Decomposing Task-Switching Costs With the Diffusion Model

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In four experiments, task-switching processes were investigated with variants of the alternating runs paradigm and the explicit cueing paradigm. The classical diffusion model for binary decisions (Ratcliff, 1978) was used to dissociate different components of task-switching costs. Findings can be reconciled with the view that task-switching processes take place in successive phases as postulated by multiple-components models of task switching (e.g., Mayr & Kliewg, 2003; Ruthruff, Remington, & Johnston, 2001). At an earlier phase, task-set reconfiguration (Rogers & Monsell, 1995) or cue-encoding (Schneider & Logan, 2005) takes place, at a later phase, the response is selected in accord with constraints set in the first phase. Inertia effects (Allport, Styles, & Hsieh, 1994; Allport & Wylie, 2000) were shown to affect this later stage. Additionally, findings support the notion that response caution contributes to both global as well as to local switching costs when task switches are predictable.

Keywords: task switching, reconfiguration, inertia, diffusion model, speed-accuracy trade-off

One of the most important abilities of humans is to adjust to new situations and to switch between different demands. However, people usually find it hard to shift from one mental set to another one. In the experimental context, requiring participants to switch between tasks is usually accompanied by longer latencies and more errors, as compared with task repetitions. Differences in performance between task-switch trials and task-repeat trials within the same task-switching block have been classically referred to as local switching costs (as opposed to global switching costs that denote differences between task-switching blocks and task-pure blocks). However, differences between task-switch trials and task-repeat trials can be accounted for by either task-switch costs, that is, particularly low performance in task-switch trials, or by task-repetition benefits, that is, particularly high performance in task repeat trials.

In the following, we will first address classical accounts of task switching costs, including task-set reconfiguration (Rogers & Monsell, 1995) and task-set inertia (Allport, Styles, & Hsieh, 1994; Allport & Wylie, 2000). Next, multiple-components models of task-switching will be considered that postulate that controlled as well as automatic processes contribute to task-switching costs at different phases of a task switch. Also repetition benefits (e.g., Schneider & Logan, 2005) will be addressed as an explanation of the observed local switching effect. We will then describe a way to model task-switching data with the classical diffusion model (Ratcliff, 1978) which may be beneficial to tease apart some of the processes contributing to different phases of a task switch. Subsequently, we will describe findings from a series of experiments in which the model was tested with different switching paradigms, including the classical alternating runs paradigm as well as variants of the explicit cueing paradigm.

Task Switching

Task Sets and Switches

A mental set has been defined as “a hypothetical, representational device that enables coherence of action over time in the face of interfering stimuli as well as flexibility of action when external or internal demands change” (Mayr, 2003, p. 223). In the domain of simple classification tasks, the narrowest functional understanding of task set is a set of stimulus-response (S-R) mapping rules. But even in the experimental context, task sets are considered to comprise a number of components, including “which locus to attend to, which attribute of the stimulus to attend to, which response mode and values to get ready, what classification of the relevant stimulus attribute to perform, how to map those classes to response values, with what degree of caution to set one’s criterion for response” (Monsell, Yeung, & Azuma, 2000, p. 252).

The demand of switching between two tasks is clearly dissociable from the primary task difficulty of the individual tasks (Allport et al., 1994; Gopher, Armony, & Greenshpan, 2000; Mayr & Kliewg, 2000; Rubinstein, Meyer, & Evans, 2001). Set-shifting has therefore been considered an executive control function (Baddeley, Chincotta, & Adlam, 2001). Additionally, this function has been shown to be separable from other components of executive control (Miyake, Friedman, Emerson, Witzki, & Howeter, 2000).

Accounts that explain the difference in performance between task-switch and task-repeat trials as reflecting switching costs point at either effortful preparation of the new task set or proactive interference of a competing task set. Especially two theories have strongly influenced theorizing since the mid 1990s. The task-set...
reconfiguration account (TSR; Rogers & Monsell, 1995) proposes that switching costs reflect the time it takes to prepare for the next task. The account emphasizes executive control and the difficulty of switching to the new task. In contrast, the task-set inertia account (TSI; Allport et al., 1994; Allport & Wylie, 2000) postulates that proactive interference arising from the processing of a prior competing task impairs the execution of the intended response. As the theory emphasizes stimulus-driven or low-level processes, it stresses the difficulty of switching away from the previous task. Both views have gained considerable support in the task-switching literature.

