Modulation of cognitive flexibility: Context- and expectancy-based effects on voluntary task choice

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Preface

Juggling different tasks at the same time is a vital part of our everyday reality. We constantly have to adjust the balance between several goals at work (Should I prepare the next seminar or edit this paper?), between different social obligations (Should I visit my mother-in-law or my best friend?), or more fundamentally between work and life. Accordingly, the underlying construct of cognitive flexibility is a major field of interest in cognitive psychology. Basic phenomena have now repeatedly been replicated and our understanding of the underlying concepts and models is more and more sharpened. One very fruitful endeavor now is to understand modulating influences on cognitive flexibility. They can tell us something about the limits of cognitive flexibility, but also the way it functions - for example, to which degree it underlies our intentional control. In addition, these modulating influences, investigated under tightly controlled laboratory conditions, are a purposeful step towards the applicability and generalizability of cognitive psychological research.

The main aim of the present thesis is to investigate how different contextual and expectancy-related variables modulate cognitive flexibility. The Introduction is targeted at explaining the relevant theoretical concepts and presenting previous empirical findings that lead up to open questions. Some of these questions will be answered in the currently presented original research. Finally, a General Discussion will present an overarching integration and interpretation of the underlying mechanisms of context- and expectancy-based effects on voluntary task choice.

Content

ABSTRACT

Performing different tasks in fast succession requires cognitive flexibility. Laboratory research shows that cognitive flexibility under controlled, predefined conditions usually incurs performance costs. However, less is known about cognitive flexibility that is chosen by the participants themselves – a situation that is not only relevant for our understanding of basic underlying processes, but is especially important concerning the practical implications of psychological research. What are beneficial conditions for deliberate cognitive flexibility and what are their limitations? The current PhD project aims to tackle these questions by investigating how voluntary task switching is influenced by (a) the context in which it occurs (the ratio of forced switches), (b) the reward expectancy within this context, and (c) the temporal predictability of the tasks themselves.

Addressing the first question, we compared electrophysiological preparatory activity in blocks of randomly intermixed voluntary and forced task switching trials to identify shared vs. unique preparation stages. Most importantly, a shared process seems to lie in a task-set reconfiguration stage, whereas task-set updating is more or less involved depending on the forced-choice context. Regarding the second question in focus, we de-confounded reward prospect and cue changes by using meaningless cues and blocked reward prospect. In blocks of high reward prospect, voluntary switch rate was globally decreased. Irrelevant cue changes had no impact on the deployment of deliberate flexibility. Finally, with respect to the third question under scrutiny, we found that participants' voluntary task choice was influenced by the foreperiod-task contingencies implemented in forced-choice trials, particularly so on switches.

In sum, motivationally relevant (reward prospect) or predictive (forced-choice context, time-based task expectancy) contextual information can have marked effects on voluntary task switching, whereas meaningless (even if salient) changes such as task-irrelevant cues reflect a limit to these context-driven effects. The results are discussed against the background of current theories on cognitive control, with a special emphasis on the flexibility-stability balance of cognitive control.

PART I – INTRODUCTION

"The myth of multitasking"[1]

"Think you're multitasking? Think again"[2]

If you search for the term 'multitasking' on any search engine, you will find a myriad of newspaper articles and blog entries informing you that multitasking of any kind is costly, inefficient, and generally leads to worse performance than if one performed each task on its own. Even if you are just rapidly switching between tasks, but at each time point focus on just one – say, you are writing an article and stop to check an incoming e-mail as soon as it pops up – the multitasking cost still applies. According to the articles, the best solution is to first finish one task before turning to the other.

Are the authors right? Scientific research of the last two and a half decades on dual-tasking and task switching agrees on one unfailingly present phenomenon: To do two tasks at the same time, or even just rapidly switching between them, is costly, slower, and more error-prone than performing just one task at a time (Monsell, 2003; Pashler, 1994). Even when participants can decide for themselves, they usually decide to repeat tasks whenever possible (Arrington & Logan, 2004; Kessler, Shencar, & Meiran, 2009). So in short: Multitasking is difficult and we prefer to avoid the associated costs.

But that is just one side of the story. Multitasking requirements have been increasing in recent years, especially in the workplace and in communication (González & Mark, 2004; Lohmann-Haislah & Schütte, 2013). This development does not seem to be coming to an end soon as it is tightly linked to new technologies and digital media: In younger generations – such as generation Z or Alpha – consuming more than one type of media at a time is the rule, not the exception (Duff & Segijn, 2019; Voorveld, Segijn, Ketelaar, & Smit, 2014). In leadership positions, multitasking is an essential part of the job, and success is built on the ability to work on several tasks and quickly shift between

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[1] Napier (2014), Goman (2011), Willingham (2019)
[2] Hamilton (2008)

them depending on current priorities (Tobak, 2011). In an even greater context, cognitive flexibility has been named one key factor for mental health (Kashdan & Rottenberg, 2010).

To me, the most important argument, though, is that multitasking is inevitable. Whether it is good or bad, it is a vital part of our everyday life. This seems to be even more the case for the generations to come. It is therefore important to find circumstances that are helpful to multitasking. This PhD project thus focused on different factors influencing the flexible allocation of cognitive control, or more generally, on flexibility in action control. What are beneficial conditions for deliberate cognitive flexibility and what are their limitations? The current thesis aims to tackle these questions by investigating how voluntary task switching is influenced by (a) the forced-choice context, (b) the underlying reward structure of the tasks, and (c) time-based task expectancy. By tackling the question at hand from various angles, we are not only able to investigate overarching functional principles, but also provide a steppingstone towards generalizability and ecological validity of multitasking research.

Let me start with a brief overview of the conceptual landscape in which we are moving, explaining the term cognitive control, how it is involved in task performance, and how its functional principles can be pinned down to a stability-flexibility balance. I will then go on with a short review of the experimental paradigm employed – the task switching paradigm and its voluntary variant – before turning to previous literature on context- and expectancy-based effects on cognitive flexibility.

A Functional Perspective on Cognitive Control: The Stability-Flexibility Balance

To understand the mechanisms behind successful multitasking, we first have to understand what processes determine successful task- or goal-directed behavior in general. To act in a goal-directed way, especially in the face of counteracting habitual and automatic response tendencies, cognitive control is required (Goschke, 2003). Cognitive control is needed to act in accordance with superordinate goals or tasks, to inhibit habitual or impulsive but non-conducive response tendencies, and to maintain goals in the face of distraction (Banich, 2009; Goschke, 2013; Miller & Cohen, 2001). Cognitive control requires active maintenance of task-relevant representations, but also the flexibility to update these representations whenever the current goal is no longer rewarding or appropriate.

For a long time, this action control has been viewed to be dependent on the activity of one control unit. That is, behavior was strictly dichotomized into fast, efficient, automatic (sub-conscious), "unwilled" behavior and slow, "willed" behavior under conscious control (Ach, 1910; James, 1890). In later investigations, Shiffrin and colleagues (Atkinson & Shiffrin, 1968; W. Schneider & Shiffrin, 1977; Shiffrin & Schneider, 1977) further specified that controlled as compared to automatic processes are much more susceptible to interference, can be executed only serially, and rely on a capacity-limited central executive. Hence, they maintained the dichotomy between controlled, "willful" behavior and automatic processing. Even though these models do not rely on the philosophical term of "free will", simply using a more mechanistic and depersonalized name ("control unit") is still shortsighted (Goschke, 2003; Hommel, 2015). How is the assumed singular control unit in itself controlled? Who controls the controller? At some point, an inexplicable "homunculus" still has to come into play as a non-measurable, non-materialistic entity that controls the control unit, the supervisory attentional system, the use of cognitive control (Miller & Cohen, 2001; Norman & Shallice, 1986).

Therefore, biologically plausible cognitive control models that incorporate the assumption of a deterministic, neuronally embedded controller have to assume some sort of self-regulatory control mechanism. Indeed, recent theories have started to withdraw from the strong dichotomy between bottom-up and top-down processing (Berridge, 2004; Goschke, 2013; Hommel, 2015). To allow for self-regulation, they incorporated a reinforcement learning mechanism that is relevant not only to basic processing, but also to higher-level goal striving processes (Berridge, 2004; Holroyd & Yeung, 2012; Inzlicht, Schmeichel, & Macrae, 2014; Shenhav, Botvinick, & Cohen, 2013; Vassena, Deraeve, & Alexander, 2017). This reinforcement learning mechanism allows the control system to regulate itself based on expected and actual outcomes. The main difference, then, between arguably more controlled and arguably more automatic behavior can be found in the recency of acquisition of S-R relationships and hence their familiarity (Hommel, 2015): Whereas "automatic" processes depend on S-R links that have been acquired much earlier, "controlled" behavior is driven by stimuli that have been instructed or acquired only recently. Indeed, the same processes that subserve goal-directed, "willed" behavior in one situation can come into conflict with it in another. The selection and subsequent pursuit of goals is driven by the same associative learning mechanisms as stimulus-driven behavior (Braem & Egner, 2018) and depends on a computed expected value of control (Kool & Botvinick, 2014; Kool, Shenhav, & Botvinick, 2017; Shenhav et al., 2013; Shenhav et al., 2017). It incorporates incentives provided by the task and the importance of goal achievement as benefits, while costs enter the computation as the required intensity of control and the subjective cost of effort. The output can be seen as the currently optimal value for control allocation. One exemplar for how this computation can be conceptualized and neuronally embedded is provided in the expected value of control theory by Shenhav et al. (2013; see also Kool et al., 2017; Shenhav, Cohen, & Botvinick, 2016), in which the authors argued that these input variables are integrated to compute expected utility, or, as the authors put it, a "willingness-to-pay signal in the currency of cognitive control" (p. 230). At the same time, prediction errors are computed that feed forward into subsequent computations and ensure a constant adjustment to current goals and environmental demands.

In recent theoretical frameworks (Dreisbach & Fröber, 2019; Goschke, 2013; Hommel, 2015), cognitive control is conceptualized as a number of control parameters that act on a dynamic and adaptable stability-flexibility balance: In some situations, stability is required, such as when a current goal or task has to be shielded against distractions. In other situations, however, goal-directed behavior depends on a flexible shift between alternatives, for instance when switching between tasks. Adaptive and successful goal-directed behavior should be characterized by a strong shielding against distraction, but at the same time, meaningful changes in the environment or the task should be faced with a swift change in behavior and priorities. If you are writing your thesis, you have to suppress the urge to look at the phone ever so often. However, if writing a specific chapter is no longer productive, going back to other chapters or checking your literature summaries is indicated.

It is important to note that stability and flexibility are necessarily antagonistic – a more stable cognitive control mode inevitably goes along with less flexibility, because focusing on the task at hand requires shielding from currently irrelevant (but potentially important) information. At the same time, increased flexibility also goes along with an increase in distractibility. To sum up, successful goal-directed behavior depends on the swiftly adapting stability-flexibility balance of cognitive control. Too rigid stability, as well as too fallible flexibility, will result in maladaptive perseveration or distractibility, as seen in mental disorders such as ADHD (Karch et al., 2014). Depending on the context, this balance has to be constantly readjusted. In the deliberations of Goschke (2013), a very fitting summary can be found:

> Contrary to the intuitive impression that intentions are immediate ‘triggering’ causes of actions, […] intentions play a much more indirect role and can be conceived of in terms of internal constraints that ‘preconfigure’ response dispositions and thereby bias which responses are subsequently activated by stimuli. (p. 409)

Hence, task performance in multitasking situations requires constant readjustments on the stability-flexibility balance: If a task is performed continually or repeatedly for some time (e.g., writing an article), a stable cognitive control mode is indicated, because in this mode information from other tasks (say, answering incoming e-mails) or completely irrelevant information should be relatively more prohibited from interfering. On the other hand, task switches (such as interrupting the writing process to answer a much-awaited phone call from a funding agency) require an efficient and swift disengagement and the rapid activation of the new task, that is, they profit from a flexible cognitive control mode.

The Neuronal Underpinnings of Cognitive Control

The question arises which mechanisms allow for this constant readjustment of control along a stability-flexibility axis. Almost all decision-making theories acknowledge the simultaneity problem (see e.g., Bogacz, 2007; Kurzban, Duckworth, Kable, & Myers, 2013; Oberauer & Kliegl, 2006; Oberauer, Souza, Druey, & Gade, 2013): Some computational mechanisms, especially those associated with working memory or central response selection processes (Oberauer et al., 2013; Oberauer & Kliegl, 2006; Pashler, 1994), can be deployed for only a limited number of simultaneous items or tasks at any given moment. Therefore, multiple representations compete at any given point in time, and cognitive control mechanisms have to implement the appropriate selection, updating, or maintenance of an alternative.

A critical functional hub in this respect, specifically for task-switching situations, is procedural working memory (Kessler, 2017; Oberauer et al., 2013; Rac-Lubashevsky & Kessler, 2016) which holds procedural representations – in the present case, task sets – in an activated state. It consists of three components: An activated part of long-term memory (in a task-switching situation, this would mean all currently relevant tasks, stimuli, and responses), a central component called the bridge (capacity-limited in nature), and the response focus (hence, the single response selected for execution). The central component is of main interest here: Because of its limited capacity, tasks compete for access to this region. Whenever a task set becomes the focus of the bridge, procedural representations of stimulus-response rules are readily available. A gating mechanism controls whether or not the information currently held active in the bridge is updated or not (Kessler, 2017; Rac-Lubashevsky & Kessler, 2016). Opening the gate and updating information are costly processes, and residual bindings in working memory or activated long-term memory can interfere with current task performance. However, this model makes no predictions how the decision as to whether the gate to the bridge should be opened or closed is managed. In that way, it merely serves as a canvas, a sort of functional background information; it does not explain who paints the picture, that is, how the balance between maintenance and updating is regulated in the short or long term.

Here, insights from neuroscientific research are of help putting an emphasis on control loops involving the anterior cingulate cortex (ACC). The ACC is suggested to learn the value of response options and feed them into subsequent controlled behavior (Holroyd & Yeung, 2012; Kennerley, Walton, Behrens, Buckley, & Rushworth, 2006). Especially the dorsal portion of the ACC (dACC) integrates information to determine subsequent control allocation by computing the expected value of control (Shenhav et al., 2013) and monitoring subsequent behavior by computing prediction errors. The specific regulation according to the expected value of control is carried out by the lateral prefrontal cortex (PFC) (Kouneiher, Charron, & Koechlin, 2009), especially the dorsolateral portion (dlPFC), together with associated structures in the basal ganglia and dopaminergic and noradrenergic nuclei in the brainstem. The active maintenance of task representations biasing the posterior cortex is ascribed to the lPFC (Miller & Cohen, 2001). One mechanism for this active maintenance may lie in recurrent excitatory connectivity between active bistable neurons in the PFC, which can create a sort of “attractor” state (O'Reilly, 2006). These bistable neurons, if sufficiently stimulated, stay active through gated ion channels (e.g., the NMDA channel) for several hundreds of milliseconds, thus arguably supporting cognitive stability (cf. Goschke & Bolte, 2014).

In contrast, widespread connectivity with subcortical structures subserves a gating function for the updating of task representations in the PFC (O'Reilly, 2006; O'Reilly & Frank, 2006). Hence, these connections may reflect the neuronal hub for adjustments on the stability-flexibility balance. This gating mechanism can be open, which allows for a rapid updating of active representations in the PFC. A closed gate, on the other hand, leads to stable maintenance of the current activity pattern. Several structures and connections have been proposed for this gating mechanism, most importantly connections with dopaminergic structures in the ventral tegmental area (VTA; for reviews see Cools, 2016; Cools & D'Esposito, 2011; Durstewitz & Seamans, 2008). It has been suggested that the relationship between the stability of working memory content and DA levels is characterized by an inverted U-shaped function. Both suboptimal and supraoptimal levels of dopamine in the PFC lead to destabilization (Fallon, Williams-Gray, Barker, Owen, & Hampshire, 2013). At the same time, different classes of DA receptors (D1 and D2) are suggested to subserve different control states (Durstewitz & Seamans, 2008). For one, DA D1 receptor activation, especially present in the PFC, yield a stabilizing effect on working memory content in the PFC, among others mediated by NMDA channel activation. Against this, D2 receptor activation aids updating processes. Whereas this D1/D2-dependent stability-flexibility modulation has quite broad and diffuse effects, activity in direct pathway “Go” neurons and indirect pathway “No Go” neurons in the basal ganglia enables selective maintenance or updating of only some regions of the PFC (O'Reilly, 2006). This is not to be viewed as mutually exclusive, though. While “Go” pathways largely depend on DA D1 neurons, “No Go” pathways depend on DA D2 neuron activity. Similarly, Cools (2016) suggests that DA activity in the striatum aids cognitive flexibility, and opposed to that, DA activity in the PFC helps cognitive stability. At the same time, DA neurons in the striatum largely have D1 receptors, whereas DA neurons in the PFC have D2 receptors. Relatedly, noradrenergic projections between the locus coeruleus (LC) and the neocortex have been proposed to mediate performance on an exploration-exploitation axis (Aston-Jones & Cohen, 2005), which is closely linked to the concepts of flexibility or stability, respectively.

To sum up, current neuroscientific research indicates that dACC and lPFC are prominently involved in the specification of a control signal – where and whether to allocate control – and the subsequent regulation of the task-relevant brain circuits. The updating and maintenance of information – or, to use a more concept-based language, cognitive flexibility and stability – are mediated via widespread dopaminergic and noradrenergic connections between the brainstem, the basal ganglia, and the PFC.

The (Voluntary) Task Switching Paradigm

Before turning to a closer description of the independent variables under scrutiny, I will first give an overview of the experimental paradigm employed. In all of the presented experiments, the task switching paradigm was used (Grange & Houghton, 2014; Kiesel et al., 2010; Monsell, 2003; Vandierendonck, Liefooghe, & Verbruggen, 2010). In this experimental setup, participants are confronted with two (or more) different tasks. On each trial, the task can either stay the same as in the previous trial (repetition) or change (switch trial). The tasks, oftentimes categorization tasks, can be contingent on different features of one so-called bivalent stimulus. One example of such a bivalent stimulus is a digit that can either be classified according to its magnitude or its parity (Kiesel et al., 2010). Alternatively, two univalent stimuli can be used for the two tasks. An exemplar would be a number magnitude task and a letter alphabet task (e.g., Fröber & Dreisbach, 2016b). In a variant of the standard design, participants are presented with a cue that announces the to-be-performed task (Kiesel et al., 2010; Meiran, 1996; Monsell, 2003). Either way, in the standard task switching design, participants are told whether to repeat or switch from the previously performed task, resulting in a comparison of task switch and task repetition trials. Behaviorally, participants display switch costs, that is, they make more errors and are slower in switch relative to repetition trials.

Opposed to this, in the voluntary task switching paradigm the participants decide themselves which task to perform on each trial (Arrington & Logan, 2004). This means, the participants have control over whether they repeat the task from the previous trial or switch to the other task. Therefore, ecological validity is increased compared to the standard task switching paradigm (Arrington & Logan, 2005; Demanet & Liefooghe, 2014; Vandierendonck, Demanet, Liefooghe, & Verbruggen, 2012). On top of investigating switch costs in performance, the voluntary task switching paradigm allows to investigate participants' voluntary task choice. One important dependent variable here is the voluntary switch rate (VSR), which is the number of trials in which participants choose to switch between tasks. In addition to the switch costs on performance, a repetition bias can be found in the VSR, that is, participants typically prefer to repeat tasks over switching between them. This is especially noteworthy because participants are typically instructed to perform both tasks equally often and in random order (Arrington & Logan, 2004, 2005).

What control processes are involved in forced and voluntary task switching? Here, the notion of task set is helpful. It is generally assumed that participants usually adopt a task set to perform a task successfully (Dreisbach, 2012; Dreisbach & Haider, 2008, 2009; Monsell, 2003). In other words, in response to or in preparation for a task the relevant stimulus-response rules of a task have to be activated or reconfigured, together with associated adjustments in attention and response-related processing (Logan & Gordon, 2001; Oberauer et al., 2013; Vandierendonck et al., 2010). One prominent class of theories assumes that switch costs largely reflect the time that control processes need to reconfigure a task set (Rogers & Monsell, 1995; Rubinstein, Meyer, & Evans, 2001), allowing to actively overcome carryover activation from previous task-set states (Allport, Styles, & Hsieh, 1994; Meiran, Kessler, & Adi-Japha, 2008). Because switch costs can be reduced, but not eliminated with increasing preparation time (Monsell & Mizon, 2006), some authors suggest that a part of this task-set reconfiguration process can be prepared in advance (termed endogenous component of task-set reconfiguration), whilst another part cannot (exogenous component of task-set reconfiguration; cf. Monsell, 2003). Still other researchers (e.g., Arrington & Logan, 2005; Arrington, Reiman, & Weaver, 2014; Demanet & Liefooghe, 2014) propose that the VSR may be a more adequate measure of control, as it is less susceptible to contextual information such as associations between tasks and stimulus features, and under tighter control of the participants themselves.

Another set of theories assumes that processes on switches are entirely the same as processes on repetitions – except for the fact that on switches, they take longer (e.g., Meiran, 2000). In studies where task predictability of cues was varied, similar effects were obtained for switches and repetitions: Higher predictability led to slower reaction times; no modulation of switch costs was found (Dreisbach, Haider, & Kluwe, 2002). That is, an updating process of the currently active task set – depending on the predictability of the cue – was equally engaged in switches and repetitions. This account is corroborated by recent electrophysiological evidence indicating a similar task-preparatory process in both switches and repetitions (M. Steinhauser, Maier, & Ernst, 2017; R. Steinhauser & Steinhauser, 2019). One overarching reason for the emergence of switch costs and the repetition bias hence may be that task-set updating is a more time-consuming process on task switch trials (Dreisbach, 2012).

Either way, both cognitive stability and cognitive flexibility are required in task switching (Dreisbach & Wenke, 2011): To perform a task repetition effectively, the task rules of the second task have to be shielded as much as possible, therefore a stable cognitive control mode is beneficial. On the other hand, a task switch requires flexibility, in the sense of efficient disengagement from the previous task and rapid activation of the now-required task. As a task switch in comparison to a task repetition (in terms of reaction times, error rates, and the VSR) is usually associated with costs, it is standing to reason that cognitive stability is the default option. Given that many everyday situations require cognitive flexibility, it is of paramount importance to identify variables that shift the stability-flexibility balance towards the flexible pole. Note that we classify conditions that specifically aid task switches – either in the form of less switch performance costs or a high VSR – as those enhancing cognitive flexibility.

Identifying Influencing Factors on the Stability-Flexibility Balance

Several findings indicate that both forced and voluntary task switching behavior is susceptible to contextual influences. For example, a series of studies argue that cue- and task-related memory processes play a role in the emergence of switch costs and the repetition bias (Altmann & Gray, 2008; Koch & Allport, 2006; Koch, Prinz, & Allport, 2005; Logan & Bundesen, 2003; Mayr & Kliegl, 2003; Waszak, Hommel, & Allport, 2005). In voluntary task switching specifically, the rate of voluntary switches can be decreased with high working memory load (Demanet, Verbruggen, Liefooghe, & Vandierendonck, 2010) or with short response-stimulus intervals (Arrington & Logan, 2004, 2005). In addition, relevant changes in the experimental situation such as variations of the stimulus or the offered reward have an impact on the VSR. Mayr and Bell (2006) could show that a change in stimulus promotes a change in the task, while a stimulus repetition usually also results in a task repetition. Even though these and other (Yeung, 2010) findings suggest that both forced and voluntary task switching is susceptible to bottom-up influences, the extent as well as the direction of this influence can differ considerably. Yeung (2010) could show that tasks that vary in task difficulty result in asymmetrical switch costs (larger costs when switching to the more difficult task), but a surprising task bias towards the more difficult task was found (participants choose to do the more difficult task more frequently than the easier task). Arrington and Yates (2009; see also Orr & Weissman, 2011) yielded evidence indicating that individuals differ in terms of their reliance on exogenous factors for task choice: The voluntary task switching paradigm thus enables the investigation of interindividual differences that may not be visible when investigating performance measures alone (see also Jurczyk, Fröber, & Dreisbach, 2018).

Despite these first promising research avenues, literature specifically investigating how self-chosen flexibility can be enhanced is rather sparse. The current PhD project therefore is aimed at filling this gap: For one, first findings on the flexibility-inducing effects of forced-choice context and reward prospect will be re-examined and extended, allowing to answer the question of how replicable and generalizable these effects are. In addition, a third factor (temporal predictability), which has been shown to affect forced task switching performance, will be applied in a voluntary task switching environment. Overarchingly, the present studies employ designs that are – in comparison to previous studies – well suited to compare task performance (RTs and ERRs) to task choice (VSR, task biases) measures and forced-choice to free-choice environments.

Influence of the context on deliberate cognitive flexibility: How and when does the forced-choice context influence voluntary task switching?

An incidental finding in a previous study (Fröber & Dreisbach, 2016b, Exp. 1-4) spurred interest in the effect of a mixture of forced and voluntary task switching trials: In four experiments, Fröber and Dreisbach randomly combined a high ratio of forced-choice and a small number of voluntary trials within blocks. Even though participants were not explicitly instructed to do so, they voluntarily switched between tasks to a surprisingly high degree (VSR between 30 and 40 %). This is especially astonishing because, as mentioned earlier the VSR is usually very low under these completely unrestricted conditions (e.g., 9-13 % in Kessler et al., 2009). Two aspects of the experiments of Fröber and Dreisbach offer an explanation for this finding: One aspect is the manipulation of the reward prospect, which will be introduced below, the other aspect are the intermixed forced-choice trials. Fröber and Dreisbach (2017) specifically investigated voluntary task choice in such a hybrid design: In two experiments, forced and voluntary trials were randomly combined within each experimental block. Using univalent stimulus material, a trial presenting just one stimulus constituted a forced-choice trial, whereas two presented stimuli (one for each task) marked a voluntary trial. In a first study, the ratio of forced to voluntary trials (25:75, 50:50, 75:25) was manipulated between participants. And indeed, the higher the number of forced choices, the higher the ratio of voluntary switches. Next, the authors pursued the question whether this was due to the number of forced choices per se, or merely due to a subgroup of these trials, namely, forced switches. That is, does the mere act of being forced frequently to do one or the other task induce a higher flexibility, or does only frequent forced *switching* elicit such a global flexibility-inducing effect? Note that in this first experiment, within the forced-choice trials, the ratio of forced repetitions to forced switches was set to 50 %. Hence, in a second experiment, Fröber and Dreisbach orthogonally manipulated the ratio of forced-choice to free-choice trials (25:75 and 75:25), and, within the forced-choice trials, the forced switch rate (25 % and 75 %). Again, they used a between-subjects design, which resulted in four experimental groups. The main effect of forced-choice ratio was again confirmed (higher VSR with a higher forced-choice ratio). This effect was qualified by an interaction with forced switch rate: Whereas the two groups with a 75:25 ratio of forced-choice to free-choice trials voluntarily switched tasks more often than the two 25:75 groups, a flexibility-inducing effect of a high forced switch rate only showed up with a sufficiently high number of forced-choice trials per se, that is, only in the 75:25 forced-choice to free-choice trials ratio group. On a theoretical level, Dreisbach and Fröber (2019) suggested that the requirement to switch tasks frequently leads to both tasks being held active in working memory. This is even more so, the more often task choice is not determined by the participants themselves (in free-choice, or voluntary trials), but by the experimenter (in forced-choice trials). This increased task uncertainty results in a global bias toward flexibility. This global bias is suggested to only aid the flexible shift between the two tasks at hand, as only these are held in a more active and readily usable state in working memory. Putting this theoretical hypothesis to a test is one of the aims of the current PhD project.

To investigate switch-specific processes in forced and voluntary task switching in conjunction with a manipulation of the forced switch rate, especially in preparation to the stimulus, we made use of the excellent temporal resolution of electroencephalographic (EEG) recordings. To obtain EEG data, electrodes are mounted onto the scalp. The signal the EEG represents usually comes from dozens of different neural sources of activity, but mostly postsynaptic potentials of large numbers of similarly oriented cortical pyramid cells (Luck, 2005). When investigating event-related potentials (ERPs) as done in the present case, electrophysiological activity in response to a certain stimulus is averaged across many trials – hence canceling out most of the noise. The resulting averaged ERP waveforms can be analyzed regarding the presence or absence of characteristic positive or negative voltage deflections, depending on the specific condition. An important technique in this regard is to compute difference waves: Here, we were specifically interested in the differential activity between task switches and repetitions. In past investigations, the switch-repeat difference wave showed two characteristic deflections, especially in a forced task switching context (for a review see Karayanidis & Jamadar, 2014): A switch positivity maximally around 400 to 600 ms after a task cue can be found at centro-parietal sites. Its magnitude varies with switch performance (as measured in RTs; e.g., Lavric, Mizon, & Monsell, 2008), but also with switch demands (Elchlepp, Lavric, Mizon, & Monsell, 2012), and has been interpreted as a specific index for task-set reconfiguration. It is often accompanied or followed by a later negative-going deflection in more frontocentral electrode sites which is usually stronger for repetitions. This pre-target negativity is discussed to reflect general task-preparatory attentional enhancements (Jamadar, Michie, & Karayanidis, 2010).

The current Study 1 conducted in collaboration with Robert Steinhauser and Marco Steinhauser from the Catholic University Eichstätt-Ingolstadt is aimed at investigating preparatory (switch-specific) activity prior to forced and voluntary choice trials. Given that previous findings on the electrophysiological signature of voluntary task switching are highly diverse and even contradicting (Chen & Hsieh, 2015; Poljac & Yeung, 2014), the proposed hybrid design may be helpful in identifying shared and distinct processes in forced and voluntary task switching. It may also be of use to link specific components and underlying processes with the switching requirements per se and the particular requirements associated with a voluntary task switch. A second aim of this study is to investigate the effects of forced switch rate (varied between 20 % and 80 % between blocks) on preparatory indices. In each block, 50 % forced and 50 % voluntary trials were presented in random order (the previously introduced hybrid design by Fröber & Dreisbach, 2016b, 2017). In order to control the onset of preparatory processes and to increase signal-to-noise ratio, cues (announcing one of two tasks in the case of a forced-choice trial, or a voluntary choice) were shown before each target. Finally, in a logistic regression-based multivariate pattern analysis (R. Steinhauser & Steinhauser, 2019; M. Steinhauser & Yeung, 2010, 2012), we extracted spatiotemporally defined activity that maximally discriminated switch from repetition trials. For one, this allows to examine switch-specific ERP potentials in the EEG, but most importantly, we can explore more directly which portions of the preparatory activity reflect a common process in forced and voluntary task switching.

