. In forced-choice trials, the cues were always white. Hence, this allowed us to measure the longevity of the cue-transition association. After the experiment, participants were asked whether they noticed anything about the colored circles. One session of the experiment lasted approximately 35 minutes.

A single trial of the practice phase started with a fixation cross for 500 ms, followed by the stimulus/stimuli above and/or below the center of the screen until a response was made. After correct responses, the feedback appeared for 500 ms in the form of the German word for correct ("Richtig!"). After errors, the feedback appeared for 1500 ms and consisted of the German word for error ("Fehler!"). During the intertrial interval, a blank screen was presented for 1000 ms. In the learning and test phase, the trial structure was largely identical. Instead of the fixation cross, the cue (white, green, or blue circle) was presented, and the feedback was shown only after errors for 2000 ms (see Figure 2).

**Figure 2**

*Schematic Depiction of a Single (Free-Choice) Trial Sequence in Experiment 1*




### *Design*

In the learning phase, for the dependent variables error rate (in %) and reaction time (RT; in ms), we used a 2 (Cue type: predictive, nonpredictive) x 2 (Transition: task repetition, task switch) repeated-measures design. In the test phase, the VSR (in %) was the dependent variable of interest in a 2 (Cue association: repetition, switch) x 2 (Phase: mixed phase, transfer phase) repeated-measures design. Higher VSRs after (implicit) switch cues would support the notion that flexibility can indeed be learned by association.

We did not examine the error rate or RT in the test phase for two reasons. First, in forced-choice trials, the cues were always predictive of the transition (thus, no analysis of the cue association was possible). Second, in free-choice trials, the number of trials per design cell varied greatly.

## Results

### *Data Preprocessing*

In the learning phase, to preprocess the data for the error rate analysis, we excluded the first trial of each block (1.04% of all trials in this phase). For the RT analysis, we additionally excluded erroneous trials (4.67%), trials following errors (4.44%), RTs faster than 100 and slower than 8000 ms (0.39%), and RTs more than 3 standard deviations above or below the individual cell mean (1.56%).

In the voluntary trials of the test phase, task choice was determined based on the chosen response hand. The VSR was calculated as the rate of voluntary task switches amongst the free choices. We included erroneous trials to capture all attempts of deliberate task switching (Arrington & Logan, 2004). In error trials, the chosen task was also derived from the response hand because responding with the wrong hand is less likely than responding with the wrong finger (Scheffers & Coles, 2000).

For all experiments, we defined the following outlier criteria: more than 50% errors in the Ishihara test for color blindness, and a mean RT or error rate with more than 3 interquartile ranges (IQR) above the third (or below the first) quartile as inspected via boxplots. In Experiment 1, we excluded one participant with an error rate of 80% in the Ishihara test for color blindness.

All analyses of variance (ANOVA) were conducted in SPSS version 28. For all Bayesian analyses, JASP version 0.17 was used with its preset parameters and default priors (Rouder et al., 2009; van Doorn et al., 2021). In the Bayesian repeated-measures ANOVAs, the Cauchy prior centered on zero was used with a fixed effects scale factor of $r$ = 0.5 and a random effects scale factor of $r$ = 1. The repeatability seed 123 was used. In Bayesian paired t-tests, the Cauchy prior centered on zero with a width of $r$ = 0.707. In the rare case that the $p$-value and the Bayes Factor of an effect did not align, we only interpreted the $p$-value. Additionally, according to Depaoli et al. (2020), sensitivity analyses were performed in which wide priors with the fixed effects scale factor of $r$ = 0.707 were used for all Bayesian repeated measures analyses (see Supplemental Materials).

### *Learning Phase*

The 2 x 2 repeated-measures ANOVA of the error rate with the independent variables cue type (predictive, nonpredictive) and transition (task repetition, task switch) revealed a significant main effect of transition, $F(1, 48) = 17.46$, $p < .001$, $\eta_p^2 = .27$, $BF_{10} = 141.237$. Participants made more errors when the task switched ($M$ = 5.81%, $SD$ = 4.66) than when the task repeated ($M$ = 3.70%, $SD$ = 3.19). The main effect of cue type ($BF_{10} = 0.190$) and the interaction ($BF_{10} = 0.323$) were not significant (all $F$s < 0.74, all $p$s > .396).

Using the same ANOVA to analyze RTs resulted in a significant main effect of transition, $F(1, 48) = 89.11$, $p < .001$, $\eta_p^2 = .65$, $BF_{10} = 4.831*10^9$. Participants responded faster on task repetitions ($M$ = 680 ms, $SD$ = 137) than on task switches ($M$ = 790 ms, $SD$ = 188). Again, the main effect of cue type ($BF_{10} = 370$) and the interaction ($BF_{10} = 0.233$) were not significant (all $F$s < 2.75, all $p$s > .104).

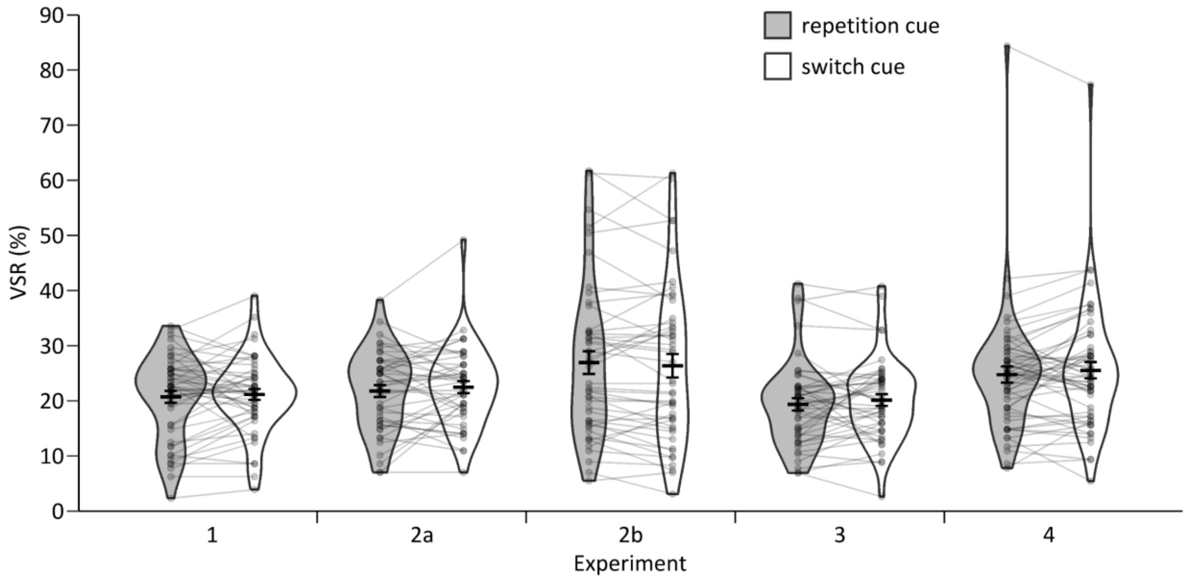
### *Test Phase*

Analyzing the VSR in a 2 x 2 repeated-measures ANOVA with the independent variable cue association (repetition, switch) and phase (mixed phase, transfer phase) revealed a significant main effect of phase, $F(1, 48) = 7.83$, $p = .007$, $\eta_p^2 = .14$, $BF_{10} = 5.508$. Participants switched tasks more often in the mixed phase ($M = 22.18\%$, $SD = 7.27$) than in the subsequent transfer phase ($M = 19.72\%$, $SD = 7.68$). The crucial main effect of cue association was not significant, $F(1, 48) = 0.38$, $p = .541$, $\eta_p^2 = .01$, $BF_{10} = 0.221$. There was no significant difference between the VSR after a task repetition cue ($M = 20.73$, $SD = 7.62$) and the VSR after a task switch cue ($M = 21.17$, $SD = 6.91$; see Figure 3). The Bayes Factor indicates moderate evidence for $H_0$ (Lee & Wagenmakers, 2013). Similarly, the interaction effect did not reach significance, $F(1, 48) = 3.38$, $p = .072$, $\eta_p^2 = .07$, $BF_{10} = 1.165$.

In the post-experiment question, no participant reported the relationship between cue color and task transition. In the Supplemental Materials, additional exploratory analyses of the cue association effect (and respective meta-analyses) are reported separately for each test phase (mixed phase, transfer phase) for all experiments. Furthermore, the task bias is reported followed by exploratory analyses of the VSR in the test phases without participants showing an extreme task bias. Last, the VSR analysis (and the meta-analysis) was repeated only including participants with signs of cue learning in the learning phase at the reasonable request of a reviewer. Overall, the pattern of the results remained the same.

**Figure 3**

*Voluntary Switch Rate (VSR) as a Function of Cue Association (Repetition Cue, Switch Cue) in the Test Phase of Experiments 1, 2a, 2b, 3, and 4*




*Note.* Bold lines represent the mean VSR per condition. Error bars represent ± one standard error of the mean. Dots represent the VSR of each participant while conditions of the same participant are connected with a grey line.

## Discussion

The results of Experiment 1 showed no effect of the color cues on the mean RT or error rate in the learning phase. Cues that predicted the upcoming task transition did not facilitate performance compared to nonpredictive cues. In the test phase, the voluntary switch rate was not influenced by the cues associated with specific transitions. Taken together, there was no evidence that associative learning modulated cognitive flexibility. The significant VSR difference between the mixed and the transfer phase can be explained by time-on-task effects, as it is a known pattern that the VSR in the hybrid paradigm tends to decrease over time (e.g., Fröber & Dreisbach, 2017).

## Experiment 2a

The results of Experiment 1 did not support the idea that flexibility can be easily triggered by association. As a next step, for Experiment 2a, we changed some aspects of the design in order to increase the potential association between the cues and cognitive flexibility. First, we extended the learning phase and removed the nonpredictive cues there (except on the first trial without any transition). This should facilitate the formation of an association because there are only two distinct events (blue or green cue) compared to the three events in Experiment 1 (blue, green, or white cue).

Apart from that, the validity of the cues in the learning phase was changed to 80% so that we could still measure the possible performance benefits of valid cues. In 80% of the trials, the corresponding transition followed (valid trial), while in 20% of the trials, the other transition followed (invalid trial). In addition, the cue was presented for a longer time, with a larger size, and remained on the screen during stimulus presentation in order to make the cue more salient, to facilitate cue usage, and to strengthen the relationship between the cue and the control mode. In the learning phase, we expected performance benefits after valid cues as compared to invalid cues. More importantly, in the test phase, the unrestricted VSR should be modulated by the learned cue-transition association. After the cue associated with task switches, participants should voluntarily switch tasks more often than after the cue associated with task repetitions.

## Method

### *Participants*

In the preregistered Experiment 2a (https://aspredicted.org/bx93p.pdf), another sample of 50 participants was collected. The number of participants was determined using the same logic as in Experiment 1. The mean age was 24.76 years (*SD* = 3.43; range 19-35). Thirty-eight subjects were female (12 male) and 47 were right-handed (three left-handed). All subjects gave informed consent prior to the experiment in accordance with the ethical standards of the national research committee and with the 1964 Helsinki declaration and its later amendments. They received course credit or 6 € for their participation. Five participants had to be excluded (see Data Preprocessing for exclusion criteria), resulting in a final sample of 45 subjects.

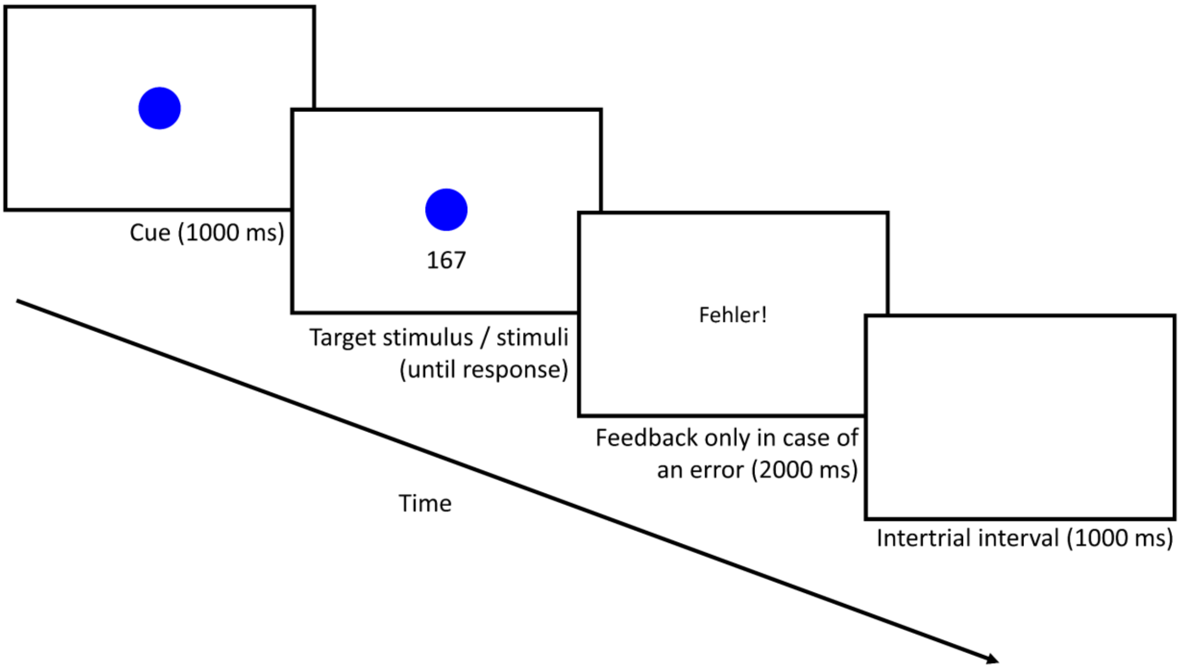
### *Apparatus, Stimuli, Procedure, and Design*

The tasks and stimuli were the same as in Experiment 1. The size of the cue was increased to a diameter of 20.83% of the screen height and no black frame was displayed around the cue (except for the white cue on the first trial of each block, and in the neutral forced-choice trials of the transfer phase). The structure of the practice phase was the same as in the previous experiment. In the following extended learning phase (two blocks, 160 trials each), only colored (predictive cues in green and blue) were presented and cue validity was set to 80%. One of the cues (blue, green) predicted task repetitions in 80% of the cases (20% task switches), whereas the other cue predicted task switches in 80% of the cases (20% task repetitions). The first 16 trials were always set to be valid to facilitate the formation of a cue-transition association. The structure of the test phase was identical to the previous experiment. This means that the cue validity in the forced-choice trials of the mixed phase was again 100% and in every forced-choice trial of the transfer phase, a nonpredictive cue was presented. Each trial of the learning and test phases started with a cue for 1000 ms, followed by the stimulus/stimuli above and/or below the cue until a response was given. Feedback appeared only after errors for

2000 ms (“Fehler!”). The intertrial interval consisted of a blank screen for 1000 ms (see Figure 4). A single session of the experiment lasted approximately 50 minutes.

**Figure 4**

*Schematic Depiction of a Single (Forced-Choice) Trial Sequence in Experiments 2a*




In the learning phase, we used a 2 (Cue validity: valid, invalid) x 2 (Transition: task repetition, task switch) repeated-measures design for the dependent variables error rate and RT. In the test phase, the 2 x 2 repeated-measures design of the VSR remained the same as in Experiment 1 with the independent variables cue association (repetition, switch) and phase (mixed phase, transfer phase).

## Results

### *Data Preprocessing*

Data preprocessing and analysis were performed according to the preregistration protocol. To analyze the error rate in the learning phase, the first trial of each block was excluded (0.63% of all trials in this phase). In addition, for the RT analysis, we excluded error trials (7.31%), trials following errors (6.07%), RTs faster than 100 and slower than 8000 ms (0.02%), and RTs more than 3 standard deviations above or below the individual cell mean (1.69%).

For the VSR in the test phase, we again included all voluntary trials. The criterion for outliers was the same as in the previous experiment (more than 50% errors in the Ishihara test for color blindness; overall error rate or mean RT with more than 3 IQR above the third or below the first quartile). Accordingly, one participant had to be excluded due to an extreme error rate (59.74%) and four additional subjects due to an extreme overall RT (1190 ms, 1498 ms, 1617 ms, 3154 ms).

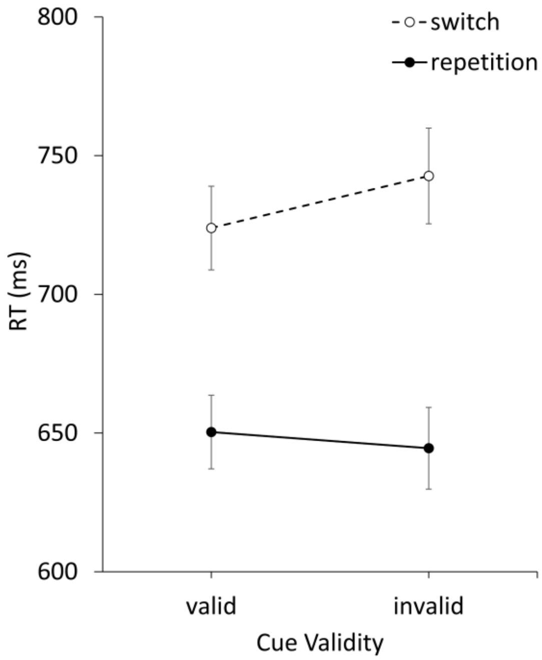
### *Learning Phase*

In the 2 x 2 ANOVA of the error rate with the independent variables validity (valid, invalid) and transition (task repetition, task switch), a significant main effect of transition was found, $F(1, 44) = 17.98$, $p < .001$, $\eta_p^2 = .29$, $BF_{10} = 77.472$. More errors were committed when the task switched ($M$ = 7.18%, $SD$ = 4.49) than when the task repeated ($M$ = 5.24%, $SD$ = 3.49). The main effect of cue validity ($BF_{10} = 0.203$) and the interaction of validity and transition ($BF_{10} = 0.620$) were not significant (all *F*s < 1.83, all *p*s > .183).

The same ANOVA with the dependent variable of mean RT revealed a significant main effect of transition, $F(1, 44) = 107.20$, $p < .001$, $\eta_p^2 = .71$, $BF_{10} = 4.160*10^{10}$. Participants responded slower on task switches ($M$ = 733 ms, $SD$ = 106) compared to task repetitions ($M$ = 647 ms, $SD$ = 92). The main effect of cue validity did not reach significance, $F(1, 44) = 2.17$, $p = .147$, $\eta_p^2 = .05$, $BF_{10} = 0.353$. However, there was a significant interaction between cue validity and transition, $F(1, 44) = 6.21$, $p = .017$, $\eta_p^2 = .12$, $BF_{10} = 6.445$. The switch costs were greater for invalid cues ($M$ = 98 ms, $SD$ = 77) compared to valid cues ($M$ = 74 ms, $SD$ = 50). This modulation was mainly driven by the task switches where participants were faster with valid cues ($M$ = 724 ms, $SD$ = 101) compared to invalid cues ($M$ = 743 ms, $SD$ = 116), $t(44) = 2.61$, $p = .012$, $d = 0.39$, $BF_{10} = 3.283$. On task repetitions, there was no significant difference between valid ($M$ = 650 ms, $SD$ = 89) and invalid cues ($M$ = 645 ms, $SD$ = 99), $t(44) = 0.98$, $p = .332$, $d = 0.15$, $BF_{10} = 0.254$ (see Figure 5).