TSR

According to the classical TSR account (Rogers & Monsell, 1995), it is assumed that in task-switch trials the new task set must be implemented first, before stimulus classification can proceed (see also Meiran, 1996; Rubinstein et al., 2001). Theories assuming an additional processing phase (or phases) inserted on task-switch trials have been referred to as additional process accounts (e.g., De Jong, 2000; Rogers & Monsell, 1995; Rubinstein et al., 2001). The additional reconfiguration process is deemed responsible for the longer latencies observed on task-switch trials. Consequently, differences in reaction time between task-switch and task-repeat trials have been interpreted as reflecting task-set preparation, whereas error rates were explained by failures to accomplish task-set reconfiguration in time. The process of preparing for the next task was considered a top-down mechanism exerted by executive functions (Meiran, 1996; Rogers & Monsell, 1995; Rubinstein et al., 2001; see also Baddeley et al., 2001; Miyake et al., 2000). For instance, it was theorized that preparation of a new task set is achieved by reprogramming of the system (Ruthruff et al., 2001), retrieving the new task set from long-term memory and “loading” it into working memory (Arbuthnott & Woodward, 2002; Goschke, 2000; Mayr & Kliegl, 2000, 2003), or by top-down biasing of relevant task-demand units from a connectionist perspective (Gilbert & Shallice, 2002).

Task-set reconfiguration has an intuitive appeal. Given that in most switching paradigms bivalent stimuli are used that can be classified according to either of the classification tasks, it was argued that it would be difficult to retrieve the correct response without having reconfigured the new task (Rogers & Monsell, 1995), or at least having shifted to the new task goal (Allport & Wylie, 2000; but see Logan’s account that is considered below).

Some of the findings supporting task-set reconfiguration were obtained with variants of the alternating runs paradigm. In this procedure, short sequences of two different tasks alternate (e.g., AAB, or AAAAAAAB BBBB). It is remarkable to note that increased latencies are observed only on trials that directly follow a task switch (Monsell, Sumner, & Waters, 2003; Rogers & Monsell, 1995). This finding was seen to support the classical task-set reconfiguration account, in which an additional (or at least substantially lengthened) reconfiguration phase is assumed only on task-switch trials.

Another highly cited argument for the role of endogenous task-set preparation was obtained with the explicit cueing paradigm. In this procedure, tasks are presented in a random order with a task cue indicating which classification is required in the next trial (Mayr & Kliegl, 2000; Meiran, 1996). Manipulating the interval between an advance task cue and the imperative stimulus was shown to affect the size of the observed switch costs, known as the cue-stimulus-interval (CSI) effect: Reduced switch costs with increasing CSI suggests that endogenous task-set preparation takes place after cue presentation in the interval prior to stimulus presentation (but see Ratcliff & McKoon, 1988; Schneider & Logan, 2005).

However, the task-set reconfiguration account was also criticized. In particular, the classical TSR assumption that the performance difference between task-switch and task-repeat trials directly corresponds to the duration of an additional processing phase of endogenous task-preparation was questioned (Wylie & Allport, 2000). In fact, a couple of other endogenous control mechanisms were recently discussed as contributing to a lengthening of processing time. For instance, Monsell and Mizon (2006) argue that additionally to (1) the time required for waiting for TSR to complete after stimulus onset, there may also be (2) slower processing due to incomplete TSR, and (3) competition between TSR and other processes.

Still, a puzzle from a TSR perspective is so-called residual switch costs that are observed even when given ample time and opportunity to prepare for the upcoming task (De Jong, 2000; Meiran & Chorev, 2005; Rogers & Monsell, 1995). It was argued that residual switch cost might reflect that certain “exclusionary components” of task sets (e.g., which stimulus characteristics not to respond to) can only be completed upon stimulus onset, possibly in the face of response conflict (Mayr & Kliegl, 2003). Alternatively, it was proposed that advance preparation is possible in principle, but participants fail to do so in a number of trials (failure to engage [FTE] hypothesis; De Jong, 2000; for a critique and generalized version of FTE see also Brown, Lehmann, & Poboka, 2006). Relatedly, it was suggested that participants might successfully prepare for some, but not all response rules prior to stimulus onset (Lien, Ruthruff, Remington, & Johnston, 2005).

TSI

The TSI account assumes that switching costs arise from some sort of proactive interference from a prior, competing task (Allport et al., 1994; Allport & Wylie, 2000). As interference is far less under intentional control than task-set reconfiguration, the TSI account has been considered as describing a more reflex-like mechanism, specifying bottom-up or stimulus-driven processes. In a first version of the theory, it was assumed that task-set inertia results from persisting patterns of activation and inhibition from the previous task that need to be suppressed or that lead to competition in processing pathways (Allport et al., 1994).