Influence of reward prospect on deliberate cognitive flexibility: Is the sequential reward effect due to the perceptual cue change or the reward change?

The “orienting and invigorating impact . . . of prospective reward” (Botvinick & Braver, 2015, p. 84) has been suggested as a working definition of motivation. In the same sense, we wanted to investigate motivational effects on the exertion and mode of cognitive control by comparing different reward contexts. It should be noted that the reward prospect effect is to be differentiated from reward reception effects (for a review see e.g., Notebaert & Braem, 2016), but also from other, related terms such as effort that describes the task-difficulty related increase in the engagement of capacity-limited, controlled processes (Inzlicht, Shenhav, & Olivola, 2018; Shenhav et al., 2017). In our Study 2, we examined the effects of performance-contingent reward prospect, which can be demarcated from those of noncontingent reward prospect (Fröber & Dreisbach, 2016a). Indeed, reward reception, noncontingent reward prospect as well as performance-contingent reward have been investigated in the context control. Performance-contingent reward prospect, in particular, has been found to increase stability and task-related processing in working memory (Jimura, Locke, & Braver, 2010), dual-tasking (Fischer, Fröber, & Dreisbach, 2018), or cue maintenance (Fröber & Dreisbach, 2014; Hefer & Dreisbach, 2017, 2020).

However, flexibility-inducing effects of performance-contingent reward prospect have been reported as well. Shen and Chun (2011) were the first to investigate the critical role of immediate reward history systematically. Over three experiments, reward prospect was varied randomly between high and low magnitude. Hence, four different reward sequences could be investigated depending on the reward prospect in the current trial as compared to the previous trial: It could either stay on a high or low level (remain-high and remain-low condition) or change (increase and decrease condition). Shen and Chun examined the impact of these four reward sequences on forced task switching behavior. Most importantly, they found that switch performance (both in absolute switch RT and the switch-repeat difference in RTs) benefitted from increasing reward prospect. This fits with findings by Kleinsorge and Rinkenauer (2012) who could show that if reward prospect is randomly changed between no reward prospect vs. reward prospect, switch costs can be specifically reduced in reward trials that were preceded by a no-reward trial. This is especially remarkable because several previous studies which globally incentivized performance did not find any switch-specific effect of reward prospect (Capa, Bouquet, Dreher, & Dufour, 2013; Dreisbach et al., 2002; Nieuwenhuis & Monsell, 2002; Savine & Braver, 2010).

Fröber and Dreisbach (2016b) pursued the question whether the VSR would equally be influenced by the reward sequence. In a first experiment, they compared how the VSR differed between unchanged high reward prospect and increasing reward prospect. And indeed the VSR was higher with increasing reward prospect than with unchanged high reward prospect. In subsequent experiments, they confirmed that this was not due to the mere physical change in the cue appearance: Even if several colors (Exp. 2) or shapes (Exp. 3) were associated with each reward magnitude, allowing for a reward cue switch even in reward-unchanged conditions, the flexibility-enhancing effect of increasing reward prospect showed up. In two final experiments, the authors additionally investigated decreasing and unchanged low reward prospect: To summarize, only unchanged high reward prospect continued to result in increased stability, whereas both increasing and decreasing reward prospect went along with more voluntary switches. The VSR following unchanged low reward prospect was of intermediate height.

As suggested by the two experiments using more than one cue per magnitude (since confirmed in several studies, Fröber, Pfister, & Dreisbach, 2019; Fröber, Pittino, & Dreisbach, 2020; Fröber, Raith, & Dreisbach, 2018; Jurczyk et al., 2018), this sequential reward effect is not due to simple perceptual priming effects: Mere physical repetition of the cue is not the main reason for the sequential reward effect. But does it not play any role at all?

Experiments 2 and 3 investigated the effect of irrelevant cue switches on voluntary task switching in high and low reward contexts. The VSR is known to be influenced by the repetition vs. switch of low-level features (Mayr & Bell, 2006) if these features carry relevant task information. A study on invertebrates (Czaczkes, Koch, Fröber, & Dreisbach, 2018) has shown that ants will switch between behaviors more often if irrelevant cues change, but only if they are presented in a high-reward context. On the other hand, if reward prospect is changed sequentially (Fröber et al., 2018; Fröber & Dreisbach, 2016b), both an increase and a decrease result in a higher VSR. The current design will use randomly changing irrelevant cues and fixed reward prospect within blocks, thereby allowing to differentiate between the two possibilities: If mere cue change triggers a higher VSR, we should find a main effect of the irrelevant cue changes. Whether or not this is influenced by the reward prospect will further our understanding of reward-induced effects on task performance and task choice. In addition, the current study 2 allows to examine the long-term nature of unchanged reward prospect effects.

Influence of temporal predictability on self-chosen cognitive flexibility: Does temporal predictability of tasks influence task choice?

The first two studies were aimed at investigating the effects of context and reward prospect. In a last study, we looked at how characteristics of the task and trial structure influence voluntary flexibility, in particular regarding the temporal structure. It has been shown that participants' task performance can adapt to different temporal correlations: If certain durations are paired with certain tasks, performing these tasks will be faster in predicted time-task conditions than unpredicted ones. More specifically, Aufschnaiter and colleagues (Aufschnaiter, Kiesel, Dreisbach, Wenke, & Thomaschke, 2018; Aufschnaiter, Kiesel, & Thomaschke, 2018) used two different foreperiods in a forced task switching design, 500 ms and 1500 ms. Unbeknownst to the participants, one of the two foreperiods was more often (90, 80, or 70 % predictability in Experiment 1, 2, and 3) paired with one task, whereas the other foreperiod was more often followed by the other task. Note that over all trials, both tasks and both foreperiods appeared equally often, just the foreperiod-task combinations appeared in different frequencies. The results showed that participants were faster (and in some experiments also less error-prone) in trials with frequent foreperiod-task associations.

Together with previous results (Thomaschke & Dreisbach, 2015), these findings indicate that participants built up time-based task expectancies. That is, over the course of the experiment, participants learn to expect a certain task based on the time passing by: Building up to the estimated time point after the short foreperiod, participants start preparing for the task associated with the short foreperiod. When the short foreperiod has passed without a stimulus appearing, participants start to shift expectancy and hence preparation to the other task. In the current context of voluntary task switching, one open question immediately comes to mind: Does this time-based expectancy also influence voluntary task choice? And in what way? Can voluntary switching between tasks be enhanced if time-based task expectancies are introduced?

A previous study suggests that predictable changes in task availabilities (Mittelstädt, Miller, & Kiesel, 2018) can influence voluntary switching. In the study, the authors presented the participants on each trial with two univalent stimuli, one for each task. Critically, they manipulated the stimulus onset asynchrony (SOA) between the two stimuli: If participants chose task A in trial N-1, the stimulus on trial N for this task A appeared shortly after the stimulus for task B. This SOA was incrementally increased every time a task was repeated, and set to the starting value again (50 ms in Exp. 1, 33 ms in Exp. 2) when the task was switched. That is, a time penalty was given for every task repetition – which indeed significantly increased the VSR. In contrast, in the current Study 3 we will investigate how time-task correlations irrespective of switch history affect voluntary task choice: We again made use of the hybrid task switching design developed by Fröber and Dreisbach (2016, 2017). Preceding the stimulus, either a short (500 ms) or long (1500 ms) waiting period (foreperiod) appeared. The forced-choice trials were meant to establish time-based task expectancies: One task was more often preceded by a short foreperiod, the other by a long foreperiod. On voluntary trials, both foreperiods were displayed equally often – voluntary task choice dependent on the current foreperiod was the main dependent variable. In addition, it was asked whether such temporal-expectancy effects on voluntary task choice would be task-transition dependent: Are participants more susceptible to choose timing-compatible tasks on switch trials? The theoretical framework of the stability-flexibility balance would indeed suggest such a hypothesis: If the current goal is temporally less shielded in order to allow for a fast switch (cf. Dreisbach & Wenke, 2011), task-irrelevant but potentially aiding information should be relatively more able to influence task performance. If similar claims can be made regarding task choice, choice rates that are compatible with the current time-task association should be higher on task switch compared to task repetition trials.

To sum up, modulating influences on the flexibility-stability balance have been shown to work on different timescales: There are long-lasting, more global effects, but also short-lived, more local effects (Dreisbach & Fröber, 2019; Hommel, 2015). It was one of the aims of the current study to investigate the functional workings behind the flexibility-inducing effects of forced-choice context, while also extending previous research to a statistically more powerful within-subject design (Fröber et al., 2018; Fröber & Dreisbach, 2017). Further, we compared electrophysiological preparatory activity to forced and voluntary task switching. In terms of more transient effects on the stability-flexibility balance, especially motivational effects seem promising, such as through the manipulation of reward prospect (Fröber & Dreisbach, 2014, 2016a, 2016b; Hefer & Dreisbach, 2017). For example, even if a global stability bias is established through the forced-choice context, specific reward conditions (i.e., a trial-to-trial increase in reward prospect, Fröber et al., 2018) can lead to a localized increase in flexibility, as reflected by the trial-wise increased VSR. In a further investigation (Study 2), we investigated whether the block-wise manipulation of reward prospect can also influence task switching behavior on a more global level. We used randomly varying, irrelevant cues in this study; this allowed to examine the effects of cue transitions and reward prospect orthogonally. Finally, in a third study, we manipulated the predictability of the tasks themselves by inducing foreperiod-task expectancies and measured their influence on voluntary task switching. In sum, we investigated the influence of global (forced-choice context, reward prospect) and local (irrelevant cue transitions, temporal predictability) contextual and expectancy-related manipulations on voluntary task switching.

PART II –
EXPERIMENTAL STUDIES

STUDY 1:

To Switch or to Repeat? – Commonalities and Differences in the Electrophysiological Correlates of Preparation for Voluntary and Forced Task Choices[3]

Jurczyk, V., Steinhauser, R., Dreisbach, G., & Steinhauser, M.

Abstract

In the laboratory, forced and voluntary task switching are usually examined in isolation. However, in our everyday life, switching between different tasks and goals often depends both on current situational demands and on our intentions. Hence, it is a reasonable assumption that both switch types share some underlying processes. To identify these, we compared electrophysiological preparatory activity in blocks of randomly intermixed voluntary and forced task switching trials. We further manipulated the forced switch rate (20 % vs. 80 %) between blocks to de-confound voluntariness with switch frequency and to investigate how switch frequency effects influence preparatory potentials. ERP analysis revealed an enhanced early P3b-like activity pattern on voluntary trials, possibly reflecting early traces of a decision process. A later pretarget negativity was enhanced on forced as compared to voluntary trials. Multivariate pattern analyses revealed that a common preparatory activity on both forced and voluntary switch trials can be found in the switch positivity time window, which we interpreted as an index of a common task-set reconfiguration process. The results are discussed with respect to current theories on switch-specific preparation and the stability-flexibility framework of cognitive control.

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[3] At the time of submission, the work described in Study 1 is in preparation for submission to a scientific journal in similar form as Jurczyk, R. Steinhauser, Dreisbach, and Steinhauser (in prep.).

Introduction

Task switching is a paradigm extensively used in order to investigate cognitive control: It allows to examine how goal-directed behavior is managed (for reviews see Kiesel et al., 2010; Monsell, 2003; Vandierendonck et al., 2010). Two tasks are learned, and then participants are instructed to perform these tasks in random succession, e.g. based on cues (cued or forced task switching, Meiran, 1996) or based on a self-chosen order (voluntary task switching, Arrington & Logan, 2004). In either case, the task switching paradigm allows to investigate the flexible shift between goals (when the task is switched between two successive trials), but also the stable maintenance of one goal (on task repetition trials; Dreisbach & Wenke, 2011). However, whether the same preparation processes underlie instructed and self-chosen task switching is a matter of ongoing debate. In the present study, we aimed to isolate preparatory brain activity that underlies both forms of preparation and thus reflects a generic task-set preparation process.

The classic and robust finding in task switching performance is that task switch trials – in comparison to task repetitions – are associated with performance decrements in reaction times (RTs) and error rates (ERRs), which are usually referred to as switch costs. Regarding task choice in the voluntary task switching paradigm, it has been found that participants display a strong bias towards repeating tasks over successive trials (for a review see Arrington et al., 2014) even though participants are typically instructed to perform both tasks equally often and in random order (Arrington & Logan, 2004, 2005). It has been argued that cue-based preparation in forced task switching is a merely stimulus-driven process in which a cue automatically activates a task set (Logan & Bundesen, 2003). Voluntary task switching on the other hand requires a choice process and might involve more endogenous, top-down preparation (Arrington et al., 2014; Arrington & Logan, 2004).

Two important ERPs reflecting preparatory activity have been identified and their occurrence may be revealing with respect to shared vs. unique processes in forced and voluntary task switching: one is a posterior positivity (sometimes also called switch positivity) that is maximal from 400-600 ms at centroparietal sites and is larger for switches. Given that its size covaries with switch RT, but not with repetition RT (Elchlepp et al., 2012; Karayanidis, Provost, Brown, Paton, & Heathcote, 2011; Lavric et al., 2008), it has been interpreted as an index of task-set reconfiguration: It is suggested that before selecting the appropriate response, the task set (that is, suitable adjustments in attention, working memory, and response selection; Monsell, 2003) has to be reconfigured from the old task set of the previous trial to the current task demands. Some portion of this task-set reconfiguration process (termed endogenous component) can be actively prepared in advance of the stimulus – resulting in a reduction in switch cost with increasing preparation interval (Monsell & Mizon, 2006). Other authors specified that similarly, interference resolution with respect to the previously active task set may account for the switch costs and their reduction with preparation (Allport et al., 1994; Meiran et al., 2008; Rubinstein et al., 2001; Ruthruff, Remington, & Johnston, 2001). Either way, the switch positivity and the associated process of task-set reconfiguration and/or interference resolution is a plausible candidate for a shared process in forced and voluntary task switching. A computational model by Masson and Carruthers (2014) suggests that reconfiguration processes play a role in the emergence of switch costs in both instructed and free-choice designs. And indeed, Poljac and Yeung (2014) found evidence for a typical switch positivity in a 100 % voluntary design (see also Forstmann, Ridderinkhof, Kaiser, & Bledowski, 2007; Orr, Gehring, and Weissman, 2010).

However, the posterior positivity on voluntary trials was clearly dissociated from a later pre-target negativity effect – diverging from some findings in the forced task switching domain (Lavric et al., 2008; but see Astle, Jackson, & Swainson, 2008). In addition, Chen and Hsieh (2015) as well as Kang, Diraddo, Logan, and Woodman (2014) found signs for a switch negativity in designs involving voluntary task switching blocks and cued task switching blocks, namely more negative amplitudes in centroparietal midline electrodes on voluntary switches as compared to voluntary repetitions.

These diverging results are more in line with another account for the switch costs, which assumes that cue encoding and task-related memory processes prevail in forced task switching (Altmann & Gray, 2008; Logan & Bundesen, 2003; Mayr & Kliegl, 2003; Waszak et al., 2005). In the compound-retrieval account by Logan and Bundesen (2003), the authors propose a strategy the completely relinquishes any control processes; instead, they assume that for every cue-stimulus pair, participants encode a cue-stimulus compound. Its retrieval from long-term memory is faster on repetitions because of residual activation in short-term memory. According to this account, the switch positivity mainly reflects memory retrieval of the cue-stimulus compound – hence it should not arise on voluntary switches. This account would predict the switch positivity to be a locus of differentiation between voluntary and forced task switching. If however the switch positivity can be found in both forced and voluntary task switching, this would be a sign that it reflects a switch-specific reconfiguration process that is necessary irrespective of the voluntariness.

Further, it should be noted that the switch positivity is a conglomerate of more than one underlying component. Nessler, Friedman, and Johnson (2012) differentiated between an early and late posterior positivity (see also Karayanidis & Jamadar, 2014) and interpreted the former as an index of task-set updating and only the latter portion as an index of task-set reconfiguration (see also Elchlepp et al., 2012). Indeed, it has been suggested that some sort of task-set updating in working memory is crucial to successful task performance in both switch and repetition trials (Dreisbach et al., 2002; Dreisbach & Wenke, 2011; Nessler et al., 2012; M. Steinhauser et al., 2017; R. Steinhauser & Steinhauser, 2019) and necessarily precedes task-set reconfiguration (for a computational model see Oberauer et al., 2013).

Interestingly in voluntary task switching, one can also differentiate between an early and late portion of the posterior positivity – though the early portion may reflect partially different processes than in cued task switching. Forstmann et al. (2007) found a difference in an early parieto-occipital activity pattern between choice and no-choice conditions and interpreted this difference as reflecting the need to categorize choice options. Hence, this early effect may reflect a first portion of a decision process that is necessary on voluntary, but not cued trials. Vandamme, Szmalec, Liefooghe, and Vandierendonck (2010) argued that voluntary switches are preceded by a first reactivation of the old task set which is then intervened by top-down control. Hence, reactivation processes – reflected in the early posterior positivity – may be more prominently present in voluntary task switching, whereas reconfiguration processes may prevail in cued task switching – reflected in modulations of the late posterior positivity, that is, the typical switch positivity.

Another important index of preparation is the pre-target negativity[4], a frontocentrally maximal component peaking at target onset or shortly after. It is thought to reflect a general process of response preparation and stimulus anticipation, given that it covaries both with repetition and switch RTs in cued task switching, and also with how much information the cue reveals (Jamadar et al., 2010; Karayanidis et al., 2009). Most voluntary task switching ERP studies yielded a larger pre-target negativity before a voluntary task switch as compared to a voluntary task repetition (Chen & Hsieh, 2015; Forstmann et al., 2007; Kang et al., 2014; Poljac & Yeung, 2014; Vandamme et al., 2010) – a difference that seems not to be present on forced trials (Chen & Hsieh, 2015; Kang et al., 2014). It is suggested to reflect the intervention of top-down control necessary on a voluntary task switch (Poljac & Yeung, 2014; Vandamme et al., 2010) in order to overcome the repetition bias. In addition, it has been proposed that the enhanced pre-target negativity on voluntary switch as compared to repetition trials indexes a strategy of effector-specific preparation ensuring proper task control if different effectors are linked with different tasks (e.g. in hand-to-task mappings such as the present case; Chen & Hsieh, 2015). Thus, current evidence points to an enhanced pre-target negativity on voluntary switch as compared to voluntary repetition trials, whereas this difference should be absent (or even reversed; Karayanidis & Jamadar, 2014) on forced trials.

The present study

To sum up, the above reviewed studies found evidence for diverging as well as common preparatory signals in forced and voluntary task switching, but differ with respect to where and when

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[4] Electrophysiological studies on voluntary task switching usually use the term contingent negative variation (CNV) to denote the late frontal negativity. However, for sake of consistency with the forced task switching literature (where the term pre-target negativity is more common), and because the CNV usually refers to a very long-going, rather central component, we use the term pre-target negativity. Note that a similar effect is also sometimes referred to as stimulus-preceding negativity (cf. Karayanidis & Jamadar, 2014).

these differences vs. commonalities were found. In order to fill this gap, we will employ the hybrid task switching design introduced by Fröber and Dreisbach (2016b, 2017), combining 50 % forced and 50 % voluntary trials within each block. This paradigm circumvents shortcomings of previous studies that compared voluntary and forced task switching: Most notably, while some studies used a warning signal as onset for the cue-locked ERPs in voluntary task switching (e.g., Chen & Hsieh, 2015; Kang et al., 2014), others used the response of the previous trial as onset point (e.g., Poljac & Yeung, 2014). It has to be noted that neither is optimal, as in a completely voluntary design, task selection and preparation can in principle happen at any random time point before task execution (Arrington & Logan, 2005), sometimes even before response execution on the preceding trial (if so-called "chunking" occurs, cf. Verbruggen, McLaren, & Chambers, 2014).

Second, combining forced and voluntary choices renders the typically used instruction that restricts voluntary choice (Arrington & Logan, 2004, 2005) superfluous. The mere presence of a sufficiently high number of forced-switch trials increases the number of voluntary switches *even though* participants are not explicitly told to switch to a certain amount (Fröber et al., 2018; Fröber & Dreisbach, 2017). Instead, instructions emphasize that participants are completely free in their decision on voluntary trials.

Third, to enable the comparison of preparatory activity between voluntary and forced choices, task cues (announcing one of two tasks, in the case of a forced-choice trial) or voluntary cues (announcing a free choice, in the case of a voluntary trial) were shown prior to each target. Thus, diverging from previous voluntary task switching experiments (except Forstmann et al., 2007), the cue has informative value and does not merely serve as a warning cue. Also, participants should not be inclined to start preparing a specific task right after the previous response (cf. Poljac & Yeung, 2014), because a forced task choice is equally likely: Consequently, in most trials, participants should start preparation with cue onset, making the cue a valid onset point for investigating preparatory potentials.

Participants alternated between blocks with a high forced switch rate (FSR) of 80 % (FSR 80 blocks) or a low FSR of 20 % (FSR 20 blocks). The latter allowed to examine possible confounding factors of switch probability: Usually, the voluntary switch rate – with intermixed forced trials – lies between 10 and 40 % making a voluntary switch a somewhat rare event (Fröber et al., 2018; Fröber & Dreisbach, 2016b, 2017). To not confound voluntary switch effects with frequency effects, it is necessary to compare voluntary task switching to a forced switching condition with comparable transition ratios. The interspersed FSR 80 blocks on the other hand are meant to increase the voluntary switch rate to a sufficient amount as to make any ERP analyses feasible. Fröber and Dreisbach (2017) could show that a high FSR increases the voluntary switch rate, and later (Dreisbach & Fröber, 2019) speculated that the flexibility-enhancing effect of a high forced switch rate may arise because the increased task uncertainty leads to both tasks being held active in working memory. As a consequence, the likelihood of a voluntary switch is increased and the repetition benefit in RTs is reduced (see also Dreisbach & Haider, 2006). On the downside, however, rare repetitions require a higher amount of task-set updating because of the stronger competition of both tasks in working memory. Nessler et al. (2012) found evidence in a pure forced design for an elevated early posterior positivity on rare repetition trials. As this early posterior positivity is taken as a sign of task-set updating, this finding corroborates the idea that a context of frequent switches is accompanied by a stronger activation level of both tasks. Exploring whether this effect extends to voluntary trials of a high-switch block was a secondary aim of the current study.

To quantify differences in the preparation for a forced and voluntary switch, we investigated ERPs. We examined an early P3b-like posterior positivity as an index for a decision process (on voluntary trials) and/or task-set updating (on both forced and voluntary trials). Further, we quantified activity within the switch-positivity (or late posterior positivity) time window as an index of task-set reconfiguration on both forced and voluntary trials. Finally, a later more frontal pre-target negativity was investigated as a representation for response or effector preparation and stimulus anticipation. We investigated commonalities between forced and voluntary task switching in the same components by means of a multivariate pattern analysis (MVPA; R. Steinhauser & Steinhauser, 2019; M. Steinhauser & Yeung, 2010, 2012). With this method, a spatiotemporally defined set of classifiers is trained to maximally discriminate between repetitions and switches for both forced and voluntary trials. If the same discriminating activity can be found on forced and voluntary switches (in comparison to repetitions), this activity pattern may be telling with respect to a common preparation process on switches. Given that earlier ERP studies on voluntary task switching found switch-specific preparation effects in the switch positivity and the pre-target negativity, both are possible candidates for such a shared process.

Method

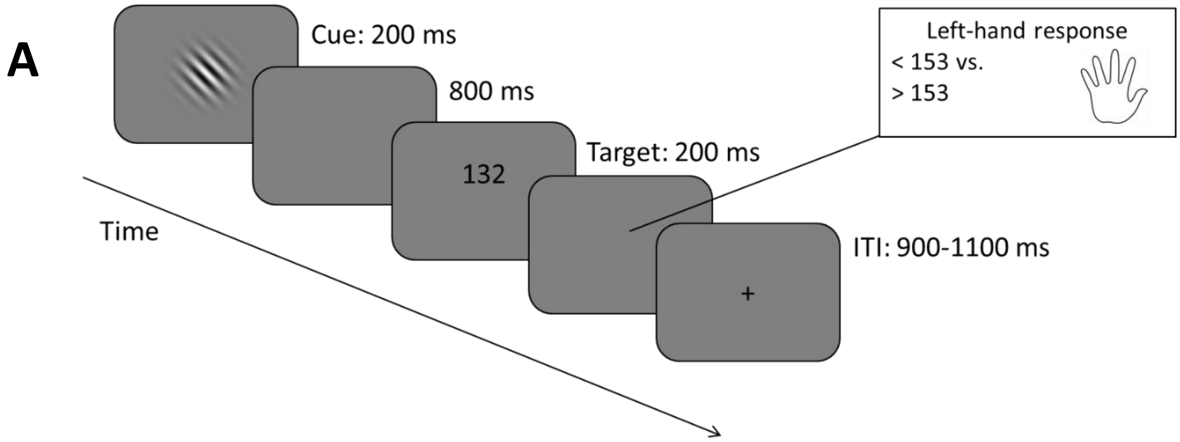
Participants. 35 students of the Catholic University of Eichstätt-Ingolstadt participated in this study (28 female; 30 right-handed; aged between 18 and 35; *M* = 22.0 years; *SD* = 3.2) for course credit or monetary compensation. The study was approved by the ethics committee of the Catholic University of Eichstätt-Ingolstadt and all participants signed informed consent prior to the experiment. They were naïve with respect to the purpose of the experiment, and had normal or corrected to normal vision. Psychology students (*N* = 20) earned course credits, all others were financially compensated for their participation (8 €/h). Two participants had to be excluded from further analysis due to a programming error, two had to be excluded due to technical failures.

Stimuli and procedure. The experiment was run using E-Prime 2.0 (Psychology Software Tools, Sharpsburg, PA) on a CRT display (display resolution at 1280 × 1024, refresh rate 75 Hz). Responses were collected with a QWERTZ-keyboard, using “y” and “x” as left and right response keys for one task (left hand), and “n” and “m” as keys for the other task (right hand). Participants were seated at approximately 70 cm from the screen (unconstrained), at which distance 1 cm on the screen corresponds approximately to 1° of visual angle.

Participants alternated between categorizing a number stimulus (125, 132, 139, 146, 160, 167, 174, or 181) as being smaller or larger than 153 and categorizing a letter stimulus (B, D, F, H, S, U, W, Y) as being closer to A or closer to Z in the alphabet (cf. Fröber & Dreisbach, 2017). Responses were given by pressing a left (smaller than 153/closer to A) or right (larger than 153/closer to Z) response key. Stimuli of both tasks appeared directly above one another in the center of the screen. Responses to the upper stimulus were always given with the left hand and responses to the lower stimulus with the right hand, while the mapping of number or letter task to position on the screen was fixed but counterbalanced across participants. All stimuli were displayed in black ink (Calibri font, size 28, bold) on a dark gray background. Contingent on a Gabor patch cue presented centrally (xx ° of visual angle) prior to each stimulus, participants either had to do the number or letter task or were free in their task choice: The task associated with the upper stimulus was announced by a leftward (45°) tilted Gabor patch, the one associated with the lower stimulus was announced by rightward (45°) tilted Gabor patch. Finally, voluntary trials were preceded by an upright oriented Gabor patch. The Gabor patch cues had spatial frequencies of either 3 or 9 cpd which were alternated in a pseudorandom order so that even on task or trial type repetitions the perceptual identity, that is, the spatial frequency, of the cue changed (cf. Logan & Bundesen, 2003; Monsell & Mizon, 2006).

Participants practiced both tasks separately in two short practice blocks (16 trials each, task order counterbalanced across subjects) already employing the task cues. These were followed by a forced task switching practice block of 16 trials in which each stimulus randomly appeared once, and a voluntary task switching practice block of another 16 trials in which stimuli to both tasks appeared twice and in random pairings. Here, the voluntary cues were introduced. After that, participants went through 14 test blocks that consisted of 80 trials each. Each block comprised 50 % forced and 50 % voluntary trials. Within subjects but between blocks the FSR within the forced trials was varied between 20 % and 80 % (in alternating order). FSR of the first block was counterbalanced across participants. Stimuli appeared pseudorandomly ensuring that all transitions between tasks and/or voluntary trials appeared equally often and about equally distributed. At the end of each block, we measured the resting eye blink rate for 10 seconds by presenting a horizontally striped Gabor patch (spatial frequency also randomly varied between 3 and 9 cpd).[5] After that, participants got a block-wise feedback on their performance in terms of mean ERR and RT with a reminder to respond as quickly and accurately as possible. Finally, they were instructed to move as minimally as possible before starting the next block.

Trial procedure is depicted in Figure 1. Each trial started with the presentation of the cue for 200 ms, followed by a blank screen for 800 ms, making up for a cue-stimulus interval of 1000 ms. After that, the target display appeared for 200 ms, either consisting of just one stimulus (in the case of a forced-choice trial) or one stimulus for each task (in the case of a voluntary trial). A blank response display stayed on screen until participants had responded. Only in practice blocks, this was followed by a 1000 ms feedback display. If participants made an incorrect response, the German word for error ("Fehler!") appeared on the screen; if they responded accurately, "Richtig!" (correct) was displayed. The intertrial interval showing a blank display had a random jittered duration between 900 and 1100 ms to prevent rhythmic responding.



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[5] Given that this measurement was intended for exploratory purposes only, we will not report results here.

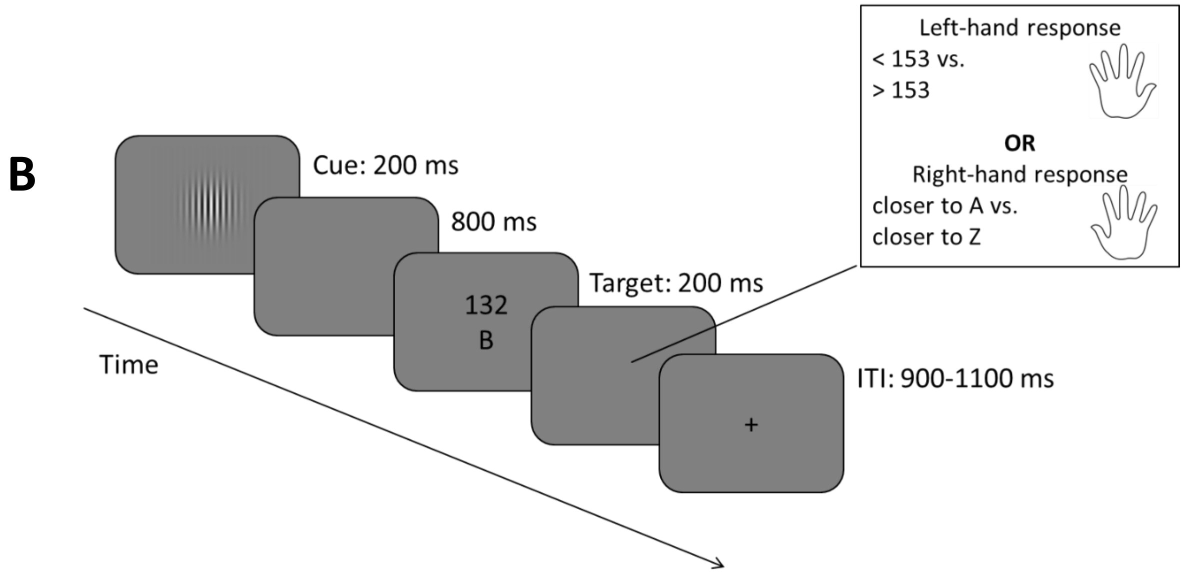

Figure 1. Course of a sample forced trial (A) and a voluntary trial (B). In both cases, the respective cue is presented for 200 ms, followed by an 800 ms blank. The target display is presented for 200 ms, followed by a blank screen until the participants responds. The length of the inter-trial interval was jittered between 900 and 1100 ms.