**Figure 5**

*Mean Reaction Times (RT) as a Function of Cue Validity (Valid, Invalid) and Transition (Task Repetition, Task Switch) in the Learning Phase of Experiment 2a*




*Note.* Error bars represent ± one standard error of the mean.

### *Test Phase*

In the 2 x 2 repeated-measures ANOVA of the VSR with the independent variables cue association (repetition, switch) and phase (mixed phase, transfer phase), none of the effects were significant (cue association $BF_{10}$ = 0.261, phase $BF_{10}$ = 0.435, cue association x phase $BF_{10}$ = 0.928; all *F*s < 2.76, all *p*s > .103). Thus, the critical main effect of cue association was not significant, $F(1, 44) = 0.80$, $p = .377$, $\eta_p^2 = .02$, $BF_{10} = 0.261$. There was no significant difference between the VSR after a task repetition cue (*M* = 21.79, *SD* = 7.31) and the VSR after a task switch cue (*M* = 22.48, *SD* = 7.33; see Figure 3). Again, the respective Bayes Factor implies moderate evidence for $H_0$ (Lee & Wagenmakers, 2013). In the post-experiment question, one of the 45 participants noticed a relationship between cue color and task transition.

## Discussion

The results of Experiment 2a indicated that the transition cues partially facilitated task performance. Valid cues led to improved RT performance on task switches. Hence, the methodological changes (transition cues were larger, displayed longer, and remained on-screen during target presentation) improved cue usage. However, the results of Experiment 2a again failed to show associative learning of cognitive flexibility. The VSR was not affected by the cues associated with specific control modes.

## Experiment 2b

In Experiment 2b, the transfer phase consisted of 100% free choices. This was the only experiment and phase in which we used a randomness instruction: Participants were instructed to perform both tasks about equally often, but in random order (Arrington & Logan, 2004). This way, we can investigate whether an entirely voluntary transfer phase facilitates the transfer of cognitive flexibility. Our expectations and the rest of the method were the same as in Experiment 2a.

### Method

#### *Participants*

Another sample of 50 participants was tested in the preregistered Experiment 2b (https://aspredicted.org/4td5t.pdf). The mean age was 22.40 years (*SD* = 3.30; range 18-33). Forty participants were female (ten male) and 47 were right-handed (three left-handed). All participants gave informed consent before the experiment started in accordance with the ethical standards of the national research committee and with the 1964 Helsinki declaration and its later amendments. Subjects received either course credit or 6 € for their participation. Four subjects had to be excluded (see Data Preprocessing for exclusion criteria) which led to a final sample of 46 participants.

#### *Apparatus, Stimuli, Procedure, and Design*

Experiment 2b differed from Experiment 2a only in one aspect. The transfer phase contained 100% voluntary trials without any interspersed forced-choice trials. Only in this phase, participants were instructed to perform both tasks about equally often in random order (Arrington & Logan, 2004). All other aspects remained the same.

### Results

#### *Data Preprocessing*

Data preprocessing and analysis were performed according to the preregistration protocol. For the analysis of the error rate in the learning phase, we excluded the first trial of each block (0.63% of all trials in the learning phase). For the RT analysis, we additionally excluded error trials (5.14%) and post-error trials (4.83%). There were no RTs faster than 100 and slower than 8000 ms. Last, we excluded RTs more than 3 standard deviations above or below the individual cell mean (1.89%).

For the VSR analysis in the test phase, we considered all voluntary trials and only excluded the first trial of each block of the transfer phase (0.52% of all voluntary trials; the first trial of the mixed phase was fixed as a forced-choice trial). One participant had to be excluded due to an extreme overall RT (1303 ms) more than 3 IQR above the third quartile. Three other participants did not comply with the randomness instruction by always repeating the same task in an entire block of the transfer phase and were therefore excluded.

### *Learning Phase*

The 2 x 2 ANOVA of the error rate with the independent variables validity (valid, invalid) and transition (task repetition, task switch) revealed a significant main effect of transition, $F(1, 45) = 12.96$, $p < .001$, $\eta_p^2 = .22$, $BF_{10} = 37.219$. Participants committed more errors on task switches ($M = 5.99\%$, $SD = 4.23$) than on task repetitions ($M = 4.03\%$, $SD = 3.87$). No other effect was significant (validity $BF_{10} =$ 0263, validity x transition $BF_{10} = 0.952$;all $F$s < 3.04, all $p$s > .088).

The same pattern of results emerged for the dependent measure of RT. The significant main effect of transition, $F(1, 45) = 101.83$, $p < .001$, $\eta_p^2 = .69$, $BF_{10} = 2.416 * 10^{10}$, indicated slower RTs on task switches ($M = 710$ ms, $SD = 125$) compared to repetitions ($M = 633$ ms, $SD = 100$). The main effect of cue validity ($BF_{10} = 0.186$) and the interaction of cue validity and transition ($BF_{10} = 0.330$) were not significant(all $F$s < 0.71, all $p$s > .405).

### *Test Phase*

The 2 x 2 ANOVA of the VSR with the independent variables cue association (repetition, switch) and phase (mixed phase, transfer phase) revealed the significant effect of phase, $F(1, 45) = 4.96$, $p = .031$, $\eta_p^2 = .10$, $BF_{10} = 1.739$. Participants switched tasks more often in the fully voluntary transfer phase ($M = 29.52\%$, $SD = 20.58$) than in the mixed phase ($M = 23.81\%$, $SD = 10.97$). The interaction of cue association and phase, $F(1, 45) < 0.01$, $p = .966$, $\eta_p^2 < 0.01$, $BF_{10} = 0.238$, and the main effect of cue association, $F(1, 45) = 0.80$, $p = .375$, $\eta_p^2 = .02$, $BF_{10} = 0.255$, did not reach significance. Again, there was no significant difference between the VSR after a task repetition cue ($M = 26.96$, $SD = 14.03$) and the VSR after a task switch cue ($M = 26.37$, $SD = 14.36$; see Figure 3). The Bayes Factor indicates moderate evidence for $H_0$ (Lee & Wagenmakers, 2013). In the post-experiment question, one of the 46 participants noticed a relationship between cue color and task transition.

### Discussion

The results of the learning phase in Experiment 2a were not replicated in the identical learning phase of Experiment 2b. Here, we found no effect of cue validity on the switch costs. Again, the VSR was not influenced by the cue-transition association. In the 100% voluntary transfer phase, participants seemed to adhere to the randomness instruction as indicated by the significantly larger VSR compared to the mixed phase (without any randomness instruction). Because participants seemed to ignore the transition cues, we changed the procedure in Experiment 3 to increase attention to the cues.

## Experiment 3

To ensure that participants would process the (to them seemingly meaningless) color cues, in Experiment 3, we presented rare catch trials that afforded a response by the participants. That is, whenever the cue was presented in yellow, participants were instructed to press the space bar in response to the following target (and refrain from responding according to the task rule; similar to Fritz & Dreisbach, 2015). Additionally, the validity of the transition cue was set to 100% to strengthen the relationship between the cue and the following transition. Because the transfer phase with 100% free choices (Experiment 2b) did not facilitate the transfer of cognitive flexibility, we again implemented the hybrid task-switching paradigm without giving a randomness instruction in the transfer phase of Experiment 3.

## Method

### *Participants*

Another sample of 50 participants took part in the preregistered Experiment 3 (https://aspredicted.org/6t2iu.pdf). The mean age was 25.66 years (*SD* = 7.20, range 18-58). Thirty-five participants were female (15 male) and 46 participants were right-handed (four left-handed). Prior to the experiment, informed consent was provided by all participants in accordance with the ethical standards of the national research committee and with the 1964 Helsinki declaration and its later amendments. Students of the University of Regensburg received course credit. Two subjects had to be excluded (see Data Preprocessing for exclusion criteria), resulting in a final sample of 48 participants.

### *Apparatus, Stimuli, Procedure, and Design*

The method was similar to Experiment 2a. An additional cue color was implemented (yellow; RGB values 255, 255, 0). This cue color appeared on catch trials where participants had to press the space bar in response to the following target (and refrain from the actual task-specific response). This ensured that participants processed the cue color. The practice phase remained the same as in the previous experiments. At the start of the learning phase, participants received general instructions about the catch trials. Sixteen catch trials were randomly interspersed among the 128 task-switching trials, resulting in 144 trials in each of the two blocks. The minimum distance between catch trials was set to three and the maximum distance to 20. The blue and green cues predicted the upcoming task transitions with 100% validity. In the test phase, 16 catch trials were added to the 128 hybrid task-switching trials (again resulting in 144 trials) in each of the two blocks of the mixed and the transfer phase. Catch trials always contained an irrelevant forced-choice stimulus. The trial structure remained the same as in Experiment 2a.

Because there was no validity manipulation in this experiment, the error rate and RT of the learning phase were analyzed only with the independent variable transition (task repetition, task switch). In the test phase, we again used a 2 (Cue association: repetition, switch) x 2 (Phase: mixed phase, transfer phase) repeated-measures design for the VSR.

## Results and Discussion

### *Data Preprocessing*

Data preprocessing and analysis were performed according to the preregistration protocol with one exception as described in the next paragraph. In the learning phase, we excluded the first trial of each block (0.69% of all trials in the learning phase), catch trials (11.11%), and all trials following catch trials (10.99%) to analyze error rates. For the RT analysis, we additionally excluded error trials (3.51%), trials following errors (3.49%), RTs faster than 100 and slower than 8000 ms (0.01%), and RTs more than 3 standard deviations above or below the individual cell mean (1.34%).

In the test phase, we excluded all trials following catch trials (10.94 % of all free choice trials in the test phase). No participant had more than 50% errors in the Ishihara test for color blindness or an overall error rate or RT more than 3 IQR above the third or below the first quartile. In the preregistration, we had defined an *overall* error rate on catch trials of more than 50% as an exclusion criterion. No participant exceeded this threshold. Overall the error rate on catch trials was rather low ($M$ = 3.94, $SD$ = 7.32, Range: 0%-35.42%). In retrospect, we realized that this exclusion criterion should also be applied separately to the learning phase, because with an error rate > 50% cue encoding and any influence on the later test phase cannot be guaranteed. This led to the exclusion of two participants that missed more than 50% of the catch trials during the learning phase (both 100%; the statistical pattern of results did not change when they were included in the analyses).

### *Learning Phase*

There were significant switch costs in the error rates of the learning phase, $t(47) = 3.13$, $p$ = .003, $d = 0.45$, $BF_{10} = 10.938$. Participants made more errors on task switches ($M$ = 5.19%, $SD$ = 3.44) than on task repetitions ($M$ = 3.50%, $SD$ = 3.64). The analysis of the RT also revealed significant switch costs, $t(47) = 11.07$, $p < .001$, $d = 1.60$, $BF_{10} = 6.421*10^{11}$. Responses were slower on task switches ($M$ = 793 ms, $SD$ = 168) compared to task repetitions ($M$ = 704 ms, $SD$ = 137).

### *Test Phase*

In the 2 x 2 repeated-measures ANOVA of the VSR with the independent variables cue association (repetition, switch) and phase (mixed phase, transfer phase), the crucial main effect of cue association did not reach significance, $F(1, 47) = 1.15$, $p = .289$, $\eta_p^2 = .02$, $BF_{10} = 0.265$. The VSR did not significantly differ between the task repetition cue ($M$ = 19.37, $SD$ = 7.73) and the task switch cue ($M$ = 20.16, $SD$ = 7.34; see Figure 3). The Bayes Factor indicates moderate evidence for $H_0$ (Lee & Wagenmakers, 2013). Similarly, all other effects were not significant (phase $BF_{10} = 0.199$, cue association x phase $BF_{10} = 0.220$; all $F$s < 0.01, all $p$s > .918). In the post-experiment question, no participant noticed a relationship between cue color and task transition.

Even with intermixed catch trials, which ensured that participants attended to the transition cue, the VSR was not modulated by the cue-transition association. Hence, there was again no evidence for associative learning of cognitive control modes.

## Experiment 4

So far, we have found no evidence that color cues that are consistently paired with the upcoming task transition trigger the associated control mode in a subsequent test phase. A transition cue in our task-switching paradigm with only two tasks could in principle also be used for task-specific preparation: based on the knowledge of the just executed task, the participants could in principle infer which task they would have to repeat or switch to in the upcoming trial and prepare accordingly. Obviously, such a strategy might be computationally costly and therefore be avoided (De Jong, 2000). In Experiment 4 we, therefore, decided to use two tasks of unequal difficulty. The rationale was that participants might be more motivated to use the transition cues (be it for task-specific or transition-specific preparation) to succeed in the more difficult task.

### Method

#### *Participants*

Experiment 4 was not preregistered. Another sample of 60 participants took part in this experiment (see Footnote 15). The participants had a mean age of 23.75 years (*SD* = 4.90; range 18-51). Forty-two were female (18 male) and 52 were right-handed (seven left-handed, one ambidextrous). All participants gave informed consent in accordance with the ethical standards of the national research committee and with the 1964 Helsinki declaration and its later amendments. Students of the University of Regensburg received course credit after completing the experiment. Four subjects had to be excluded before the final analysis (see Data Preprocessing for exclusion criteria), resulting in a final sample of 56 participants.

#### *Apparatus, Stimuli, Procedure, and Design*

The used method was largely similar to Experiment 1. The main difference was that instead of the previous number task participants had to categorize numbers as prime (17, 19, 23, 29) or non-prime numbers (15, 21, 25, 27). This prime task is considerably more difficult than the number task in the previous experiments as has been shown in previous studies (Dreisbach & Jurczyk, 2022; Jurczyk et al., 2019). The structure of the phases and the individual trials remained the same as in Experiment 1. One session lasted approximately 40 minutes.

The independent variable task difficulty was added to all designs. For the error rate and RT analysis in the learning phase, we used a 2 (Cue type: predictive, nonpredictive) x 2 (Transition: task repetition, task switch) x 2 (Task difficulty: difficult, easy) repeated-measures design. In the test phase, the VSR served as dependent variable in a 2 (Cue association: repetition, switch) x 2 (Phase: mixed phase, transfer phase) x 2 (Task difficulty: difficult, easy) repeated-measures design.

## Results

### *Data Preprocessing*

The same preprocessing as in Experiment 1 was performed. In the learning phase, to analyze the error rate we first excluded the first trial of each block (1.04% of all trials in this phase). In addition, for the RT analysis, we excluded error trials (6.79%), post-error trials (6.18%), RTs faster than 100 and slower than 8000 ms (0.05%), and RTs more than 3 standard deviations above or below the individual cell mean (1.50%).

Again, all voluntary trials of the test phase were included to account for all cases of deliberate switching. Please note that in contrast to the previous experiments, because we analyzed the VSR per task difficulty, the VSR was calculated as the proportion of voluntary switches to one task in a particular cue and phase condition relative to all possible free-choice trials in that condition (Jurczyk et al., 2019). Participant outliers were defined the same way as in Experiment 1 (more than 50% errors in the Ishihara test for color blindness; overall error rate or mean RT with more than 3 IQR above the third or below the first quartile). One participant was excluded due to an extreme overall error rate (35.80%). Three more participants had extreme overall RTs (1699 ms, 1730 ms, 2225 ms) and were therefore excluded.

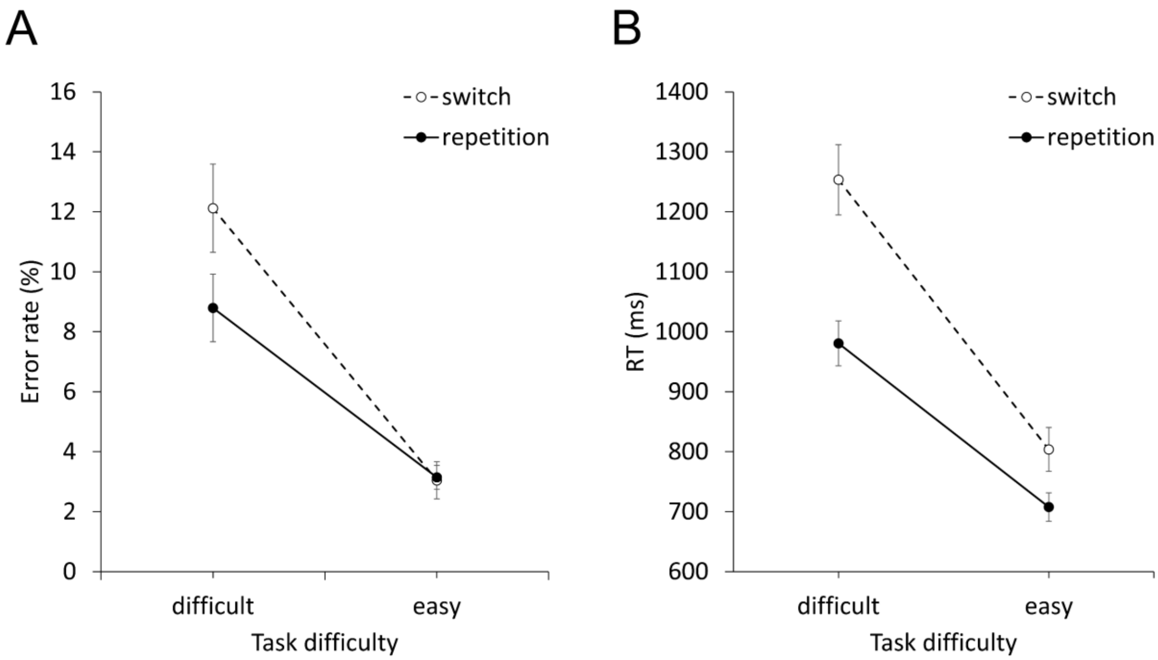
### *Learning Phase*

The 2 (Cue type) x 2(Transition) x 2 (Task difficulty) repeated-measures ANOVA of the error rates revealed significant main effects of task difficulty, $F(1, 55) = 43.43$, $p < .001$, $\eta_p^2 = .44$, $BF_{10} = 4.809$, and transition, $F(1, 55) = 8.98$, $p = .004$, $\eta_p^2 = .14$, $BF_{10} = 6.782*10^5$. Participants made more errors on the difficult task ($M = 10.46\%$, $SD = 9.23$) compared to the easy task ($M = 3.09\%$, $SD = 3.15$). Similarly, participants made more errors on task switches ($M = 7.58\%$, $SD = 6.52$) than on task repetitions ($M = 5.97\%$, $SD = 5.08$). Furthermore, the interaction between task difficulty and transition reached significance, $F(1, 55) = 9.87$, $p = .003$, $\eta_p^2 = .15$, $BF_{10} = 11.120$. There were significant switch costs only when switching to the difficult task ($M = 3.32\%$, $SD = 6.66$; $M_{switch} = 12.12\%$, $SD_{switch} = 11.00$; $M_{repeat} = 8.79\%$, $SD_{repeat} = 8.45$), $t(55) = 3.73$, $p < .001$, $d = 0.50$, $BF_{10} = 56.323$. The switch costs to the easy task were not significant ($M = -0.10\%$, $SD = 4.61$; $M_{switch} = 3.04\%$, $SD_{switch} = 4.66$; $M_{repeat} = 3.14\%$, $SD_{repeat} = 2.96$), $t(55) = 0.16$, $p = .875$, $d = 0.02$, $BF_{10} = 0.148$ (see Figure 6, Panel A). The cue type showed no significant main effect ($BF_{10} = 0.243$), interaction with task difficulty ($BF_{10} = 0.223$) or transition($BF_{10} = 0.246$), or three-way interaction ($BF_{10} = 0.187$; all $F$s < 1.63, all $p$s > .207).

The same pattern of results emerged for the dependent measure of RT. The analysis revealed a significant effect of task difficulty, $F(1, 55) = 93.73$, $p < .001$, $\eta_p^2 = .63$, $BF_{10} = 7.642*10^8$, and transition, $F(1, 55) = 72.81$, $p < .001$, $\eta_p^2 = .57$, $BF_{10} = 3.082*10^{10}$. Participants responded slower to the difficult task ($M$ = 1117 ms, $SD$ = 347) than to the easy task ($M$ = 756 ms, $SD$ = 220). There were slower responses to task switches ($M$ = 1029 ms, $SD$ = 317) as compared to task repetitions ($M$ = 844 ms, $SD$ = 207). Moreover, the interaction of task difficulty and transition reached significance, $F(1, 55) = 33.51$, $p < .001$, $\eta_p^2 = .38$, $BF_{10} = 5.077*10^4$. Again, the switch costs were higher when switching to the difficult task ($M$ = 273 ms, $SD$ = 245; $M_{switch}$ = 1254 ms, $SD_{switch}$ = 438; $M_{repeat}$ = 981 ms, $SD_{repeat}$ = 281), $t(55) = 8.35$, $p < .001$, $d = 1.12$, $BF_{10} = 3.969*10^8$, compared to the easy task, ($M$ = 96 ms, $SD$ = 137; $M_{switch}$ = 804 ms, $SD_{switch}$ = 273; $M_{repeat}$ = 708 ms, $SD_{repeat}$ = 178), $t(55) = 5.26$, $p < .001$, $d = 0.70$, $BF_{10} = 7028.599$ (see Figure 6, Panel B). The cue type showed no significant main effect ($BF_{10} = 0.156$), interaction with task difficulty ($BF_{10} = 0.230$) or transition ($BF_{10} = 0.204$), or three-way interaction($BF_{10} = 0.202$; all $F$s < 0.28, all $p$s > .600).

**Figure 6**

*Mean Error Rates (Panel A) and Reaction Times (RT; Panel B) as a Function of Task Difficulty (Difficult, Easy) and Transition (Task Repetition, Task Switch) in the Learning Phase of Experiment 4*




*Note.* Error bars represent ± one standard error of the mean.

### *Test Phase*

The 2 x 2 x 2 repeated-measures ANOVA with the independent variable cue association (repetition, switch), phase (mixed phase, transfer phase), and task difficulty (difficult, easy) resulted in the significant effect of task difficulty, $Fs(1, 55) = 29.83$, $p < .001$, $\eta_p^2 = .35$, $BF_{10} = 7216.016$. Participants switched more often to the easy task ($M$ = 17.49%, $SD$ = 8.60) than to the difficult task ($M$ = 7.66%, $SD$

= 8.64). The critical main effect for cue association did not reach significance, $F(1, 55) = 1.30$, $p = .259$, $\eta_p^2 = .02$, $BF_{10} = 0.230$. The VSR did not differ significantly between the task repetition cue ($M = 24.76$, $SD = 9.63$) and the task switch cue ($M = 25.54$, $SD = 11.17$; see Figure 3). The corresponding Bayes Factor suggests moderate evidence for $H_0$ (Lee & Wagenmakers, 2013). All other effects including the main effect of phase ($BF_{10} = 0.252$), and the interactions between cue association and phase ($BF_{10} = 0.193$), cue association and task difficulty ($BF_{10} = 0.230$), phase and task difficulty ($BF_{10} = 0.501$), and the three-way interaction ($BF_{10} = 0.197$) were not significant (all *F*s < 1.86, all *p*s > .178).[16] In the post-experiment question, two of the 56 participants noticed a relationship between cue color and task transition.

### Discussion

As in the previous experiments, we found no influence of the cue-transition association on performance in the forced-choice trials of the learning phase or later on the decision to switch tasks in the free-choice trials of the test phase. Thus, the difficult task did not sufficiently increase participants' motivation to use the transition cues for preparation. Similarly, cognitive flexibility as measured by the VSR was not modulated by the cues associated with specific control modes.

The finding of higher error rate and RT switch costs for the more difficult task stands in contrast to previous findings of asymmetric switch costs. There, the switch costs to the easier task are more pronounced and participants are biased towards the more difficult task due to between-task interference (Yeung, 2010). The discrepancy can be explained by the fact that the present experiment used univalent stimuli, which reduces between-task interference, and did not include an instruction to perform both tasks equally often. This method reliably results in increased switch costs to the more difficult task and a general bias toward the easier task (Dreisbach & Jurczyk, 2022; Jurczyk et al., 2019).

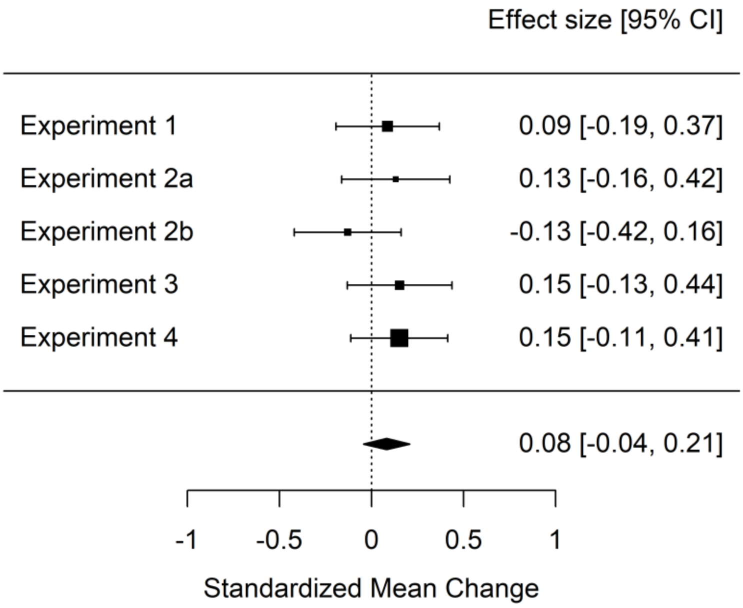
## Meta-Analysis of Experiments 1-4

In an attempt to get a more precise picture of the cue effect, a meta-analysis across Experiments 1-4 was performed by using the metafor package for R (Viechtbauer, 2010) where effect sizes were corrected according to Gibbons et al. (1993). As can be seen in Figure 7, the analysis overall shows no significant effect of cue association (repetition, switch) on the VSR ($d = 0.08$, $p = .206$, 95% CI [-0.04, 0.21]).

[16] Excluding one participant with an extreme overall VSR (80.86%, more than 3 IQRs above the third quartile; see Figure 3) led to the same statistical pattern of results.

**Figure 7**

*Meta-Analysis of the Effect of Cue Association (Repetition, Switch) on the VSR in the Test Phase Across Experiments 1-4.*




## Experiment 5

In Experiments 1-4 we found no evidence for associative learning of cognitive flexibility using task-irrelevant cues. To rule out the possibility that voluntary task choice in general cannot be influenced by learned cue associations, we conducted Experiment 5. Here, the cue no longer predicted the upcoming task transition to measure associative learning of flexibility, but instead directly predicted the upcoming task. We expect that such cues would successfully trigger the associated task in the test phase and consequently make a choice of the respective task more likely. This would show that the null effects from Experiments 1-4 cannot be ascribed to the insensitivity of our experimental approach.

### Method

#### *Participants*

The data of 50 participants was collected for the preregistered Experiment 5 (https://aspredicted.org/qj7bd.pdf). The mean age was 24.98 years (*SD* = 3.94, range 19-36). Thirty-five participants were female (14 male, 1 N/A) and 45 participants were right-handed (four left-handed, 1 ambidextrous). All participants provided informed consent in accordance with the ethical standards of the national research committee and with the 1964 Helsinki declaration and its later amendments. Students of the University of Regensburg received course credit. One subject had to be excluded (see Data Preprocessing for exclusion criteria), resulting in a final sample of 49 participants.

### *Apparatus, Stimuli, Procedure, and Design*

The method was similar to Experiment 2a. The main difference was that the blue and green cues now predicted the identity of the upcoming task (instead of the task transition). The cue-to-task mapping was counterbalanced across participants. Additionally, the colors predicted the upcoming task with 100 % validity. The number of trials per block and the trial structure remained the same as in Experiment 2a.

Because there was no validity manipulation in this experiment, the error rate and RT of the learning phase were analyzed only with the independent variable transition (task repetition, task switch). In the test phase, we used a 2 (Cue association: number task, letter task) x 2 (Phase: mixed phase, transfer phase) repeated-measures design to investigate the influence on the task choice (rate of choices in favor of the number task relative to the letter task).

## Results and Discussion

### *Data Preprocessing*

Data preprocessing and analysis were performed according to the preregistration protocol. In the learning phase, we excluded the first trial of each block (0.78% of all trials in the learning phase) to analyze error rates. For the RT analysis, we additionally excluded error trials (4.39%), trials following errors (4.20%), RTs faster than 100 and slower than 8000 ms (0.02%), and RTs more than 3 standard deviations above or below the individual cell mean (1.61%).

In the test phase, we included all voluntary trials to investigate the choice rate of the number task as a function of phase (mixed phase, transfer phase) and cue association (number task, letter task). No participant made more than 50% errors in the Ishihara test for color blindness or showed an extreme overall error rate. One participant was excluded due to an extreme mean RT (2242 ms) more than 3 IQR above the third or below the first quartile.

### *Learning Phase*

The switch costs in the error rates of the learning phase were not significant, $t(48) = 0.21$, $p = .418$, $d = 0.030$, $BF_{10} = 0.159$. Participants had similar error rates on task switches ($M = 4.49\%$, $SD = 3.88$) and task repetitions ($M = 4.39\%$, $SD = 2.77$). The analysis of the RT revealed significant switch costs, $t(48) = 6.64$, $p < .001$, $d = 0.95$, $BF_{10} = 5.062*10^5$. Responses were slower on task switches ($M = 694$ ms, $SD = 154$) compared to task repetitions ($M = 630$ ms, $SD = 108$).

### *Test Phase*

In the 2 x 2 repeated-measures ANOVA of the choice rate of the number task with the independent variables cue association (number task, letter task) and phase (mixed phase, transfer phase), the crucial main effect of cue association was significant, $F(1, 48) = 35.97$, $p < .001$, $\eta_p^2 = .43$,

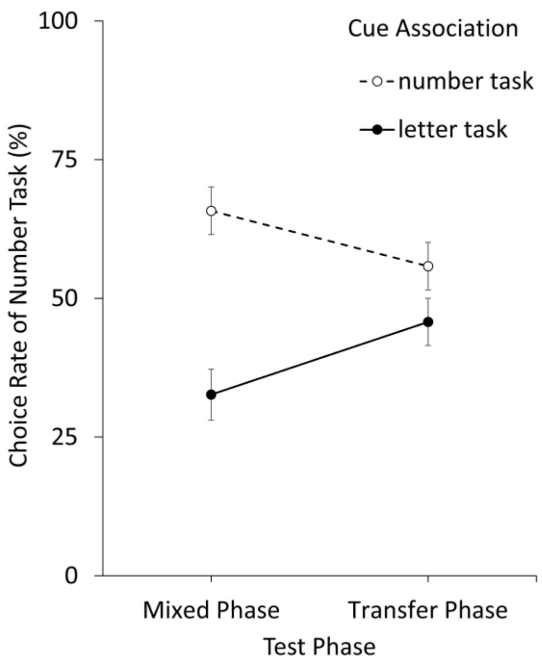
$BF_{10}$ = 6.193*10$^4$. The choice rate of the number task was higher following the cue associated with the number task ($M$ = 60.79%, $SD$ = 27.17) than the cue associated with the letter task ($M$ = 39.21%, $SD$ = 29.13). The Bayes Factor indicates strong evidence for $H_1$ (Lee & Wagenmakers, 2013). Additionally, the interaction between phase and cue association was significant, $F(1, 48) = 29.45$, $p < .001$, $\eta_p^2 = .38$, $BF_{10}$ = 3.918*10$^4$. The effect of cue association was more pronounced in the mixed phase (cue associated with number task: $M$ = 65.78%, $SD$ = 30.06; cue associated with letter task: $M$ = 32.65%, $SD$ = 32.21), $t(48) = 6.22$, $p < .001$, $d = 0.88$, $BF_{10}$ = 1.240*10$^5$, than in the transfer phase (cue associated with number task: $M$ = 55.80%, $SD$ = 29.94; cue associated with letter task: $M$ = 45.76, $SD$ = 29.81), $t(48) = 3.92$, $p < .001$, $d = 0.56$, $BF_{10}$ = 89.780 (see Figure 8). The main effect of phase did not reach significance, $F(1, 48) = 0.36$, $p = .550$, $\eta_p^2 < .01$, $BF_{10} = 0.209$.

In the post-experiment question, 33 participants reported the cue-task association. An additional exploratory analysis, only including participants who did not report the cue-task association, still revealed a significant influence of the cue on task choice (see Supplemental Materials).

Experiment 5 confirmed that learned cue-task associations modulate task choice accordingly. Therefore, the observed null effects in Experiments 1-4 cannot be explained by the insensitivity of the chosen experimental approach.

**Figure 8**

*Mean Choice Rate of the Number Task as a Function of Test Phase (Mixed Phase, Transfer Phase) and Cue Association (Number Task, Letter Task) in the Test Phases of Experiment 5*




*Note.* Error bars represent ± one standard error of the mean.

## General Discussion

The present study investigated whether it is possible to trigger cognitive flexibility by association using task-irrelevant color cues. In Experiments 1-4, participants first completed two blocks of trials in which one color cue consistently predicted a task switch and another color cue predicted a task repetition. In the subsequent test phase, we measured the VSR to assess whether the cues modulated cognitive flexibility. Taken together, the results of Experiments 1-4 showed no evidence of associative learning of transition cues. There was no significant VSR difference between the color cues associated with task repetitions or switches. The respective Bayes factors indicate moderate evidence for the null hypothesis (all $BF_{10} < 0.271$), and a meta-analysis of all experiments revealed no significant effect of cue association on the VSR ($d = 0.08$, 95% CI [-0.04, 0.21]). Thus, the task-irrelevant cues did not trigger the associated control mode. Furthermore, in the learning phase, a facilitating effect of predictive transition cues was only evident in Experiment 2a. Here, we found a reduction in switch costs with valid compared to invalid cues. However, this finding was not replicated in the identical learning phase of Experiment 2b. Critically, the results of Experiment 5 show that the null effects reported in Experiments 1-4 cannot be ascribed to the insensitivity of the experimental approach. When cues were associated with a specific task in the learning phase, these cues primed the associated task in the following test phase and increased the likelihood of voluntarily choosing the respective task, accordingly. Now, how can the lack of evidence for the associative learning of flexibility be reconciled with the associative learning account of cognitive control?

Recent theories argue that cognitive control is based on lower-level associative learning principles (Abrahamse et al., 2016; Braem & Egner, 2018). Accordingly, the present study tested a very basic form of associative learning by implementing task-irrelevant color cues. The learning accounts would have predicted that the associated control mode can be triggered by the cues in a bottom-up manner. Consistent with these theories, numerous studies have already demonstrated a variety of bottom-up influences on task choice. Stimulus repetitions (Mayr & Bell, 2006), repetitions of even unattended stimulus features (Yeung, 2010), stimulus availability (Arrington, 2008; Mittelstädt, Miller, & Kiesel, 2018), hemispheric asymmetries in perception (Arrington & Rhodes, 2010) and location repetitions (Arrington & Weaver, 2015) all influence voluntary task choice. However, in the present study, the associations between the color cues and the transition did not influence cognitive control.

One possibility is that the cue-transition association was not successfully formed. This explanation is supported by the present results, which showed largely no facilitating effect of predictive or valid cues on performance in the learning phase. At first glance, this is at odds with previous studies which indicated that participants can utilize implicit transition cues. In previous research, using the pre-target interval as a transition cue improved performance (Aufschnaiter et al., 2018; Aufschnaiter et al., 2021). Also, the item identity when tied to a certain switch proportion can implicitly modulate the switch costs (Chiu & Egner, 2017; Chiu et al., 2020; Leboe et al., 2008). Similarly, the location can be successfully used as a transition cue (Crump & Logan, 2010). On the other hand, Bejjani et al. (2020) showed that participants were able to utilize numerical cues that predicted the upcoming transition only when the cues were presented supraliminal and, more importantly, when participants were instructed about their meaning. As in the present experiments, there was no evidence that participants used implicit (uninstructed) cues to prepare for the following transition. Moreover, Dreisbach et al. (2002) showed that explicit transition cues are not used to prepare for a task switch. A key difference between the studies in question, which may explain their contrasting results, is the perceived task relevance of the transition-predicting feature. The pre-target interval, the target location, and the target itself share a stronger connection to the task at hand than simple numerical or color cues presented before the target. Therefore, the latter cues may tend to be ignored and not associated with specific task events, i.e., the task transition, whereas only task-relevant cues are used to facilitate performance.

It is important to note that the lack of performance-enhancing effects of predictive or valid cues in the learning phase does not automatically preclude the possibility of modulations of cognitive flexibility in the subsequent test phase. Bejjani et al. (2018) showed significant transfer effects of stimulus-control associations on cognitive control in a Stroop paradigm even when there was no control modulation during the initial pairing of stimulus and control mode (Experiment 1). Furthermore, the present study used different dependent variables in the learning phase (error rates and RTs) and test phase (VSR). Even if performance in forced-choice task switching is unaffected by the cue-transition association, later voluntary choice behavior might still be influenced by these subtle bottom-up modulations. Thus, we still need to explain why there was no evidence for flexibility by association.

The current study found no evidence for associative learning of cognitive control modes using a direct and simple approach. On the other hand, an overwhelming number of studies found convincing evidence consistent with the predictions of the learning account. In addition to the aforementioned bottom-up influences and effects of predictive transition cues on performance, operant conditioning (Braem, 2017) and item-specific switch proportions (Chiu et al., 2020) triggered cognitive flexibility as measured by the VSR. Thus, lower-level learning processes can substantially modulate the voluntary switch rate. Why was this not the case in the current experiments? The present study was the first to test the associative learning account using informative but completely task-irrelevant color cues. Even when the transition cue remained on-screen during target presentation and cue encoding was ensured by the use of catch trials, the cue-transition association did not trigger the associated control mode. The present results suggest that perceived task relevance may be integral to the retrieval of control associations. The reward (Braem, 2017) and the stimulus itself (Chiu et al., 2020) as used in the previous studies might be considered more important for the participant's current task, whereas simple color cues are not essential for successful task performance. Only in the former case, the rewarded or associated control mode is automatically triggered. Without task relevance, the control association may not be retrieved. This rationale is consistent with the finding that bottom-up influences of cue repetitions on the VSR disappear when completely task-irrelevant cues are used that vary randomly prior to target onset (Arrington & Logan, 2005; Jurczyk et al., 2021). Furthermore, following Hommel (2022), it is still possible that the association between cue and control mode was learned but not retrieved, because the retrieval (but not the creation) of event files is assumed to depend on task relevance (but see Kikumoto & Mayr, 2020).