EEG data acquisition. The BIOSEMI Active-Two system (BioSemi, Amsterdam, The Netherlands) was used for EEG recording. 64 Ag-AgCl electrodes were placed on the scalp according to the extended International 10-20 system in the positions Fp1, AF7, AF3, F1, F3, F5, F7, FT7, FC5, FC3, FC1, C1, C3, C5, T7, TP7, CP5, CP3, CP1, P1, P3, P5, P7, P9, PO7, PO3, O1, Iz, Oz, POz, Pz, CPz, Fpz, Fp2, AF8, AF4, AFz, Fz, F2, F4, F6, F8, FT8, FC6, FC4, FC2, FCz, Cz, C2, C4, C6, T8, TP8, CP6, CP4, CP2, P2, P4, P6, P8, P10, PO8, PO4, O2, as well as the left and right mastoid. The CMS (Common Mode Sense) and DRL (Driven Right Leg) electrodes were used as reference and ground electrodes during recording, and all electrodes were offline re-referenced to linked mastoids. EEG and EOG data were continuously recorded at a sampling rate of 512 Hz.

Data analysis. Regarding task choice, the voluntary switch rate (VSR, in %) in 20 % FSR vs. 80 % FSR blocks will be compared using one-sample *t* tests. Transition was determined on the basis of the chosen hand (cf. Arrington & Logan, 2004; Scheffers & Coles, 2000). Regarding task performance, RTs in ms and ERRs in % will be analyzed in repeated-measures ANOVAs with the factors choice (forced vs. voluntary), FSR (20 % vs. 80%) and transition (repeat vs. switch).

For the statistical analyses, the experimental blocks were considered. The first trial of each block was omitted of all analyses (1.2 % of all trials). Only prior to the behavioral RT analyses, we also excluded error trials (5.8 %), trials following errors (5.3 %), and trials in which the RT deviated more than three *SD*s from the individual mean of the condition (1.4 %). In addition, two subjects were identified as RT outliers via boxplots created using the SPSS built-in tool. These subjects deviated more than three interquartile ranges from the overall mean in the baseline block. Consequently, they were not considered for any further analyses. Thus, the final sample used for the statistical analyses contained 29 subjects.

All EEG analyses were performed using MATLAB v9.0 (The Mathworks, Natick, MA) scripts in combination with EEGLAB v12.0 (Delorme & Makeig, 2004) functions. EEG data were band-pass filtered to exclude frequencies above 40 Hz and below 0.1 Hz, divided into epochs from 500 ms before to 1500 ms after cue onset and baseline-corrected to the interval 200 ms before cue onset. If necessary, electrodes were interpolated using spherical spline interpolation if the joint probability criterion (threshold 5) and the kurtosis criterion (threshold 5) in EEGLAB's channel rejection routine (pop_rejchan.m) were met. Epochs were removed if they contained activity exceeding ± 300 µV in any channel (except AF1, FP1, Fpz, Fp2, AF8 in order to prevent exclusion due to blink artifacts) and their joint probability deviated more than five *SD*s from the epoch mean. To correct for eye blinks and muscular artifacts, an infomax-based ICA (Bell & Sejnowski, 1995) was computed and components whose time courses and topographies were typical of these artifacts were removed after visual inspection. Regarding the investigation of the preparatory activity on forced- and voluntary trials, we will focus on the switch positivity, P3, and the pre-target negativity, each as a function of the within-subject factors FSR, choice, and transition. Based on the time windows described by Karayanidis and Jamadar (2014; see also Karayanidis et al., 2011) and the grand average topography obtained, mean amplitudes for the switch positivity were computed at electrode POz in the time window from 400 to 800 ms, for the P3 in electrode POz from 200 to 400 ms, and for the pre-target negativity in electrode Fz from 800 to 1000 ms.

Decoder Analysis. To identify shared processes in forced and voluntary task switching, we extracted switch-specific preparatory activity from the EEG signal on forced and voluntary trials as training sets by means of MVPA (M. Steinhauser & Yeung, 2010) and used this as decoder of the switch-specific activity in this data set. That is, a decoder of forced switch-specific activity was trained on the forced (switch - repetition) difference, and another decoder of voluntary switch-specific activity was trained on the voluntary (switch - repetition) difference. For that, we used a linear integration method (Parra et al., 2002) that allows to extract specific spatial components from the ERP data that discriminate maximally between two conditions (R. Steinhauser & Steinhauser, 2019; M. Steinhauser & Yeung, 2010, 2012). A set of classifiers is computed as spatiotemporal weighting coefficients in a logistic regression where the output distinguishes between two conditions as much as possible. For every time window, an averaged prediction value is assigned to each trial as the output of the logistic regression

$$y(t) = \boldsymbol{v}^T x(t)$$

where x(t) is the vector of electrode activity at time t and v^T describes the optimal spatial weighting coefficient for the particular condition difference. We first computed weight vectors v_{forced_switch} discriminating optimally between forced repetition trials and forced switch trials in partially overlapping time windows of 50 ms, separated by 10 ms during the cue-stimulus interval (0-1000 ms post cue). Each of these classifiers was trained on an equal, randomly drawn number of forced-repetition and forced-switch trials per participant. They can be interpreted as filters for switch-specific preparatory activity on forced trials. We did the same for switch-specific preparatory activity on voluntary trials by training a classifier in the same way to maximally discriminate voluntary switches from voluntary repetitions. The resulting weight vectors $v_{voluntary\_switch}$ serve as filters for switch-specific preparatory activity as it is present on voluntary trials. Leave-one-out (LOO) cross-validation was applied in the training of each weighted vector used for further analysis to prevent overfitting such that each weight vector equaled the mean of N weight vectors trained with T*(N-1) samples of N-1 trials to predict the T samples of the remaining trial (R. Steinhauser & Steinhauser, 2019), where N denotes the number of trials and T denotes the number of time samples.

To describe the sensitivity of the resulting classifiers, we report the area under the Receiver Operating Characteristic curve (Az score) where Az = 0.5 would indicate classification at chance level and Az = 1 signifies perfect identification. To test whether sensitivity significantly exceeded chance level, a permutation test was applied (M. Steinhauser & Yeung, 2012). For each participant a test distribution under the Null hypothesis (i.e., no difference in preparatory activity for switch and repeat trials) was generated by recomputing *Az*-scores 1000 times with random assignment of the switch/repeat categories. The resulting 1000 values represent the test distribution and were used to determine critical Az-values associated with significance levels of 0.05. Overall critical Az-values were computed by averaging across participants. This was done separately for both the forced and voluntary trials training set. For visualization of the spatial distribution of the weights of each discriminating component, we computed a coupling coefficient vector representing the activity at each electrode site that correlated with the discriminating component and is interpreted as the "sensor projection" (Parra et al., 2002, p. 225) of that component.

Finally, an averaged prediction value $\bar{y}_e$ as the output of a logistic regression can be calculated for every time window t of each trial, even for those trials that were not part of the training data set. In our case, $\bar{y}_e$ denominates the probability of a trial to be classified as being a switch trial (and thus can take on values between 0 and 1). Hence, computed for voluntary trials over v_{forced_switch}, this output mirrors the amount of forced switch-specific preparatory activity present on voluntary trials. Similarly, we computed such averaged prediction values for forced trials over $v_{voluntary\text{-}switch}$. Statistical testing of differences was conducted at 50 ms time windows around the peaks of the three tested ERP components.

Results

Behavioral data. We first investigated participants' voluntary choice pattern dependent on FSR. As expected, participants switched tasks significantly more often in the FSR 80 blocks than in the FSR 20 blocks, *t*(28) = 11.20, *p* < .001, *d* = 1.35 (see Fig. 2A). The similarity in switch rate on forced and voluntary trials in the FSR 20 blocks makes those blocks especially suitable for later forced vs. voluntary comparisons in the ERP and MVPA analyses.

In the RT analysis, the main effect of FSR, *F*(1, 28) = 4.18, *p* = .050, η_p^2 = .13, and of transition, *F*(1, 28) = 77.37, *p* < .001, η_p^2 = .73, were significant. Furthermore, the two-way interaction of FSR and transition, *F*(1, 28) = 4.21, *p* = .050, η_p^2 = .13, as well as the three-way interaction with choice reached significance level, *F*(1, 28) = 12.49, *p* = .001, η_p^2 = .31 (see Fig. 2B). Switch costs on forced-choice trials were reduced but not eliminated in the FSR 80 blocks (48 ms) as compared to the FSR 20 blocks (102 ms), which was both due to slower repetitions, *F*(1, 28) = 22.05, *p* < .001, η_p^2 = .44, and faster switches, *F*(1, 28) = 4.59, *p* = .041, η_p^2 = .14, in the FSR 80 blocks. Switch costs on voluntary choice trials were not significantly influenced by FSR, *F*(1, 28) = 0.21, *p* = .653 – on a descriptive level, both voluntary switches and repetitions were performed faster in FSR20 blocks, but only significantly so on repetitions, *F*(1, 28) = 6.63, *p* < .016, η_p^2 = .19. No other effect was significant (all *F*s < 1.66, all *p*s > .20).

For the ERRs, a main effect of transition was significant, *F*(1, 28) = 11.22, *p* = .002, η_p^2 = .29, with switch costs of 2.4 % (repetition: 4.7 % vs. switches: 7.1 %). In addition, only the interaction of FSR and choice was significant, *F*(1, 28) = 4.41, *p* = .045, η_p^2 = .14 (see Fig. 2B). In FSR 20 blocks, participants made less errors in voluntary than in forced-choice trials, *F*(1, 28) = 5.23, *p* = .030, η_p^2 = .16, an effect that was not present in FSR 80 blocks, *F*(1, 28) = 0.20, *p* = .657. No other effect reached significance (all *F*s < 3.75, all *p*s > .06).

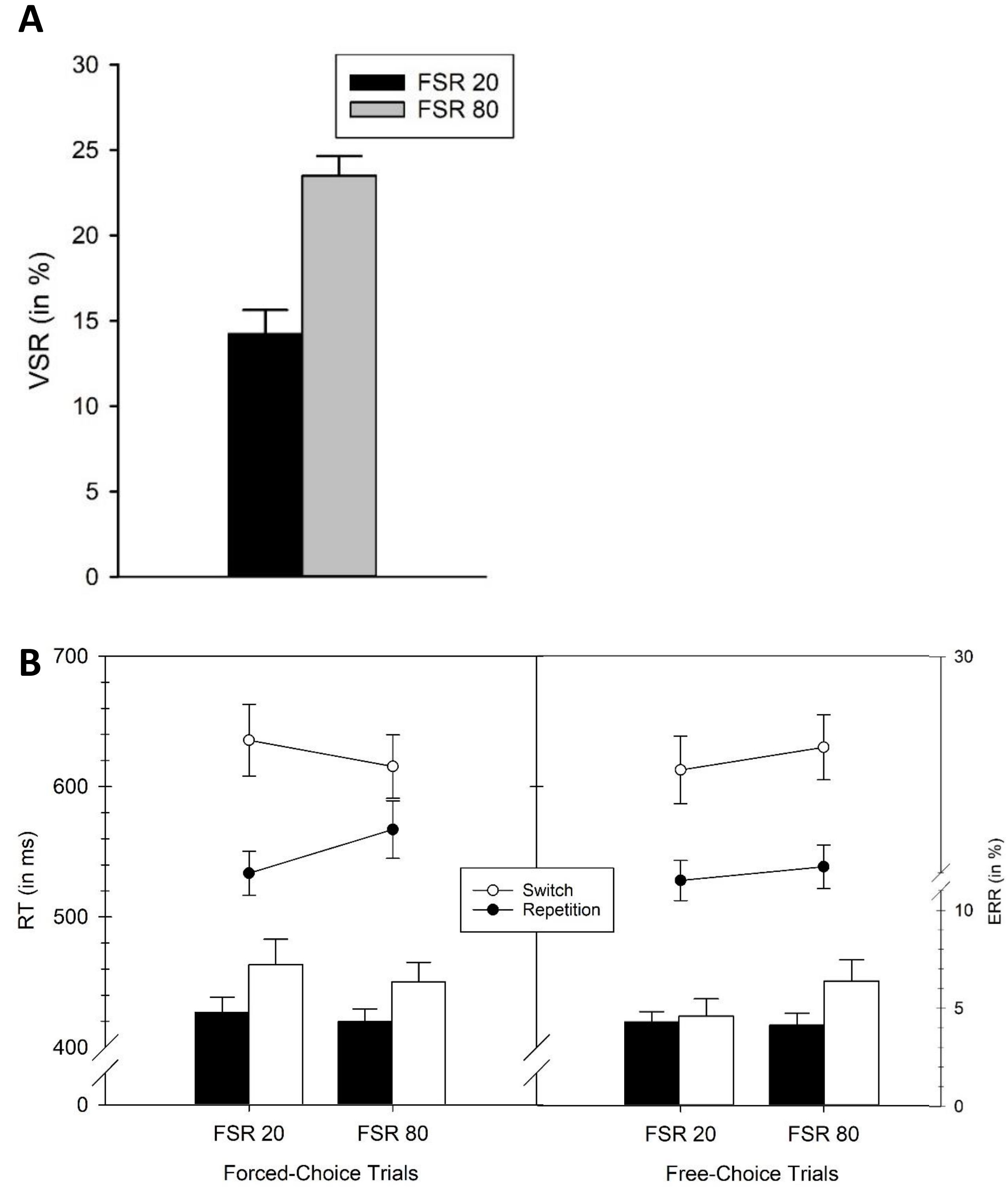

Figure 2. Behavioral results. A: VSR (in %) in FSR 20 blocks as compared to FSR 80 blocks. B: RTs (in ms) and ERRs (in %) as a function of FSR, choice, and transition. Error bars represent ± 1 *SEM*. (FSR = forced switch rate; ms = milliseconds; RT = reaction time; VSR = voluntary switch rate; *SEM* = standard error of the mean)

ERPs. In the ERP analysis, guided by the task switching literature (for a review see Karayanidis & Jamadar, 2014), we focused on three cue-locked ERP components: an early P3b-like positivity, a later extended posterior positivity with a similar scalp topography (often named "switch positivity"), and finally a pre-target negativity that peaks at or shortly after target onset. We checked for effects of FSR, choice, and transition on mean amplitude (see Figure 3). Scalp topographies of the switch-repeat difference wave in all conditions can be found in Figure 4.

In the P3 time window (200 to 400 ms after cue onset), both a two-way interaction of choice and FSR, $F(1, 28) = 11.55$, $p = .002$, $\eta_p^2 = .29$, as well as a three-way interaction of transition, choice, and FSR, $F(1, 28) = 8.18$, $p = .008$, $\eta_p^2 = .23$, reached significance (see Fig. 4). The switch-repeat difference was only significant on voluntary trials of FSR 20 blocks, $t(28) = - 2.95$, $p = .006$, Cohen's $d = -0.27$ (all other differences: *t*s < 1.40, *p*s > .17). All other effects were nonsignificant (all *F*s < 2.45, all *p*s > .12). In line with the results of Vandamme et al. (2010) and Forstmann et al. (2007), voluntary switch trials were markedly preceded by an enhanced early posterior positivity, which is interpreted as a sign for a choice cue evaluation process – hence reflecting an early portion of a decision process that does not need to be made on cued task switching trials. The pronounced effect in rare voluntary switches of the FSR 20 blocks is likely due to the associated enhanced task-set updating demands in this condition.

For the amplitude of the switch positivity, a significant main effect of transition, $F(1, 28) = 19.32$, $p < .001$, $\eta_p^2 = .41$, was observed. As expected, switch trials were accompanied by a more positive deflection in POz (difference: 1.15 µV, 95 % CI [.62 µV; 1.69 µV]) in the tested time window. Furthermore, the interaction of choice and FSR became significant, $F(1, 28) = 5.01$, $p = .033$, $\eta_p^2 = .15$. A tendentially more positive deflection in FSR 20 as compared to FSR 80 blocks was observed on voluntary trials (difference: 0.80 µV; 95 % CI [-0.05 µV; 1.67 µV], but not on forced-choice trials (difference: 0.04 µV; 95 % CI [-0.94 µV; 1.02 µV]. No other effect reached significance (all *F*s < 1.05, all *p*s > .30). Thus, switches indeed showed a more positive going deflection than repetitions, which was neither significantly influenced by choice nor by FSR – possibly reflecting a common component for forced and voluntary task switching. In accordance with the ERP literature on forced task switching, we suggest that the main underlying process is a necessary task-set reconfiguration and/or interference resolution process that is involved in both forced and voluntary task switching. In the same time window and electrode, an interaction of choice and FSR was observed. Similar to the effect in the P3b time window, voluntary trials in the FSR 20 blocks were preceded by an enhanced positivity; thus, this interaction could reflect an overspill of the earlier P3b-like effect signifying a task-set updating process.

By contrast, the same analysis on the pre-target negativity only revealed a main effect of choice, $F(1, 28) = 7.82$, $p = .009$, $\eta_p^2 = .22$. Preceding forced choices, the pre-target negativity in electrode Fz on average showed a more negative amplitude (difference: -1.27 µV; 95 % CI [-2.20 µV; -.34 µV]). Neither transition nor FSR significantly impacted the pre-target negativity (all *F*s < 3.03, all *p*s > .09). This result is not in line with some previous voluntary and forced task switching studies that found a marked switch-repeat difference in the pre-target negativity window. Instead, the current effect could reflect stronger response preparation processes on forced trials irrespective of task choice. Given that the cue unequivocally determines the appearing task and the associated stimulus and response set only on forced trials, the current design may allow for stronger response preparation and stimulus anticipation on forced trials which are reflected in this enhanced pre-target negativity.

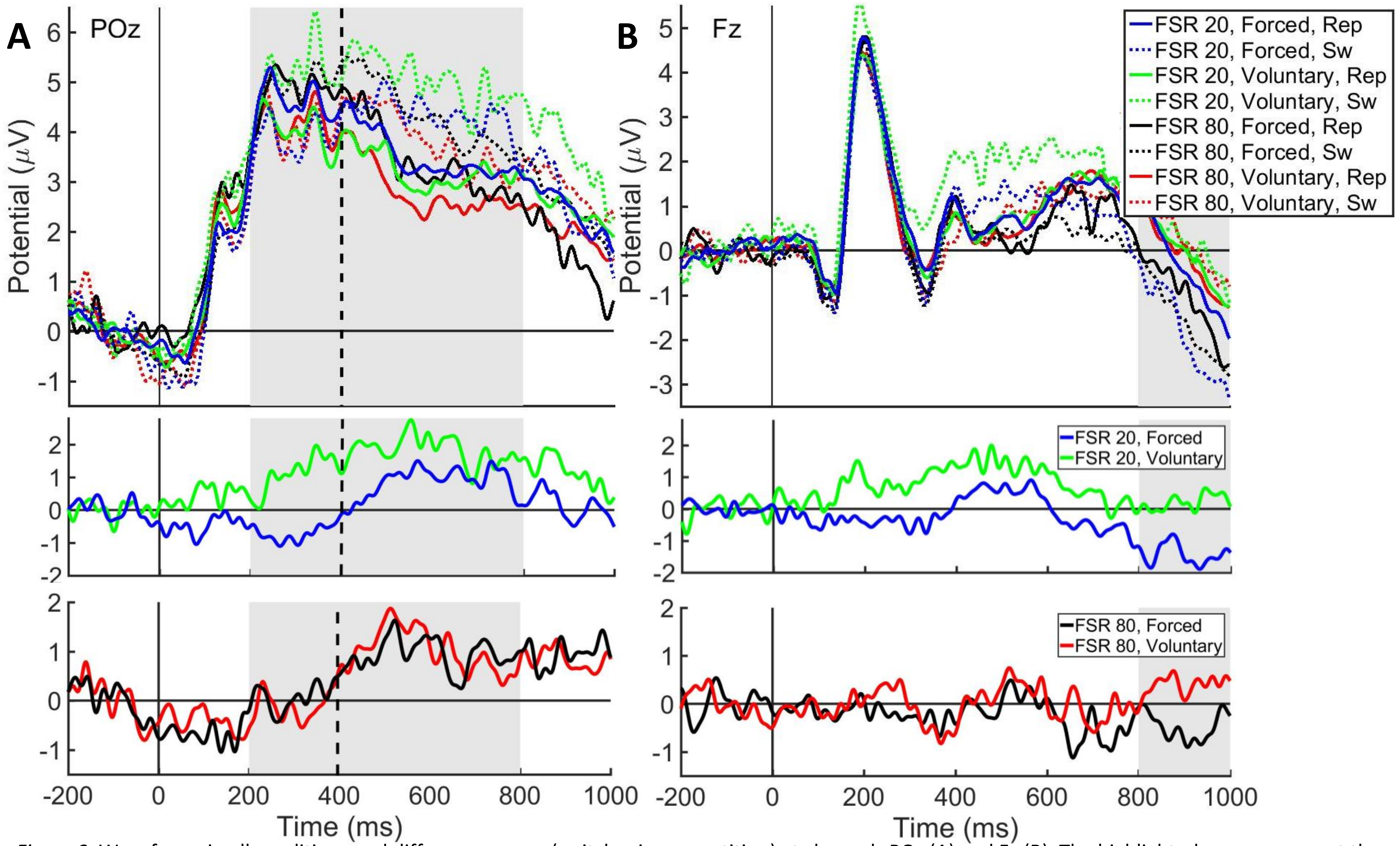

Figure 3. Waveforms in all conditions and difference waves (switch minus repetition) at channels POz (A) and Fz (B). The highlighted areas represent the time window used for testing the mean amplitudes of ERP components. Time point zero refers to cue onset.

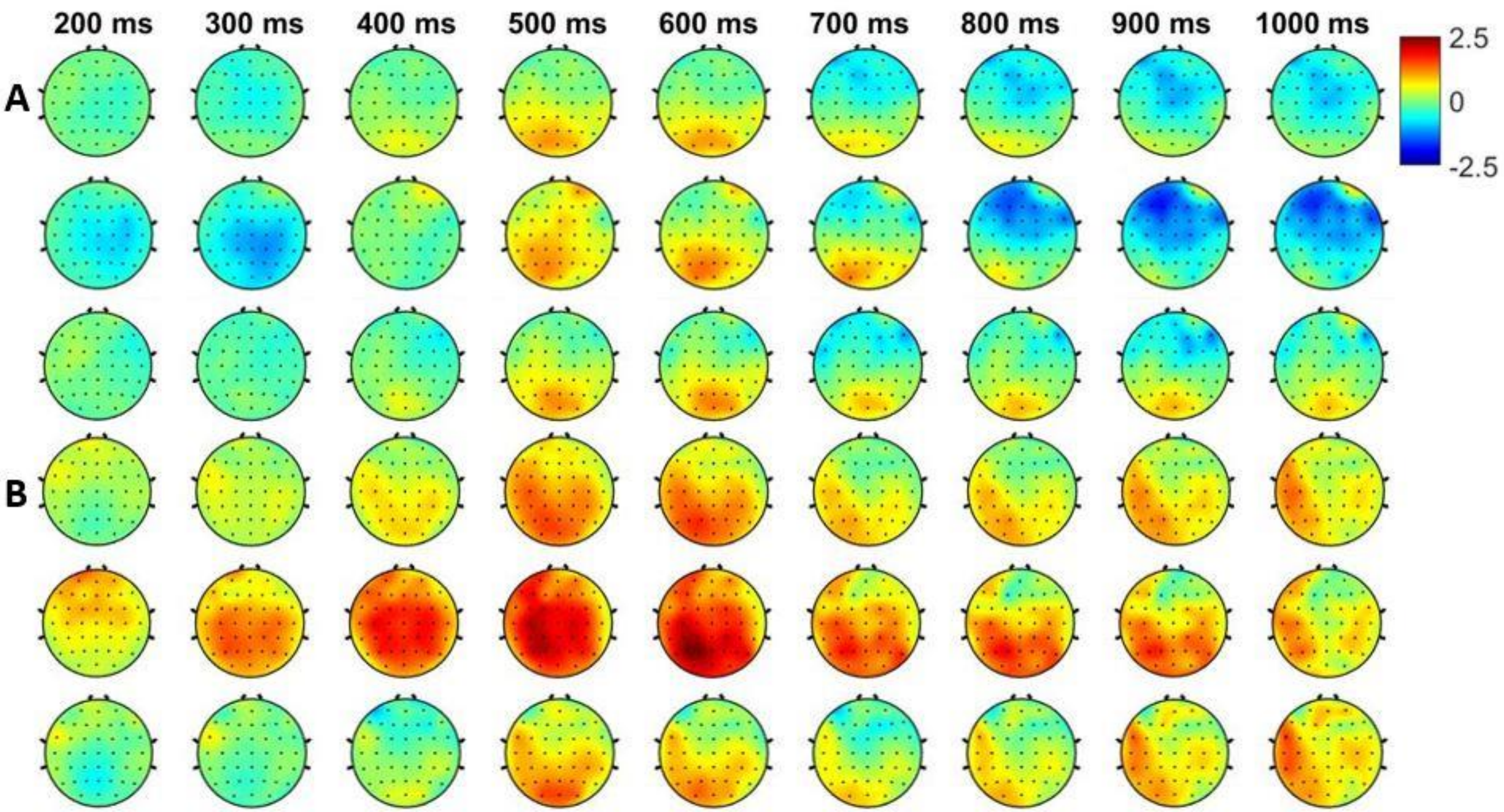

Figure 4. A: Topography of the difference wave between forced switch and forced repetition trials in the cue-locked ERP over all blocks (upper), in FSR 20 blocks (middle), and in FSR 80 blocks (lower). B: Topography of the difference wave between voluntary switch and voluntary repetition trials in the cue-locked ERP over all blocks (upper), in FSR 20 blocks (middle), and in FSR 80 blocks (lower)

MVPA. The ERP analyses suggest that forced and voluntary waveforms are comparable in the late posterior positivity time window as the main effect of transition was neither significantly influenced by choice nor by FSR – but of course, absence of evidence is not evidence of absence. A stronger test regarding the commonality between both choice types is provided by a MVPA because it allows to quantify common activation between different conditions. Since this methodological approach takes interindividual variability in the scalp topographies into account, signal-to-noise ratio is increased with regard to differential comparisons between conditions (R. Steinhauser & Steinhauser, 2019; M. Steinhauser & Yeung, 2010, 2012). In a first step, we extracted switch-specific preparatory activity separately for both forced-choice and voluntary trials by applying the linear integration method by Parra et al. (2002; 2005). A set of classifiers was trained to maximally discriminate between forced switch and repetition trials, and another for the voluntary switch-repeat difference. The time course of classification and the discriminating topography matched those of the late posterior positivity (switch positivity), and to a lesser degree, that of the pre-target negativity in the ERP analysis (Fig. 5). Further, the sensitivity analysis against a random distribution generated via permutation tests showed significant discrimination performance of both classifier sets (see Fig. 5A). Notably, the classifier set of the voluntary switch-specific preparatory activity shows a lower signal-to-noise ratio and hence more variability in the ensuing Az scores. This may in large part be due to lower and interindividually more variable switch trial numbers which determine the trial number that can be used for training the classifiers. The key issue, though, is that time windows of significant differences in the training data set do not preset or limit time windows of significant differences in the test data set, because they may reflect forced- or voluntary-specific preparatory activity that does not generalize to the other condition.

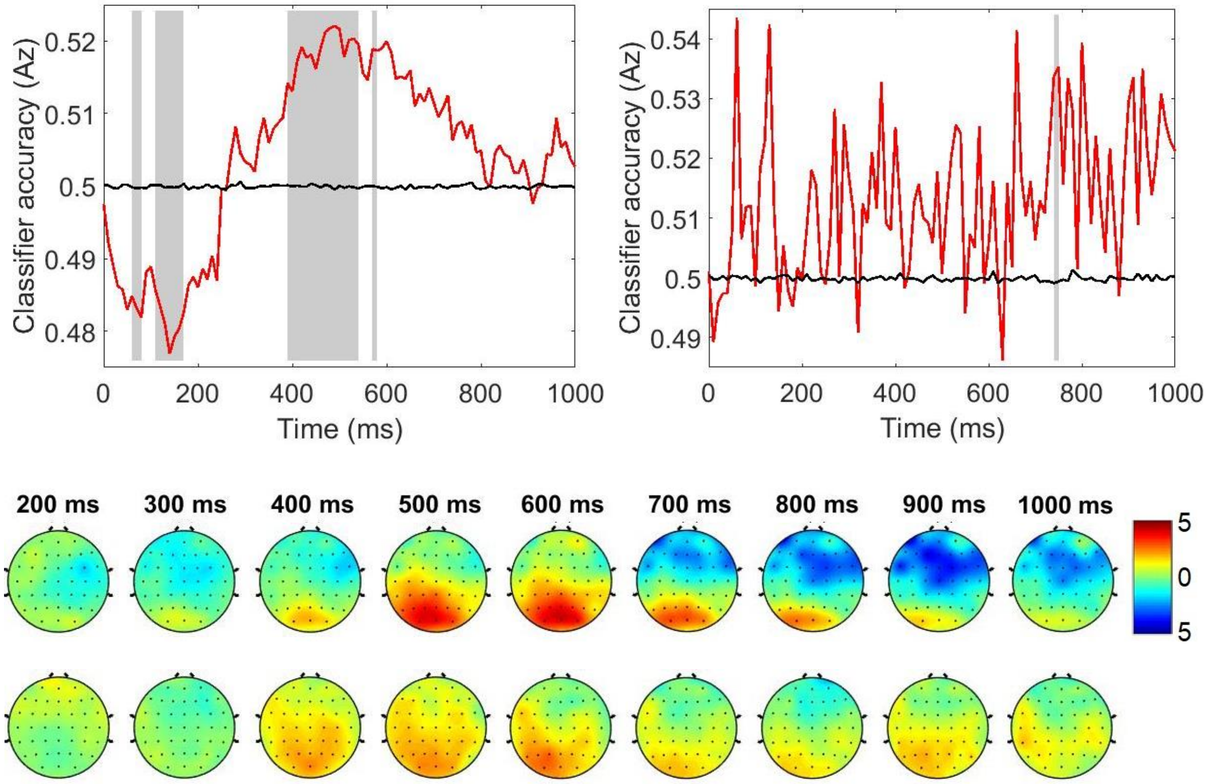

Figure 5. A: Classifier accuracy (in red) for the switch-specific preparatory activity on time windows of 50 ms width, every 10 ms in forced (left) and voluntary (right) trials. Black line indicates the test distribution under a random permutation test. Gray areas mark time points with a significant accuracy against a random permutation test. B: Discriminating topography of the classifier in contiguous time windows of 100 ms for forced (upper) and voluntary (lower) trials.

We then tested (1) whether the forced switch-specific preparatory activity was able to significantly distinguish voluntary switches from voluntary repetitions, and (2) whether the voluntary switch-specific preparatory activity was able to discriminate forced switches from forced repetitions. If both sets of classifiers are analogously able to distinguish between switches and repetitions in the test data set, we can crossvalidate that the activity pattern we extracted reflects inherent switch-specific preparatory activity that precedes a prepared switch irrespective of voluntariness. Time windows for testing were defined as the 50 ms around the peak within the time windows of the three tested ERP components, that is: for the P3 at 350 ms, for the posterior positivity at 560 ms, and the pre-target negativity at 940 ms.

As visualized in Figure 6, the classifiers were able to distinguish between repetition and switch trials in the respective test data set, but apparently only in the time window of the switch positivity. To further test this, we examined local peaks of the three components under scrutiny. For the P3, both the effect of the forced switch-specific activity, $F(1, 28) < 0.01$, $p = .985$, $\eta_p^2 < .001$, as well as the effect of the voluntary switch-specific activity, $F(1, 28) = 0.22$, $p = .643$, $\eta_p^2 < .01$, were nonsignificant. That is, neither classifier was able to significantly distinguish switches from repetitions in the other data set. At the peak of the posterior positivity, however, both for the forced switch-specific activity, $F(1, 28) = 6.10$, $p = .020$, $\eta_p^2 = .18$, and the voluntary switch-specific activity, $F(1, 28) = 6.28$, $p = .018$, $\eta_p^2 = .18$, a significant difference in the discrimination activity between switches and repetitions was found in the respective other data set. That is, some portion of the differential preparatory activity preceding voluntary switches resembles the activity preceding forced switches, and analogously, the preparatory activity preceding forced switches as compared to forced repetitions bears similarities to forced switch-specific activity. Topography and time windows of significant discrimination activities are remarkably similar (see Fig. 6 and 5B). In accordance with the ERPs, the sensory projection of both classifiers shows a posterior and slightly left-lateralized activity pattern in the time window of the posterior positivity.

Finally, for the peak of the pre-target negativity, again, no significant effect was found, neither for forced switch-specific activity, $F(1, 28) = 0.04$, $p = .838$, $\eta_p^2 < .01$, nor for voluntary switch-specific activity, $F(1, 28) = 0.11$, $p = .740$, $\eta_p^2 < .01$.

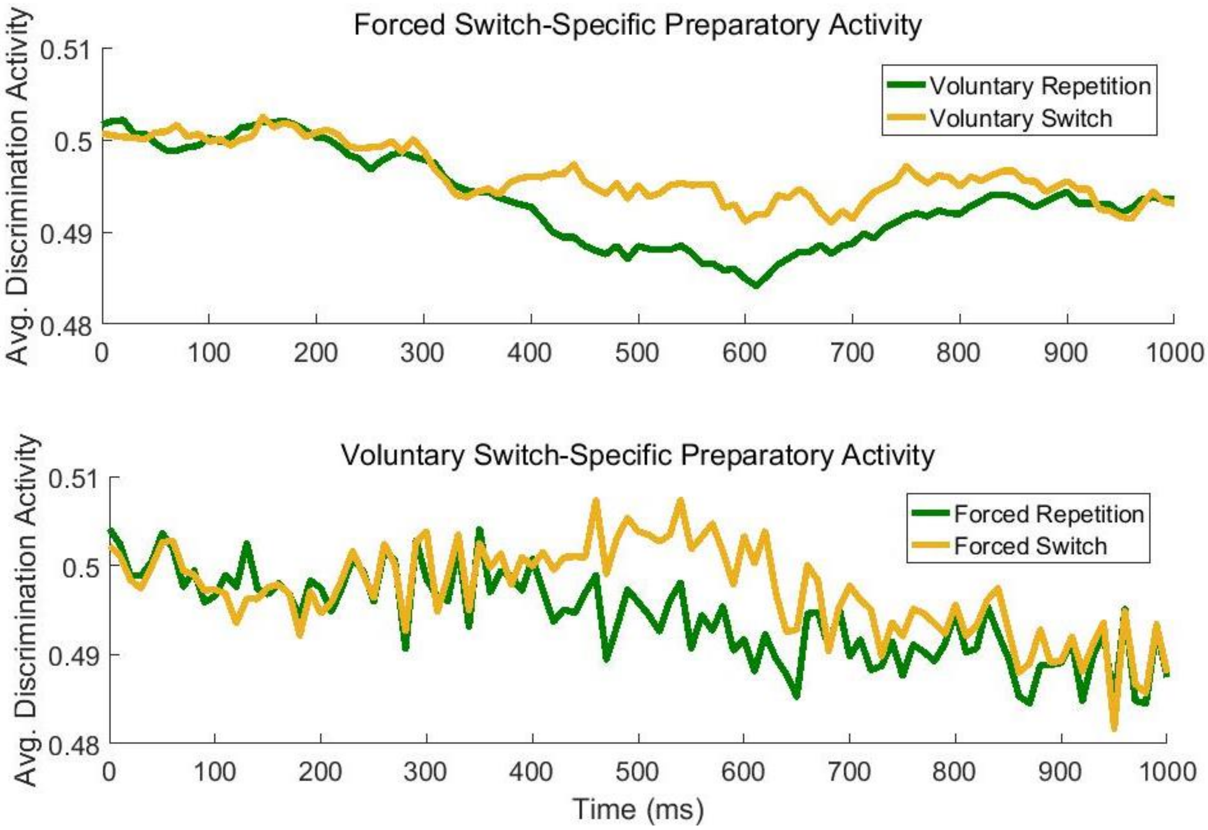

Figure 6. Average discrimination activity on voluntary trials based on the set of classifiers trained on the switch-specific preparatory activity on forced trials (upper); average discrimination activity on forced trials based on the voluntary switch-specific preparatory activity (lower).

To sum up, both sets of classifiers were able to discriminate between switches and repetitions in the other data set in the time window and with the topography of the late posterior positivity. Together with the ERP findings, these results provide strong evidence that the process reflected in the late posterior positivity – possibly an increased demand for task-set reconfiguration (see below) – is necessary on both voluntary and forced task switch trials. The current results also do not indicate that the switch-repeat difference in late posterior positivity was larger preceding forced than voluntary trials, as found by several previous studies (Forstmann et al., 2007; Kang et al., 2014). Even though the MVPA classifier was able to distinguish switches from repetitions forced trials in a larger time window (see Fig. 5A) this is mainly due to the higher classifier accuracy that is achieved by larger trial numbers. Kang et al. (2014) found evidence for a switch negativity preceding voluntary task switching, and argued that memory processes prevail in cued task switching, whereas voluntary task switching is able to truly capture an endogenous task-set reconfiguration process. However, neither our results nor other studies (e.g., Chen & Hsieh, 2015; Poljac & Yeung, 2014) are in line with this hypothesis. Instead, we think the current study is a clear indication that switch-specific preparation is largely similar between forced and voluntary task switching.

In an exploratory Transition × FSR ANOVA, we checked whether these effects were influenced by FSR. Neither for the P3 peak nor the posterior positivity, any significant effects of FSR arose (all *F*s < 1.23, all *p*s > .27). At the peak of the pre-target negativity, however, a significant interaction between FSR and transition arose, $F(1, 28) = 5.10$, $p = .032$, $\eta_p^2 = .15$. Planned comparisons revealed only a trend effect in forced repetitions, in that the activity preceding forced repetition trials in FSR 80 blocks had more similarity to a voluntary switch than FSR 20 forced repetitions, $t(28) = 2.02$, $p = .053$, $d = .32$, with no other single comparison approaching significance (all *t*s < 1.42, all *p*s > .16). Given that similarities between forced and voluntary switch trials were not restricted to FSR 20 blocks, these similarities cannot be ascribed to a simple switch frequency effect. In the same way, differences between forced and voluntary switch trials were similarly visible in FSR 20 and FSR 80 blocks.

Discussion

In the current experiment, we compared electrophysiological preparatory activity in forced and voluntary task switching. By combining both choice types within blocks, each trial preceded by a task or choice cue, this study is methodologically distinguished from previous studies and hence allows to compare forced and voluntary task switching more directly. The manipulation of forced switch rate between blocks ensured that switch frequency and choice type could be investigated independently. In a multivariate pattern analysis on the preparatory activity differentiating switches from repetitions, we examined commonalities between forced and voluntary task switching. This similarity can be pinpoint to a task-set reconfiguration process captured by the late posterior positivity. ERP analyses allowed us to quantify differences between forced and voluntary task switching: First, the decision process on voluntary trials showed up in an enhanced early posterior positivity. In contrast and against our initial hypothesis, forced-choice stimuli were preceded by an enhanced pretarget negativity, possibly reflecting enhanced response preparation and preparatory attentional processes. Finally, switch-frequency effects on voluntary trials were only accompanied by an enhanced posterior positivity, likely an index of the increased task-set updating demands.

Commonalities and differences of forced and voluntary task switching

The extracted markers for switch-specific preparatory activity were significantly able to distinguish switches from repetitions. Moreover, similarity between forced and voluntary switch-specific preparatory activity around the peak of the posterior positivity (at 560 ms) was confirmed, as predictability around this peak was significant when MVPA classifiers of forced or voluntary switch-specific preparatory activity were applied to the respective other data set. Moreover, we could exclude that these effects are due to less frequent voluntary than forced switches: The forced switch rate mainly affected an early posterior positivity but did not interact with transition in later phases. Hence, the current results offer another piece of evidence that switch-specific preparation on forced trials does not merely reflect the retrieval of a cue-stimulus compound (cf. Logan & Bundesen, 2003; D. W. Schneider & Logan, 2005) – instead, it reflects a generic process of switch-specific preparation. Other varied designs offer support for this rather universal role of the switch-specific posterior positivity in task switching, as it was similarly found in intermittently instructed cued task switching (Barceló, Periáñez, & Nyhus, 2007), in both response-effector and stimulus-dimension shifts (Hsieh, Wu, & Lin, 2014), and with transition instead of task cues (West, Langley, & Bailey, 2011). More importantly, against the background of methodologically diverse and hence partially contradicting ERP studies in voluntary task switching (Chen & Hsieh, 2015; Forstmann et al., 2007; Kang et al., 2014; Orr et al., 2010, November; Poljac & Yeung, 2014; Vandamme et al., 2010), the current results offer strong support that switching under forced- and voluntary-choice conditions is not so different, after all.

Authors suggest that in order to perform a successful task switch, the cognitive system has to reconfigure the currently active set of task representations in order to match the now relevant task

(Monsell, 2003). This reconfiguration process seems to be necessary irrespective of whether the task switch is predetermined or self-chosen by the participant. Similarly, a modeling study has shown that task-set reconfiguration is equally involved in both forced and voluntary task switching, whereas cue-related task retrieval processes are confined to forced task switching (Masson & Carruthers, 2014). This reconfiguration process hence does not only reflect the retrieval of task codes from memory, but also the appropriate attentional and response-related adjustments (Demanet & Liefooghe, 2014; Meiran, Chorev, & Sapir, 2000). However, it should be noted that the current results do not allow to infer that this switch-specific posterior positivity effect reflects a qualitative difference between switch and repetition trials. It is equally possible that it merely reflects a task-preparatory process that is more engaged on switches than on repetitions.

As reviewed in the introduction, it has been argued that the early P3b-like portion of the posterior positivity provides an index of task-set updating, while the later typical switch positivity reflects task-set reconfiguration (Karayanidis & Jamadar, 2014; Nessler et al., 2012). Hence, the early portion may reflect a more general process of task-set updating in working memory, that is engaged on switches and repetitions (Altmann & Gray, 2008; Dreisbach et al., 2002; Dreisbach, 2012; Meiran, 2000). This task-set updating process ensures that stimulus-response links in working memory are sufficiently activated to ensure successful task performance. Indeed, M. Steinhauser and colleagues (M. Steinhauser et al., 2017; R. Steinhauser & Steinhauser, 2019) found evidence that task errors (that is, forced trials in which participants perform the wrong task) are characterized by the absence of an early posterior positivity. In voluntary task switching, it is suggested that proper task control is characterized by a first reactivation (that is, updating) of the previously performed task (Forstmann et al., 2007; Vandamme et al., 2010), that is later in the preparation interval intervened when participants decide to switch tasks. Therefore, the enhanced early posterior positivity that we found on voluntary trials, particularly on voluntary switches in FSR 20 blocks, may reflect a (frequency-dependent) decision process, and the associated demands on task-set updating.

In the pre-target negativity time window, the ERP analysis revealed a larger enhancement preceding forced than voluntary trials. In line with both voluntary task switching (Poljac & Yeung, 2014) and forced task switching (Karayanidis et al., 2011) evidence, this effect seemed dissociable from the posterior positivity, as it was not significantly influenced by task transition and differed significantly between choice types. However, the effect is not in line with previous ERP studies on voluntary task switching (Chen & Hsieh, 2015; Kang et al., 2014; Poljac & Yeung, 2014; Vandamme et al., 2010) that found a (transition-dependent) enhancement on voluntary trials. A possibility for the diverging effects could be the different stimulus types: Chen and Hsieh (2015) and Kang et al. (2014) let participants either alternate between choice types between blocks or only once in the middle of the experiment. Hence, the information transmitted through the stimulus display did not differ within a block (cf. Koechlin & Summerfield, 2007). In our design, the target display on voluntary trials comprised two stimuli, whereas on forced trials, only one stimulus appeared. Thus, preparatory processes that are targeted at the stimulus anticipation should be enhanced on forced trials – and this should be the case irrespective of the decision to switch or repeat tasks, as may be reflected in the enhanced pre-target negativity on forced trials in the present study.

Switch-frequency based effects on voluntary task switching

The behavioral evidence in this study corroborates and extends previous findings. We found an increased voluntary switch rate in FSR 80 blocks as compared to FSR 20 blocks. This effect was mirrored in RTs especially on forced trials: In FSR 80 blocks with a high number of forced switches, repetitions were performed slower and switches were performed faster than in FSR 20 blocks. The present results therefore line up with previous performance (Dreisbach & Haider, 2006) and choice (Fröber & Dreisbach, 2017) results. They extend previous choice results from a between-subjects to a within-subjects design, hence providing further evidence that cognitive control mode seems to be able to adapt to changing task contexts on the basis of a few trials (Abrahamse, Braem, Notebaert, & Verguts, 2016; Braem & Egner, 2018; Goschke & Bolte, 2014).

Dreisbach and Fröber (2019) suggest that the forced-choice context modulates the extent to which both tasks are held simultaneously in working memory: Even though this goes along with higher interference between concurrently active task representations, being frequently forced to switch means that this cost is offset by a higher switch efficiency needed in a majority of trials (Dreisbach & Haider, 2006). In turn, also the probability of a voluntary switch is increased because of continued parallel activation of both tasks. On the other hand, if forced switches are rare (as in the current FSR 20 blocks), switch trials should be characterized by a particularly demanding updating process of task rules in working memory. Therefore, the current switch-frequency dependent effect of an enhanced early P3b-like posterior positivity in voluntary switches in blocks with infrequent forced switches (FSR 20 blocks) is in line with this theoretical account.

Future outlook

The current results suggest that preparation in anticipation of a task switch is similar in both forced and voluntary task switching. Whether the same is true regarding a general preparation process – that is, a process that is similarly engaged in switches and repetitions – will remain to be investigated: As R. Steinhauser and Steinhauser (2019)'s findings suggest, such a process can be captured by comparing single-task to mixed-task blocks and is reflected in an early P3b-like activity pattern. A similar cue-locked P3 activity in an intermittently instructed cued task switching paradigm (Barceló et al., 2007) was taken to reflect a necessary re-activation of the already active task set, consistent with a task-set updating process. The authors argued that this re-activation reflects a sort of default operation (cf. O'Reilly, 2006), as they found strong correlations between the early and late part of the posterior positivity. In the present study, we found the strongest early parietal positivity preceding rare *voluntary* switches, providing first preliminary evidence that a general preparatory process is indeed involved in voluntary switching. However, devising a useful voluntary analog to a single-task condition to quantify general preparatory activity will be challenging. Future studies could be guided by voluntary Go/No go ERP studies (Karch et al., 2014; Karch et al., 2016). Though Karch and colleagues found significant effects during intentional actions only in time-frequency analysis, this may be different when voluntary Go conditions are compared to voluntary task switching.

Conclusion

The present study sought out to clarify the commonalities and differences between forced and voluntary task switching by comparing electrophysiological preparatory processes. We found clear evidence that both choice types share a common switch-specific process that is reflected in a late posterior positivity. Most task switching evidence corroborates the notion that this effect reflects a common task-set reconfiguration process. In contrast, a decision process on voluntary trials – possibly linked with a task-set updating process (Forstmann et al., 2007; Nessler et al., 2012; Vandamme et al., 2010) – was mirrored in an enhanced P3b-like posterior positivity particularly on voluntary switches in blocks of low switch frequency. This finding further supports accounts suggesting increased activation of both task sets in working memory to be the main reason for the flexibility-enhancing effect of a high forced switch rate (Dreisbach & Fröber, 2019).

STUDY 2:

Bottom-Up Influences on Voluntary Task Switching in Different Reward Contexts?[6]

Jurczyk, V., Fröber, K., & Dreisbach, G.

Abstract

In humans, voluntary task switching is susceptible to bottom-up influences like a switch of the target stimulus identity (Mayr & Bell, 2006). A recent study with ants (Czaczkes et al., 2018) has shown that even irrelevant cue changes increase switching behavior, but only if they are presented within a high-reward context. To investigate whether a reward context would also increase switching behavior in response to meaningless cue changes in humans, we conducted two voluntary task switching experiments. On each trial, participants chose between two tasks preceded by one of two different color cues. Reward prospect was manipulated between blocks (Experiment 1: no vs. high reward; Experiment 2: low vs. high reward). In both experiments, the cue change did not modulate the voluntary switch rate. However, the voluntary switch rate was significantly lower in high-reward blocks as compared to no-reward or low-reward blocks. This suggests that bottom-up influences on deliberate task switching in humans are limited to task-relevant information. Moreover, the finding of a decreased voluntary switch rate within a high-reward context further supports the claim that unchanged high reward prospect promotes cognitive stability.

Introduction

Cognitive control describes the ability to act in a goal-directed manner. Humans need to be able to maintain goals over time, particularly in the face of distraction. But they also need the ability to flexibly switch between goals and overcome habits and needs in response to significant changes.

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[6] This study is currently under review in identical form, citable as Jurczyk, Fröber, and Dreisbach (under review).

How this so-called flexibility-stability balance of cognitive control (Goschke, 2003; Hommel, 2015) is modulated has been in the focus of cognitive and neuroscientific research over the past decade (for reviews see Botvinick & Braver, 2015; Dreisbach & Fröber, 2019; Goschke & Bolte, 2014). For example, there is much evidence that reward prospect promotes cognitive stability and proactive control (Chiew & Braver, 2014; Fröber & Dreisbach, 2014; Hefer & Dreisbach, 2017, 2020), whereas changing (especially increasing) reward prospect increases cognitive flexibility (Fröber & Dreisbach, 2016b; Kleinsorge & Rinkenauer, 2012; Savine, Beck, Edwards, Chiew, & Braver, 2010; Shen & Chun, 2011). There is also evidence, though, that just any cue or context change might promote cognitive flexibility (Logan & Bundesen, 2003; Mayr & Bell, 2006), calling into question the aforementioned flexibility-increasing effect of changing reward prospect. Here, we aim to disentangle the effects of reward prospect and cue changes on cognitive flexibility in a voluntary task switching paradigm.

How reward prospect promotes cognitive stability and cognitive flexibility

Reward prospect as one of the strongest motivational factors for behavior has been under investigation for decades – in recent years, the question of how motivation affects cognition has been under close examination (Botvinick & Braver, 2015; Goschke & Bolte, 2014; Notebaert & Braem, 2016). In the domain of cognitive tasks, it has been shown that performance-contingent reward prospect leads to increased cognitive stability, in the sense of stronger maintenance of task-relevant information and increased cue usage for task performance (van den Berg, Krebs, Lorist, & Woldorff, 2014; for reviews see Botvinick & Braver, 2015; Dreisbach & Fröber, 2019; Goschke & Bolte, 2014). Evidence from different tasks and paradigms shows that the prospect of performance-contingent reward enhances cue usage in context-processing tasks (Chiew & Braver, 2011, 2014; Fröber & Dreisbach, 2014; Hefer & Dreisbach, 2017, 2020), reduces dual task costs by enhanced task shielding (Fischer et al., 2018), promotes the inhibition of currently irrelevant information (Jiang & Xu, 2013), and increases the attentional blink (Bijleveld, Custers, & Aarts, 2011). There is thus converging evidence from different labs and paradigms that the prospect of reward increases cognitive stability, which however comes at the cost of decreased flexibility.

Nevertheless, one can identify conditions in which reward prospect enhances cognitive flexibility. Researchers making use of the task switching paradigm report findings where the switch costs were systematically reduced by reward prospect (Fröber & Dreisbach, 2016b; Kleinsorge & Rinkenauer, 2012; Savine et al., 2010; Shen & Chun, 2011). Shen and Chun (2011) were the first to show that it critically depends on the immediate reward history whether reward prospect promotes stability or flexibility: In three experiments, they used a standard (forced-choice) task switching paradigm and showed reward cues of changing magnitudes: Announced by a cue, participants could either earn high or low performance-contingent reward on a given trial. Hence, from one trial to the next, reward prospect could either remain low, increase, remain high, or decrease. The authors investigated task switch and task repetition performance as a function of reward sequence and consistently found the fastest repetition RTs when reward prospect remained on a high level – corroborating the stability-inducing effect of unchanged performance-contingent reward prospect found in previous studies. Importantly however, an increase in reward prospect specifically benefitted switch performance: Participants were fastest to switch tasks when reward prospect increased, and the switch costs were smallest in this condition (to some part also due to slowed repetition RTs as compared to unchanged high reward). Thus, Shen and Chun (2011) provided first evidence that an increase in reward prospect enhances cognitive flexibility.

Fröber and colleagues (Fröber et al., 2018; Fröber & Dreisbach, 2016b; Jurczyk et al., 2018) investigated the effect of different reward sequences on voluntary task switching (Arrington & Logan, 2004, 2005). The voluntary switch rate (VSR, that is, the percentage of trials in which participants voluntarily decide to switch tasks) as a function of reward sequence (again with the factor levels remain low, increase, remain high, and decrease) was the main variable of interest. Fröber and Dreisbach (2016) could replicate and extend the finding that unchanged high reward prospect leads to high cognitive stability – now in the form of a low VSR. Yet, both increasing and decreasing reward prospect resulted in increased VSR and did not significantly differ from one another. Unchanged low reward prospect resulted in an intermediate VSR. Hence, in the context of deliberate switching, any change in reward prospect resulted in increased flexibility (but see Fröber et al., 2018; Jurczyk et al., 2018).

Contextual influences in voluntary task switching

However, an alternative interpretation may be that a change of the reward cue per se provokes voluntary task switching. Even though previous studies had used several cues to announce a high vs. low reward, such that the cue in fact changed its physical appearance on every trial, the reward cues within one reward magnitude were more similar (e.g., different shades of gray) than cues of a different reward magnitude (i.e., different colors). So, it might still be that the change of cue category contributed to the sequential reward effect. This would also converge with other findings from the voluntary task switching literature where it was shown that contextual information – particularly transitions from one trial to the next - influence the VSR. Mayr and Bell (2006) ran a voluntary task switching experiment where they investigated whether target stimulus repetitions influenced the VSR. They could show that stimulus repetitions lead to more task repetitions, even though participants were instructed to choose tasks randomly. Even more simply, the task-irrelevant repetition of the stimulus location results in more voluntary task repetitions (Arrington & Weaver, 2015; Mittelstädt et al., 2018). Relatedly, irrelevant shape repetitions reduced the VSR, particularly under concurrent working memory load (Demanet et al., 2010).

Here, we aim to investigate whether meaningless cue changes (a cue that does not convey any advance information about the upcoming task) would promote switching depending on the respective reward context. Note that we use the term „reward context“ to denote the block-wise manipulation of the opportunity of reward, whereas we use the term „reward prospect“ to denote the opportunity to earn reward that is announced by cues that are presented at the beginning of a given trial. Interestingly, there are already indications that reward context and changes of the cue interact: In an animal study by Czaczkes et al. (2018) investigating spontaneous alternation behavior – as an adapted variant of the voluntary task switching paradigm – in the ant species *Lasius niger*, it was shown that the sequential reward effect resembled the one shown for humans. Ants were cued to expect low or high reward at the beginning of a T-maze. They switched paths in the T-maze most often when the incentive offered at the end of both paths changed, and switched less frequently when it stayed on a high level. Importantly, in a second experiment using irrelevant cues – that is, cues that were uninformative about the potential reward –, it was shown that the sequential reward effect is to some part a sequential cue effect: In a high-reward context (i.e., offering in each trial a high-molarity syrup at both ends of a T-maze), irrelevant cue switches resulted in more behavioral switches between two different alternatives (i.e., two different moving paths associated with equally rewarded path goals). In a context of unchanged low reward prospect, the influence of irrelevant visual cues, landmarks, and odor cues was significantly reduced. Thus, at least in ants, irrelevant cue switches on their own result in a higher voluntary switch rate, but only so in a high-reward context. If the same is true for humans, a part of the sequential reward effect may be due to the change in reward cue category.

Current Study

Investigating the effect of irrelevant, that is, meaningless cue switches on voluntary task switching was the main aim of Experiment 1. In addition, different reward contexts were compared, both in terms of a global effect of reward prospect on voluntary task switching, but also with respect to a possible interaction with cue transition. Human participants worked through several blocks of a voluntary task switching paradigm. By using irrelevant cues presented prior to each trial that did not convey any information regarding the reward or the task, cue and reward context could be examined separately. We used brightly colored dots as irrelevant cues, that either kept or changed their color from one trial to the next. We manipulated the reward context in a block-wise fashion by comparing rewarded blocks to no-reward blocks. If contextual influences on voluntary task switching extend to irrelevant changes, meaningless cue switches might also induce a higher VSR irrespective of the current reward condition. In the present experiments, the cue switch on its own did not carry information, because potential reward prospect is instructed at the beginning of each block – in contrast to changes in the target stimulus (Mayr & Bell, 2006) or changes in the prospective reward magnitude (Fröber & Dreisbach, 2016b) of previous experiments. Hence, whether or not the VSR is influenced by these task-irrelevant cues may be informative with regard to potential limits of contextual influences on deliberate task choice.

Our design further allowed to analyze a possible interaction between (irrelevant) cue changes and reward context with respect to the VSR. Assuming that such an interaction exists, previous literature suggests that the effect of cue transitions on the VSR should be enhanced in the rewarded blocks. The findings by Czaczkes and colleagues (2018) with ants suggest that irrelevant cue changes increase switching behavior, but only if they are presented within a high-reward context. Similarly, in the sequential reward effect literature in humans, the effect of increasing reward prospect seems to be stronger or at least more robust than the effect of a decreasing reward, suggesting a higher sensitivity to high as compared to low reward: In a context of global stability (as induced by a lower number of forced task switches in a hybrid task switching paradigm combining forced- and free-choice task switching, Fröber et al., 2018), only an increase in reward prospect resulted in an increased VSR. Similarly, when confronted with tasks of unequal difficulty, only the increase in reward prospect results in increased switching to the more difficult task (Jurczyk et al., 2018).

Experiment 1

Method

Participants. Thirty-two students of the University of Regensburg and two non-students participated in this study (30 female; 30 right-handed; aged between 18 and 39; *M* = 22.7 years; *SD* = 4.4) for course credit. We chose this number of participants based on the main effect of reward sequence reported in a previous study (Fröber & Dreisbach, 2016b, Exp. 5) for a one-way repeated-measures ANOVA in a 100 % voluntary task switching design. A sample size of N=26 has proven sufficient to detect the effect with a level of statistical power of 80 % and a significance level of 5 % using the bias- und uncertainty-corrected sample size planning tool available at www.DesigningExperiments.com (see also Anderson, Kelley, & Maxwell, 2017). The software allows to conduct power analysis based on a previous empirical effect while correcting for publication bias and uncertainty (we assumed an assurance level of 50 %).

It was ensured that all participants had normal color vision by means of the Ishihara test (Ishihara, 2003). All participants signed informed consent prior to the experiment and were naïve with respect to the purpose of the experiment. Psychology students earned course credits for their participation.

Apparatus. The experiment was run using E-Prime 2.0 (Psychology Software Tools, Sharpsburg, PA) on a 19-inch TFT display (display resolution at 1280 x 1024, refresh rate 60 Hz). Responses were collected with a QWERTZ-keyboard, using “y” and “x” as left and right response keys for one task (left hand), and “n” and “m” as keys for the other task (right hand). Participants were seated at approximately 60 cm from the screen (unconstrained), at which distance 1 cm on the screen corresponds approximately to 1° of visual angle.