The present experiments identified a blind spot in previous theories of cognitive control. The learning accounts propose that lower-level learning processes can trigger associated control modes (Abrahamse et al., 2016; Braem & Egner, 2018). However, without task-relevant cues, the cue-transition association did not trigger associated control representations even when the cues were salient (Experiments 2a and 2b), cue processing was ensured (Experiment 3), and cue usefulness was increased (Experiment 4). This means that not every feature can serve as a contextual cue. At least one limiting factor seems to be task relevance. In a related vein, Eder and Dignath (2022) recently reevaluated the work of Kurt Lewin and argued that existing associations do not automatically energize behavior. Action readiness, intention, or a specific goal are required to trigger behavior. In the same way, we propose that not every contextual cue can immediately activate associated control modes. To modulate behavior, the cues must be relevant (or perceived as relevant) to the current task.

A potential limitation of the present study is the overall short learning period. The number of learning trials ranged from 192 to 320. This may simply not be enough time to form a sufficiently strong cue-transition association (see Xu et al., 2023). Notably, with the current procedure, learning can continue into the first part of the test phase where the cues are still predicting the upcoming transition in forced-choice trials. This adds another 128 "learning trials". However, the formation of a cue-transition association may take even more time. Potentially, more trials or learning sessions over several days are necessary when using task-irrelevant cues. Nevertheless, because previous studies have shown that task-relevant contextual cues can quickly modulate cognitive flexibility (e.g., Leboe et al., 2008), and because Experiment 5 demonstrated associative learning of task cues in the present paradigm, the current study highlights an important boundary condition with respect to task-irrelevant cues.

Conversely, it could be argued that the test phase was too long and that the cue-transition association was quickly unlearned in this new context. However, as mentioned above, in the current procedure learning can still take place in the forced-choice trials of the first test phase. Nevertheless, no effect of the cue association on cognitive flexibility was found here (see Supplemental Materials for separate analyses per test phase). Furthermore, by adding the two test phases as a separate factor in the main analysis, we should have detected reduced effects of the cue-transition association over time (in the form of a phase x cue association interaction as found in Experiment 5 for the association of cues with task identity). Finally, previous studies with a similar number of trials have detected the effect of the transition association on cognitive flexibility when using task-relevant cues (Chiu & Egner, 2017; Chiu et al., 2020).

## Conclusion

Taken together, the present study provided no evidence for associative learning of cognitive flexibility using task-irrelevant but predictive color cues, even though flexibility here only refers to switching between two highly practiced tasks. Previous theories suggest that cognitive control can be guided by lower-level learning processes. However, the integration or retrieval of control associations seems to require task-relevant or at least target-related features. Overall, this study shows that associative learning of cognitive flexibility is not as ubiquitous as recently thought.

# PART III:

# DISCUSSION

# Summary of the Present Findings

The present project investigated three different reasons to switch, namely objective and subjective switch costs, associated temporal costs of switching, and associative learning processes using task-irrelevant cues. In other words, we investigated factors that may influence the motivation to engage in cognitive flexibility in order to switch tasks. We explored the evidence for two distinct approaches, i.e. an economic account based on the Expected Value of Control (EVC) model (Shenhav et al., 2021) and the alternative non-economic associative learning account of cognitive flexibility (Braem & Egner, 2018). Study 1 and Study 2 investigated the perspective that the decision to switch is the result of an economic cost-benefit analysis. As one cost factor, **Study 1** showed that objective and introspective switch costs can guide the decision to switch. **Study 2** considered that switch costs not only indicate the associated effort cost but also the associated temporal costs. The results revealed that these temporal costs (independent of the effort costs) are a relevant factor influencing the willingness to switch. Last, **Study 3** showed that lower-level learning of task-irrelevant cues associated with task switches did not trigger increased voluntary switching. Taken together, the first two studies provided a deeper understanding of the costs of switching, while the third study showed that these costs are not easily overcome by associative learning.

An overview of all experiments and their respective results is provided in Table 2 and described in the following section. Then, I highlight the implications of the present results for cognitive effort. In this context, I discuss the connection between flexibility and effort. After comparing the economic account and the associative learning account of cognitive flexibility, I address limitations and provide an outlook with a conclusion.

**Table 2**

*Broad Overview of All Experiments in the Present Project*

| Study | Experiment | Main manipulation | Main results |
|---|---|---|---|
| Study 1: Objective and Introspective Switch Costs | Exp. 1 | Measuring objective switch costs and the VSR in an HTS paradigm (50% FSR), and introspective switch costs in a separate phase | Only objective switch costs significantly predicted the VSR |
| | Exp. 2 | Measuring objective and introspective switch costs together in an HTS paradigm (alternating 25% and 75% FSR) | Neither objective nor introspective switch costs predicted the VSR; introspective estimations correctly captured smaller switch costs with a higher FSR |
| | Exp. 3 | Measuring objective switch costs and the VSR in an HTS paradigm (alternating 25% and 75% FSR), and introspective switch costs in a separate phase | Introspective switch costs significantly predicted the VSR overall (especially in the 75% FSR condition); objective switch costs predicted the VSR in the 25% FSR condition |
| Study 2: Temporal Costs | Exp. 1 | Manipulation of the intertrial interval following *free and forced* task switches between blocks | Increased VSR in blocks with a short interval after switches |
| | Exp. 2 | Manipulation of the intertrial interval following *forced* task switches between blocks | Increased VSR in blocks with a short interval after forced switches |
| Study 3: Associative Learning | Exp. 1 | Learning phase: Cue colors predict the *task transition*; Test phase: Measuring the VSR in response to the cue colors (HTS) | No influence of cue association (repetition or switch) on the VSR |
| | Exp. 2a | Stronger association by excluding neutral cues | |
| | Exp. 2b | Testing transfer to 100% voluntary test phase | |
| | Exp. 3 | Including catch trials to ensure cue processing | |
| | Exp. 4 | Using tasks of unequal difficulty | |
| | Exp. 5 | Cues directly predict the *task* | Increased choice rate of the task associated with cue |

*Note.* VSR = Voluntary Switch Rate, HTS = Hybrid Task Switching, FSR = Forced Switch Rate.

## Study 1

Study 1 investigated whether the objective switch costs or introspection about the switch costs guides the decision to switch. According to an economic cost-benefit analysis, participants with higher objective and/or introspective switch costs should switch less often to avoid these costs. In Experiment 1 and 3, introspection and voluntary task switching were measured in separate phases, whereas in Experiment 2, both phases were combined. First of all, in all experiments, the introspective RT estimations showed significant switch costs in line with the previous study by Bratzke and Bryce (2019). The results of the second experiment further demonstrated that participants' estimations are sensitive to subtle modulations of the switch costs through the forced switch rate. The RT estimations reflected the typical finding that a context with frequent forced task switches leads to reduced switch costs (Crump & Logan, 2010; Dreisbach & Haider, 2006; Fröber & Dreisbach, 2017; Fröber, Jurczyk, & Dreisbach, 2021; Siqi-Liu & Egner, 2020). Secondly, the objective switch costs predicted the VSR in Experiment 1 (with a 50 % FSR) and in the 25 % FSR condition of Experiment 3, while the introspective switch cost predicted the VSR only in Experiment 3 (especially within the 75 % FSR condition). In blocks with a medium or low switch frequency, a task switch may require an especially strong reactive engagement of effortful cognitive flexibility (compared to blocks with a high switch frequency where flexibility is overall increased). This reactive flexibility indicated by the objective switch costs may be a better measure of the actual costs of switching and guide the decision to switch. In contrast, in an overall effortful high switch context, effort avoidance might drive participants to base the decision to switch on the available introspective switch costs. In Experiment 2, the VSR appeared to be influenced by neither objective nor introspective switch costs. Here, voluntary task switching with concurrent introspection at random time points may have been overly demanding, making the measured objective and introspective switch costs less accurate and less useful for decision-making. Taken together, Study 1 showed that participants generally tend to consider the costs of switching when deciding to switch. The context can influence whether objective or introspective switch costs are taken into account.

Objective or subjective influences on decision-making are a timely topic. A recent study investigated whether subjective ratings of confidence, physical effort, and reward satisfaction or their objective counterparts influence the decision to allocate control (Corlazzoli et al., 2023). They found that mainly the subjective ratings modulate control allocation. However, Study 1 demonstrated that the dominance of subjective measures does not always apply. With a medium or low switch frequency only the objective switch costs were considered when deciding to switch.

## Study 2

Study 2 disentangled the nature of the switch costs which can indicate both the effort cost and the temporal cost of switching. We manipulated the intertrial duration following task switches between blocks to modulate the associated temporal costs of switching independent of effort. The ITI after a task switch was either short or long. In two experiments, the results showed an increased VSR in blocks with a shorter ITI after task switches. With reduced temporal costs associated with switching, participants were more inclined to switch tasks. This was still the case in Experiment 2 where switching tasks was not profitable (participants could not reduce the overall duration of the experiment by switching voluntarily). This way, Study 2 provided direct evidence that the associated temporal costs are a relevant factor for the typical avoidance of task switches. The temporal costs may represent a cost factor in the function of the expected value of control (Shenhav et al., 2021). Additionally, the reduced temporal cost may be perceived as rewarding and through operant conditioning (Thorndike, 1911) bias the decision to switch. Increased temporal costs, on the other hand, are aversive because they impose higher opportunity costs (Kurzban et al., 2013). However, even when temporal costs of switching were more than neutralized (with a short duration following switches), participants still overall showed a repetition bias. This suggests that the avoidance of the effort costs of switching still prevails. Taken together, both temporal costs and effort costs appear to be taken into account when deciding to switch.

## Study 3

Study 3 explored whether task-irrelevant cues can be associated with cognitive flexibility and later trigger an increased willingness to switch in line with the associative learning account (Abrahamse et al., 2016; Braem & Egner, 2018). Two meaningless colored cues were associated with a certain task transition (repetition or switch). In the test phase, we investigated the VSR in response to the two cues. However, the results of Experiments 1-4 revealed no VSR difference between the two transition cues (using Bayesian statistics). Hence, the task-irrelevant cues did not trigger the associated control mode. Multiple aspects of the procedure were adjusted between the experiments to produce the strongest possible association between cue and control mode. We excluded neutral cues (Experiment 2a, 2b, and 3), included catch trials to ensure cue encoding (Experiment 3), and used tasks of unequal difficulty to increase the motivation to utilize the cues (Experiment 4). Additionally, in Experiment 2b, we showed that there was still no effect of the cue association when using a 100 % voluntary test phase (instead of a hybrid task-switching paradigm). Critically, in Experiment 5, cues were directly associated with the task (rather than the transition) and accordingly influenced choice behavior in the test phase. This finding rules out that the null effects can be explained by an insensitivity of the present experimental approach. Taken together, Study 3 showed that task-irrelevant transition cues cannot trigger the associated control mode. Increased task relevance or target-related features appear to be required to bias choice behavior towards effortful task switches. Otherwise, the general avoidance of task switches prevails.

# Cognitive Effort

### Implications for Effort Avoidance

Together, the three studies of the present project showed that voluntary task switches tend to be avoided due to their objective switch costs, introspective switch costs, and effort-independent temporal costs, while task-irrelevant transition cues cannot override this avoidance. Especially Study 1 and Study 2 offered new insight into factors underlying effort avoidance during voluntary task choice which connects the two research fields of cognitive flexibility and cognitive effort. The effort costs associated with a task switch are (to some extent) reflected in the switch costs. However, the switch costs can be interpreted in two different ways. First, high individual switch costs may suggest that switching is especially costly and therefore effortful for the participant. Second, high switch costs may suggest that the participant did not invest much effort in the task at all resulting in poorer switching performance. Consequently, the findings of Study 1 indicated either that participants consider their objective and introspective switch costs reflecting the effort of switching, or that participants who avoid investing effort during task-switching performance (resulting in larger switch costs) also avoid the effort of voluntary switches. Both interpretations are in line with the interpretation that the effort of task switches is a cost factor in decision-making. Study 2 investigated the temporal cost independent of effort as a determinant of switching. Participants indeed showed a lower VSR with higher associated temporal costs of switching. However, even when the temporal costs of switching were more than compensated for by the reduced ITI following switches, participants still avoided the effort of task switches (indicated by VSRs below 50 %). Last, Study 3 showed that task-irrelevant transition cues cannot trigger cognitive effort engagement in the form of voluntary switches.

The notion that the waste of cognitive effort tends to be avoided has been the subject of several previous studies and theories (e.g., Brehm & Self, 1989; Dreisbach & Jurczyk, 2022; Kool et al., 2010; Richter et al., 2016; Shenhav et al., 2021; Silvestrini et al., 2023; Westbrook & Braver, 2015). The present findings are consistent with this general principle. A task switch is more difficult than a task repetition. According to the Motivation Intensity Theory (Brehm & Self, 1989; Richter et al., 2016), effort is a direct function of task difficulty. It follows that switching tasks is more effortful than a task repetition. The EVC model is even more relevant for the present project because it comprehensively describes the decision to allocate effort based on cost-benefit analyses (Shenhav et al., 2021) while the Motivation Intensity Theory explains the engagement of effort in a given task primarily based on task difficulty and success importance.

In the EVC model, the decision to switch or repeat can be viewed as the result of an economic cost-benefit analysis to maximize the expected value of control (Shenhav et al., 2021). The expected value of control is a function of the probability of the outcome, the value of the outcome, and the costs of control (Shenhav et al., 2013). When the effort costs of switching (indicated by the switch costs) are larger, the resulting expected value of control is smaller and individuals should avoid costly task switches. This perspective is in line with previous findings where higher switch costs were associated with reduced VSRs (Mayr & Bell, 2006; Mittelstädt, Dignath, et al., 2018; Mittelstädt, Miller, & Kiesel, 2018, 2019). One strength of the present project is the fact that this relationship was directly tested in Study 1 and embedded in a framework of effort-based decision-making. The key principle of this framework is that effort engagement needs sufficient justification because effort represents a cost factor. But why is effort costly?

If cognitive effort is a key determinant of success, why do individuals not always actively engage in effort? The simple part of the answer is that effort incurs a cost. The difficult part of the answer is to define what exactly constitutes these costs. Intuitively, early theories argued that there is a tangible resource that is depleted when applying cognitive control, directly representing the cost of control (ego depletion; Baumeister et al., 1998). In this context, blood glucose was the main hypothesized candidate for the central resource of self-control (Gailliot et al., 2007). However, follow-up studies showed that the assumptions regarding blood glucose could not be confirmed (Kurzban, 2010). Additionally, there are further issues with the resource model of control (for a review see Inzlicht & Berkman, 2015), mainly how motivation can counteract ego depletion and, therefore, replenish a supposedly depleted resource (Masicampo et al., 2014). As a result, the cost of control must be sought elsewhere. Opportunity cost (Kurzban et al., 2013) and the related process model of self-control (Inzlicht et al., 2014) provide solutions for this issue.

The cognitive system has a limited capacity and cannot engage in two different tasks simultaneously. Therefore, prioritization is needed meaning that we need to decide which task to perform at a given time. Performing one task incurs opportunity costs which refer to the value of the alternative task (Kurzban, 2016; Kurzban et al., 2013). While using the cognitive capacities for one behavior option, one is giving up on the potential benefits of other options. Together with the concept of delay discounting (da Matta et al., 2012), suggesting that individuals value immediate rewards more than delayed rewards, this creates a problem: Cognitive effort may have beneficial consequences in the future but offers no immediate reward while other behavioral options do. In this sense, the cost of cognitive effort reflects the opportunity cost of alternative behaviors (Shenhav et al., 2017). The opportunity costs have solely motivational consequences. Human beings may be technically able to always invest high levels of cognitive effort, but due to opportunity costs, cognitive effort is avoided. From an evolutionary view, opportunity costs encourage exploration. New behaviors should be explored because they can yield better rewards (Ainslie, 2013). The optimal trade-off between exploitation and exploration is also a central topic in the process model of self-control which builds upon the opportunity cost account (Inzlicht et al., 2014). This model assumes that the cost of control stems from changes in task motivation due to an optimal balance between labor (have-to goals) and leisure (want-to goals).

Applied to task switching, a task switch requires additional processes and offers no immediate reward. Therefore, a task switch incurs opportunity costs of not being able to apply the cognitive capacities to other options with higher immediate values, for example, daydreaming (Kurzban et al., 2013). A task repetition requiring less control incurs lower opportunity costs and is therefore preferred. Study 2 may have directly influenced the opportunity costs of a task switch by manipulating the waiting time after a task switch. With a longer duration after switches, the behavioral option of a task switch took up more time that could have been used elsewhere. As a result, the opportunity cost of switching may have been increased in this context. Generally, a task switch takes up more time than a task repetition (defined as the switch costs; Kiesel et al., 2010; Koch & Kiesel, 2022; Monsell, 2003; Vandierendonck et al., 2010). This time difference should influence the opportunity cost implying that task switches are also avoided because they simply take more time as shown in Study 2. Taken together, task switching appears to be avoided due to the associated effort costs and temporal costs. Both types of costs are related to opportunity costs representing a critical mechanism in human decision-making.

## The Effort Paradox

So far, the present project and most economic decision-making models have put cognitive effort in a rather bad light. Effort is often described as unpleasant, difficult, and demanding. Therefore, as a main principle of human behavior, people tend to avoid the waste of effort(Hull, 1943). This raises the question of why some people still voluntarily invest very high effort. Why do people go mountain climbing, run a marathon, play chess, or consider an academic career? Below, I elaborate on the four answers, namely that (1) the perception of effort is subjective, (2) effort investment can be justified, (3) effort can add value, and (4) effort itself can have a positive value.

(1) First, the perception of effort is subjective. The same effort may feel very aversive to one person but not discomforting at all to another (Dreisbach & Jurczyk, 2022). Moreover, the perception of the same effort can vary intraindividually. This high variability in subjective effort experience can explain why people willingly go for supposedly effortful activities. The activities simply do not feel that effortful to them. One way to measure subjective effort is the cognitive effort discounting paradigm (Westbrook et al., 2013).

(2) Second, effort investment can be justified. Even though effort is a cost factor in decision-making, it is sometimes required to reach one’s goals. Therefore, with enough associated payoff, people are willing to pay this cost (Silvestrini et al., 2023). Study 1 demonstrated that lower objective

and introspective switch costs can justify the decision to switch. In Study 2, we showed that effort in the form of a task switch can be justified to some extent when the associated temporal costs are reduced. The reduced temporal costs may be perceived as reward and through operant conditioning (Thorndike, 1911) increase the willingness to switch. That reward can generally increase effort investment during task switching was shown by Otto et al. (2022) where higher reward resulted in reduced switch costs. Interestingly, this effect vanished over time in a context with a high switch frequency because the participants learned that it was not worth investing effort in this demanding context. Apart from reward, boredom may justify effort. Doing nothing can be even more aversive than doing something effortful (Wu et al., 2021). Hence, in order to reduce boredom individuals may be inclined to switch tasks. In everyday life, there may again be substantial variability between individuals in how different activities are valued and, therefore, which effort is justified or not.