Stimuli and procedure. Stimuli consisted of numbers (125, 132, 139, 146, 160, 167, 174, 181) or letters (B, D, F, H, S, U, W, Y), which had to be categorized as smaller or larger than 153 (number task) or nearer to A or nearer to Z in the alphabet (letter task) by pressing a left or right response key, respectively. We used this seemingly arbitrary number task (opposed to the typically used smaller/larger than 5 task) in order to match tasks in terms of difficulty – it has been shown that single-figure numbers are easier to categorize than letters (see Fröber & Dreisbach, 2017). Stimuli of one task appeared always above a central fixation point and stimuli of the other task below (1.3 ° of visual angle). Mapping of number or letter task to position on the screen was fixed but counterbalanced across participants, while responses to the upper task were always given with the left hand. All stimuli were displayed in black ink (Calibri font, size 28, bold) on a light gray background (RGB values: 192, 192, 192). On every trial, one stimulus for each task appeared on screen – but participants just had to respond to one of them. We used the classic voluntary task switching instruction (introduced by Arrington & Logan, 2004) that tells participants to perform both tasks about equally often but in random order. To explain randomness, participants were instructed to imagine flipping a coin on every trial where one side is equivalent to choosing the number task and the other side to choosing the letter task. They were further instructed to neither count number of individual task choices nor switch tasks on every trial.

The critical manipulation of our study was to randomly vary the color of the fixation point (presented in Origin font, 26 pt.): In each test block, possible colors were either light blue (RGB values:

80, 169, 255) and orange (RGB values: 255, 130, 0), or pink (RGB values: 255, 100, 255) and green (RGB values: 0, 191, 0). These colors were chosen as they constitute contrasting colors of comparable luminance. One pair of colors was used for the reward blocks, while the other pair was used for the no-reward blocks (assignment counterbalanced across participants). Crucially on each trial, fixation color was randomly changed or repeated. Participants were told about these cues, but the instruction only emphasized that two colors were used to indicate reward blocks, and two others to indicate no-reward blocks. In all practice blocks, this colored point was replaced by a black point.

Participants practiced both tasks separately in two short practice blocks (16 trials each so that each stimulus appeared two times, order of tasks counterbalanced across subjects). These were followed by one voluntary task switching practice block, already preceded by the voluntary task switching instruction (32 trials). A subsequent, longer baseline block of 128 voluntary task switching trials followed where all possible stimulus pairs appeared two times in pseudorandom order where stimulus repetitions were not allowed. During this baseline block individual RT thresholds for earning the reward in the following test phase were calculated as the lower tercile (correct RTs ordered from fast to slow) separately for repetitions and switches.

The following test phase consisted of four blocks of 128 trials each, where two blocks allowed to earn points (reward block) and the other two did not (no-reward block; in alternating order, with starting block counterbalanced across participants). In the reward blocks, participants could earn 7 points for fast and correct responses, irrespective of task choice. All stimulus pairs appeared equally often (and were all paired equally often with each color of the fixation point), pseudorandomized so that stimulus repetitions were prohibited and for every cue, cue repetitions and switches were equally probable and appeared roughly equally distributed. Overall, cue repetitions occurred in 50 % of the trials. After each block in the baseline and test phase, participants were given feedback about their task bias and repetition bias to encourage them to follow the instructions on “random” switching as described above.

Trial procedure of the test phase is depicted in Figure 1. Each trial started with presentation of the colored fixation point. After 500 ms, the two stimuli appeared above and below a then blackened fixation point. The target display stayed on screen until participants had responded. Feedback was displayed for 500 ms, telling the participants whether they answered correctly and, in the reward blocks, whether they had earned the points. When they made an incorrect response, the German word “Falsch” (wrong) appeared on the screen and in case of the reward blocks, a correct but too slow response was followed by a feedback screen where the words “Zu langsam” (too slow) were shown. In case of a correct (and in reward blocks, fast enough) response, “Richtig” (correct) was displayed. The intertrial interval showing a blank display had a random duration that was shorter following correct responses (150-250 ms) and longer following errors (900-1200 ms).

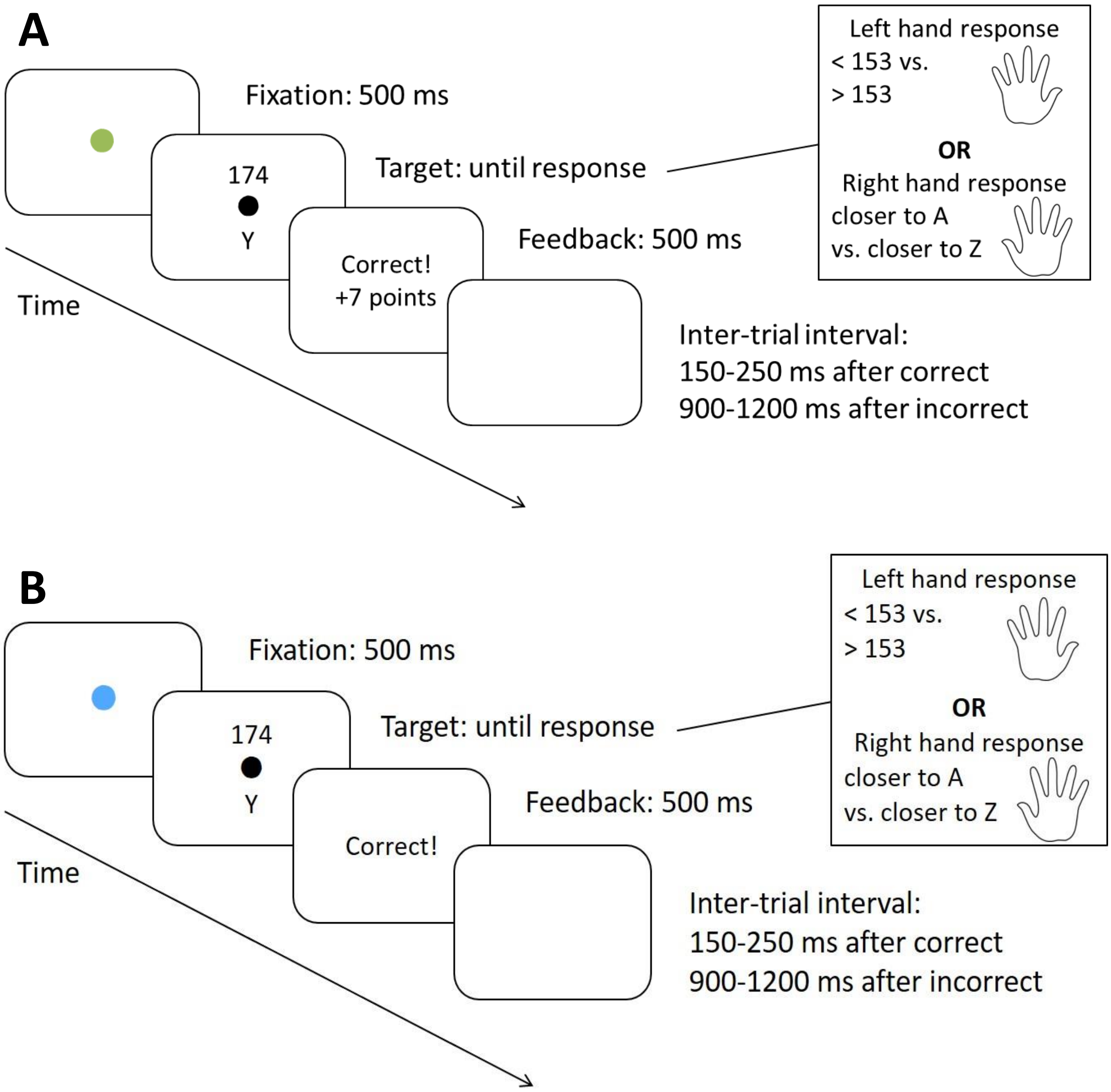

Figure 1. Time course of a sample trial in the reward blocks (A) and in the no-reward blocks (B). In both cases, a colored fixation point is presented for 500 ms followed by the stimulus display. Depending on reaction time and accuracy, feedback is displayed, followed by a variable inter-trial interval.

At the beginning of the test phase, participants were informed about the reward scheme, but instructed that they should also try to answer fast and accurately in no-reward blocks. With their individual high score, people took part in a social competition: The best three participants in the experiment were awarded Amazon vouchers (15 €, 10 €, and 5 € for the best, second-best, and third-best participant). Overall, the experiment took about 35 minutes.

Design. As main dependent variable, we examined the choice measure voluntary switch rate (VSR) in % in a two-factorial repeated-measures design with the factors reward context (reward block vs. no-reward block) and cue transition (cue repetition vs. cue switch). To quantify potential null effects, we calculated Bayes Factors in addition to conventional significance tests.

Additionally, we looked at reaction times (RTs) in ms and error rates (ERRs) in %. In both cases, we calculated 2 (reward context) × 2 (task transition: task repetition vs. task switch) × 2 (cue transition) repeated-measures analyses of variance (ANOVAs).

Results

Data preprocessing. Raw data of this and the next experiment can be found under https://epub.uni-regensburg.de/43217/. For the statistical analyses, we used SPSS v23 (IBM, Armonk, NY), Statistica v14 (StatSoft, Tulsa, OK), and JASP v0.11.1 (JASP Team, Amsterdam, Netherlands) software. The four experimental blocks were considered in the analysis. The first trial of each block was omitted (0.8 % of all trials). Only prior to RT analyses, we also excluded error trials (9.9 %), trials following errors (9.0 %), and trials in which the RT deviated more than three *SD*s from the individual mean of the condition (1.2 %). In addition, two subjects were identified as RT outliers via boxplots created using the SPSS built-in tool. These subjects deviated more than three interquartile ranges from the overall median in the baseline block. Another two subjects did not follow the task choice instructions in the baseline block at all – they exclusively performed one task throughout. Consequently, they were not considered for further analyses. Thus, the final sample used for the statistical analyses contained 28 subjects.

Voluntary switch rate. The ANOVA yielded a main effect of reward context, $F(1, 27) = 42.86$, $p < .001$, $\eta_p^2 = .61$. Participants switched more often in blocks without reward ($M = 40.9$ %, $SD = 15.3$) in comparison to blocks with reward ($M = 30.4$ %, $SD = 16.0$). However, in both cases, participants were not able to follow the instructions to switch and repeat tasks equally often, as would have been expected if they had followed the instructions of random task choice, indicated by *t* tests against 50 %, $t(27) = -3.14$, $p = .004$, $d = -3.14$ for the non-rewarded blocks and $t(27) = -6.51$, $p < .001$, $d = -6.55$ for reward blocks. The main effect of cue transition, *F*(1, 27) = 1.43, *p* = .242, η_p^2 = .05, as well as the interaction, *F*(1, 27) = 0.80, *p* = .380, η_p^2 = .03 (see Fig. 2) were not significant. Bayes Factor analyses were conducted to substantiate these findings. Including in the model a main effect of cue transition revealed a BF_{01}[7] of 3.422; additionally including the interaction effect yielded a BF_{01} of 3.588, hence providing moderate evidence against any effect of cue transition. Just for comparison, the BF_{10} only including the main effect of reward prospect was 4.423e +11, hence the data showed very strong support for this effect.

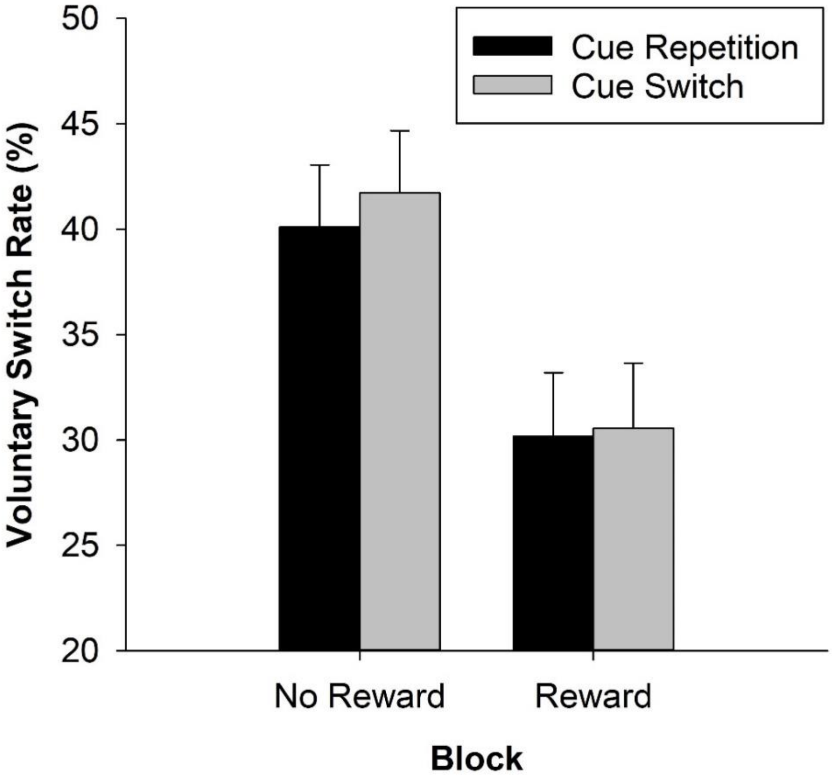

Figure 2. Voluntary switch rate (in %) in Experiment 1 as a function of block and cue transition. Error bars represent + 1 *SEM*.

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[7] We used the preset scale parameter of r = 0.5 for the Cauchy prior as implemented in the JASP software, see also Rouder, Morey, Verhagen, Swagman, and Wagenmakers (2017).

RTs and ERRs. Mean RTs and ERRs as a function of reward prospect, cue transition, and task transition can be found in Table 1. The main effect of reward context, $F(1, 27) = 102.84$, $p < .001$, $\eta_p^2 = .79$, as well as the main effect of task transition, $F(1, 27) = 25.37$, $p < .001$, $\eta_p^2 = .48$, were significant. Participants were faster in reward blocks (442 ms) than in no-reward blocks (551 ms). They showed the typical switch costs (task repetitions: 467 ms vs. task switches: 486 ms). For sake of completeness, we also want to report the marginally significant main effect of cue transition, $F(1, 27) = 3.06$, $p = .091$, $\eta_p^2 = .10$, as well as the trend interaction of reward prospect and task transition, $F(1, 27) = 3.11$, $p = .089$, $\eta_p^2 = .10$. Participants displayed a tendency to respond slower after cue switches (478 ms vs. 474 ms after cue repetitions). Switch costs tended to be larger in no-reward blocks. No other effect reached significance (all *F*s < .90, all *p*s > .360).

The ERR analysis revealed a main effect of reward prospect, $F(1, 27) = 19.56$, $p < .001$, $\eta_p^2 = .42$: Participants made errors in 13 % of all reward-block trials, but only in 8.7 % of all no-reward trials. No other effect in the ERR ANOVA was significant (all *F*s < .75, all *p*s > .397).

Table 1

Mean RTs (in ms) and error rates (in %) of Experiment 1 as a function of reward context, cue transition, and task transition

	No-Reward Blocks				**Reward Blocks (7 points)**			
	Cue Repetition		**Cue Switch**		**Cue Repetition**		**Cue Switch**	
	Repetition	**Switch**	**Repetition**	**Switch**	**Repetition**	**Switch**	**Repetition**	**Switch**
RT	495	526	500	524	434	443	439	451
(*SD*)	(*59*)	(*77*)	(*64*)	(*72*)	(*47*)	(*45*)	(*47*)	(*45*)
ERR	7.76	8.48	7.58	7.46	13.0	13.1	12.0	13.8
(*SD*)	(*5.62*)	(*6.84*)	(*5.79*)	(*5.03*)	(*5.56*)	(*9.09*)	(*5.87*)	(*9.39*)

Discussion

The aim of this first experiment was to investigate the influence of irrelevant cue transitions on voluntary task switching in the context of performance-contingent reward vs. no reward. Next to investigating the two main effects, we were also interested whether we would find indications of a presumed interaction between the two variables (Czaczkes et al., 2018; Fröber et al., 2018; Hefer & Dreisbach, 2017, 2020).

The analyses yielded a significant main effect of reward context on the VSR. In blocks offering reward prospect, participants switched less often between the two tasks than in blocks without an incentive. This result is in line with previous studies that have associated unchanged high reward prospect with stability (e.g., Fröber & Dreisbach, 2016b). Opposed to that, our hypothesis regarding a main effect of cue transition on the VSR could not be confirmed. Irrelevant cue switches did not lead to a significantly higher number of task switches. This stands in contrast to previous studies that have found evidence that relevant changes do affect the VSR (Arrington & Weaver, 2015; Demanet et al., 2010; Fröber & Dreisbach, 2016b; Mayr & Bell, 2006; Mittelstädt et al., 2018), and that irrelevant cue changes affect switching behavior in ants, at least in a context of high reward (Czaczkes et al., 2018). Additionally, an interaction between cue transition and reward context was not significant. Indeed, a Bayes Factor analysis revealed moderate evidence against any effect of cue transition. Hefer and Dreisbach (2017, 2020) compared reward blocks to no-reward blocks, just as in the present Experiment 1, and did find increased cue usage in the rewarded blocks as compared to no-reward blocks. In the task they used, the cue was always relevant (even if not predictive) for the response rule – thus, the irrelevant cues used here might reflect a boundary condition for cue-driven reward effects in humans.

However, before drawing strong conclusions we conducted a second experiment replacing the no reward blocks with low reward blocks. This way, we are able to compare the influence of high vs. low reward prospect between blocks. On the level of consecutive trials, a difference between unchanged high and low reward prospect has been obtained several times (Fröber et al., 2018; Fröber et al., 2019; Fröber et al., 2020; Fröber & Dreisbach, 2016b; Jurczyk et al., 2018; Shen & Chun, 2011) in a sense of increased stability in unchanged high reward prospect trials and increased flexibility in unchanged low reward trials. Irrelevant cue transitions had a marked effect on path switch rate in ants in a high-reward context, whereas such an effect could not be obtained in the low-reward context (Czaczkes et al., 2018). The sequential reward effect in humans equally builds on a comparison between low-reward and high-reward trials. If this reward-magnitude driven effect is a long-lived one, it should also be present in our block-wise manipulation of reward prospect.

On top of that, we are interested whether under these changed reward conditions, an interaction between cue transition and reward prospect will be observable. Experiment 2 therefore aims to answer whether the null effect in the interaction of cue transition and reward prospect in Experiment 1 is owed to the lacking reference of high reward prospect when compared to a no reward context. In other words, without a low-reward reference, it is not clear whether it was indeed perceived as “high” reward. In fact, rather than the absolute reward magnitude, the relative difference between a high and a low reward prospect may play the crucial role for the interaction.

Experiment 2

Method

Participants. A total of 33 subjects participated in Experiment 2. Of these, all were psychology students, all were right-handed, none was colorblind, and all but one were female. On average, participants were 19.8 years old (18-25 years, *SD* = 1.7).

Apparatus, stimuli, and procedure. Everything was exactly the same as in Experiment 1 except for the following changes. No-reward blocks were replaced with low-reward blocks, in which participants could earn 1 point for correct responses. That is, the overall score for winning the Amazon vouchers was the total of the points earned in low-reward and high-reward blocks.

Design. The design was exactly the same as in Experiment 1, except for the factor reward context which now comprised the factor levels low-reward block vs. high-reward block.

Results

Data preprocessing. Data preprocessing criteria were the same as in the first experiment and resulted in the omission of 0.8 % of all test trials for the VSR and ERR analyses. For the RT analyses, we again additionally excluded incorrectly answered trials (8.9 %), trials following errors (8.1 %), and RT outliers calculated for individual conditions (1.2 %). In addition, two subjects were identified as RT outliers via boxplots created using the SPSS built-in tool. These subjects deviated more than three interquartile ranges from the overall median in the baseline block and more than two interquartile ranges from the overall median in the test phase. Thus, 31 subjects were included in the final sample.

Voluntary switch rate. Again only the main effect of reward context was significant, $F(1, 30) = 21.41$, $p < .001$, $\eta_p^2 = .42$. Participants switched more often in blocks with low reward prospect ($M = 31.8$, $SD = 16.5$) in comparison to blocks with high reward prospect ($M = 23.3$, $SD = 12.5$). Again participants' voluntary switch rates differed significantly from chance, both for the high-reward blocks, $t(30) = -11.87$, $p < .001$, $d = -2.14$, and the low-reward blocks, $t(30) = -6.12$, $p < .001$, $d = -1.10$. Neither the main effect of cue transition nor its interaction with reward context approached significance level (both *F*s < .2, both *p*s > .670; see Fig. 3). To substantiate these null findings, we again computed Bayes Factors to quantify the evidence in the data against exclusion of the main effect of cue transition and the interaction. A model including the main effect of cue transition yielded a BF_{01} of 5.144, and further including the interaction of cue transition and reward context a BF_{01} of 4.279. That is, the second experiment again provided moderate evidence against any effect of cue transition. For the sake of completeness, the BF_{10} for the main effect of reward context was at 6.430e+7, again yielding very strong support for this effect.

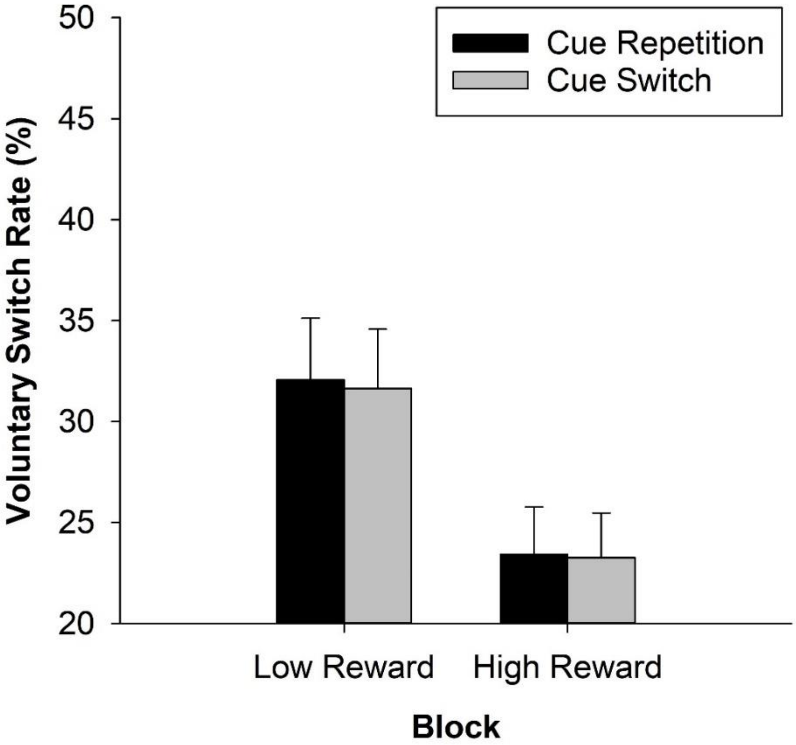

Figure 3. Voluntary switch rate (in %) in Experiment 2 as a function of block and cue transition. Error bars represent + 1 *SEM*.

RTs and ERRs. Mean RTs and ERRs of Experiment 2 as a function of reward context, cue transition, and task transition can be found in Table 2. Analogous to the first experiment, the RT ANOVA yielded a main effect of reward context, *F*(1, 30) = 68.34, *p* < .001, η_p^2 = .70, and of task transition, *F*(1, 30) = 34.04, p < .001, η_p^2 = .53. Participants were faster when offered high reward (455 ms) compared to low reward (529 ms). They showed typical switch costs (task repetitions: 479 ms vs. task switches: 505 ms). All other effects were nonsignificant (all Fs < 1.52, all *p*s > .220).

In the ERR analysis, there was a significant main effect of reward context, *F*(1, 30) = 25.74, *p* < .001, η_p^2 = .46, and a main effect of cue transition, *F*(1, 30) = 5.57, *p* = .025, η_p^2 = .16, which were qualified by a trend significant interaction, *F*(1, 30) = 3.98, *p* = .055, η_p^2 = .12. Cue transitions descriptively had a larger impact on ERR in high-reward blocks (12.3 % vs. 9.6 % for cue repetitions and switches, respectively) than in low-reward blocks (6.9 % vs. 6.5 %). It is important to note that in both cases, cue repetitions resulted in more errors than cue switches. We will turn to this unexpected finding in the discussion. Apart from that, no other effect reached significance (all *F*s < .2.50, all *p*s > .120).

Table 2

Mean RTs (in ms) and error rates (in %) of Experiment 2 as a function of reward context, cue transition, and task transition

	Low-Reward Blocks				**High-Reward Blocks**			
	Cue Repetition		**Cue Switch**		**Cue Repetition**		**Cue Switch**	
	Repetition	**Switch**	**Repetition**	**Switch**	**Repetition**	**Switch**	**Repetition**	**Switch**
RT	514	545	514	542	445	468	443	465
(*SD*)	(*72*)	(*90*)	(*70*)	(*92*)	(*66*)	(*67*)	(*50*)	(*53*)
ERR	6.73	7.07	6.46	6.52	12.7	11.8	10.8	8.42
(*SD*)	(*3.69*)	(*5.15*)	(*3.84*)	(*6.84*)	(*5.59*)	(*9.22*)	(*5.03*)	(*6.53*)

Discussion

Experiment 2 replicated the key findings of Experiment 1. We again examined whether irrelevant cue transitions in a reward context influence voluntary task switches – however, here we compared blocks with low reward to blocks with high reward prospect. The analysis of the VSR again revealed a significant main effect of reward context. Participants switched tasks more often in a context of low reward as compared to a high reward context. This result converges with the findings of Experiment 1 where we found higher switch rates in the reward as compared to the no-reward blocks. In addition, it extends previous studies indicating that unchanged high reward from one trial to the next leads to increased stability and a reduced VSR, whereas unchanged low reward prospect yields a higher VSR in comparison (e.g., Fröber & Dreisbach, 2016b). Experiment 2 replicated the null effect in the interaction of irrelevant cue transitions and reward prospect. Furthermore, the main effect of cue transitions was again nonsignificant. Like in Experiment 1, Bayes Factor analysis revealed moderate evidence against any effect of cue transition.

General Discussion

In two voluntary task switching experiments, we examined the effect of irrelevant cue transitions in the context of either reward vs. no reward or high vs. low reward. We used colored dots as irrelevant cues, that could randomly either change their color (cue switch) or stay the same color (cue repetition) from one trial to the next. Reward prospect in Experiment 1 was manipulated in blocks associated with unchanged reward of 7 points for fast and correct responses vs. blocks with no reward. In Experiment 2, blocks with unchanged high reward of 7 points were contrasted with blocks with low reward of 1 point for correct responses. In both experiments, the reward context had an impact on the VSR: Participants switched tasks less often when offered high reward of 7 points. An interaction between cue transition and reward context or a main effect of cue transition on the VSR could not be confirmed, though. This suggests that the often observed increased VSR when reward prospect (and thus the reward cue) changes cannot be ascribed to bottom-up influences of changing cues.

Reward prospect enhances cognitive stability over extended periods of time

Reward prospect has repeatedly been shown to bias the cognitive system toward more stability by enhancing task-related processing (Botvinick & Braver, 2015; Dreisbach & Fröber, 2019; Goschke & Bolte, 2014). This has specifically been investigated in the context of standard and voluntary task switching (Fröber & Dreisbach, 2016b; Shen & Chun, 2011), where unchanged (high) reward prospect – manipulated in a trial-wise fashion – resulted in selectively fast performance on task repetition trials and a repetition bias in task choice. The results of the current experiments extend these findings to block-wise manipulations of reward prospect. In blocks with stable reward prospect (as compared to no reward) and in blocks with high reward prospect (as compared to low reward), we consistently found a reduced VSR. This result is especially notable because it increases the generalizability of the stability-inducing effect of performance-contingent (high) reward and therefore its relevance for our everyday life.

The stability-inducing effect of reward prospect is often ascribed to an enhanced attentional focus on the task and a selective strengthening of task-related processing: Both electrophysiological (van den Berg et al., 2014) and neuroimaging (Padmala & Pessoa, 2011) evidence point to an increased involvement of a fronto-parietal attentional-control network by reward prospect, possibly mediated by subcortical noradrenergic and dopaminergic structures (Aston-Jones & Cohen, 2005; Botvinick & Braver, 2015; Cools & D'Esposito, 2011; Demanet, Liefooghe, & Verbruggen, 2011). In that way, unchanged (high) reward prospect seems to aid one basic function of the prefrontal cortex that lies in a robust maintenance of task-relevant information (O'Reilly, 2006). Because active maintenance in working memory is capacity-limited (Oberauer et al., 2013), this is directly linked to a decreased flexibility (Goschke, 2003; Hommel, 2015).

Irrelevant cues as a limit to contextual influences on task choice?

Various studies using the voluntary task switching paradigm have revealed factors that enhance flexible behavior and increase the VSR. For example, task relevant changes such as a change in stimulus lead to higher rates of task switches (Arrington & Weaver, 2015; Mayr & Bell, 2006). Both experiments of the current study have found limitations of these contextual influences on voluntary task choice. We have investigated the effect of a situational change that is not of direct importance to the participant. For that, we used irrelevant color cues that were presented in the middle of the screen at the beginning of each trial. Against our initial hypothesis, such cues did not exert a significant effect on the VSR. Both experiments yielded moderate evidence against any effect of cue transitions – in comparison to relevant changes such as stimulus changes or reward cue changes (Fröber & Dreisbach, 2016b). Thus, the results of Experiments 1 and 2 suggest that the effect of environmental changes on the VSR are restricted to relevant changes. Our results can be classed with the more and more spreading research perspective that contexts can be associated with different control settings. That is, cognitive control mode – such as stability or flexibility – is not only guided by the current goals and task requirements, but is also determined by contextual influences that are guided by associative principles (Abrahamse et al., 2016). For example, it was shown that cues, locations, or stimuli that are associated with a greater switch probability result in decreased switch costs (Braem & Egner, 2018; Crump & Logan, 2010; Dreisbach & Haider, 2006). Similarly, we have shown that dependent on the reward context, voluntary switching may either be globally increased or decreased. However, irrelevant but centrally presented cues that could switch or repeat did not further affect voluntary choices.

The analyses of reaction times and error rates show that the absence of a cue-dependent effect on task choice was not due to missed perception and processing of the cue transitions. In Experiment 1, participants showed a tendency to answer more slowly after cue switches. In Experiment 2, they made more errors when the cue switched. This could be due to partial repetition costs (Hommel, 2004; Zmigrod, Sonneville, Colzato, Swaab, & Hommel, 2013): When an event – such as a trial – consists of several features (say, the cue, the stimulus, task decision, and response), these are bound together in a temporal event file. When the next event (i.e., the next trial) ensues and a feature repeats, the whole previous event file is retrieved, facilitating performance in full repetition trials, and hindering performance in partial repetition trials, as the previous binding first has to be dissolved. Hence, if the cue that is presented before the task retrieves a certain event file, it should interact with performance. We therefore assume that the effect of cue transition on performance may indicate such feature integration processes. However, future research will have to disentangle how these integration processes interact across cue, stimulus, task, and response repetitions (cf. Demanet et al., 2010; Schmidt & Liefooghe, 2016). It is important to note that performance and choice indices may capture the influence of variables such as reward or cue transitions in different ways, such as in the current experiments.

Because our VSR data also provided moderate evidence against a model assuming an interaction between cue transition and reward context, we can conclude that the reward context does not seem to modulate the effect of irrelevant cue switches. Thus, our results are not in line with the study on ants by Czaczkes et al. (2018). Their study was conceptually similar to the current Experiment 2. They did find an interaction between irrelevant cue switches and reward prospect, though. The ants changed paths in a T-maze more often if presented with irrelevant cue switches, and they did so only in a context of unchanged high reward prospect. It should be mentioned that in the ant study, visual color cues, orientations helps, and olfactory cues were used as irrelevant cues. Here, the study by Czaczkes and colleagues could inspire future research, for example if different modalities like auditory or olfactory stimulation are used, and maybe even concurrently.

Reward prospect has direct impact on participants' behavior. Changes in the environment that are not directly relevant to the task at hand or are not salient should not have such a marked effect on behavior. Such irrelevant environmental changes – as the irrelevant cues in the current example – seem to be only of negligible importance for behavior and also for choice patterns. The cue transitions affected performance merely in marginally significant effects. Thus, it is another important result of the current study that the readiness to switch is more dependent on top-down control than previous studies have suggested (Mayr & Bell, 2006), at least when feature repetitions or switches are not part of the task-relevant stimulus.

Future outlook

In both experiments, participants only received the high reward when they answered not only correctly, but also below an individually determined response time threshold. The low reward in Experiment 2, however, could already be earned with a correct response and was not dependent on the reaction time. In Experiment 1, participants similarly only got a feedback on their accuracy, but not on the reaction time in the no-reward blocks. Even though the RT thresholds were determined separately for task repetitions and task switches, it could be that participants simply switched less often between the two tasks with high reward prospect because of the experienced time pressure. Indeed, reaction time and error rate data in both experiments revealed a speed-accuracy tradeoff: In that high-reward blocks were associated with faster, but less accurate responses. For a clear dissociation between response strategy or reward strategy, future studies should adapt reward reception criteria. However, it has to be noted that in a trial-wise examination, using the same response time threshold for both high and low reward still resulted in increased stability in trials of unchanged high reward (Fröber & Dreisbach, 2016b, Exp. 3).

Conclusion

In two voluntary task switching experiments we have investigated the impact of irrelevant cue transitions on voluntary task switching in the context of different reward prospects. Reward prospect was manipulated between blocks and was varied between unchanged high-reward blocks and no-reward blocks in Experiment 1, and between unchanged high-reward blocks and unchanged low-reward blocks in Experiment 2. While the cues did not have any effect on the switch rate, the reward context did influence the VSR in a systematic manner: In high-reward blocks, participants switched between tasks less often. Our results thus confirm and extend the performance-stabilizing effect of unchanged (high) reward and show that the bottom-up influences on voluntary task switching are restricted to task-relevant stimulus features.

STUDY 3:

Does Temporal Predictability of Tasks influence Task Choice?

Jurczyk, V., Mittelstädt, V., & Fröber, K.

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Abstract

Task performance improves when the required tasks are predicted by the preceding time intervals, suggesting that participants form time-based task expectancies. In the present study, we pursued the question whether temporal predictability of tasks can also influence task choice. For this purpose, we conducted three experiments using a hybrid task switching paradigm (with two tasks) combining forced-choice and free-choice trials. Each trial was preceded by either a short (500 ms) or a long (1500 ms) foreperiod. In forced-choice trials, the instructed task was predicted by the length of the foreperiod (Exp. 1A and 1B: 100 % foreperiod-task contingencies; Exp. 2: 80 % foreperiod-task contingencies). In the remaining trials, participants were free to choose which task to perform. In all three experiments, we found that participants' task choice was influenced by the foreperiod-task contingencies implemented in forced-choice trials. Specifically, participants were overall biased to choose tasks compatible with these contingencies; these compatible choice rates were larger for the short compared to the long foreperiod. Our findings suggest that learned time-based task expectancies influence subjects' voluntary task choice and that an initially present task bias toward the "short" task

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is not always overcome at the long foreperiod. We discuss potential underlying mechanisms against the background of voluntary task switching and interval timing.

Introduction

The timing of events is an important predictor for action planning and action selection (Kolling & O'Reilly, 2018; Petter, Gershman, & Meck, 2018): Imagine meeting with a friend. While you are waiting at your arranged meeting place, time goes by. At first you will probably be prepared for your friend to be on time – your planned course of action accordingly is to interact with that person. When you have been waiting for some minutes, you will start to overthink that plan. Maybe it would be wise to call or message your friend in case they forgot or had another meeting place in mind? That is, depending on the time passing by, different events and also different actions will become more likely. As a consequence, your readiness for doing certain actions will shift slowly from one action to the next as time passes. There are many cases in which the relationship between passing time and expected events has high informative value for task selection. Previous research has shown that participants learn the temporal predictability of events as reflected in observable changes in performance. For example, task performance improves for trials in which the foreperiod is predictable of the upcoming task (i.e., so-called time-based task expectancy; Aufschnaiter, Kiesel, Dreisbach et al., 2018). However, it is unclear whether this information is also actively used to guide task selection. In the present study, we therefore aim to investigate whether and how temporal predictability can also influence task choice behavior.

General and specific temporal predictability effects

In our introduction, we will first review previous studies demonstrating influences of temporal predictability on performance before turning to a discussion of contextual influences on task choice behavior. Lastly, we will elaborate on whether and how time-based task expectancies may influence task choice behavior. In general, timing studies have primarily focused on investigating the expectancy of certain time durations (general time expectancy; Los & Heslenfeld, 2005; Niemi & Näätänen, 1981), conscious time estimation (interval timing; Balcı & Simen, 2016), and the neural underpinnings of time perception (see, e.g., Merchant & Lafuente, 2014; Wearden, 2016). One basic finding is that subjects can learn to incorporate the expected duration of a foreperiod in their behavior: If the foreperiod duration is predictable or precued, performance (e.g., reaction time, RT) is improved in predicted compared to unpredicted or unpredictable intervals (Coull & Nobre, 1998; Coull, Frith, Büchel, & Nobre, 2000; Los, 2013; Miniussi, Wilding, Coull, & Nobre, 1999). Interestingly, if the interval duration is fixed within a block, performance profits from short intervals compared to long foreperiods (the fixed foreperiod effect, Niemi & Näätänen, 1981); however, with variable foreperiods, performance improves with prolonged foreperiod durations (the variable foreperiod effect; e.g., Los & Heslenfeld, 2005; Los & van den Heuvel, 2001; Steinborn, Rolke, Bratzke, & Ulrich, 2008). A large portion of this latter effect can be ascribed to an asymmetric sequential effect, in that short-foreperiod trials suffer from a preceding long-foreperiod trial, whereas long foreperiod trials are answered fast no matter the preceding foreperiod length (Los, Kruijne, & Meeter, 2014; Steinborn et al., 2008; Vallesi & Shallice, 2007). On a theoretical level, the effects can be most parsimoniously explained by a trace conditioning account (Los et al., 2014; Los & Heslenfeld, 2005): The beginning of the foreperiod (often indicated by a warning stimulus) marks the beginning of a temporally structured cascade where unspecific readiness to respond is shaped by a conditioning process. The conditioned strength of each time point is reinforced whenever the imperative stimulus appears at that time point. It remains unchanged whenever the foreperiod ends before passing that time point and is weakened when that time point passes without an imperative stimulus appearing. Other accounts favor a more strategic point of view (Niemi & Näätänen, 1981), assuming participants actively prepare according to the estimated probability of stimulus occurrence, or a mixture of both intentional preparation and unintentional conditioning (Langner, Steinborn, Eickhoff, & Huestegge, 2018; Vallesi & Shallice, 2007).

More relevant for the purpose of the present study are findings of temporal expectancies for specific events (Wagener & Hoffmann, 2010). A growing body of literature shows that time-event correlations can be exploited for improving behavior: If an event appears more likely after a certain foreperiod (than after another foreperiod, or than another event), this foreperiod-event correlation will influence behavior: Expected time-event associations will lead to better performance (faster RTs, smaller error rates) than unexpected ones (Thomaschke, Wagener, Kiesel, & Hoffmann, 2011). Thus, in contrast to the variable foreperiod effect mentioned above, the time-event correlation paradigm aims at temporally manipulating readiness to respond in a specific way, rather than shaping unspecific readiness. This has been shown for events such as specific stimulus-response associations (Schröter, Birngruber, Bratzke, Miller, & Ulrich, 2015), stimulus-effector associations (Thomaschke & Dreisbach, 2013), and even affective qualities (Bogon, Thomaschke, & Dreisbach, 2017). The time-based event expectancy effects were also found robustly across different frequency distributions of events and foreperiods and across different foreperiod durations (Thomaschke et al., 2011).

On a theoretical level, the time-event correlation effect is presumably due to learning associations between intervals and the respective stimulus-response events (Thomaschke & Dreisbach, 2015). Participants then use these learned expectations to prepare for the required task or event, depending on the passing time. Several studies suggest that it is primarily action preparation rather than perceptual preparation that profits from the time-event correlations (Thomaschke & Dreisbach, 2013, Thomaschke & Dreisbach, 2015; Volberg & Thomaschke, 2017). For example, when a certain effector (e.g., the right digit finger) is more often required after a certain time interval (e.g., after a short rather than after a long foreperiod), responses using this effector will be faster - even when controlling for stimulus-time associations (Thomaschke & Dreisbach, 2013). While these studies suggest that action preparation is an important contributing factor to time-based expectancy effects, they do not preclude other forms of preparation to play a role, too. For example, (task) performance is also improved if the upcoming stimulus location can be predicted based on the time interval (compared to an unpredicted stimulus location; Pfeuffer, Aufschnaiter, Thomaschke, & Kiesel, under revision; Wagener & Hoffmann, 2010).

Evidence for time-based (task) expectancies

It is important, though, that specific time expectancy goes beyond simple time-effector correlations: Providing evidence for this, Wendt and Kiesel (2011) have found that if high or low response-conflict likelihood is associated with a short or long foreperiod, respectively, attentional adjustments will be stronger if the current foreperiod predicts large conflict likelihood and vice versa. Another example is provided by recent studies demonstrating the formation of time-based *task* expectancies (Aufschnaiter, Kiesel, Dreisbach et al., 2018; Aufschnaiter, Kiesel, & Thomaschke, 2018). These studies make use of a version of the task switching paradigm in which a cue indicates which task to perform on a given trial (e.g., if a target number is displayed in blue, participants should perform a parity classification, if presented in red, they should perform a magnitude classification). Critically, the task can either switch or repeat from one trial to the next. The classic finding is that performance (RTs and/or error rates) is worse in switch trials compared to repetition trials (for reviews see Monsell, 2003; Kiesel et al., 2010; Vandierendonck et al., 2010). These so-called switch costs indicate that additional time is needed to reconfigure a new task set and/or overcome the previously required one. Consequently, switch trials especially profit from longer (temporally nonpredictive) preparation intervals (Monsell & Mizon, 2006). Here, the elapsed time neither serves as a cue for unspecific readiness (as in the variable FP paradigm) nor as a task-specific trigger (as in the time-event correlation paradigm). Instead, passing time just means more time for passive decay of the old (interfering) task-set and – if a specific task cue is given – more time to prepare for the upcoming task.

To investigate the influence of *predictive* preparation intervals on task performance, Aufschnaiter, Kiesel, Dreisbach et al. (2018; Aufschnaiter, Kiesel, & Thomaschke, 2018) combined this task switching procedure with the variable foreperiod paradigm by implementing contingencies between foreperiods and tasks: Participants were presented with digits that were either to be classified according to their parity or according to their magnitude (smaller or larger than five) depending on the color of the appearing digit. Importantly, one task was more often preceded by one foreperiod (e.g., parity task after a short foreperiod) and the other task was more often preceded by another foreperiod (e.g., magnitude task after long), while the overall frequency of short and long foreperiods was kept equal. Over three experiments that differed in terms of the degree of predictability (90 %, 80 %, 70 %), subjects were faster (and in Experiment 1 also less error-prone) in trials with frequent foreperiod-task associations. This effect of temporal predictability did not depend on awareness.

Contextual influences on task choice behavior

Crucially, in these studies the specific task to be performed in a given trial was always specified (e.g., a color indicated the appropriate task in a trial – so-called forced-choice trials). However, temporal predictability in our environment may also be used when we voluntarily decide which course of action to pursue. Following up on the above-mentioned example of meeting with an unpunctual friend, the passing time provides information about which task is most appropriate (e.g., making a phone call as time passes). In general, people are usually free which task they want to perform and thus they need to flexibly schedule whether they perform the same task again or whether they want to switch to another task (e.g., Fröber & Dreisbach, 2017). Recent research suggests that people are able to adapt their task choice behavior to changing multitasking environments, such as changes in rewards for task completion (Fröber & Dreisbach, 2016b) or to predictable changes in task availabilities (Mittelstädt et al., 2018). As far as we are aware, though, there are no previous studies investigating whether and how time-based task expectancy influences voluntary task choice.

This question is not trivial: To further our theoretical understanding of time-based task expectancies, it is important to understand not only how they influence task *performance*, but also task *selection* processes. Task performance and task selection seem to involve partially diverting cognitive control mechanisms (Arrington & Yates, 2009; Chen & Hsieh, 2013; Orr & Weissman, 2011). Consequently, even though studies have found factors influencing task choice behavior such as preparation time (Arrington & Logan, 2004), stimulus repetition (Mayr & Bell, 2006), or task difficulty (Yeung, 2010), these effects are not directly deducible from task performance effects: For example, when confronted with the voluntary choice between two tasks that vary in task difficulty, participants perform better in the relatively easier task, while task choice is biased toward the more difficult task.

In voluntary task choice, action selection seems to precede stimulus selection (Herbort & Rosenbaum, 2014). Thus, variables that primarily affect action selection (e.g., differential response interference; Jurczyk et al., 2018; Yeung, 2010) should influence free choice behavior to a greater degree than variables affecting stimulus selection (e.g., by specifying the stimulus, but not the response hand, Herbort & Rosenbaum, 2014). Time-based expectancy effects largely reflect the expectancy of certain *actions* (Volberg & Thomaschke, 2017), suggesting that a modulating influence on task choice is possible.

The present experiments

The present study aims to investigate people's voluntary task choice behavior in a temporally structured task environment. For this purpose, we will use the hybrid task switching paradigm (i.e., a combination of free-choice and forced-choice trials) introduced by Fröber and Dreisbach (2016b, 2017): Here, in each block there is a combination of both free- and forced-choice trials. Using univalent stimuli, trials where just one stimulus of one task appears constitute forced-choice trials, while two appearing stimuli of both tasks indicate free choice. If free-choice trials are paired with a sufficient ratio of forced-choice trials, a reasonable amount of voluntary switching can be obtained (e.g., voluntary switch rates of over 20 % with 50 % forced-choice trials; Fröber et al., 2018) even without telling participants explicitly to do so (as opposed to instructing participants to do both tasks equally often but in random order, see Arrington & Logan, 2004, 2005; Arrington et al., 2014). Thus, in contrast to the standard voluntary task switching paradigm, task choice behavior can be investigated without additional instructions that restrict participants' choice.

The critical point here is that we varied contingencies between time intervals and tasks for forced-choice trials (see Aufschnaiter, Kiesel, Dreisbach et al., 2018). The main question we are pursuing is whether temporal contingencies implemented in forced-choice trials influence task choice in free-choice trials. Based on findings of temporal predictability effects on task performance and findings that people are able to adapt their task choice behavior to different task environments, task choice behavior may be biased toward tasks compatible with the foreperiod-task associations formed in forced-choice trials. On the other hand, as mentioned earlier, it is not clear whether task choice behavior is biased at all in this temporally structured environment.

It is in particular interesting to explore how participants will adapt their task choice behavior to the different foreperiod lengths. Depending on whether the size of the effect differs between the short and the long foreperiod, we can make tentative inferences about the underlying mechanisms. If the compatible choice rate is higher for the longer foreperiod, participants presumably rather built time-based task expectancies for the long foreperiod-task association. By design, as long as the short foreperiod has not passed, the probabilities for the foreperiod to continue or of a stimulus to be shown are equal (for a similar argument along the lines of hazard function in the variable foreperiod effect see e.g., Nobre, Correa, & Coull, 2007). The long foreperiod, however, will always be followed by a stimulus display, most likely of one specific task. Also, even in voluntary task switching, preparation processes profit from longer pretarget intervals (Arrington & Logan, 2004). This might extend to processes related to temporal predictability.

On the other hand, compatible choice rate might also be stronger or the same size for the short foreperiod: In the variable foreperiod paradigm, sequential and modality-based modulations are restricted to the short foreperiod (Steinborn, Rolke, Bratzke, & Ulrich, 2009, 2010). For task performance, time-based task expectancy effects for the short foreperiod were found, and sometimes even resulted in an RT benefit for this short foreperiod. Aufschnaiter and colleagues (2018) speculate that this may be due to higher phasic alertness after the short foreperiod (cf., Meiran, Chorev, & Sapir, 2000; Niemi & Näätänen, 1981) or less precise time-keeping ability, the more time passes (the scalar property of timing, for a review see Hass & Durstewitz, 2014). However, one could also argue that when participants start out the trial preparing for one task, they may not always be able to switch preparation to the other task (see also General Discussion). Lastly, the temporal predictabilities established in the forced-choice trials might affect voluntary choice behavior on the level of general task preferences rather than in a fine-grained manner depending on the foreperiod: As participants were completely free in their task-choice behavior – which makes the emergence of such task biases more likely (Kessler et al., 2009) –, we also checked whether systematic overall task biases emerged.

In our analyses, we also considered whether participants switched or repeated tasks because these task transitions have considerable effects on both task performance and task choice. Specifically, robust switch costs are also present in voluntary task switching settings and participants usually show a strong bias to repeat tasks (Arrington & Logan, 2004, 2005; Fröber & Dreisbach, 2017; Kessler et al., 2009). Time-based task expectancy effects on performance are sometimes stronger for task switches, but no clear picture has emerged yet. The present findings might help to further our understanding of the interaction between task transition and temporal predictability.

To foreshadow, even though participants' choice behavior was overall biased to select the compatible task in Experiment 1, the specific length (short vs. long) of the foreperiod additionally modulated task choice. In Experiment 2 and 3, we demonstrate the robustness of these (partially surprising) timing-induced choice biases across modified paradigms.

Experiment 1A

In the first experiment, the contingencies between foreperiods and tasks in the forced-choice trials were kept at 100 % - thus, on forced-choice trials one task was always preceded by a short foreperiod (i.e., 500 ms), while the other task was always preceded by a long foreperiod (i.e., 1500 ms, Aufschnaiter, Kiesel, Dreisbach et al., 2018). A first learning block consisted only of those forced-choice trials. In the subsequent hybrid test phase, we used a 50:50 free- to forced-choice ratio – free-choice trials were randomly preceded by short and long foreperiods. Participants were instructed to voluntarily select one of the two possible tasks in each free-choice trial (i.e., without any randomness instruction as in typical voluntary task switching studies; for a review see Arrington et al., 2014). The percentage of choices compatible with the temporal predictabilities of the forced-choice trials (hereafter designated "compatible choice rate") was the main dependent variable. The hybrid test phase was followed by a 100 % forced-choice test block where foreperiods were no longer predictive of the upcoming task. Thus, this final phase allowed us to assess whether typical time-based task expectancy effects on forced-choice task performance (Aufschnaiter, Kiesel, Dreisbach et al., 2018; Aufschnaiter, Kiesel, & Thomaschke, 2018) can also be observed in this hybrid task switching environment.

Although our main goal was to investigate whether temporal predictability influences task-choice behavior, we also investigated whether time-based task expectancies on forced-choice trials would influence task performance on free-choice trials in a similar manner (i.e., faster RTs for expected interval-task combinations than for unexpected interval-task combinations). Even though this is not a necessary preliminary for effects on task choice, the analysis provides an important extension of previous time-based task expectancy effects from forced-choice to free-choice trials. If so, this would demonstrate that time-based expectancy effects on task performance presumably build on similar mechanisms both for forced-choice and free-choice trials.

Method

Participants. Thirty-two students of the University of Regensburg took part in this study (26 female; 26 right-handed; age range = 18-26; *M* = 20.2; *SD* = 2.0) for course credit or money (6 €). We chose this number of participants on the basis of the main effect of temporal predictability on task performance reported in a previous study (Aufschnaiter, Kiesel, Dreisbach et al., 2018). A sample size of 26 has proven sufficient to detect the effect with a level of statistical power of 80% and a significance level of 5% using the bias- and uncertainty-corrected sample size planning tool available at www.DesigningExperiments.com (see also Anderson et al., 2017). The software allows to conduct power analysis based on a previous empirical effect while correcting for publication bias and uncertainty (we assumed an assurance level of 50%). For reasons of counterbalancing we rounded to 32 participants. All participants signed informed consent prior to the experiment and were naïve with respect to the purpose of the experiment. Participants were treated in accordance with the ethical standards of the American Psychological Association.

Apparatus. The experiment was run using E-Prime 2.0 (Psychology Software Tools, Sharpsburg, PA) on a 19-inch TFT display (display resolution at 1280 * 1024, refresh rate 60 Hz). Responses were collected with a QWERTZ-keyboard, using “y” and “x” as left and right response keys for one task (left hand), and “n” and “m” as keys for the other task (right hand). Responses were to be given with the digit and middle finger of the respective hand. Participants were seated at approximately 60 cm from the screen (unconstrained), at which distance 1 cm on the screen corresponds approximately to 1° of visual angle.

Stimuli and procedure. Stimuli consisted of numbers (125, 132, 139, 146, 160, 167, 174, 181) or letters (B, D, F, H, S, U, W, Y), which had to be categorized as smaller or larger than 153 (number task) or nearer to A or nearer to Z in the alphabet (letter task) by pressing a left or right response key, respectively. We used this seemingly arbitrary number task (opposed to the typically used smaller/larger than 5 task) in order to match tasks in terms of difficulty.[9] Stimuli of one task appeared always above a central fixation cross and stimuli of the other task below (1.3 cm). Mapping of number or letter task to position on the screen was fixed but counterbalanced across participants, while responses to the upper task were always given with the left hand. All stimuli were displayed in black ink (Calibri font, size 28, bold) on a silver (RGB: 192, 192, 192) background. A central fixation cross was displayed in Calibri font, size 24, bold, 0.4 cm. On forced-choice trials only one stimulus appeared on screen, on free-choice trials two stimuli were shown simultaneously and participants were free to choose which task to perform (voluntary task switching).

Participants practiced both tasks separately in two short practice blocks (16 trials each so that each stimulus randomly appeared two times, order of tasks counterbalanced across subjects, already paired with the respective foreperiods, see below). These were followed by one practice block of forced-choice switch trials (16 trials). A subsequent longer learning block of 128 trials also consisted of just forced-choice trials and was meant to establish foreperiod-task contingencies: One task was always paired with a 500 ms foreperiod (short fixation duration), the other with a 1500 ms foreperiod (long fixation duration) prior to stimulus onset. The foreperiod-task contingencies were fixed during the experiment for each participant but were counterbalanced across participants. A following hybrid test phase consisted of five blocks of 128 trials each. 50 % of the trials were forced-choice trials, 50 % free-choice trials. All stimuli appeared equally often, but without direct stimulus repetitions, and the order of stimuli was pseudorandomized so that all combinations of trial type (forced, free) x foreperiod/task (short, long; numbers, letters) appeared about equally often and equally distributed. In the free-choice trials, each foreperiod appeared with equal probability, while also controlling for equally distributed foreperiod-foreperiod transitions.

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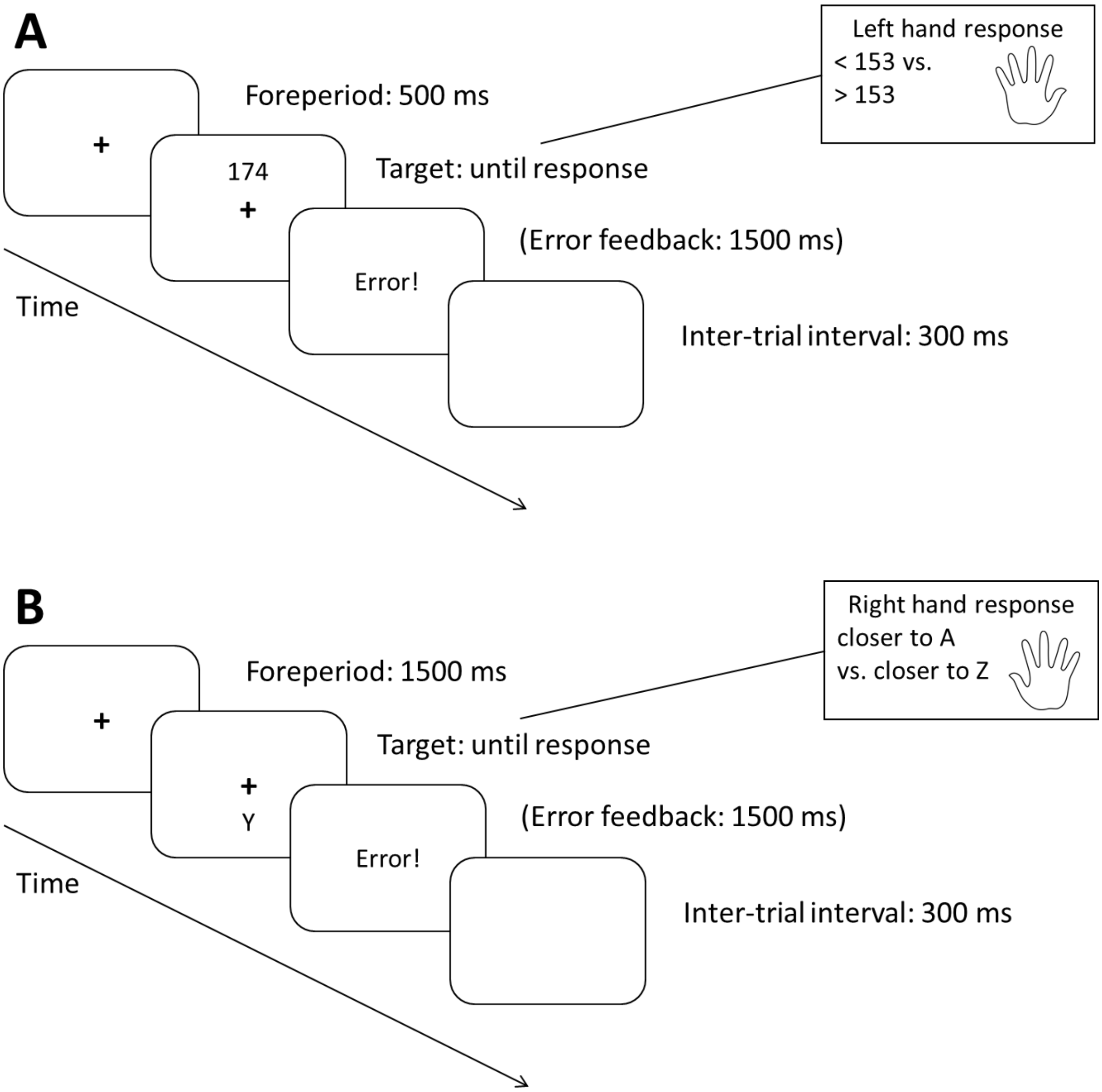
[9] It has been shown that differences in task difficulty induce a task bias toward the easier task under unrestricted free choice (Jurczyk et al., 2018), and that the three-digit number and the alphabet letter task are comparable in task difficulty without inducing a task bias (Fröber & Dreisbach, 2017).

The trial procedure for both forced-choice and free-choice trials is depicted in Figure 1. Each trial began with the presentation of the fixation cross for either 500 ms (short) or 1500 ms (long). Then the stimulus or, for free-choice trials, the stimuli were presented until a response was made. A feedback display was only displayed if an error was made. It lasted for 1500 ms and the word “Fehler!” (German for “Error”) was displayed. The intertrial interval was a blank screen and lasted for 300 ms.



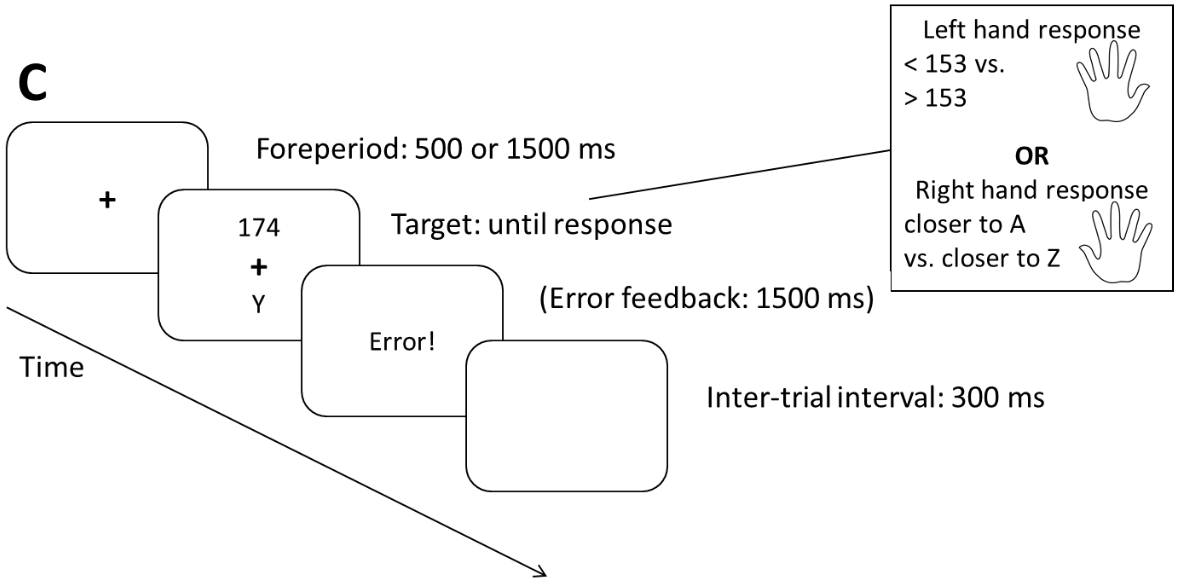


Figure 1. Illustration of a single trial. A: Example of a forced-choice trial with a short foreperiod, followed by one of the tasks. B: Example of a forced-choice trial with a long foreperiod, followed by the other task. C: Example of a free-choice trial with variable foreperiod, short or long, and voluntary choice between both tasks.