(3) Third, effort can add value (Inzlicht et al., 2018). The results of effortful activities are often evaluated positively. The most famous example is the IKEA effect (Norton et al., 2012) where self-made products are valued higher than ready-made products. While building the product, effort has to be invested. Later, people try to justify that effort resulting in an increased valuation. This effect may motivate people to further invest effort in the future.

(4) Fourth, effort itself can have a positive value. The positive side of effort was explicitly highlighted by Inzlicht et al. (2018). The notion is grounded in the concept of learned industriousness. Effort is typically rewarded in everyday life (e.g., studying more for a test resulting in better grades, thinking harder in chess resulting in better moves, and regular workouts resulting in better health and fitness). Through reinforcement learning, effort itself can inherit reward value and function as a secondary reinforcer (Eisenberger, 1992). Depending on the reward history, some people may be more inclined to willingly invest effort than others. In other words, some people learned that it pays off to be industrious. On the one hand, learned industriousness may be a strong individual trait that adds to the interindividual variability in effort investment. This may explain a large part of the observed variability in voluntary switching behavior in the present studies and previous research. This disposition may be related to the need for cognition (Cacioppo et al., 1984; A. R. Cohen et al., 1955) which represents an individual tendency towards cognitive effort. Interestingly, the need for cognition appears to be independent of the individual ability in cognitive control tasks (Gärtner et al., 2021), thus reflecting a motivational tendency. On the other hand, learned industriousness offers a means for experimental manipulations to (temporarily) influence the motivation to engage effort. Previous studies showed that rewarding effort influences future effort investment in rats (Eisenberger et al., 1979) and humans (Eisenberger & Leonard, 1980). Furthermore, recent research revived the idea of learned industriousness by modulating the intrinsic value of effort through reward (Clay et al., 2022).

However, applying the right reward schedule appears to be challenging (see Lin et al., 2024) because reward can also undermine intrinsic motivation (Deci et al., 1999).

Taken together, there are several reasons why people would engage in effortful activities. Critically, effort also has a positive side, either adding value to the product (3) of effort or being valued itself (4; Inzlicht et al., 2018). It is still an open question what situational or interindividual characteristics determine whether effort is costly or valued in a given context. The potential dispositions towards effort investment are quite diverse. Sometimes people may want to relax and avoid the waste of effort, sometimes seek a challenge, explore, avoid boredom, or reach personal goals (see Zerna et al., 2023). Future studies should seek to uncover the mechanisms that influence the negative or positive perception of effort.

# Flexibility and Effort

The present project brought the two research fields of cognitive flexibility and cognitive effort together. Until now, the two fields have developed largely independently from each other. Research on cognitive control has only recently started to consider the role of effort (Musslick & Cohen, 2021). As the present studies show, cognitive flexibility and effort can strongly impact each other. Therefore, this project highlights that we should always take cognitive effort into account when studying flexibility, and vice versa, or even better, integrate the two topics.

The present project utilized the VSR as a measure of cognitive flexibility but also as a measure of cognitive effort investment. This raises the question of whether cognitive flexibility itself is effortful and whether it is more effortful than its counterpart, i.e., cognitive stability. When looking at task-switching experiments, a task switch requires cognitive flexibility and is more effortful than a task repetition which benefits from cognitive stability (Dreisbach & Fröber, 2019). Previous research had a similar perspective on the effort of flexibility. For example in the demand selection task, the frequency of task switches was used to modulate cognitive effort and thereby decision-making (Kool et al., 2010). In sum, task switches appear to relate to cognitive effort. On the other hand, being cognitively stable and repeating tasks more often results in better performance and is generally preferred. So based on task-switching research, we may be drawn to the conclusion that flexibility is more effortful than stability. However, the task-switching paradigm is specifically designed to measure the costs of switching (Kiesel et al., 2010). When we look at other control paradigms, like the Stroop task (Stroop, 1935) stable task shielding required on incongruent trials can be much more effortful than flexibly processing even task-irrelevant information on congruent trials. In this case, cognitive stability appears to be the more effortful control mode. In other words, depending on the paradigm, either cognitive flexibility or stability is associated with effort.

Even within the task-switching paradigm, one may argue that a task repetition can sometimes be more effortful than a task switch. Just like cognitive work, boredom can also induce effort. Previous studies showed that individuals rather applied electroshocks to themselves (Wilson et al., 2014) or engaged in cognitive work (Wu et al., 2021) when the alternative was to do nothing. Constantly repeating the same simple task in a task-switching paradigm may be easy, but also quite boring. A comparable situation from everyday life is assembly line work. The task may be easy, but individuals crave stimulation. Given the task context, task repetitions may therefore be more boring and effortful than task switches which can provide some form of variation. On a general note, the effortful nature of boredom may facilitate exploration which is key in human development (Muentener et al., 2018).

From a theoretical perspective, the EVC model argues that cognitive control in general should be effortful (Shenhav et al., 2017). Any instance where high control is needed incurs effort costs. Hence, both control modes of cognitive flexibility and stability should require cognitive effort. A given task may require especially high task focus (e.g. stability in the Stroop task) or a high ability to switch (e.g. flexibility in the task-switching paradigm). In both cases, applying control to successfully complete the task is effortful. This feeling of effort depends on potential alternative actions that may offer higher short-term rewards incurring opportunity costs (Kurzban, 2016; Kurzban et al., 2013). Staying focused while reading a complicated paper can be very effortful while switching to chatting with a colleague can be a pleasant distraction. Conversely, when being swamped with work, it is very effortful to constantly switch between different tasks, while staying focused on one task is easier and potentially more productive. Critically, experiences with task switches in everyday life may be confounded with the absolute number of tasks. Situations that require high flexibility in the form of task switching may be more effortful simply because there are more tasks that have to be completed and not because of the switching itself.

Taken together, cognitive effort is associated with cognitive control in general, i.e., both stability and flexibility. Depending on the current task demands, cognitive flexibility or stability can feel more effortful (Dreisbach & Mendl, 2024). This feeling of effort can have a critical impact on motivation and decision-making.

# Associative Learning

The present project explored evidence for an economic account (Study 1 and Study 2) and an alternative non-economic associative learning account of cognitive flexibility (Study 3). The results of Study 1 and Study 2 are in line with the economic approach based on the EVC model (Shenhav et al., 2021). Voluntary switching behavior appears to be based on economic cost-benefit analyses. When exploring associative learning, Study 3 found evidence against general claims of the associative learning account of flexibility (Braem & Egner, 2018). The results highlighted a critical limitation of the associative learning account. Task-irrelevant cues did not automatically trigger the associated control mode. To allow the learning or retrieval of such control associations some degree of task relevance appears to be necessary (see Hommel, 2022). Humans are not biased that easily towards using a certain control mode. Cognitive flexibility is only shaped by the experience of relevant changes in the environment (for example in a context of frequent task switches; Fröber & Dreisbach, 2017). Similarly, Braem et al. (2024) recently argued that intentional regulations of control are difficult to achieve. Extensive training or experience is necessary to get individuals to adjust cognitive control. Taken together, associative learning of cognitive flexibility appears to require task-relevant cues to take effect in line with the notion that only relevant experiences can modulate control.

At first glance, the associative learning account differs from effort-based decision-making in key aspects. Associative learning should automatically influence cognitive flexibility (Braem & Egner, 2018) while effort-based decision-making is controlled and economic. When considering the results of Study 2, the distinction between the two accounts blurs. In Study 2 participants learned that a certain control mode is associated with higher or lower temporal costs which may be perceived as reward and punishment. Through the mechanism of operant conditioning (Thorndike, 1911) the following choices are biased towards the rewarded control mode. This result suggests that economic decisions can be influenced by associative learning. Hence, when associating effort with reward or punishment (influencing the result of economic cost-benefit-analyses) the predictions of both accounts align. A similar economic *and* associative effect of reward on cognitive flexibility was shown by Braem (2017) who rewarded participants higher for task repetitions or task switches which influenced the VSR in a subsequent phase. Additionally, Brosowsky & Egner, 2021 showed that associating repetitions with a higher task demand (an economic cost influencing decision-making) increases voluntary switching. These findings together with the results of Study 2 show that associative learning can produce economic behavior. On a general note, economic behavior often requires associative learning through experience to form representations of the costs and rewards associated with behavior options. Taken together, associative learning of relevant experiences can influence cognitive flexibility. This associative learning account can align with economic accounts when operant conditioning is used to bias the decision to invest effort. This highlights the close link between learning, effort, and flexibility.

# Limitations

In the present project, especially in Study 1 and Study 2, we used performance (the switch costs) as a measure of cognitive effort. If the switch costs were high, we assumed that switching was more effortful. Per definition, effort reflects the factor translating task characteristics and individual ability into performance (Shenhav et al., 2017). However, task performance not only depends on effort but also on other factors. Therefore, task performance only offers an indirect approximation of the invested effort (Silvestrini et al., 2023). In some situations, performance and effort may be independent. No matter how much effort one invests, performance can stay the same. Conversely, performance between participants can vary greatly while the invested effort is actually similar. This issue arises because the perception of effort is subjective and depends on task characteristics and individual ability. In the same situation, different people experience different levels of effort. Still, when all other factors are kept constant, task performance should, as the direct result of effort investment, correlate with effort.

Instead of performance, physiological measures may be more closely associated with effort. To measure cognitive effort, previous studies often used pupillometry (for a review, see van der Wel & van Steenbergen, 2018) or cardiovascular measures, especially the pre-ejection period indicating sympathetic activity (for reviews, see Richter et al., 2016; Silvestrini et al., 2023). These measures may provide a better picture of the subjective experience of effort. Recent research highlighted the viability of pupillometry (da Silva Castanheira et al., 2021; Fröber et al., 2020) to investigate motivation and effort during task switching. To date, cardiovascular measures have not been used to explore effort during task switching. Here the beta-adrenergic cardiac reactivity indicated by the RZ interval can be useful for assessing trial-by-trial changes in effort investment (Kuipers et al., 2017). Future studies may utilize these measures to investigate effort-based decision-making during voluntary task choice. It may also help to include explicit subjective ratings of effort, e.g. an adapted version of the NASA task load index (Hart & Staveland, 1988), to directly capture the subjective experience of the effort of switching. Such subjective measures may even be more sensitive than physiological measures (Ayres et al., 2021).

The concept of effort is rather imprecise and can differ between theories and studies. This gets especially clear when trying to discriminate effort from related concepts like motivation, fatigue, task load, difficulty, or performance. In the present project, we used the established definition of effort as the intensification of mental activity (Eisenberger, 1992) translating task characteristics and individual ability into performance (Shenhav et al., 2017). However, others argue that a further distinction needs to be made between cognitive work (related to the present definition of effort) and the subjective experience of effort (Wolpe et al., 2024). This experience may be especially relevant from a clinical perspective to better understand related cognitive disorders. Therefore, future research may focus on the subjective experience of cognitive work associated with switching tasks which again may require using physiological or explicit subjective measures.

Similar to effort, cognitive flexibility is a rather broad concept. As highlighted in the introduction, task switching captures the attentional shifting function of cognitive flexibility (Sacharin, 2009). The term flexibility can also refer to associative flexibility (creativity) and regulative flexibility (adjustments to changing demands; see Figure 1). The present results and conclusion only apply to attentional shifting between two simple tasks with equal difficulty (Monsell, 2003). How the other levels of flexibility or more complex tasks relate to economic decision-making and associative learning is an interesting topic for future research. Especially creativity may behave differently compared to task switching because it involves more advanced cognitive processes (Mehta & Dahl, 2019). Furthermore, exploring meta-flexibility (the ability to switch between different control modes; Dreisbach & Fröber, 2019; Hommel, 2015) in the context of effort-based decision-making may provide novel insights into cognitive control.

Taken together the present conclusions about the influences of the switching ability, temporal costs, and associative learning are limited to effort defined as cognitive work which we indirectly measured through performance. Furthermore, the findings relate to cognitive flexibility as shifting between two simple tasks. Other types of flexibility like creativity, meta-flexibility, and adjustments to changing demands require further investigation.

# Outlook

Cognitive effort plays a critical role in shaping our actions and decisions. For this reason, especially the tendency to avoid effort has been the subject of numerous studies. However, there exist several different ways to capture effort avoidance. We used the VSR to measure the avoidance of effortful task switches. Others used the demand selection task to measure effort avoidance (Kool et al., 2010). Additionally, the cognitive effort discounting paradigm pits reward against effort to obtain a subjective value of effort (Westbrook et al., 2013). As a last prominent example, the need for cognition scale measures the individual's tendency to engage and enjoy cognitive effort (inverse effort avoidance) through a self-report questionnaire (Cacioppo et al., 1984). The short descriptions already show that these measures capture different aspects and are either explicit or implicit. The critical question is how these measures relate to each other and what is the best measure of effort avoidance. Regarding the first question effort discounting appears to relate to the need for cognition score (Mækelæ et al., 2023; Westbrook et al., 2013). However, the demand selection task showed no correlation to effort discounting or the need for cognition (Mækelæ et al., 2023). Overall, it is unclear whether the different measures relate to the same underlying construct. Hence, regarding the second question, there is no "best" measure of effort avoidance. We always need to consider the used paradigms and tasks when drawing conclusions. Future research should aim to provide a comprehensive breakdown of the concept of effort and its measures. Additionally, the relationship of the VSR with these distinct measures of effort avoidance requires further investigation to get a better understanding of what aspect of effort avoidance the VSR relates to.

Cognitive flexibility is closely related to working memory functions (Diamond, 2013). In order to be flexible, working memory content has to be updated. To switch between tasks, individuals have to activate the new task set (and deactivate the previous one; Monsell, 2003). Dreisbach and Fröber

(2019) proposed two potential mechanisms for how cognitive flexibility can be modulated. First, lowering the *updating threshold* in working memory should facilitate the entry of new information into working memory. Second, keeping *two tasks active* in working memory should facilitate the switching between these tasks. The first mechanism of a lower updating threshold can generalize to new tasks while the second mechanism of keeping both tasks active is task-specific. For example, previous research indicated that the context effect of the forced switch frequency operates through the second mechanism because the effect only transfers to new items of the same tasks but not to new task sets (Fröber, Jurczyk, & Dreisbach, 2021). For the present findings, it is unclear through which mechanism the task-switching ability (Study 1) or the associated temporal costs (Study 2) influenced flexibility. A better switching ability may suggest being better at lowering the updating threshold in working memory. Alternatively, the ability may only capture how the specific tasks are kept active in working memory. The two options can be tested against each other by measuring the switching ability between two tasks to predict the VSR between two different tasks. For the associated temporal costs, a similar method of using more than two task sets may help uncover the underlying mechanism. Due to similarities to the contextual switch frequency effect, one may expect a similar task-specific nature (Fröber, Jurczyk, & Dreisbach, 2021) indicating that flexibility is increased by keeping both tasks active in working memory. The same can be true for the associative learning effects that may be found when using more task-relevant cues (as suggested by Study 3). Future studies are needed to test these predictions and uncover the underlying mechanisms of the reasons to switch.

Generally, when we analyze voluntary task-switching behavior, we typically use the VSR as the main dependent variable of interest. How often do the participants switch voluntarily between the two simple tasks? In the present project and previous studies, much weight is given to this decision to switch. However, we sometimes overlook what the participant is actually doing: The participant is choosing to perform a certain task (which may or may not happen to be a task switch). The overt decision process relates primarily to the chosen tasks but it is unclear how much thought is given to the transition between tasks. The latter would require processing the current task relative to the previous task which participants simply may not do (because that would be effortful). Introspective switch costs show that individuals are able to detect their larger RTs when switching tasks (Bratzke & Bryce, 2019; Study 1). However, this introspection provides no evidence that people have access to abstract representations about task repetitions and task switches. They simply have a (largely) accurate feeling about the speed of their current response. Thus, a participant's choice may first and foremost reflect the preference for the chosen task at a given moment. Even when using tasks of similar difficulty, without further instructions, this task bias can be rather pronounced (Arrington et al., 2014). For the tasks of the present project, the task bias defined as the choice rate of the more preferred task amounted to about 75 % (see Supplemental Material of Study 3). So, when we register a task switch this may simply imply that the alternative task was more attractive. When thinking about voluntary task switching in everyday life, it becomes clear that the decision to switch highly depends on the actual tasks. When reading a captivating book, you may only switch the task if you have to do so, for example, to take an important phone call. And after the call, you may be inclined to quickly switch back to reading. In this sense, the hybrid task-switching paradigm (Fröber & Dreisbach, 2017) has high ecological validity for measuring voluntary choice behavior because the participants can work on the preferred task but are sometimes forced to switch to the alternative task. Hence, we can measure the voluntary choice to switch back to the preferred task. It is an open question for future research whether or not these kinds of task switches are perceived as effortful. They may even feel positive because they lead to the intended goal of performing the preferred task. Alternatively, if these switches are still effortful, the task preference may justify this investment of effort. Taken together, the task bias may reflect an ecologically valid factor influencing voluntary switching. In future research, one may disentangle the decision for a certain task from the decision to switch for example by using the double registration procedure (see Arrington & Logan, 2005; Fröber et al., 2019).

As a final note, describing cognitive control as a balance of cognitive flexibility and stability implies an antagonistic nature (Goschke, 2013; Hommel, 2015). Increasing one side should automatically decrease the other. High flexibility should be accompanied by low stability. In this view, cognitive flexibility and stability are two sides of a single dimension. This perspective may be particularly intuitive because the task-switching paradigm is designed to capture this one-dimensional balance. The main measures of the switch costs and the VSR contain both control modes. More precisely, the switch costs describe the performance difference between task switches and task repetitions. Repetitions benefit from cognitive stability while switches benefit from cognitive flexibility (Dreisbach & Fröber, 2019). If a participant has lower switch costs, this could be the result of fast switches (high flexibility) *or* slow repetitions (low stability). The same is true for the VSR. A high VSR may indicate a tendency towards task switches (high flexibility) or an avoidance of task repetitions (low stability). The two cases are normally interpreted in the same way: The balance is shifted towards cognitive flexibility (away from cognitive stability). However, some findings suggest that stability and flexibility can be independent (for a review, see Egner, 2023). For example, Fröber and Dreisbach (2023) showed that stability induced by remaining high reward can promote behavioral flexibility in the form of switching back to the preferred task (which has just been denied). Therefore, in different dimensions, contexts, or tasks, people may be flexible and stable at the same time. Even on the same task, a strong task focus indicating stability does not preclude a high preparedness to switch tasks (Egner, 2023). The present project investigated the reasons to switch relating to cognitive flexibility in the task-switching paradigm. Therefore, another interesting topic would be the reasons to be stable in a task where stability (task shielding) is the effortful option, e.g. in the Stroop task (Stroop, 1935).

Regarding cognitive stability, the factors investigated in the present studies (individual ability, temporal costs, and associative learning) may have similar effects. At least theoretically, the decision to invest cognitive control in the form of high stability should also be influenced by effort-based decision-making and cost-benefit analyses (Shenhav et al., 2021). Outweighing the avoidance of high cognitive stability with simple associative learning using task-irrelevant cues should again prove difficult.