In a last 100 % forced-choice test phase (another block of 128 trials), the contingencies between tasks and foreperiods were abolished, meaning that each task was preceded by the short foreperiod in 50 % of trials, and by the long foreperiod in the remaining 50 % of the trials.

Design. Task-choice behavior on free-choice trials in the test phase was of main interest, especially whether participants preferentially chose the task compatible with the foreperiod-task contingencies implemented on forced-choice trials. Compatible choice rate was defined as the percentage of trials in which participants chose the task compatible with the current foreperiod (as established in forced-choice trials). This compatible choice rate was assessed via one-sample *t* tests against compatibility choice rates predicted by chance (i.e., 50 %) and in a more fine-grained 2 (foreperiod: short, long) x 2 (transition: repetition, switch) repeated-measures analysis of variance (ANOVA). We also analyzed via one-sample *t* tests (against a 50 % chance level) whether general choice biases emerged: Task biases toward the task associated with the short or long foreperiod and the repetition bias were considered.

Furthermore, we analyzed RTs and ERRs on free-choice trials in 2 (foreperiod) x 2 (compatibility: compatible vs. incompatible task choice) x 2 (transition) repeated-measures ANOVA.

In the last test block (forced-choice trials only), RTs and ERRs were analyzed as a function of foreperiod (short, long), previous compatibility (compatible, incompatible) of the current foreperiod-task association, and transition (repetition, switch).

Results

Data preprocessing. Raw data of this and the following two experiments can be found under https://epub.uni-regensburg.de/41403/. We excluded the first trial of each block (0.8 %), as it does not entail task transition. Task-choice analyses used all remaining trials including errors to cover all attempts of voluntary switching (Arrington & Logan, 2004). Errors in free-choice trials were assigned to task by selected hand as it can be assumed that participants rather choose the wrong finger than choose the wrong hand (Scheffers & Coles, 2000). In the RT analyses, we additionally excluded trials with excessively slow or fast reaction times (over 3 *SD*s from the subject's mean in a condition; 1.8 % of all trials), error trials (4.6 %) as well as post-error trials (4.3 %). We further excluded the data set from two participants as they were considered outliers (as identified by boxplots) in mean ERRs.[10]

Free-choice trials, task choice. We first examined whether general task biases occurred. Overall, participants showed a bias toward the task associated with the short foreperiod (*M* = 56.6, *SD* = 27.7), *t*(29) = 11.19, *p* < .001, *d* = 0.24, and also toward the number task (*M* = 52.2, *SD* = 28.4), *t*(29) = 10.07, *p* < .001, *d* = 0.08. They repeated tasks more often than they switched (24.3 % switches, *SD* = 12.2), *t*(29) = -11.56, *p* < .001, *d* = -2.11. A paired *t* test between switch rates of the foreperiod conditions indicated that this repetition bias was stronger for the short foreperiod (short: 22.6 % switches vs. long: 26.1 % switches), *t*(29) = -3.46, *p* = .002, *d* = -0.26.

Most importantly with respect to our research question, a *t* test showed that participants overall chose the compatible task more often than predicted by chance (*M* = 55.2, *SD* = 7.6), *t*(29) =

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[10] Boxplots were created using the SPSS built-in tool with mean RT and ERRs collapsed across all design cells. By default, values deviating more than 3 interquartile ranges from Q3 or Q1 are marked as outliers. Exclusion of these participants had no impact on the statistical pattern of results obtained.

3.75, $p < .001$, $d = 0.69$.[11] This effect was present for the short foreperiod ($M = 61.8$, $SD = 7.6$), $t(29) = 12.13$, $p < .001$, $d = 0.42$, but not the long foreperiod ($M = 48.7$, $SD = 7.61$), $t(29) = 9.02$, $p < .001$, $d = -0.05$, where overall the "short" task was still chosen more often than the "long" task. In a 2 x 2 ANOVA on compatible choice rate with the factors foreperiod and transition (see Fig. 2A), only the main effect of transition was significant, $F(1, 29) = 12.83$, $p = .001$, $\eta_p^2 = .31$. The compatible choice rate was higher on voluntary switch trials as compared to voluntary repetition trials (59.4 % vs. 54.0 %). No other effect reached significance (both *F*s < 2.2; both *p*s > .14).

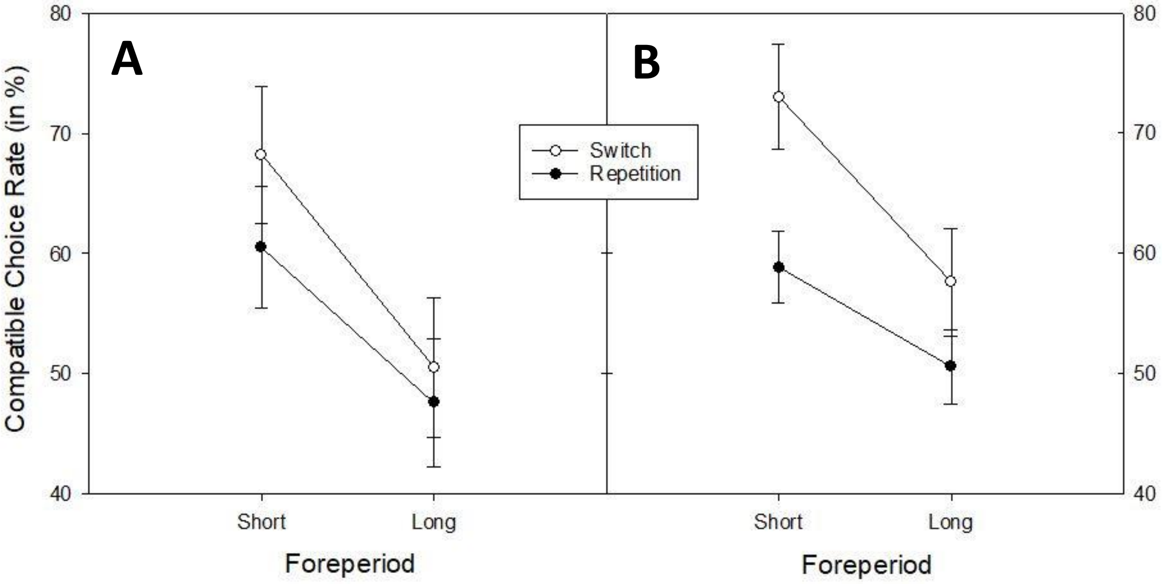

Figure 2. Compatible Choice Rate (in %) in Experiment 1A (panel A) and 1B (panel B) as a function of foreperiod and transition. Error bars represent ± 1 *SEM*.

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[11] In post-hoc analysis, we excluded participants who displayed extreme task biases. A cut-off value of 95 % was chosen based on the distribution of task choice rates, as displayed via histograms: While the majority of data points could best be described by a widespread but rather smooth bell-shaped distribution around the 50 % value with gradually less occurrences the more extreme the task choice, seven participants were clotted at the end points at over 95 % task bias to either task. Note that five of these participants had a task bias toward the "short" task, irrespective of whether it was a number or letter task. When excluding those participants, the "short" task bias was no longer significant ($M = 54.2$, $SD = 19.8$), $t(23) = 1.04$, $p = .311$, $d = 0.21$, while the compatible choice rate still was ($M = 56.3$, $SD = 8.2$), $t(23) = 3.78$, $p < .001$, $d = 0.77$.

Free-choice trials, RTs and ERRs. RTs and ERRs for all conditions are shown in Table 1. The 2 (foreperiod) x 2 (compatibility) x 2 (transition) repeated-measures ANOVA on RTs revealed significant switch costs, $F(1, 21) = 36.59$, $p < .001$, $\eta_p^2 = .64$, further qualified by an interaction of transition and compatibility, $F(1, 21) = 4.35$, $p = .049$, $\eta_p^2 = .17$. The compatibility effect only showed up on repetition trials, with slower repetitions after an incompatible foreperiod. No other effect reached significance (all *F*s < 2.00, all *p*s > .180).

The same analysis on ERRs yielded only a significant effect of compatibility, $F(1, 21) = 5.47$, $p = .029$, $\eta_p^2 = .21$. Furthermore, a marginally significant interaction of compatibility and transition, $F(1, 21) = 3.36$, $p = .081$, $\eta_p^2 = .14$, was found. Compatibility effects were only found for switch trials. All other effects were nonsignificant (all *F*s < 2.00, all *p*s > .170).

Table 1

Mean RTs (in ms) and ERRs (in %) in free-choice trials of Experiment 1A as a function of foreperiod (short vs. long), compatibility (compatible vs. incompatible), and transition (repetition vs. switch)

	Short Foreperiod				**Long Foreperiod**			
	Compatible		**Incompatible**		**Compatible**		**Incompatible**	
	Repetition	**Switch**	**Repetition**	**Switch**	**Repetition**	**Switch**	**Repetition**	**Switch**
RT	674	827	715	841	677	818	704	789
(*SD*)	(*83.4*)	(*140*)	(*89.9*)	(*150*)	(*112*)	(*203*)	(*105*)	(*119*)
ERR	4.24	1.88	3.43	5.50	2.26	1.36	4.34	4.32
(*SD*)	(*3.93*)	(*4.17*)	(*4.16*)	(*8.23*)	(*1.75*)	(*2.44*)	(*4.62*)	(*5.09*)

Forced-choice trials, final test block. In the last forced-choice only block, foreperiod was no longer predictive of the upcoming task. A 2 (foreperiod) x 2 (previous compatibility) x 2 (transition) repeated-measures ANOVA on RTs revealed both a main effect of previous compatibility (676 ms vs. 693 ms for compatible and incompatible trials, respectively), $F(1, 29) = 5.07$, $p = .032$, $\eta_p^2 = .15$, and of transition (633 ms vs. 736 ms), $F(1, 29) = 42.68$, $p < .001$, $\eta_p^2 = .60$. Foreperiod and transition interacted significantly, $F(1, 29) = 4.71$, $p = .038$, $\eta_p^2 = .14$, reflecting larger switch costs at short (117 ms) compared to the long foreperiod (89 ms). Compatibility effects showed a tendency to be larger on repetition trials as compared to switch trials (28 ms vs. 8 ms, respectively), but the interaction between compatibility and transition was only marginally significant, $F(1, 29) = 3.00$, $p = .094$, $\eta_p^2 = .09$.

The analogous ERR ANOVA only showed a significant interaction of compatibility by transition, $F(1, 29) = 8.22$, $p = .008$, $\eta_p^2 = .22$, driven by a large compatibility effect of 2.2 % on repetition, and a descriptively reversed effect of -0.9 % on switch trials.

Discussion

On free-choice trials, participants showed a substantial bias to choose the task compatible with the current foreperiod (i.e., the task associated with the current foreperiod on forced-choice trials). This effect was especially pronounced on switch trials. The compatibility effect found in task choice was mirrored in free-choice performance, with smaller RTs (on repetitions) and lower ERRs for compatible foreperiod-task pairings. In a final forced-choice only test block (without foreperiod-task correlations), we tested whether time-based task expectancy effects on forced-choice task performance could be replicated (Aufschnaiter, Kiesel, Dreisbach et al., 2018; Aufschnaiter, Kiesel, & Thomaschke, 2018) in our task environment: Indeed, task-to-foreperiod pairings that matched previously learned ones resulted in faster RTs and better ERRs.

Interestingly, participants showed a tendency to preferably do the task associated with the short foreperiod – a tendency that was reduced, but still descriptively present, after the long foreperiod. Even though this may very well lie in the mechanics of how time-based task expectancy works (see General Discussion), a single new finding has to be treated with caution. Therefore, before drawing strong conclusions about the generality of this “short” task bias[12], Experiment 1B sought out whether this “short” task bias was also present with an increased ratio of forced-choice trials.

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[12] Here and hereafter we will use the term “short” task bias whenever participants display a bias – that is, a choice rate of > 50 % - toward the task that is associated with the short foreperiod on forced-choice trials. This might either be the number or letter task depending on the particular participant (as

Experiment 1B

In Experiment 1A, we observed that participants were biased to select the task compatible with the presented foreperiod. Interestingly, a general bias toward the task associated with the short foreperiod (a "short" task bias) was also observed. However, this latter effect was primarily present in some participants with very strong task biases. The main aim of Experiment 1B was to investigate whether a "short" task bias would also be present in a setting with a higher ratio of forced-choice trials (75:25). Fröber and Dreisbach (2017) have shown that a larger variability in terms of task choices – and thus, less extreme task biases – can be achieved by this manipulation: When forced to switch tasks frequently, a context of increased cognitive flexibility is established that transfers to the free-choice trials. Furthermore, we expected to again observe an overall preference for the compatible task. Given that a higher ratio of forced-choice trials means a higher ratio of trials where the foreperiod is predictive of the upcoming task, it is very well possible that participants' tendency to select the compatible task will be larger in Experiment 1B as compared to Experiment 1A.

We also included a post-questionnaire to ask participants if they could consciously recall the foreperiod-task association and if so, whether they deliberately used it for their task-choice behavior. If participants become aware of the foreperiod-task contingencies, this may open the way to an explicit, conscious strategy of using these associations for free choice (e.g., Bijleveld, Custers, & Aarts, 2012). Previous studies have shown that specific temporal predictability does not critically depend on awareness (Aufschnaiter, Kiesel, Dreisbach et al., 2018; Aufschnaiter, Kiesel, & Thomaschke, 2018). Even though these findings are not suggestive of it, there is some indication that this might be different under voluntary task switching conditions, given that it involves more intentional, top-down control (Arrington & Logan, 2005).

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foreperiod-task assignments were counterbalanced). In particular, we want to emphasize that it does not necessarily refer to the task for which participants show shorter RTs.

Method

Participants. 32 participants of the Albert Ludwigs University of Freiburg took part in this study (23 female, 31 right-handed, age range = 20-30; *M* = 23.8, *SD* = 2.6) for course credit or money (6 €). All participants signed informed consent prior to the experiment and were naïve with respect to the purpose of the experiment.

Apparatus, stimuli, procedure, and design. Everything was exactly the same as in Experiment 1A except for the following changes. In the test phase of combined free- and forced-choice trials, the ratio of forced- to free-choice trials was now set to 75:25. Additionally, participants filled in a short questionnaire after the experiment asking them whether they were aware of the foreperiod-task contingencies and, if so, whether they used this knowledge for their task-choice behavior.[13]

Results

Data preprocessing. Exclusion criteria were the same as in Experiment 1A and resulted in the removal of 0.8 % of the data (for RT analysis: 11.2 %).

Free-choice trials, task choice. We first checked for general biases in task choice behavior. Participants chose the task associated with the short foreperiod more often than predicted by chance (*M* = 62.4, *SD* = 21.1), *t*(26) = 3.32, *p* = .003, *d* = 0.59. There was no significant bias toward either the number or letter task (bias toward number task: *M* = 51.5, *SD* = 24.5), *t*(31) = 0.36, *p* = .725, *d* = 0.06. Participants repeated tasks more often than they switched (switch rate: 29.7 %, *SD* = 11.3), *t*(31) = -10.18, *p* < .001, *d* = -1.80; this repetition bias did not differ significantly between the short and long foreperiod (short: 28.7 %, long: 30.7 %), *t*(31) = -1.54, *p* = .133, *d* = -0.17.

Most important to our hypothesis, a *t* test again revealed a significant overall task choice bias toward the compatible task (*M* = 55.8, *SD* = 7.8), *t*(31) = 4.19, *p* < .001, *d* = 0.74. This compatible choice bias was significantly present after the short foreperiod (*M* = 68.1, *SD* = 20.5), *t*(31) = 5.00, *p* < .001, *d*

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[13] Of 32 participants, ten reported that they were aware of the foreperiod-task contingencies. Excluding these participants from the task-choice analyses did not alter the statistical pattern of results obtained – in particular, the compatible choice bias was still significant, (*M* = 55.6 %), *t*(21) = 3.58, *p* = .002, *d* = 0.76. Thus, the additional analyses excluding those participants suggests that awareness was not vital for the effects obtained.

= 0.88, and descriptively reversed after the long foreperiod (*M* = 48.5, *SD* = 24.3), *t*(31) = -1.52, *p* = .138, *d* = -0.27. The ANOVA on the compatible choice rate including foreperiod and transition as within-subjects factors revealed both a significant main effect of foreperiod, *F*(1, 31) = 13.15, *p* = .001, η_p^2 = .30, and of transition, *F*(1, 31) = 22.62, *p* < .001, η_p^2 = .43, as well as a marginally significant interaction between the two, *F*(1, 31) = 3.30, *p* = .079, η_p^2 = .10 (see Figure 2B). Again, compatible choice rate was larger on voluntary switch trials as compared to voluntary repetition trials (64.2 % vs. 53.4 %).

We again investigated whether the results on task choice differed when participants with an extreme task bias (participants with a bias toward one of the two tasks of > 95 %[14]; 5 participants) were excluded. Importantly, while the “short” task bias was then no longer significant (55.5 %), *t*(26) = 1.94, *p* = .063, *d* = 0.37, the compatible choice bias remained significant (56.9 %), *t*(26) = 4.52, *p* < .001, *d* = 0.87.

In further analyses, we explored whether the differences in experimental manipulation between Experiment 1A and 1B significantly impacted free choice. However, neither in terms of overall compatible choice rate nor “short” task bias (neither on their own nor in a Duration x Transition ANOVA including the between-subjects factor Experiment) did the two experiments differ significantly (all *p*s > .330).

Free-choice trials, RTs and ERRs. RTs and ERRs for all conditions are shown in Table 2. The ANOVA on RTs revealed the significant main effects compatibility, *F*(1, 22) = 9.55, *p* = .005, η_p^2 = .30, and transition, *F*(1, 22) = 25.98, *p* < .001, η_p^2 = .54, and a marginally significant effect of foreperiod, *F*(1, 22) = 3.93, *p* < .060, η_p^2 = .15. Subjects were faster after a long foreperiod (806 ms vs. 833 ms), showed a compatibility effect (801 ms vs. 838 ms) and switch costs (746 ms vs. 894 ms). None of the interactions was significant (all *F*s < 2.70, all *p*s > .110).

The same analysis on ERRs yielded a significant effect of foreperiod, *F*(1, 23) = 12.08, *p* = .002, η_p^2 = .34. Participants made less errors after a long foreperiod (2.6 % vs. 5.8 %). Similar to Experiment

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[14] Note that similar to Experiment 1A, all of these participants had a task bias toward the “short” task irrespective of whether it was a number or letter task.

1A, a significant interaction of compatibility and transition arose, *F*(1, 23) = 4.37, *p* = .048, η_p^2 = .16. A typical compatibility effect was found only for repetition trials, whereas a reversed effect was found for switch trials. All other effects were not significant (all *F*s < 1.90, all *p*s > .180).

Table 2

Mean RTs (in ms) and ERRs (in %) in free-choice trials of Experiment 1B as a function of foreperiod, compatibility, and transition

	Short Foreperiod				**Long Foreperiod**			
	Compatible		**Incompatible**		**Compatible**		**Incompatible**	
	Repetition	**Switch**	**Repetition**	**Switch**	**Repetition**	**Switch**	**Repetition**	**Switch**
RT	674	827	715	841	677	818	704	789
(*SD*)	(*83.4*)	(*140*)	(*89.9*)	(*150*)	(*112*)	(*203*)	(*105*)	(*119*)
ERR	4.24	1.88	3.43	5.50	2.26	1.36	4.34	4.32
(*SD*)	(*3.93*)	(*4.17*)	(*4.16*)	(*8.23*)	(*1.75*)	(*2.44*)	(*4.62*)	(*5.09*)

Forced-choice trials, final test block. In the last forced-choice only block, foreperiod-task contingencies were no longer valid. In the RT analysis, main effects of previous compatibility, *F*(1, 25) = 20.64, *p* < .001, η_p^2 = .45, transition, *F*(1, 25) = 35.19, *p* < .001, η_p^2 = .59, as well as an interaction of foreperiod and transition, *F*(1, 25) = 13.72, *p* = .001, η_p^2 = .35, were significant. Compatibility in the preceding test phase resulted in an advantage of 39 ms; switch costs were smaller after a long foreperiod, though this was mostly driven by slower RTs on long repetition trials. None of the other effects was significant (all *F*s < 0.40, all *p*s > .540).

ERRs showed only a significant main effect of transition (repetitions: 2.7 % vs. switches: 5.4 %), *F*(1, 31) = 16.49, *p* < .001, η_p^2 = .35, with no other significant effects (all *F*s < 1.40, all *p*s > .250).

Discussion

As in Experiment 1A, there was an overall significant compatible choice bias in Experiment 1B that tended to be stronger on switch compared to repetition trials. Thus, participants adapted their task-choice behavior to the current foreperiod and the task associated with it on forced-choice trials. Not only task choice, but also task performance on free-choice trials were influenced by the foreperiod-task correlations (in RTs, and repetition ERRs). In the last forced-choice only test block (i.e., without foreperiod-task correlations), previous compatibility still affected RTs.

Experiment 1B employed a higher ratio of forced-choice trials than Experiment 1A. Previous research (Fröber et al., 2018; Fröber & Dreisbach, 2017) has indicated that this increases the variability of task-choice behavior as it creates a context of increased cognitive flexibility. The same was true for the current experiments, as the overall VSR increased from 24.3 % in Experiment 1A to 29.7 % in Experiment 1B. Yet in Experiment 1B, extreme task biases occurred with similar frequency as in Experiment 1A and were always directed toward the short foreperiod. Thus, this “short” task bias seems to reflect a systematic effect of temporal predictability on general task preference rather than being caused by a high number of free-choice trials. Additional analyses were run excluding participants with extreme task biases. These analyses rendered the “short” task bias insignificant while the compatible choice bias was still present – suggesting that the former is largely driven by a few participants whereas the latter is not.

Experiment 2

The previous two experiments have established that temporal predictabilities indeed influence voluntary task-choice behavior: Specifically, participants showed an overall bias to select the compatible task, but this compatible choice rate was modulated by a bias to select the task associated with the short foreperiod. An important limitation of the previous experiment is that 100 % contingencies were used. This is rather atypical, particularly within time-event correlation paradigms (Aufschnaiter, Kiesel, Dreisbach et al., 2018; Thomaschke et al., 2011; Wagener & Hoffmann, 2010), and it could have influenced time-based task expectancy processing in a different way than is normally seen in these paradigms. Also, in our everyday environment, contingencies are hardly ever perfect, but rather probabilistic. Thus, the purpose of Experiment 2 was to check whether the current findings would replicate in a task environment with more common contingencies (i.e., 80 %). This also has the advantage that the difference between compatible and incompatible trials in forced-choice trials can be analyzed already within the test block.

Method

Participants. Another 32 participants of the University of Regensburg took part in this experiment for course credit or money (6 €). Of these, 30 were female and 28 were right-handed. They were between 19 and 34 years old (*M* = 22.7, *SD* = 3.4). All participants signed informed consent prior to the experiment, were naïve with respect to the purpose of the experiment, and did not participate in Experiment 1.

Apparatus, stimuli, and procedure. Except for the following changes, everything was exactly the same as in Experiments 1A and 1B. On forced-choice trials, foreperiod-task contingencies were now fixed to 80 %. The learning block was accordingly prolonged to 160 trials. The following test phase consistent of five blocks of 120 trials each; two thirds of the trials were forced-choice trials (again with 80 % foreperiod-task contingency) and one third were free-choice trials. Since the compatibility effect on forced-choice trials could now already be calculated in the test block, the last forced-choice only test block was dropped. As in Experiment 1B, after completing the experiment, participants filled in a short post-questionnaire regarding their awareness of the foreperiod-task contingencies.[15]

Design. The design was analogous to Experiments 1A and 1B except that forced-choice trials in the hybrid test phase could already be analyzed in a 2 (foreperiod) x 2 (compatibility) x 2 (transition) repeated-measures ANOVA.

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[15] Six of all 32 participants indicated that they grew aware of the foreperiod manipulation. The statistical result pattern was basically identical when excluding these participants.

Results

Data preprocessing. Exclusion criteria were the same as in Experiments 1A and 1B and resulted in the removal of 0.8 % of the data (for RT analysis: 10.9 %).

Free-choice trials, task choice. We again first checked for the emergence of general task biases. An overall bias toward the task associated with the short foreperiod was present (*M* = 62.0, *SD* = 20.9), *t*(31) = 3.25, *p* = .003, *d* = 0.58, while no bias toward either number or letter task emerged (bias toward number task: *M* = 54.2, *SD* = 23.9), *t*(31) = 0.99, *p* = .330, *d* = 0.18. A strong repetition bias was found (switch rate: *M* = 25.6, *SD* = 9.14), *t*(31) = -25.62, *p* < .001, *d* = -2.67, which was again larger after a short foreperiod (short: 22.6 % switches, long: 28.7 % switches), *t*(31) = -6.13, *p* < .001, *d* = -0.60.

Crucially, an overall compatible choice bias was again significant (*M* = 54.4, *SD* = 3.99), *t*(31) = 6.30, *p* < .001, *d* = 1.11. It was stronger for the short foreperiod (*M* = 66.4, *SD* = 19.1), *t*(31) = 4.86, *p* < .001, *d* = 0.86, with a tendency toward the incompatible (“short”) task after the long foreperiod (*M* = 42.4, *SD* = 23.3), *t*(31) = -1.85, *p* = .074, *d* = -0.33. The ANOVA with the factors foreperiod and transition revealed a main effect of foreperiod, *F*(1, 31) = 12.92, *p* = .001, η_p^2 = .29, and an interaction of foreperiod and transition, *F*(1, 31) = 6.81, *p* = .014, η_p^2 = .18 (see Figure 3). Compatible choice rate was only significant for the short foreperiod. This effect was even stronger for voluntary switches. Five participants displayed very extreme task biases (> 95 % bias toward one of the two tasks). Irrespective of whether it was a number or letter task, all these participants displayed a bias toward the “short” task. In an analysis excluding these participants, the “short” task bias was no longer significant (*M* = 55.4 %), *t*(26) = 1.85, *p* = .076, *d* = 0.36, while there was still a significant bias to select the compatible task (*M* = 55.3 %), *t*(26) = 7.37, *p* < .001, *d* = 1.42.

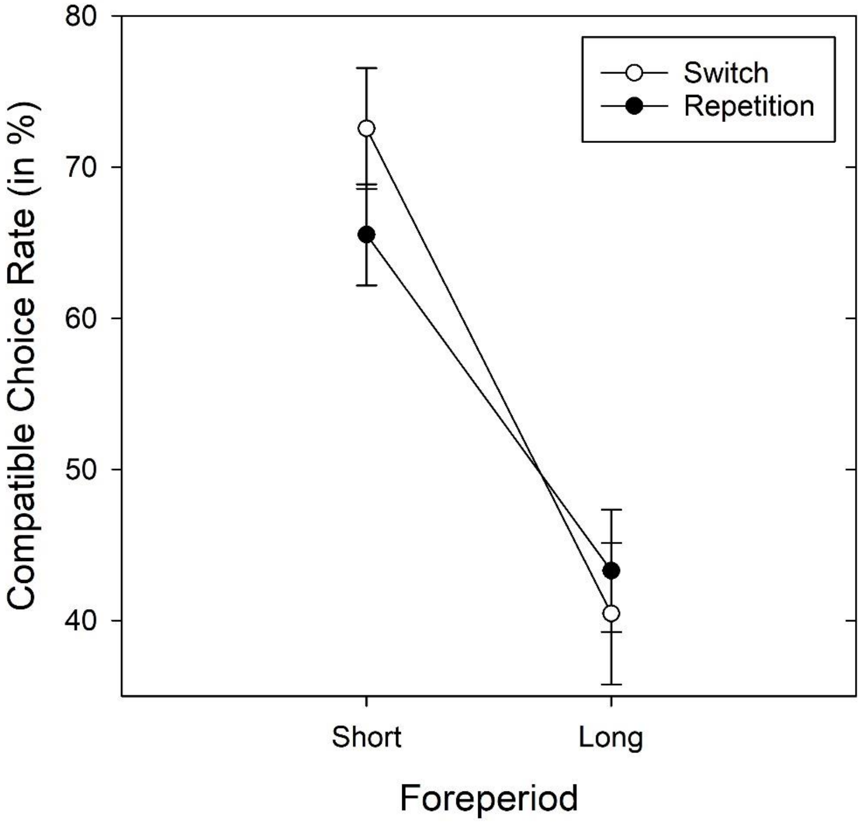

Figure 3. Compatible Choice Rate (in %) in Experiment 2 as a function of foreperiod and transition. Error bars represent ± 1 *SEM*.

Free-choice trials, RTs and ERRs. Mean RTs and ERRs can be found in Table 3. The RT ANOVA yielded only a significant main effect of transition, $F(1, 23) = 34.47$, $p < .001$, $\eta_p^2 = .60$, and a marginally significant main effect of compatibility, $F(1, 23) = 4.07$, $p = .055$, $\eta_p^2 = .15$. Participants showed switch costs of 128 ms and a trend compatibility effect of 24 ms. No other effect reached significance (all *F*s < 1.90, all *p*s > .190).

The analogous ERR analysis revealed a marginally significant main effect of transition, $F(1, 25) = 4.12$, $p = .053$, $\eta_p^2 = .14$, which was further qualified by an interaction with compatibility, $F(1, 25) = 6.41$, $p = .018$, $\eta_p^2 = .20$. A substantial compatibility effect was only found on switch trials. All other effects were nonsignificant (all *F*s < 2.40, all *p*s > .130).

Table 3

Mean RTs (in ms) and ERRs (in %) in free-choice trials of Experiment 2 as a function of foreperiod, compatibility, and transition.

	Short Foreperiod				**Long Foreperiod**			
	Compatible		**Incompatible**		**Compatible**		**Incompatible**	
	Repetition	**Switch**	**Repetition**	**Switch**	**Repetition**	**Switch**	**Repetition**	**Switch**
RT	674	830	725	822	691	800	699	846
(*SD*)	(*120*)	(*226*)	(*169*)	(*234*)	(*121*)	(*209*)	(*135*)	(*240*)
ERR	3.07	3.78	1.94	6.68	4.24	2.80	2.91	9.91
(*SD*)	(*2.74*)	(*7.26*)	(*2.45*)	(*16.3*)	(*4.21*)	(*5.05*)	(*3.43*)	(*20.3*)

Forced-choice trials. Divergent from the previous experiments, incompatible trials were already included in the test phase. Thus, performance on the forced-choice trials in the test phase was analyzed. We conducted Foreperiod x Compatibility x Transition ANOVAs on RTs and ERRs. For RTs, this analysis yielded a main effect of compatibility, *F*(1, 31) = 9.90, *p* = .004, η_p^2 = .24, as compatible trials were on average responded to 14 ms faster than incompatible trials, and a main effect of transition, *F*(1, 31) = 113.10, *p* < .001, η_p^2 = .79, reflecting switch costs of 124 ms. Furthermore, the interaction of foreperiod and transition was significant, *F*(1, 31) = 30.70, *p* < .001, η_p^2 = .50. Smaller switch costs arose after a long foreperiod, both owing to slower repetition RTs as well as faster switch RTs. No other effect was significant (all *F*s < 1.10, all *p*s > .330).