# Conclusion

The present project connected the topics of cognitive effort and cognitive flexibility. Both are highly relevant concepts in everyday life. In most situations, effort is a key determinant of success. Similarly, cognitive flexibility is fundamental in order to show the appropriate behavior in our dynamic lives. Therefore, uncovering the factors that motivate cognitive effort or facilitate cognitive flexibility is a critical area of research. In this context, the present studies provided novel insights into effort-based decision-making and associative learning processes. Individuals consider their ability and the temporal costs when deciding to invest effortful cognitive flexibility by switching between two simple tasks. Furthermore, associative learning processes appear to require relevant experiences to trigger this type of effortful cognitive flexibility. While the waste of effort generally tends to be avoided, the present project showed that there are multiple valid reasons to switch. These reasons can motivate us to invest substantial effort. Understanding these reasons is crucial because, on a larger scale, effort is required to achieve challenging goals such as completing a dissertation project.

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# APPENDIX

## Supplemental Material: Study 1

Here we present analyses that take the order of phases in Experiments 1 and 3 into account to rule out potential order effects in our design. Participants would either start with the HTS phase or with the introspection phase. This two-level factor was added to the analyses of the objective RT in the HTS and the introspection phase, and the subjective RT. Furthermore, we conducted separate multiple linear regression analyses for each order predicting the VSR with the objective switch costs of the HTS phase and the subjective switch costs of the introspection phase.

### Experiment 1

In Experiment 1, the 2 x 2 mixed ANOVA with the within-subjects factors transition (repetition, switch) and the between-subjects factor order (HTS first, introspection first) of the objective switch costs in the HTS phase resulted in a significant effect of transition, $F(1, 116) = 330.91$, $p < .001$, $\eta_p^2 = .74$, with faster RTs on task repetitions ($M$ = 632 ms, $SD$ = 131) compared to task switches ($M$ = 746 ms, $SD$ = 180). The main effect of order got significant, $F(1, 116) = 6.47$, $p = .012$, $\eta_p^2 = .05$, with faster RTs in the HTS phase when starting with the introspection phase ($M$ = 676 ms, $SD$ = 212; HTS first: $M$ = 746 ms, $SD$ = 212) Finally, the interaction effect reached significance, $F(1, 116) = 4.84$, $p = .030$, $\eta_p^2 = .04$. There were higher switch costs in the HTS first condition ($M$ = 178 ms, $SD$ = 103) than in the introspection first condition ($M$ = 139 ms, $SD$ = 85; see Figure S1, panel A).

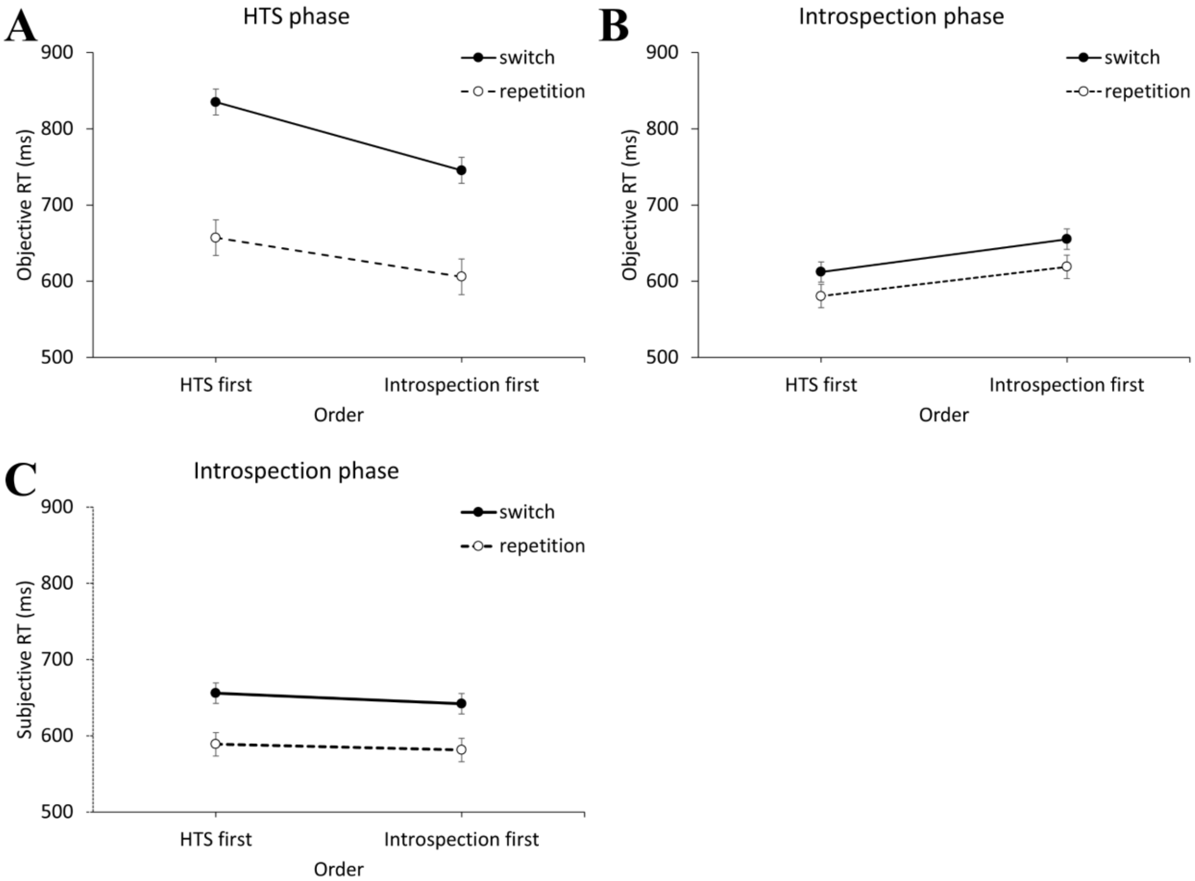
The same ANOVA was conducted using the objective RT of the introspection phase. The analysis revealed a significant effect of transition, $F(1, 116) = 66.03$, $p < .001$, $\eta_p^2 = .36$. Participants were faster on task repetitions ($M$ = 600 ms, $SD$ = 103) compared to task switches ($M$ = 634 ms, $SD$ = 118). Furthermore, there was a significant main effect of order, $F(1, 116) = 4.18$, $p = .043$, $\eta_p^2 = .04$. In the introspection phase, RTs were faster when participants started with the HTS phase ($M$ = 596 ms, $SD$ = 153; Introspection first: $M$ = 637 ms, $SD$ = 153). The interaction was not significant, $F(1, 116) = 0.28$, $p = .601$, $\eta_p^2 < .01$ (see Figure S1, panel B).

The analysis of the subjective RT yielded a significant effect of transition, $F(1, 116) = 122.73$, $p < .001$, $\eta_p^2 = .51$. Estimated RTs were lower on repetitions ($M$ = 585 ms, $SD$ = 179) compared to switches($M$ = 649 ms, $SD$ = 177). Neither a significant effect of order nor a significant interaction was observed (all $F$s < 0.28, all $p$s > .595; see Figure S1, panel C).

Taken together, when starting with the introspection phase, participants showed better performance and lower switch costs in the later HTS phase. This finding represents typical practice effects. Similarly, when starting with the HTS phase, the general performance in the later introspection phase was facilitated. There was no beneficial effect on the already very low switch costs. The subjective reaction times were not modulated by the order of the two phases.

**Figure S1**

*Objective RTs in the HTS phase (panel A), objective RTs in the introspection phase (panel B), and subjective RTs (panel C) as a function of order (HTS first, Introspection first) and transition (repetition, switch) in Experiment 1*




*Note.* Error bars represent ±1 standard error of the mean.

We conducted a separate multiple regression when participants started with the HTS phase with the objective switch costs of the HTS phase and subjective switch costs of the introspection phase as predictors for the VSR. The model equation was marginally significant, $F(2, 56) = 3.01$, $p = .057$, $R^2 = .10$, adjusted $R^2 = .07$. The VSR was equal to 26.57 - .022 (objective switch costs) - .015 (subjective switch costs). Only the objective switch costs were a significant predictor, $\beta = -0.28$, $t(56) = -2.16$, $p = .035$. The beta of the subjective switch costs did not reach significance $\beta = -0.12$, $t(56) = -0.96$, $p = .344$.

When starting with the introspection phase, the regression analysis yielded a significant model, $F(2, 56) = 4.73$, $p = .013$, $R^2 = .15$, adjusted $R^2 = .11$. Here, the VSR was equal to 30.51 - .034

(objective switch costs) - .019 (subjective switch costs). Again, only the objective switch costs significantly predicted the VSR, $\beta$ = -0.34, $t(56) = -2.67$, $p = .010$, while the subjective switch costs did not, $\beta$ = -0.13, $t(56) = -1.01$, $p = .315$. In sum, only the objective switch costs predicted the VSR especially when participants started with the introspection phase.

## Experiment 3

In Experiment 3, the 2 (transition: repetition, switch; within) x 2 (FSR: 25 %, 75 %) x 2 (order: HTS first, introspection first, between) mixed ANOVA of the objective switch costs in the HTS phase brought up a significant main effect of transition, $F(1, 97) = 198.18$, $p < .001$, $\eta_p^2 = .67$, with faster RTs on task repetitions (*M* = 634 ms, *SD* = 115) compared to task switches (*M* = 806 ms, *SD* = 207). Apart from that, the interaction between transition and FSR reached significance, $F(1, 97) = 50.96$, $p < .001$, $\eta_p^2 = .34$, indicating higher switch costs in the 25 % FSR condition compared to the 75 % FSR condition. The main effect of phase order was not significant, $F(1, 97) = 3.92$, $p = .051$, $\eta_p^2 = .04$. Descriptively, participants responded faster in the HTS phase when starting with the introspection phase (*M* = 689 ms, *SD* = 222) than when starting with the HTS phase (*M* = 751 ms, *SD* = 220). All other effects did not reach significance (all *F*s < 1.16, all *p*s > .285).

For the objective RTs in the introspection phase, a 2 (transition) x 2 (order) ANOVA yielded a significant effect of transition, $F(1, 97) = 182.83$, $p < .001$, $\eta_p^2 = .65$. Participants were faster on task repetitions (*M* = 566 ms, *SD* = 84) compared to task switches (*M* = 679 ms, *SD* = 147). No other effect reached significance (all *F*s < 1.34, all *p*s > .251).

The same analysis with subjective RTs resulted in a significant effect of transition, $F(1, 97) = 78.15$, $p < .001$, $\eta_p^2 = .45$. Participants estimated faster RTs on task repetitions (*M* = 549 ms, *SD* = 195) compared to task switches (*M* = 609 ms, *SD* = 209). All other effects failed to reach significance (all *F*s < 0.18, all *p*s > .673). Taken together, the order of phases did not significantly influence the results of the objective and subjective RTs or the FSR effect.

For the participants that started with the HTS phase, the multiple regression with objective and subjective switch costs as predictors for the VSR did not reach significance, $F(2, 47) = 1.80$, $p = .177$, $R^2 = .07$, adjusted $R^2 = .03$. The VSR was equal to 24.33 - .007 (objective switch costs) - .022 (subjective switch costs). Neither the objective switch costs, $\beta$ = -0.11, $t(47) = -0.79$, $p = .436$, nor the subjective switch costs, $\beta$ = -0.23, $t(47) = -1.61$, $p = .113$, were a significant predictor.

When starting with the introspection phase, the multiple regression model was significant, $F(2, 46) = 4.33$, $p = .019$, $R^2 = .16$, adjusted $R^2 = .12$. In the model, the VSR was equal to 26.82 - .016 (objective switch costs) - .042 (subjective switch costs). The beta weights of the objective switch costs, $\beta$ = -0.28, $t(46) = -2.03$, $p = .048$, and the subjective switch costs, $\beta$ = -0.32, $t(46) = -2.33$, $p = .024$, reached significance. In sum, the prediction of the VSR was the strongest when participants started with the introspection phase.

# Supplemental Material: Study 2

Here, we report violin plots with individual data points and the descriptive statistics of the RT and error rate analyses. Furthermore, in both experiments, we ran additional exploratory analyses on the performance (RT and error rate) in voluntary trials. Here, the number of trials per design cell can vary heavily depending on the participant's choice behavior (by predominantly repeating or switching tasks). Therefore, these results should be considered with caution. Last, in Experiment 2 we analyzed the VSR when excluding trials directly following forced choice (where the ITI was manipulated) to avoid influences of the preceding ITI on the VSR. Because in Experiment 1 the ITI was also manipulated following voluntary switches, this exploratory analysis was only performed in Experiment 2.

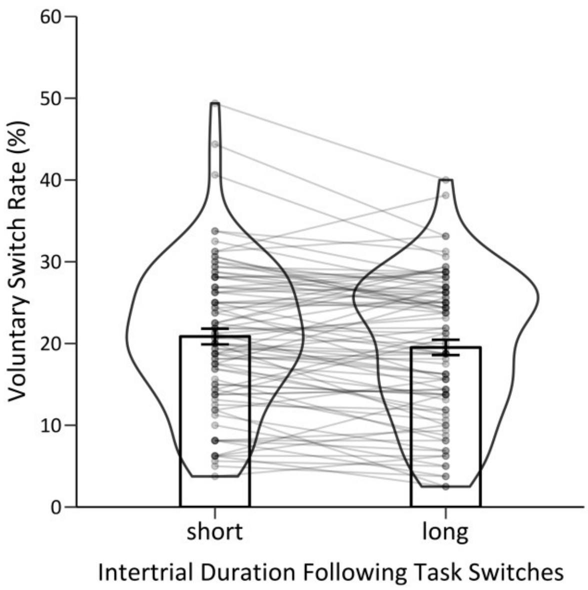
## Experiment 1

### Violin Plots with Individual Data Points

Figure S1 shows the VSR data in a violin plot with individual data points of Experiment 1. Figure S2 depicts the RT and error rate in a similar plot.

**Figure S1**

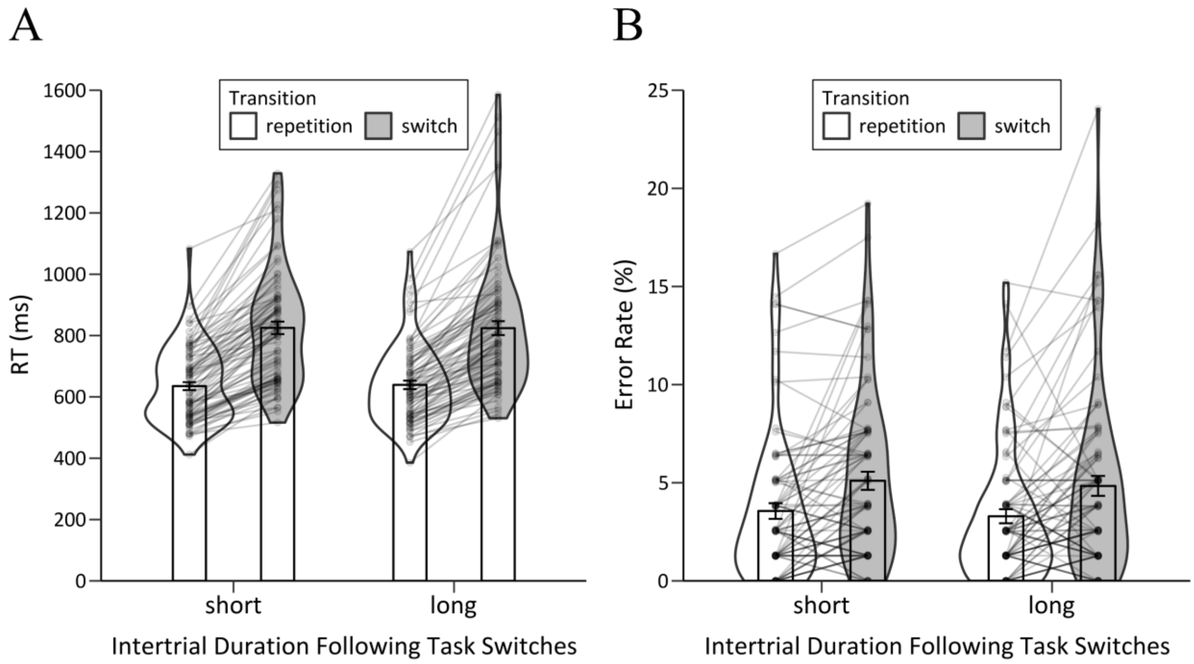
*Voluntary Switch Rate (VSR, in %) as a Function of Duration Condition (Short Duration After Task Switches=500 ms, Long Duration After Task Switches=1500 ms) in Experiment 1*




*Note.* Violin plot. Bars represent the overall mean VSR per condition. Error bars represent ± one standard error of the mean. Dots represent the VSR of each participant while conditions of the same participant are connected with a grey line. Excluding four participants with the largest VSR led to the same statistical pattern of results.

**Figure S2**

*Reaction Time (RT, in ms; Panel A) and Error Rates (in %; Panel B) as a Function of Duration Condition (Short Duration After Task Switches=500 ms, Long Duration After Task Switches=1500 ms) and Transition (Task Repetition, Task Switch) in Experiment 1*




*Note.* Violin plot. Bars represent the overall mean per condition. Error bars represent ± one standard error of the mean. Dots represent the individual mean of each participant while conditions of the same participant are connected with a grey line.

## Descriptive Statistics of RT and error rate

Table S1 contains the mean RT per condition of the forced-choice RT analysis in Experiment 1. Table S2 shows the mean error rate per condition of the forced-choice error rate analysis in Experiment 1.

**Table S1**

*Mean (SD) Reaction Time (in ms) in Forced-Choice Trials as a Function of Duration Condition (Short Duration After Task Switches=500 ms, Long Duration After Task Switches=1500 ms) and Transition (Task Repetition, Task Switch) in Experiment 1*

| | Task Repetition | Task Switch | Σ |
|---|---|---|---|
| Short Duration After Task Switches | 635 (*119*) | 825 (*188*) | 730 (*148*) |
| Long Duration After Task Switches | 639 (*129*) | 824 (*211*) | 732 (*165*) |
| Σ | 637 (*120*) | 825 (*196*) | 731 (*154*) |

**Table S2**

*Mean (SD) Error Rate (in %) in Forced-Choice Trials as a Function of Duration Condition (Short Duration After Task Switches=500 ms, Long Duration After Task Switches=1500 ms) and Transition (Task Repetition, Task Switch) in Experiment 1*

| | Task Repetition | Task Switch | Σ |
|---|---|---|---|
| Short Duration After Task Switches | 3.56 (*3.73*) | 5.10 (*4.28*) | 4.33 (*3.76*) |
| Long Duration After Task Switches | 3.29 (*3.33*) | 4.84 (*4.71*) | 4.07 (*3.64*) |
| Σ | 3.43 (*3.28*) | 4.97 (*4.23*) | 4.20 (*3.57*) |

## Performance in Voluntary Trials

### *RT*

In Experiment 1, the 2 (duration condition: short duration after task switches, long duration after task switches) x 2 (transition: task repetition, task switch) repeated-measures ANOVA of the RT in voluntary trials showed the significant main effect of transition, $F(1, 85)=114.26$, $p<.001$, $\eta_p^2=.57$. Reaction times were faster for voluntary task repetitions than for voluntary task switches. The main effect of the duration condition and the interaction were not significant (all *F*s<0.33, all *p*s>.568; see Table S3).

**Table S3**

*Mean (SD) Reaction Time (in ms) in Voluntary Trials as a Function of Duration Condition (Short Duration After Task Switches=500 ms, Long Duration After Task Switches=1500 ms) and Transition (Task Repetition, Task Switch) in Experiment 1*

| | Task Repetition | Task Switch | Σ |
|---|---|---|---|
| Short Duration After Task Switches | 649 (*159*) | 784 (*215*) | 716 (*181*) |
| Long Duration After Task Switches | 647 (*169*) | 775 (*246*) | 711 (*197*) |
| Σ | 648 (*162*) | 780 (*220*) | 714 (*185*) |

### *Error Rate*

Similarly, the 2 (duration condition) x 2 (transition) repeated-measures ANOVA of the error rate in voluntary trials revealed a significant main effect of transition, $F(1, 85)=8.77$, $p=.004$, $\eta_p^2=.09$. Participants made fewer errors on voluntary repetitions compared to voluntary switches. The main effect of the duration condition and the interaction were not significant (all *F*s<0.27, all *p*s>.605; see Table S4).

**Table S4**

*Mean (SD) Error Rate (in %) in Voluntary Trials as a Function of Duration Condition (Short Duration After Task Switches=500 ms, Long Duration After Task Switches=1500 ms) and Transition (Task Repetition, Task Switch) in Experiment 1*

| | Task Repetition | Task Switch | Σ |
|---|---|---|---|
| Short Duration After Task Switches | 2.58 (*2.63*) | 3.68 (*4.60*) | 3.13 (*3.17*) |
| Long Duration After Task Switches | 2.77 (*2.51*) | 3.83 (5.41) | 3.30 (*3.40*) |
| Σ | 2.68 (*2.37*) | 3.75 (*4.15*) | 3.22 (*2.93*) |

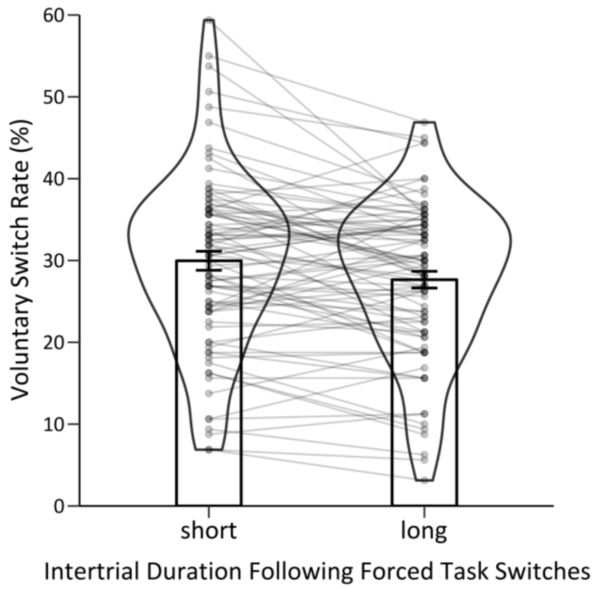
## Experiment 2

### Violin Plots with Individual Data Points

Figure S3 shows the VSR data in a violin plot with individual data points of Experiment 2. Figure S4 depicts the RT and error rate in a similar plot.

**Figure S3**

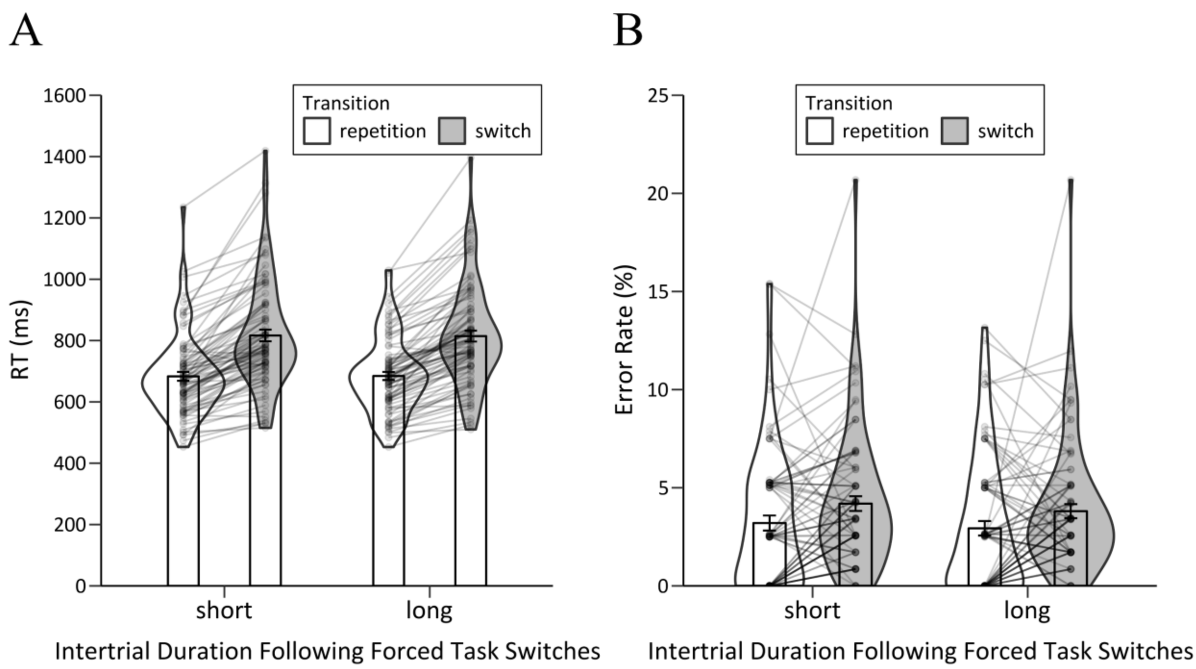
*Voluntary Switch Rate (VSR, in %) as a Function of Duration Condition (Short Duration After Forced Task Switches=500 ms, Long Duration After Forced Task Switches=1500 ms) in Experiment 2*




*Note.* Violin plot. Bars represent the overall mean VSR per condition. Error bars represent ± one standard error of the mean. Dots represent the VSR of each participant while conditions of the same participant are connected with a grey line. Excluding five participants with the largest VSR led to the same statistical pattern of results.

**Figure S4**

*Reaction Time (RT, in ms) as a Function of Duration Condition (Short Duration After Forced Task Switches=500 ms, Long Duration After Forced Task Switches=1500 ms) and Transition (Task Repetition, Task Switch) in Experiment 2*




*Note.* Violin plot. Bars represent the overall mean per condition. Error bars represent ± one standard error of the mean. Dots represent the mean of each participant while conditions of the same participant are connected with a grey line.

## Descriptive Statistics of RT and error rate

Table S5 contains the mean RT per condition of the forced choice RT analysis in Experiment 2. Table S6 shows the mean error rate per condition of the forced choice error rate analysis in Experiment 2.

**Table S5**

*Mean (SD) Reaction Time (in ms) in Forced-Choice Trials as a Function of Duration Condition (Short Duration After Forced Task Switches=500 ms, Long Duration After Forced Task Switches=1500 ms) and Transition (Task Repetition, Task Switch) in Experiment 2*

| | Task Repetition | Task Switch | Σ |
|---|---|---|---|
| Short Duration After Forced Task Switches | 683 (*132*) | 817 (*175*) | 750 (*148*) |
| Long Duration After Forced Task Switches | 685 (*121*) | 815 (*165*) | 750 (*139*) |
| Σ | 684 (*123*) | 816 (*168*) | 750 (*142*) |

**Table S6**

*Mean (SD) Error Rate (in %) in Forced-Choice Trials as a Function of Duration Condition (Short Duration After Forced Task Switches=500 ms, Long Duration After Forced Task Switches=1500 ms) and Transition (Task Repetition, Task Switch) in Experiment 2*

| | Task Repetition | Task Switch | Σ |
|---|---|---|---|
| Short Duration After Forced Task Switches | 3.20 (*3.54*) | 4.19 (*3.48*) | 3.70 (*3.14*) |
| Long Duration After Forced Task Switches | 2.93 (*3.42*) | 3.81 (*3.29*) | 3.37 (*2.83*) |
| Σ | 3.07 (*2.85*) | 4.00 (*3.18*) | 3.53 (*2.74*) |

## Performance in Voluntary Trials

### *RT*

In Experiment 2, the 2 (duration condition: short duration after forced task switches, long duration after forced task switches) x 2 (transition: task repetition, task switch) repeated-measures ANOVA of the RT in voluntary trials revealed a significant main effect of transition, $F(1, 84)=109.49$, $p<.001$, $\eta_p^2=.57$. Participants responded faster on voluntary task repetitions compared to voluntary task switches. There was also a significant main effect of duration condition, $F(1, 84)=7.65$, $p=.007$, $\eta_p^2=.08$, with faster RTs with a long duration following forced task switches compared to a short duration following forced task switches. The main effects were further qualified by a significant interaction between duration condition and transition, $F(1, 84)=4.67$, $p=.034$, $\eta_p^2=.05$. The switch costs in voluntary trials were larger in blocks with a short duration following switches ($M$=119 ms, $SD$=111) than in blocks with a long duration following switches ($M$=99 ms, $SD$=99; see Table S7). Follow-up t-tests revealed that this effect was driven by slower RTs on voluntary task switches in blocks with a short duration following switches compared to blocks with a long duration following switches, $t(84)=2.89$, $p=.005$, $d=0.31$. RTs on voluntary task repetitions did not significantly differ between duration conditions, $t(84)=0.92$, $p=.359$, $d=0.10$.

**Table S7**

*Mean (SD) Reaction Time (in ms) in Voluntary Trials as a Function of Duration Condition (Short Duration After Forced Task Switches=500 ms, Long Duration After Forced Task Switches=1500 ms) and Transition (Task Repetition, Task Switch) in Experiment 2*

| | Task Repetition | Task Switch | Σ |
|---|---|---|---|
| Short Duration After Forced Task Switches | 692 (*141*) | 811 (*201*) | 751 (*165*) |
| Long Duration After Forced Task Switches | 687 (*143*) | 786 (*193*) | 736 (*162*) |
| Σ | 689 (*140*) | 798 (*193*) | 744 (*162*) |

### *Error Rate*

The 2 (duration condition) x 2 (transition) repeated-measures ANOVA of the error rate in voluntary trials revealed no significant main effect of transition or duration condition, and no significant interaction (all *F*s<2.95, all *p*s>.090). Overall the error rate in voluntary trials was rather low (see Table S8).

**Table S8**

*Mean (SD) Error Rate (in %) in Voluntary Trials as a Function of Duration Condition (Short Duration After Forced Task Switches=500 ms, Long Duration After Forced Task Switches=1500 ms) and Transition (Task Repetition, Task Switch) in Experiment 2*

| | Task Repetition | Task Switch | Σ |
|---|---|---|---|
| Short Duration After Forced Task Switches | 2.22 (*2.20*) | 2.72 (*3.56*) | 2.47 (*2.51*) |
| Long Duration After Forced Task Switches | 1.92 (*1.82*) | 2.19 (*3.42*) | 2.05 (*2.19*) |
| Σ | 2.07 (*1.79*) | 2.45 (*2.90*) | 2.26 (*2.07*) |

### VSR Analysis Excluding Trials Directly Following Forced Choice

When excluding voluntary trials directly following forced choice, the VSR was still significantly larger in blocks with a short duration following forced task switches (*M*=14.77%, *SD*=13.19) than in blocks with a long duration following forced task switches (*M*=13.26%, *SD*=11.23), *t*(84)=1.91, *p*=.030, *d*=0.21. That the VSR within voluntary trials is overall reduced (compared to the overall VSR), is in line with the previous finding that the context effect of forced switches on the VSR predominantly takes place in the first trial after forced choice (Fröber, Jurczyk, & Dreisbach, 2021).

## Summary

Taken together, only in Experiment 2, there was a significant influence of the duration condition on the voluntary switch costs with increased switch costs in blocks with a short duration following forced switches. This exploratory finding may be critical because the decision to switch is often guided by the individual switch costs (Mayr & Bell, 2006; Mendl & Dreisbach, 2022; Mittelstädt, Dignath, et al., 2018). Participants with larger switch costs tend to switch tasks less often. Considering this typical relationship between switch costs and the VSR, the reduced voluntary switch costs in blocks with a long interval following switches should have resulted in increased VSRs. However, because we found the opposite pattern, participants appear to base their decision to switch in a given context more on the associated temporal costs of task switches relative to task repetitions instead of their actual voluntary switch costs. Last, the effect of the associated temporal costs on the VSR was still evident when excluding voluntary trials directly following forced choice (where the ITI duration was manipulated). This rules out that the VSR effect is primarily based on the ITI duration directly preceding the current trial and supports the notion that the associated temporal costs influence the willingness to switch.

# Supplemental Material: Study 3

Additional exploratory analyses are reported here. First, analyses of the test phase of Experiments 1-4 investigated the effect of the cue association on the voluntary switch rate (VSR) separately for the mixed block and the transfer block (for details about methods, see main text). These analyses were performed because the two test phases contained considerably different procedures. Furthermore, the task bias was calculated irrespective of the specific task (ranging from 50%, meaning no bias, to 100%, meaning always choosing the same task), and additional exploratory analyses were conducted without participants showing a strong bias to one of the two tasks (of at least 90%). This way, we can investigate whether the task bias influenced the present results. Next, exploratory analyses of Experiments 1, 2a, 2b, and 4 were performed only including participants who showed signs of cue learning in the learning phase (indicated by an overall positive influence of predictive cues on the RTs in Experiments 1 and 4, and valid cues on the RTs in Experiments 2a and 2b). This analysis was not possible in Experiment 3 or 5, where cue learning could not be measured. Additionally, sensitivity analyses of the Bayes Factor are reported for all Experiments by using a different, wide fixed effects scale factor prior ($r$ = 0.707; see Tables S1-S6) in the repeated measures ANOVAs of the main results.

Separate meta-analyses of Experiments 1-4 were performed for the cue association effect in the mixed block and the transfer block, as well as an additional meta-analysis of Experiments 1, 2a, 2b, and 4 only including participants with signs of cue learning. Last, an exploratory analysis of Experiment 5 was conducted where we only included participants who did not report the cue-task association.

### Experiment 1

In Experiment 1, additional paired-sample t-tests (two-sided) revealed no significant difference between cue associations (repetition, switch) in the mixed test phase, $t(48) = 0.66$, $p = .510$, $d = 0.09$, $BF_{10} = 0.191$. Hence, the VSR did not differ between the repetition cue ($M$ = 22.55%, $SD$ = 8.62) and the switch cue ($M$ = 21.81%, $SD$ = 7.83). In the following transfer phase, there was a just significant difference, $t(48) = 2.01$, $p = .050$, $d = 0.29$, $BF_{10} = 0.986$, with lower VSRs for the repetition cue ($M$ = 18.91%, $SD$ = 8.48) compared to the switch cue ($M$ = 20.54%, $SD$ = 7.88). This difference was not confirmed by the Bayes Factor.

The mean task bias was 73.34% ($SD$ = 15.96) in the mixed phase and 74.03% ($SD$ = 17.16) in the transfer phase. When excluding 14 participants with a task bias of 90% or more in one of the two test phases (leaving 35 participants in the analysis), the 2 (Phase: mixed phase, transfer phase) x 2 (Cue association: repetition, switch) ANOVA of the VSR in the test phases showed the same pattern of results as the analysis with the full sample. There was a significant main effect of phase, $F(1, 34) = 12.81$, $p = .001$, $\eta_p^2 = .27$, $BF_{10} = 28.083$, with a higher VSR in the mixed phase ($M$ = 20.83%, $SD$ = 7.83) compared to the transfer phase (*M* = 17.28%, *SD* = 7.47). The main effect of cue association ($BF_{10}$ = 0.447) and the interaction ($BF_{10}$ = 0.374) were not significant (all *F*s < 1.72, all *p*s > .198).

When only including the 20 participants who showed an RT benefit after predictive cues in the learning phase, the 2 (phase) x 2 (cue association) ANOVA of the VSR in the test phase revealed a significant main effect of phase, $F(1, 19) = 15.38$, $p < .001$, $\eta_p^2 = .45$, $BF_{10} = 26.64$, with a higher VSR in the mixed phase (*M* = 22.34%, *SD* = 6.99) compared to the transfer phase (*M* = 17.54%, *SD* = 8.21). The main effect of cue association ($BF_{10}$ = 0.371) and the interaction ($BF_{10}$ = 1.123) were not significant (all *F*s < 2.38, all *p*s > .140). The sensitivity analysis of Experiment 1 is reported in Table S1.

**Table S1**

*Bayes Factor sensitivity analysis with default (r = 0.5) and wide (r = 0.707) fixed effects scale factor prior in Experiment 1*

| Dependent Variable | Effect | $BF_{10}$ (default prior) | $BF_{10}$ (wide prior) |
|---|---|---|---|
| Error rate (learning phase) | cue type | 0.190 | 0.138 |
| | transition | 141.237 | 22.788 |
| | cue type x transition | 0.323 | 0.241 |
| RT (learning phase) | cue type | 0.370 | 0.276 |
| | transition | $4.831*10^9$ | $6.479*10^9$ |
| | cue type x transition | 0.233 | 0.203 |
| VSR (test phase) | cue association | 0.221 | 0.161 |
| | phase | 5.508 | 4.373 |
| | cue association x phase | 1.165 | 0.934 |

## Experiment 2a

The same t-test in Experiment 2a showed no significant VSR difference between cue associations in the mixed phase, $t(44) = 1.90$, $p = .064$, $d = 0.28$, $BF_{10} = 0.833$. Descriptively, the VSR was lower for the repetition cue (*M* = 21.81%, *SD* = 7.90) compared to the switch cue (*M* = 23.72%, *SD* = 8.68). Similarly, there was no significant influence of the cue association in the transfer phase, $t(44) = 0.46$, $p = .646$, $d = 0.07$, $BF_{10} = 0.179$. The VSR did not differ between the repetition cue (*M* = 21.77%, *SD* = 8.94) and the switch cue (*M* = 21.25%, *SD* = 8.26).

In Experiment 2a, the mean task bias was 73.70% (*SD* = 16.69) in the mixed phase and 77.05% (*SD* = 16.50) in the transfer phase. When excluding 13 participants with a strong task bias of 90% or more in one of the two test phases (leaving 32 participants in the analysis), the 2 (Phase) x 2 (Cue association) ANOVA showed the same pattern as the analysis with all participants included with no significant main effect or interaction (cue association $BF_{10}$ = 0.492, phase $BF_{10}$ = 0.632, cue association x phase $BF_{10}$ = 0.728; all *F*s < 2.21, all *p*s > .147).

When only including the 21 participants who showed a positive cue-validity effect in RTs in the learning phase, the 2 (phase) x 2 (cue association) ANOVA of the VSR resulted in a significant main effect of cue association, $F(1, 20) = 4.54$, $p = .046$, $\eta_p^2 = .19$, $BF_{10} = 1.081$, with a higher VSR for the switch cue ($M = 24.59\%$, $SD = 7.59$) compared to the repetition cue ($M = 22.24\%$, $SD = 8.06$). The Bayes Factor indicates anecdotal evidence for this effect. The main effect of phase ($BF_{10} = 1.332$) and the interaction ($BF_{10} = 0.798$) were not significant (all $F$s < 3.76, all $p$s > .067). The sensitivity analysis of Experiment 2a is reported in Table S2.