The analogous ERR analysis revealed only a main effect of transition, *F*(1, 31) = 39.13, *p* < .001, η_p^2 = .56, with participants making errors in 5.89 % of all switch trials, but only in 3.29 % of all repetition trials. All other effects were nonsignificant (all *F*s < 0.80, all *p*s > .400).

Discussion

The results of this experiment replicated the findings obtained in the previous two experiments and thus generalize them to a task environment with 80 % foreperiod-task contingencies. In particular, a significant compatible choice bias was found, which was larger for switch trials. Performance on free-choice trials was influenced by compatibility (marginally so in RTs, and only on switches for ERRs). Divergent from the previous two experiments, foreperiod-task contingencies were kept at 80 % (not 100 %), thus allowing to compare compatible and incompatible forced-choice trials already in the hybrid task switching phase. This comparison yielded significant RT compatibility effects, extending typical time-based task expectancy effects on forced-choice trials to the free-choice context (Aufschnaiter, Kiesel, Dreisbach et al., 2018; Aufschnaiter, Kiesel, & Thomaschke, 2018).

Thus, Experiment 2 corroborates the finding of the previous experiments: A robust compatible choice bias was found, but was limited to (or stronger for) the short foreperiod. The compatible choice rate is larger for task switches. And overall, participants display a bias toward the task associated with the short foreperiod. This bias seems to be largely driven by a few participants with very strong task preferences.

General Discussion

In three experiments, we investigated whether task-choice behavior is biased by temporal cues in a temporally structured voluntary task environment. Specifically, in a task switching setting forced-choice and free-choice trials were randomly intermixed (hybrid design) and the task in forced-choice trials was predicted by the foreperiod (Exp. 1A and 1B: 100 % predictability; Exp. 2: 80 % predictability). Our main interest was whether the randomly interspersed short and long foreperiods in free-choice trials biased task choice. Across all experiments, we found that compatible foreperiod-task combinations were chosen more often than incompatible ones. In the first two experiments, this effect was stronger on task-switch trials (in the last experiment, this transition-dependent dissociation was only found for the short foreperiod). Furthermore, over all experiments a compatible choice bias was only significant for the short foreperiod. Finally, temporal predictability also affected general task preferences: Overall, participants were biased toward the task which was associated with the short foreperiod on forced-choice trials.

Task-choice behavior and temporal preparation: A preparation-switch account

The present experiments provide first evidence that time-based task expectancy effects can not only be found for task performance, but also for task choice. As reviewed in the introduction, temporal predictability may influence task performance in forced-choice settings due to time-dependent changes in task preparation. In a similar vein, task-choice behavior might also be influenced by these changes in task preparation: Specifically, we suggest that task selection in our experiments utilized bottom-up biases as introduced by time-based task expectancy. Throughout the time course of the foreperiod, the preparatory activation level for either task changed as a function of temporal predictability, which further translated to task selection (Arrington, 2008).

On a more functional level, this suggests that temporal predictability does not only influence the course of (active) task preparation, but also of task selection processes. In studies on voluntary task switching, these two processes have been found to be distinguishable (Arrington et al., 2014) – but in many cases task preparation informs task selection (Arrington & Logan, 2004; Mittelstädt et al., 2018). On the one hand, while task preparation is mainly reflected in performance indices like the switch costs (in RTs and ERRs), task selection processes are marked by task choice indices like the VSR and task bias – and correlations between these markers have been found to be rather small (Arrington & Logan, 2005). Also, substantial interindividual differences emerge in terms of how much task selection is driven by exogenous factors (Arrington & Yates, 2009; Orr & Weissman, 2011), supporting a view of (partially) diverting processing streams. However, our results indicate that task selection behavior incorporates the temporal predictabilities of tasks in forced-choice trials. This would also fit with the study by Mittelstädt et al. (2018) in which participants used the predictable waiting time for a repetition stimulus when deciding to switch or repeat tasks. A number of studies further substantiate the claim that contextual factors exert a huge effect on voluntary task choice (e.g., preparation time, stimulus repetitions, task difficulty; Arrington & Logan, 2004; Mayr & Bell, 2006; Yeung, 2010). We suggest that the same may be true for the temporal predictability effects we found here. In terms of the selection process, Herbort and Rosenbaum's (2014) as well as Volberg and Thomaschke's (2017) studies suggest that action selection – in this case, preparing the response rule and/or response hand associated with one task (Demanet & Liefooghe, 2014) – is biased by the foreperiod-task contingencies, favoring compatible task choices.

This would also fit with our finding of a significant "short" task bias, that is, a bias toward the task associated with the short foreperiod. Even though this effect was considerably reduced when excluding participants with very strong task biases (that is, with less variability in their task-choice behavior), the compatible choice bias still was larger after the short foreperiod. In our view, the emergence of time-based task expectancy effects requires that participants first prepare for one task, and then switch to preparing for the other – it is highly likely that they fail more often to do so after the long foreperiod (see also de Jong, 2000). This idea is in accordance with other findings of the literature. First, Pfeuffer et al. (under revision) provide evidence that the frequency of anticipatory eye movements to the temporally predicted location (and task) is larger for a short compared to a long foreperiod. Second, Volberg and Thomaschke (2017) show that preparatory activity related to a certain temporally expected effector switches roughly at the expected end point of the short duration. Furthermore, Aufschnaiter, Kiesel, Dreisbach et al. (2018) found an RT benefit for the short compared to the long foreperiod in a temporally structured task environment. Relatedly, it is a well-known fact that participants tend to avoid switching tasks (Kessler et al., 2009), at least partially because of the effortful cognitive operations (e.g., reconfiguration of task sets) needed to implement those switches (Kool, McGuire, Rosen, & Botvinick, 2010). The same may be true for temporally predictable tasks, where a switch in preparation after the short foreperiod passed may equally be avoided.

Effects of foreperiod length can also be found in the variable foreperiod paradigm mentioned in the introduction (e.g., Steinborn et al., 2008), where sequential modulations can only be found with a current short foreperiod (Los & van den Heuvel, 2001). Steinborn and colleagues (Steinborn et al., 2009, 2010) used varying warning stimulus modalities or features to show that this sequential modulation was largely reduced when modality (or sufficiently distinct features within one modality) shifted, further providing evidence that short-foreperiod trials are influenced by preceding trials in a way that long-foreperiod trials are not: After-effects of the previous trial, e.g., its reinforced time point of peak readiness, thus seem to be limited to comparably short foreperiods. Other processes, such as conceptually driven, more intentional preparation processes may prevail for longer foreperiods (Langner et al., 2018).

Similar effects can be seen in the task-switching domain, where interference from the previous task set is largest with no or very short preparation intervals (Meiran, 1996). Consequently, task switching performance as well as voluntary switch rates increase with more time between trials (Arrington & Logan, 2004; Monsell & Mizon, 2006). In our paradigm, stronger compatibility effects were obtained for the short foreperiod and also the task associated with it. Thus, one could assume that while task selection of short-foreperiod trials depends on availability biases induced by the foreperiod-task contingency manipulation, any such biases are reduced the more time passes. Hence, the “short” task bias is a temporal bias in effect propagation of foreperiod compatibility: The compatibility bias may simply fade with time or become noisier (as a sort of passive decay, cf. Meiran, 1996) or may sometimes be overruled by other factors impacting task choice (Arrington & Logan, 2005; Langner et al., 2018).

The present results could also be interpreted in terms of an episodic-retrieval account (Hommel, 2004; Los et al., 2014; Mayr & Bell, 2006; Thomaschke & Dreisbach, 2015). According to this account, a binding between the current foreperiod and the task is established on each trial, that carries over to the next trial(s): Repetition of the current foreperiod automatically retrieves the task that was associated with it in the previous trial. Given that foreperiods and tasks in forced-choice trials were highly correlated, task-choice behavior in a following free-choice trial could simply reflect such an automatic retrieval of previous foreperiod-task bindings (for a similar argument see Los et al., 2014) – thus most likely results in a compatible choice. However, this account would predict more compatible choice repetitions than switches (cf., Los et al., 2014; Mayr & Bell, 2006), which we did not find in the current results. Nevertheless, episodic retrieval is suggested to be an aiding factor during learning of foreperiod-task associations (Thomaschke & Dreisbach, 2015) and could influence task choice in the current paradigm in addition to learned foreperiod-task associations. Future studies should directly test this account by contrasting trials where both foreperiod and task repeat or switch with trials where only one repeats.

Temporal predictability effects on (voluntary) task performance

Importantly, we also found time-based task expectancy effects on task performance. RTs and, somewhat attenuated, ERRs on free-choice trials mirrored the advantage of compatible task choice. Repetition and switch trials seemed to profit in a similar way by compatibility; or at least, a clear dissociation is not possible with the current results (Exp. 1A: no effect for switch RTs; first two experiments: no effect for switch ERRs; Exp. 2: no effect for repetition ERRs). The similarity between task choice and task performance results fits well with the preparation-switch account we introduced earlier: Time-based task expectancy acts on task preparation processes (as reflected in task performance indices) that inform task selection (reflected in task-choice indices). However, the face validity of the performance-choice similarities has to be treated with caution – causal attributions cannot be made so far and further investigation is needed to corroborate this claim.

In a last 100 % forced-choice test block in Exp. 1A and 1B, we checked whether effects of compatibility as established in the previous test phase would transfer to a block where foreperiod-task combinations were completely random. Here, we could show that indeed previously established foreperiod-task combinations were responded to faster and (only in the case of repetitions in Exp. 1A) more accurately. This reflects a replication and extension of the findings by Aufschnaiter, Kiesel, and Thomaschke (2018), who showed for forced-choice task switching that time-based task expectancy survives a change in absolute time environment. The current results extend these findings to a free-choice context.

Implications for future research

Using different or a larger variety of foreperiods may be interesting with respect to the “short” task bias. Given that preparation in voluntary task switching as well as in foreperiod and interval timing studies is known to be successful only after some hundred milliseconds have passed, the question arises whether the “short” task bias would still be found if the short foreperiod was considerably shortened (e.g., to 100 ms). In this case, the default may rather be to start preparation only after this short foreperiod has passed – given that only then task preparation (Kiesel et al., 2010) and accurate timing (Lewis & Miall, 2009) is possible. Furthermore, previous research on the variable foreperiod paradigm (Langner et al., 2018; Steinborn et al., 2009, 2010) has shown that the short-foreperiod "bias" in terms of susceptibility to sequential modulations can be largely increased when using more than two foreperiods and a greater temporal range. In an experimental setup that was optimized for revealing differential effects of foreperiod length, Langner et al.'s (2018) Experiment 2 employed foreperiods of 800 ms, 1600 ms, and 2400 ms. Note that using more than two foreperiods also means that participants have to not only learn two foreperiod-task associations, but three. While increasing the overall task demand, this would also allow to investigate other task-switching phenomena in the context of temporal predictability, such as backward inhibition (Koch, Gade, Schuch, & Philipp, 2010).

This methodological approach may be informative in more than one respect: It may also allow to examine whether the effect of temporal predictability on task-choice behavior is based on relative or absolute timing. Previous studies on time-based expectancy effects on performance seem to prompt the relative timing idea (Aufschnaiter, Kiesel, & Thomaschke, 2018; Thomaschke, Kunchulia, & Dreisbach, 2015). That is, participants learn that one event appears after the interval that is *relatively* shorter/longer than another interval, rather than learning the correlation between the exact time period and the event. If we think of the example at the beginning of this chapter of waiting for an unpunctual friend, relative timing information seems sufficient for changing the anticipated course of action: The more time passes, the likelier it is that the person will not show up and I will have to act accordingly. Aufschnaiter, Kiesel, and Thomaschke (2018) make the legitimate claim that finding time-based task expectancy to only involve relative timing information may be due to the experimental design involving only two foreperiods. Given that many real-life scenarios require absolute timing (e.g., a pilot operating in a cockpit), it may be a fruitful endeavor to see whether time-based task expectancy effects, particularly on task-choice behavior, can be shifted toward absolute timing.

Conclusion

The present experiments show for the first time that time-based task expectancies influence free choice. The current research extends findings which showed that contingencies between onset latency and tasks can be learned (Aufschnaiter, Kiesel, Dreisbach et al., 2018; Aufschnaiter, Kiesel, & Thomaschke, 2018) and will influence performance, by extending the findings to a free-choice environment: Both for choice and performance parameters, an advantage for predicted foreperiod-task combinations was found. The fact that these effects were more pronounced for the short foreperiod suggests that participants started out each trial with preparing for the task associated with the short foreperiod and, if it passed without a target appearing, were not always able to switch preparation to the other task. On a more general level, the current research adds to existing findings demonstrating contextual influences on voluntary task switching (Mayr & Bell, 2006; Mittelstädt et al., 2018; Yeung, 2010).

PART III – DISCUSSION

The present PhD project investigated the impact of different contextual variables (forced-choice context, reward prospect, temporal predictability) on the expenditure of cognitive control in a multitasking setting. At the same time, voluntary- and forced-choice conditions were compared. The current studies can be integrated into the growing research perspective that goal-directed behavior is in large part dictated by the same associative principles as more "automatic" behavior and hence more reliant on contextual information than previously assumed. Indeed, we found evidence that voluntary task choice is influenced by the forced-choice context, block-wise manipulated reward prospect, and learned time-based task expectancies in predictable ways: A high forced switch rate induces flexibility, whereas unchanged high reward prospect induces stability, as indicated in an increased or reduced VSR, respectively. Participants selected tasks that were compatible with learned temporal contingencies more often than incompatible ones, particularly on switches. At the same time, we found moderate evidence that irrelevant cue transitions do not impact task choice, reflecting a possible limit to contextual influences on voluntary task switching.

Summary of Main Findings

I started from the premise that multitasking is difficult and that the associated costs are avoided whenever possible (Kool et al., 2010), making it a vital research aim to identify factors and situations that systematically increase the ability to flexibly switch between tasks. To operationalize cognitive flexibility, we made use of the task switching paradigm which either requires participants to switch between or repeat tasks (forced task switching) or provides them with the opportunity to decide for themselves which task they want to perform on each given trial (voluntary task switching) (Grange & Houghton, 2014; Kiesel et al., 2010; Monsell, 2003; Vandierendonck et al., 2010). As we were primarily interested in how contextual information modulates self-chosen flexibility, the main dependent variable of interest was the voluntary switch rate, that is, the percentage of trials in which subjects chose to switch between tasks. Before turning to an overarching integration and a theoretical discussion of the current findings, I will first give a brief summary of the studies carried out: What are the questions being raised and how are they answered by the current research?

In **STUDY 1**, we examined ERPs locked to a task or task choice cue (on forced or voluntary task switching trials, respectively) preceding the stimulus by 1000 ms. An early P3b-like posterior positivity was particularly enhanced on rare voluntary switches. This electrophysiological index has widely been associated with the updating of working memory in the presence of new, unpredicted, or surprising contextual events (Barceló et al., 2007; Donchin & Coles, 1988; Goffaux, Phillips, Sinai, & Pushkar, 2006). That is, this component can be interpreted as early signs of a decision process (Forstmann et al., 2007; Vandamme et al., 2010) that is only necessary on voluntary trials as well as the associated need for task-set updating (Nessler et al., 2012). In addition, we found evidence that a switch-related posterior positivity is present in comparable way for both forced and voluntary task switching. That is, a similar switch-related preparation process is involved in either task switching type, irrespective of whether the choice to switch is made by the participant or the experimenter. This finding speaks against accounts arguing that forced task switching merely reflects cue-driven retrieval of a task set (Logan & Bundesen, 2003; D. W. Schneider & Logan, 2011), but instead, a common process of task-set reconfiguration may be necessary for switch-specific preparation irrespective of whether the decision to switch is under participant's or experimenter's control (Masson & Carruthers, 2014).

In the two experiments of **STUDY 2**, blocks associated with a high reward prospect of 7 points yielded a significantly smaller number of voluntary switches than blocks with no or low reward prospect. In contrast, we found moderate evidence against any effect of irrelevant color-cue transitions, neither as a main effect nor in interaction with reward prospect. Cue transitions did have an effect on performance, however: Subjects displayed a tendency to respond faster after cue repetitions (in Experiment 1), and made more errors after cue switches (in Experiment 2). This result is noteworthy because it shows that motivationally relevant information may be incorporated in the decision process on voluntary trials, whereas irrelevant information (even though not ignored, as shown by the effects on task performance) is not. Moreover, the current results are in line with previous findings of a stabilizing effect of high performance-contingent reward prospect (Botvinick & Braver, 2015; Dreisbach & Fröber, 2019; Goschke & Bolte, 2014), even if reward prospect stays on a constant level for extended periods of time. A possible mechanism for this effect may lie in the modulation of the updating threshold in working memory by reward prospect, as will be discussed below.

STUDY 3 employed a hybrid task switching design where on forced trials, the foreperiod (time between trial onset and stimulus onset) was predictive of the upcoming task. The resulting time-based task expectancies significantly influenced task choice on voluntary trials: Tasks compatible with the current foreperiod were chosen more often than incompatible tasks. Particularly after the short foreperiod of 500 ms and on switches, these time-based expectancies exerted an influence on voluntary task choice. The learned time-task associations presumably led to biases in task preparation that influenced task selection processes (cf. Mittelstädt et al., 2018). Such an expectancy-based preparatory process is particularly evident for the relatively shorter foreperiod (Aufschnaiter, Kiesel, Dreisbach et al., 2018; Langner et al., 2018): If participants start out a trial expecting – and hence preparing – one task, they may not always switch preparation to the other task when the shorter foreperiod passes and the long foreperiod ensues.

Overarching Integration: How can Cognitive Flexibility be Modulated?

The theoretical stance adopted here is the stability-flexibility balance of cognitive control (Goschke, 2003, 2013; Hommel, 2015): To act in a goal-directed manner, cognitive control processes are acting on a stability-flexibility balance that should adapt swiftly and appropriately to changing task demands or goals. Whenever a change in task or goal is required or doing the current task is no longer adaptive, a flexible shift is indicated. At the same time, if a suitable cognitive control mode – that is, a certain bias on the stability-flexibility axis – is adopted, it should be maintained for longer periods of time as long as appropriate and rewarding. In multitasking situations, then, a flexible cognitive control mode ensures the swift and successful switch from one task to the next. If a task is repeated or executed for longer periods of time, a stable cognitive control mode is indicated.

Shifts on the stability-flexibility axis are not only subject to goal-driven influences, but also subject to contextual effects as evidenced in the current studies. Indeed, the stability-flexibility balance of cognitive control seems to be highly sensitive to internal and external context factors. This adaptability not only to current goals but also to the demands and associative qualities of the task environment has been noted to be a key principle of the cognitive control system (Abrahamse et al., 2016; Braem & Egner, 2018). Similar effects regarding the modulation of the stability-flexibility balance have been found for other manipulations of the reward prospect (e.g., Braem, 2017; Fröber & Dreisbach, 2016b; Hefer & Dreisbach, 2017), conflict-associated contexts and items (Chiu & Egner, 2017; Crump & Logan, 2010; Dreisbach, Reindl, & Fischer, 2018), and affect (Dreisbach & Goschke, 2004; Fröber & Dreisbach, 2012, 2014). Cues, locations, or stimuli selectively associated with a high switch probability yield smaller switch costs compared to contexts that possess a low switch probability (Chiu & Egner, 2017; Dreisbach et al., 2002; Dreisbach & Haider, 2006; Nessler et al., 2012). Very recent research indicates that even single items associated with a high switch probability result in an increased voluntary switch rate (Chiu, Fröber, & Egner, 2020). If you think of the Introduction, I have referenced the “old” picture of a dichotomy between controlled vs. automatic behavior. This idea is more and more replaced by the assumption that there is no such dichotomy, but rather that control processes are more or less involved depending on the familiarity, recency, and difficulty of particular tasks or S-R relationships (Hommel, 2015; Yeung, 2010), but also the interference caused by other information in the environment (Goschke, 2013). Thus, cognitive stability and flexibility are to be understood as mostly context-specific in nature (Braem & Egner, 2018).

What mechanisms bring about a context-driven shift on the stability-flexibility balance? Dreisbach and Fröber (2019) suggest that modulations of the stability-flexibility balance by contextual information may either come from the extent to which multiple tasks are held active in working memory, or from shifts in the updating threshold in working memory. That is, working memory seems to be the central functional hub for cognitive control modulations in the context of task switching (Oberauer et al., 2013; Schmidt & Liefooghe, 2016). Because it is generally capacity-limited (presumably because maintaining multiple bindings at the same time leads to interference; Oberauer et al., 2013), task representations compete for access to working memory. Therefore, only if the situation or the current goal calls for it, one or multiple tasks are held active (Dreisbach & Haider, 2008, 2009).

Let us first take a closer look at the first assumed mechanism, that is, the extent to which tasks are held active in working memory. Fröber and Dreisbach (2019) suggest that the effect of the forced-choice context may be due to this mechanism. In high-switch blocks, participants make the frequent experience of having to switch between tasks – therefore, task uncertainty is quite high. Hence, keeping both tasks in a somewhat activated or readily available state helps to manage those frequent switches successfully, even though it goes along with a higher distractibility on repetition trials (Dreisbach & Wenke, 2011; Dreisbach & Haider, 2008, 2009; Dreisbach, 2012). In turn, not only forced, but also voluntary switching is facilitated, because not only the most recently performed task is easily accessible through carryover activation (cf. Arrington & Logan, 2005), but also the other task. In contrast, in blocks of rare forced switches, voluntary switching equally poses higher task-set updating demands on working memory. In Study 1, we indeed found that rare voluntary switches elicited an elevated early P3b-like posterior positivity, which has been interpreted as an index of task-set updating (Karayanidis & Jamadar, 2014; Nessler et al., 2012). This frequency-specific effect can therefore be seen as one piece of evidence regarding the task-uncertainty account proposed by Fröber and Dreisbach (2019)

Here, it should be noted that task-set updating can be distinguished from task-set reconfiguration. Task-set updating refers to the updating of task rules in working memory – and therefore is a process that is engaged to varying degrees, depending on switch demands in the current context or trial (e.g., Kessler, 2017; Rac-Lubashevski & Kessler, 2016; Dreisbach & Haider, 2008, 2009; Dreisbach et al., 2002). It is also suggested that task-set updating occurs to some degree even on task repetitions (Altmann & Gray, 2008; Dreisbach et al., 2002; Meiran, 2000). Task-set reconfiguration on the other hand refers to the adjustments made in attentional, memory- and response-related networks in accordance with the shift in task demands on task-switch trials (Demanet & Liefooghe, 2014; Meiran, Chorev, & Sapir, 2000). In Study 1, we found evidence that task-set reconfiguration –

reflected in the switch positivity – is equally involved in voluntary and forced task switches, both in blocks of high and low switch demands.

The second mechanism proposed by Dreisbach and Fröber (2019) is a modulation of the updating threshold. This parameter refers to the probability that information is updated in working memory, as it modulates the stability of attractor states in working memory, that is, the extent to which activation patterns are sustained for longer periods of time (cf. Goschke & Bolte, 2014). Note that this mechanism is rather unspecific: A lower updating threshold means that just about any salient or motivationally relevant information may become the content of working memory. Dreisbach and Fröber (2019) suggest that affect- and reward-based modulations of the stability-flexibility balance are due to this mechanism. Goschke and Bolte (2014) point out that flexibility-enhancing motivational or affective conditions (positive affect, non-contingent, low, or no reward) should serve as signals for exploration, either because they serve as a safety signal (non-contingent reward or positive affect; Fröber & Dreisbach, 2014; Müller et al., 2007) or because they indicate that higher reward may be obtainable (low or changing reward prospect; Aston-Jones & Cohen, 2005; Shen & Chun, 2011). Therefore, these conditions should naturally be characterized by unspecific and domain-general flexibility. In contrast, high (performance-contingent) reward prospect plays a prime role in motivating the sustained recruitment of effortful control (Botvinick & Braver, 2015; Muraven & Slessareva, 2003). Hence, it goes along with increased task shielding, that is, a rather high updating threshold. This can be seen in Study 2, were blocks of high reward prospect were associated with a particularly low voluntary switch rate. Importantly, reward context did not influence the extent to which uninformative and motivationally irrelevant cues impact performance – hence, even a lower updating threshold (as presumably present in low- or no-reward blocks) does not mean that such irrelevant information impacts task choice.

Combining Different Contextual Manipulations

It should be noted that other researchers (e.g., Goschke, 2013; Hommel, 2015) also bring into play other mechanisms regarding the modulation of the stability-flexibility balance. Hommel (2015) differentiates between a competitor-receptivity and a goal-biasing mechanism: Both the extent to which active representations in working memory mutually inhibit each other as well as the extent to which currently goal-relevant representations are maintained offer viable explanations for modulations of the stability-flexibility balance. Goschke (2013) on the other hand proposes a list of so-called meta-control parameters that are adjusted depending on the current situational demands and goals, such as the attentional breadth or the learning rate. It is currently not conclusive whether these proposed mechanisms complement each other or have to be viewed as partly interchangeable.

Notably, contextual factors seem to work on different time scales, and therefore open up the possibility that the stability-flexibility balance may rely on several mechanisms: Different control states can either be changed on a local trial-wise basis (simple task transition effects, time-based expectancy, sequential reward effect), but also as part of a global adjustment (forced switch rate, unchanged reward prospect) (Dreisbach & Fröber, 2019). As several dopaminergic and noradrenergic connections between the brainstem, the basal ganglia, and the PFC (see Introduction) seem to be involved in the interplay of flexibility and stability, the possibility is opened up that several mechanisms work concurrently.

By combining several contextual manipulations within one study, one can determine whether two variables exert their stability- or flexibility-inducing effect via shared or different mechanisms, and interact in an additive, underadditive, or overadditive way. For example, by combining a working memory gating paradigm with task switching, Kessler (2017) and Rac-Lubashevsky and Kessler (2016) were able to dissociate gate opening from task switching costs: They could show that the cost of gate opening was eliminated in switch trials, hence suggesting parallel processes. Also, for many variables, one effect will overrule the other, such as reward and affective manipulations (Fröber & Dreisbach, 2012, 2014): The transient stability-inducing effect of performance-contingent reward usually seems to override the more long-lasting flexibility-enhancing effect of positive mood induction.

More relevant to the current investigations, Fröber et al. (2018) simultaneously manipulated reward history (to investigate the sequential reward effect) and the forced-choice context (that is, the ratio of forced-choice to free-choice trials). The ratio of forced-choice to free-choice trials resulted in a main effect, in that more forced-choice trials resulted in a high voluntary switch rate. For the reward sequence, though, an interaction with forced-choice context was observed: In a context of global stability (induced by a context of few forced-choice trials), only an increase in reward prospect induced a higher voluntary switch rate – the flexibility-inducing effect of decreasing reward prospect could not be observed. However, the typical sequential reward effect of higher flexibility (more voluntary switches) with both an increase and a decrease in reward prospect, accompanied by a selectively low voluntary switch rate with unchanged high reward prospect was observed as soon as the number of a forced-choice trials was high enough, hence, in a context of global flexibility. The authors thus concluded that reward prospect results in temporally distinct, short-term effects that depend on the global context.

Another explanation that was entertained by the authors is a motivational one: An increase in reward prospect results in an increase in motivation which is not the case for a decrease in reward prospect. Hence, in three follow-up experiments (Jurczyk et al., 2018), we have combined a sequential reward manipulation with voluntary and forced task switching between tasks of varying difficulty. In this context, switching to the other task does not only depend on flexibility, but, in the case of a switch to the more difficult task, also on the willingness to choose a more demanding option. In theoretical accounts on motivational arousal or effort expenditure (Brehm & Self, 1989; Gendolla & Richter, 2010; Shenhav et al., 2013), it has been shown that motivational arousal (that is, the relative increase in motivation) is a function of task difficulty. This relationship may be bidirectional: Eisenberger (1992) showed that subjects can learn to associate higher task difficulty and the associated higher effort expenditure with higher reward. Therefore, we predicted that an increasing reward prospect should specifically increase the voluntary switch rate to the more difficult task, which was confirmed across all three experiments: Even though participants largely refrained from choosing the difficult task voluntarily, an increase in reward prospect was associated with a selective increase in switches to the more difficult task.

Therefore, variables influencing global biases on the stability-flexibility balance (forced-choice or reward context) interact with factors inducing local, trial-wise biases (reward prospect of the current trial, time-based task expectancy). For increasing the ecological validity of cognitive research, it is not only important to investigate voluntary choice, but it is also crucial to increase the degrees of freedom in other ways – for example by combining variables that induce the short-term and long-term biases on the stability-flexibility balance – to increase similarity to real-life scenarios. Future research will have to disentangle how compatible different contextual manipulations are, over and above the mentioned conjoint manipulation of reward prospect and forced-choice context or task difficulty.

CONCLUSION

The current studies replicated and extended the finding that self-chosen cognitive flexibility can be modulated by various contextual manipulations that range from motivational factors such as reward prospect over the manipulation of the forced-choice context to variations of temporal expectancy. It was shown that forced switch rate had a marked influence on voluntary switch rate even in the course of a few trials. Further, the current findings indicated that reward prospect can have a marked stabilizing influence on voluntary switch rate even for extended periods of time. Time-based task expectancies lead to more time-task compatible task choices, particularly on voluntary switches. A limit to these contextual effects may be represented by purely bottom-up driven effects like irrelevant color-cue transitions. Hence, it can be concluded that even though mostly foregone, self-chosen cognitive flexibility can be facilitated by various contextual manipulations.

On a theoretical basis, the present results indicate that the stability-flexibility balance of cognitive control is influenced by several factors and thus may reflect the interplay of several so-called meta-control parameters, most importantly the updating threshold in working memory and the extent to which several tasks are held active in working memory. With regard to the underlying mechanisms, we have found evidence that a common switch-related preparation process – arguably reflecting task-set reconfiguration – can be found in forced and voluntary task switching, whereas a shared general preparatory process (possibly reflecting a task-set updating mechanism) awaits investigation.

In sum, the current results can be viewed as an outlet to the cost of multitasking: If chosen appropriately, contextual information can aid cognitive flexibility – hence maybe breaking the spell on “the myth of multitasking”.

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