**Table S2**

*Bayes Factor sensitivity analysis with default (r = 0.5) and wide (r = 0.707) fixed effects scale factor prior in Experiment 2a*

| Dependent Variable | Effect | $BF_{10}$ (default prior) | $BF_{10}$ (wide prior) |
|---|---|---|---|
| Error rate (learning phase) | cue validity | 0.203 | 0.148 |
| | transition | 77.472 | 70.941 |
| | cue validity x transition | 0.620 | 0.484 |
| RT (learning phase) | cue validity | 0.353 | 0.264 |
| | transition | $4.160*10^{10}$ | $5.518*10^{10}$ |
| | cue validity x transition | 6.445 | 5.627 |
| VSR (test phase) | cue association | 0.261 | 0.193 |
| | phase | 0.435 | 0.331 |
| | cue association x phase | 0.928 | 0.739 |

## Experiment 2b

In the mixed phase of Experiment 2b, there was no significant difference between the repetition cue ($M = 24.11\%$, $SD = 12.23$) and the switch cue ($M = 23.51\%$, $SD = 10.89$), $t(45) = 0.56$, $p = .575$, $d = 0.08$, $BF_{10} = 0.186$. Likewise, no significant difference was found in the transfer phase (repetition Cue: $M = 29.80\%$, $SD = 20.41$; switch Cue: $M = 29.24\%$, $SD = 21.04$), $t(45) = 0.79$, $p = .436$, $d = 0.12$, $BF_{10} = 0.214$.

The mean task bias amounted to 74.30% ($SD = 16.73$) in the mixed phase and 56.68% ($SD = 10.33$) in the transfer phase. The descriptively lower task bias in the transfer phase can be explained by the given instruction. Only in the transfer phase of Experiment 2b, participants were asked to perform both tasks equally often (and in random order). When excluding 15 participants with a task bias of 90% or more in one of the two test phases (leaving 31 participants in the analysis), the 2 (Phase) x 2 (Cue association) ANOVA of the VSR in the test phases showed the significant main effect of phase, $F(1, 30) = 6.22$, $p = .018$, $\eta_p^2 = .17$, $BF_{10} = 2.731$, with a lower VSR in the mixed phase ($M = 22.98\%$, $SD = 13.16$) compared to the transfer phase ($M = 29.39\%$, $SD = 19.30$). The Bayes Factor indicated only anecdotal evidence for this effect. The main effect of cue association ($BF_{10}$ = 0.281) and the interaction ($BF_{10}$ = 0.374) were not significant (all *F*s < 1.22, all *p*s > .279).

When only including the 23 participants who showed a positive cue-validity effect in RTs in the learning phase, the 2 (phase) x 2 (cue association) ANOVA of the VSR revealed no significant effect (main effect phase: $BF_{10}$ = 0.686; main effect cue association: $BF_{10}$ = 0.315, interaction: $BF_{10}$ = 0.308; all *F*s < 1.01, all *p*s > .326). The sensitivity analysis of Experiment 2b is reported in Table S3.

**Table S3**

*Bayes Factor sensitivity analysis with default (r = 0.5) and wide (r = 0.707) fixed effects scale factor prior in Experiment 2b*

| Dependent Variable | Effect | $BF_{10}$ (default prior) | $BF_{10}$ (wide prior) |
|---|---|---|---|
| Error rate (learning phase) | cue validity | 0.263 | 0.193 |
| | transition | 37.219 | 34.690 |
| | cue validity x transition | 0.952 | 0.760 |
| RT (learning phase) | cue validity | 0.186 | 0.135 |
| | transition | $2.416*10^{10}$ | $3.191*10^{10}$ |
| | cue validity x transition | 0.330 | 0.247 |
| VSR (test phase) | cue association | 0.255 | 0.188 |
| | phase | 1.739 | 2.073 |
| | cue association x phase | 0.238 | 0.168 |

## Experiment 3

In the mixed phase of Experiment 3, the VSR again did not differ between the repetition cue (*M* = 19.38%, *SD* = 7.18) and the switch cue (*M* = 20.08%, *SD* = 8.14), *t*(45) = 0.58, *p* = .567, *d* = 0.08, $BF_{10}$ = 0.183. In the transfer phase, there was also no significant difference between the repetition cue (*M* = 19.37%, *SD* = 10.02) and the switch cue (*M* = 20.24%, *SD* = 9.09), *t*(45) = 0.87, *p* = .388, *d* = 0.13, $BF_{10}$ = 0.224.

In Experiment 3, the mean task bias was 79.03% (*SD* = 17.34) in the mixed phase and 75.66% (*SD* = 18.14) in the transfer phase. When excluding 20 participants with a strong task bias of more than 90% in one of the two test phases (leaving 28 participants in the analysis), the 2 (Phase) x 2 (Cue association) ANOVA resulted in the same pattern as the full analysis indicating no significant main effect or interaction (cue association $BF_{10}$ = 0.236, phase $BF_{10}$ = 0.342, cue association x phase $BF_{10}$ = 0.283; all *F*s < 0.55, all *p*s > .466). The sensitivity analysis of Experiment 3 is reported in Table S4.

**Table S4**

*Bayes Factor sensitivity analysis with default (r = 0.5) and wide (r = 0.707) fixed effects scale factor prior in Experiment 3*

| Dependent Variable | Effect | $BF_{10}$ (default prior) | $BF_{10}$ (wide prior) |
|---|---|---|---|
| VSR (test phase) | cue association | 0.265 | 0.196 |
| | phase | 0.199 | 0.145 |
| | cue association x phase | 0.220 | 0.161 |

## Experiment 4

In Experiment 4, we calculated a 2 (Cue association) x 2 (Task difficulty: difficult, easy) repeated-measures ANOVA with the VSR in each test phase. In the mixed phase, the main effect of task difficulty was significant, $F(1, 55) = 36.14$, $p < .001$, $\eta_p^2 = .40$, $BF_{10} = 5.320*10^4$, with a lower VSR to the difficult ($M = 7.59\%$, $SD = 9.09$) compared to the easy task ($M = 18.00\%$, $SD = 8.11$). There was no significant main effect of cue association (repetition: $M = 25.02\%$, $SD = 10.03$, switch: $M = 26.14\%$, $SD = 10.34$), $F(1, 55) = 1.59$, $p = .213$, $\eta_p^2 = .03$, $BF_{10} = 0.291$, nor a significant interaction between cue association and task difficulty, $F(1, 55) = 0.05$, $p = .817$, $\eta_p^2 < .01$, $BF_{10} = 0.211$. In the transfer phase, the same pattern emerged. There was a significant main effect of task difficulty, $F(1, 55) = 22.35$, $p < .001$, $\eta_p^2 = .29$, $BF_{10} = 839.816$, with a lower VSR to the difficult ($M = 7.73\%$, $SD = 9.69$) compared to the easy task ($M = 16.99\%$, $SD = 9.74$). The main effect of cue association (repetition: $M = 24.50\%$, $SD = 12.88$, switch: $M = 24.94\%$, $SD = 13.55$), $F(1, 55) = 0.24$, $p = .630$, $BF_{10} = 0.175$, and the interaction were not significant, $F(1, 55) = 0.03$, $p = .856$, $\eta_p^2 < .01$, $BF_{10} = 0.208$.

The mean task bias amounted to 76.63% ($SD = 16.85$) in the mixed phase and 76.20% ($SD = 17.90$) in the transfer phase. When excluding 21 participants with a task bias of 90% or more in one of the two test phases (leaving 35 participants in the analysis), the 2 (Phase) x 2 (Cue association) x 2 (Task difficulty) ANOVA of the VSR in the test phases showed the significant main effect of task difficulty, $F(1, 34) = 17.35$, $p < .001$, $\eta_p^2 = .34$, $BF_{10} = 122.326$, with a lower VSR to the difficult task ($M = 9.58\%$, $SD = 7.66$) compared to the easy task ($M = 14.69\%$, $SD = 7.53$). Furthermore, there was a significant interaction between phase and task difficulty, $F(1, 34) = 5.80$, $p = .022$, $\eta_p^2 = .15$, $BF_{10} = 2.729$. This interaction was not confirmed by the anecdotal Bayes Factor. Post-hoc t-tests revealed that the VSR to the difficult number task did not differ between the mixed phase ($M = 9.33\%$, $SD = 6.54$) and the transfer phase ($M = 9.82\%$, $SD = 9.42$), $t(34) = 0.44$, $p = .586$, $d = 0.09$, $BF_{10} = 0.209$. The VSR to the easy task was significantly higher in the mixed phase ($M = 15.74\%$, $SD = 7.26$) compared to the transfer phase ($M = 13.64\%$, $SD = 8.52$), $t(34) = 2.52$, $p = .017$, $d = 0.43$, $BF_{10} = 2.786$. This post hoc test was not confirmed by the anecdotal Bayes Factor. No other effects or interactions were significant (all $F$s < 2.04, all $p$s > .162, phase $BF_{10} = 0.427$, cue association $BF_{10} = 0.361$, phase x cue association $BF_{10}$ =

0.245, cue association x task difficulty $BF_{10}$ = 0.346, phase x cue association x task difficulty $BF_{10}$ = 0.233).

When only including the 29 participants who showed an RT benefit after predictive cues in the learning phase, a 2 (phase) x 2 (cue association) ANOVA of the VSR in the test phase revealed no significant effect (main effect phase: $BF_{10}$ = 1.292; main effect cue association: $BF_{10}$ = 0.473, interaction: $BF_{10}$ = 0.270; all $F$s < 3.81, all $p$s > .061). The sensitivity analysis of Experiment 4 is reported in Table S5.

**Table S5**

*Bayes Factor sensitivity analysis with default (r = 0.5) and wide (r = 0.707) fixed effects scale factor prior in Experiment 4*

| Dependent Variable | Effect | $BF_{10}$ (default prior) | $BF_{10}$ (wide prior) |
|---|---|---|---|
| Error rate (learning phase) | cue type | 0.243 | 0.177 |
| | transition | $6.782*10^5$ | $8.159*10^5$ |
| | task difficulty | 4.809 | 3.786 |
| | cue type x transition | 0.246 | 0.184 |
| | cue type x task difficulty | 0.223 | 0.166 |
| | transition x task difficulty | 11.120 | 9.526 |
| | cue type x transition x task difficulty | 0.187 | 0.135 |
| RT (learning phase) | cue type | 0.156 | 0.112 |
| | transition | $3.082*10^{10}$ | $4.197*10^{10}$ |
| | task difficulty | $7.642*10^8$ | $9.538*10^8$ |
| | cue type x transition | 0.204 | 0.152 |
| | cue type x task difficulty | 0.230 | 0.172 |
| | transition x task difficulty | $5.077*10^4$ | $5.746*10^4$ |
| | cue type x transition x task difficulty | 0.202 | 0.147 |
| VSR (test phase) | cue association | 0.230 | 0.167 |
| | phase | 0.252 | 0.184 |
| | task difficulty | 7216.016 | 9685.465 |
| | cue association x phase | 0.193 | 0.140 |
| | cue association x task difficulty | 0.230 | 0.168 |
| | phase x task difficulty | 0.501 | 0.382 |
| | cue association x phase x task difficulty | 0.197 | 0.143 |

## Meta-Analyses

Regarding the effect of cue association only in the mixed phase, a meta-analysis (described in more detail in the main text) over all experiments indicated no significant effect on the VSR (*d* = 0.07, *p* = .315, 95% CI [-0.07, 0.21], see Figure S1). Similarly, in the transfer phase, a meta-analysis showed no significant influence of cue association on the VSR (*d* = 0.06, *p* = .381, 95% CI [-0.07, 0.19], see Figure S2).

An additional meta-analysis of the effect of cue association was conducted across Experiments 1, 2a, 2b, and 4 including only participants with signs of cue learning. The meta-analysis showed no significant influence of cue association on the VSR (*d* = 0.16, *p* = .180, 95% CI [-0.08, 0.41], see Figure S3).

**Figure S1**

*Meta-Analysis of the Effect of Cue Association (Repetition, Switch) on the VSR in the Mixed Phase Across Experiments 1-4*

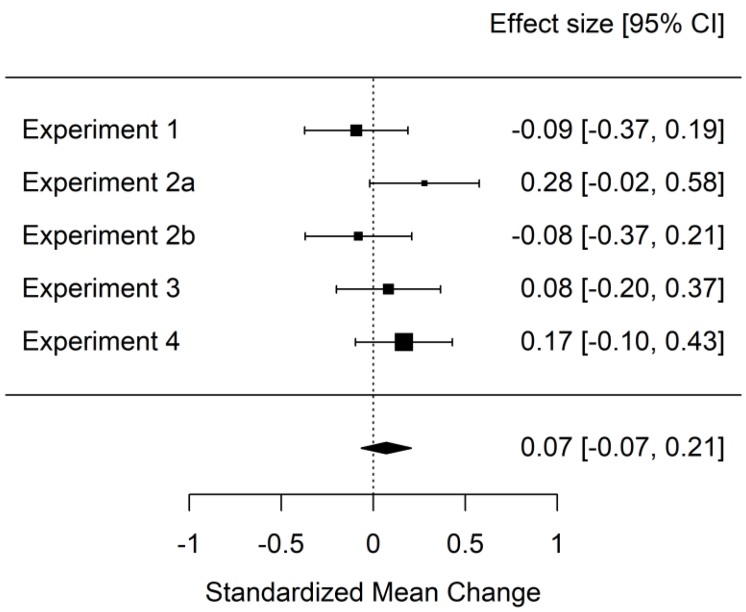


**Figure S2**

*Meta-Analysis of the Effect of Cue Association (Repetition, Switch) on the VSR in the Transfer Phase Across Experiments 1-4*

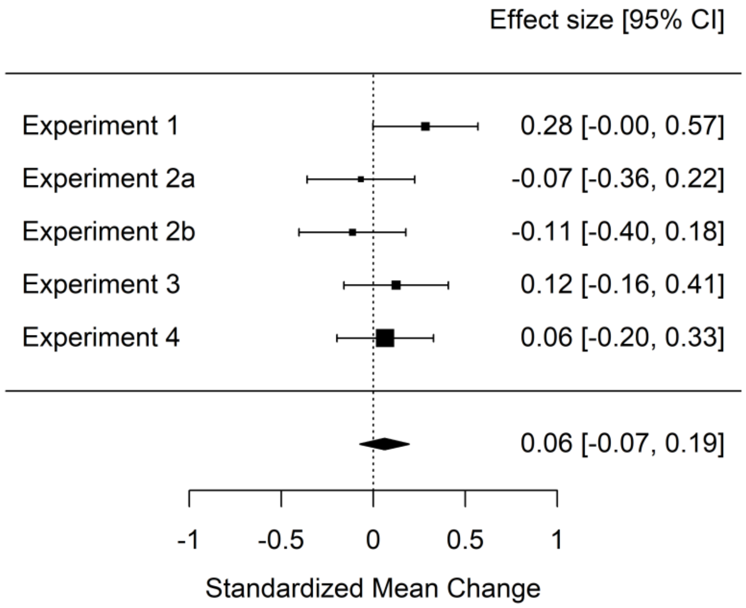
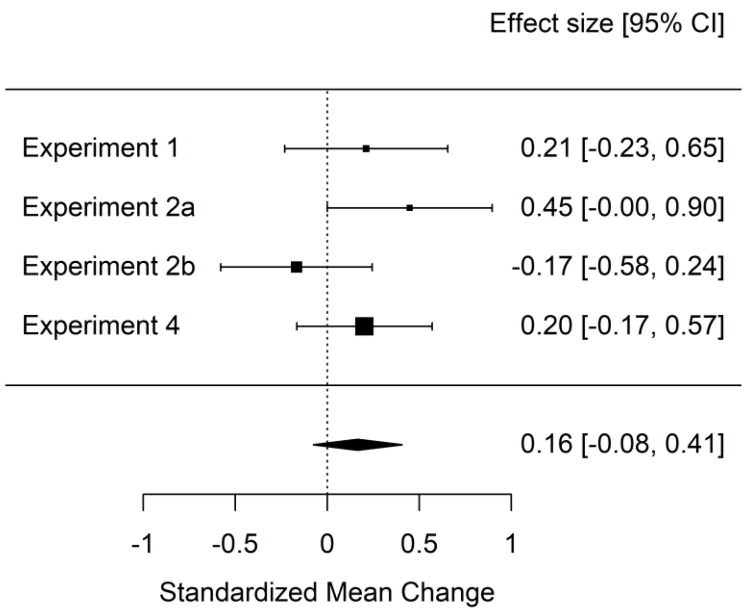



**Figure S3**

*Meta-Analysis of the Effect of Cue Association (Repetition, Switch) on the VSR in the Transfer Phase Across Experiments 1, 2a, 2b, and 4 Only Including Participants with Signs of Cue Learning*



## Experiment 5

In Experiment 5, 33 participants reported the association between cue and task in the post-experiment question. For that reason, an additional exploratory analysis was conducted only including participants who did not report the cue-task association ($n$=16). The 2 (Phase) x 2 (Cue association: number task, letter task) ANOVA of the choice rate of the number task revealed a significant effect of cue association, $F(1, 15) = 5.87$, $p = .029$, $\eta_p^2 = .28$, $BF_{10} = 1.711$, with a higher choice rate of the number task after cues associated with the number task ($M$ = 57.91%, $SD$ = 26.16) compared to cues associated with the letter task ($M$ = 49.46%, $SD$ = 27.66). The Bayes Factor indicated only anecdotal evidence for this effect. The main effect of phase ($BF_{10}$ = 0.769) and the interaction ($BF_{10}$ = 0.634) were not significant (all $F$s < 2.07, all $p$s > .171). The sensitivity analysis of Experiment 5 is reported in Table S6.

**Table S6**

*Bayes Factor sensitivity analysis with default (r = 0.5) and wide (r = 0.707) fixed effects scale factor prior in Experiment 5*

| Dependent Variable | Effect | $BF_{10}$ (default prior) | $BF_{10}$ (wide prior) |
|---|---|---|---|
| Choice rate of number task (test phase) | cue association | $6.193*10^4$ | $6.926*10^4$ |
| | phase | 0.209 | 0.152 |
| | cue association x phase | $3.918*10^4$ | $4.416*10^4$ |

## Summary

The meta-analyses separately for each test phase showed that there was no significant influence of cue association on the VSR in the mixed phase or the transfer phase across experiments. Only in the transfer phase of Experiment 1, there was a just significant effect of cue association on the VSR. This explorative difference was not expected as the effect should be more pronounced in the mixed phase, where the association is frequently reinforced.

Even though some participants exhibit a strong task bias, the pattern of results by and large remained the same when excluding these participants. Therefore, the strong task biases did not appear to mask potential effects of the cue association.

When only including participants with signs of cue learning in the learning phase, the results remained largely the same. The only difference is reflected in the now significant effect of cue association in Experiment 2a. However, the respective meta-analysis again indicates overall no significant effect of cue association on the VSR.

Overall, the sensitivity analyses showed that the results did not substantially change with different priors. Irrespective of the chosen priors, the results of Experiments 1-4 indicated moderate evidence for $H_0$, hence, no effect of the cue-transition association on the VSR. In contrast, Experiment

5 suggested that the learned cue-task association influenced voluntary choice behavior in the current paradigm even when only those participants were included who did not report the cue-task association in the post-experiment question.