In a newer version of the TSI theory it was predicted that carry-over effects may also result from stimulus triggered retrieval of previous S-R (Allport & Wylie, 2000) or stimulus-task-set associations (Waszak, Hommel, & Allport, 2003) that were acquired (or strengthened) in previous trials. The retrieval version of the account is more flexible and can explain relatively long-lasting proactive interference occurring after a lag of several trials. Recently it was argued that what is actually retrieved, triggered by the stimulus, is not a particular response, but an abstract representation of a competing task (Koch & Allport, 2006; Waszak & Hommel, 2007), although there is also evidence for the role of more direct S–R associations (Kiesel, Wendt, & Peters, 2007).
TSI is not a unitary account; instead, it subsumes a number of different bottom-up or stimulus-driven mechanisms. Persisting patterns of activation and inhibition (inertia in a narrower sense) as well as stimulus-triggered retrieval of responses or entire task sets may contribute to interference. Especially on trials directly following a task switch there should be competitor priming of the last (now irrelevant) task set as well as negative priming of the currently relevant task set which needed to be suppressed on the directly preceding trial. Finally, S-R or S-Set bindings may contribute to task-switching costs (Waszak et al., 2003; Waszak, Hommel, & Allport, 2004, 2005). Additionally, inertia effects were made responsible for residual switching costs (Allport et al., 1994; Mayr & Kliegl, 2000; Rogers & Monsell, 1995).

The task-set inertia account is well supported in the literature. A particular compelling finding is that the proportion of trials in which the stimulus has been processed in the same task set relative to trials in which the stimulus has been classified according to the competing task set determines the size of the switching effect (Wylie & Allport, 2000). The sensitivity to previous stimulus processing clearly supports the switching-away-from perspective (TSI), rather than the switching-to (TSR) perspective.

Multiple Components of Task Switching

As highlighted in the preceding sections, there is evidence for both, task-set preparation as well as carry-over effects. Accordingly, leading inertia theorists consider that some form of voluntary switch, such as goal shifting as a minimum request, to take place (e.g., Allport & Wylie, 2000), and inertia effects were included in task-set reconfiguration accounts (e.g., Yeung & Monsell, 2003a, 2003b). Today, multiple-components models are generally accepted that conceptualize task-set activation as a result of both, carry-over effects as well as endogenous control functions. So a number of factors likely contribute to the readiness to perform the currently relevant task (Gilbert & Shallice, 2002; Koch & Allport, 2006; Mayr & Kliegl, 2003; Meiran, 1996; Meiran, Chorev, & Sapir, 2000; Monsell, 2003; Rubinstein et al., 2001; Ruthruff et al., 2001; Sohn & Anderson, 2001; Yeung & Monsell, 2003a). A systematic taxonomy of sources and components of task-switching costs was offered by Fagot (1994) and, based on the latter, by Meiran and colleagues (Meiran et al., 2000).

In addition to the sources of task-switching costs already discussed, global selection costs and global control strategies (Mayr, 2001; Mayr & Liebscher, 2001; as to the latter see also Fagot, 1994; De Jong, 2000) were considered as contributors to task-switching effects. Their role was especially emphasized for global switching costs, when performance in task-switching blocks is compared with performance in task-pure blocks: Task-switching blocks are usually characterized by higher working memory demands; additionally, they are perceived as more difficult, which should result in increased response caution.

Phases of Task Switching

Recently, a number of models were proposed that help reconcile most findings in the task switching literature (Gopher et al., 2000; Mayr & Kliegl, 2003; Rubinstein et al., 2001; Ruthruff et al., 2001). These accounts can be described as multiple-components models as they assume that top-down as well as bottom-up factors contribute to switching costs, but at different phases of information processing. Rubinstein and colleagues (2001) have proposed that two complementary sets of phases exist: executive control processes and task processes. Task processes are those basic processes required in all classification tasks, including trials of task-pure blocks. They comprise stimulus identification, response selection, and movement production. In turn, executive functions are assumed to be additionally drawn in task-switching trials, enabling set-shifting and comprising goal shifting and rule activation.

Executive processes, were proposed to govern the preparation of the relevant task set (e.g., Fagot, 1994). These processes at the earlier preparation phase may comprise retrieving relevant task-set information from long-term memory (Arbuthnott & Woodward, 2002; Goschke, 2000; Mayr & Kliegl, 2000, 2003) or top-down biasing of the relevant task-demand units (Gilbert & Shallice, 2002). It is assumed that processes at the preparation phase can be completed prior to stimulus onset, but in the absence of advance task cues, preparation has to wait until relevant information is given. At a later phase, information from the stimulus is used to select the correct response in accord with the prepared task set. Therefore this phase can only begin with the onset of the imperative stimulus and will end when the decision process is terminated and a response initiated. Inertia effects were postulated to cause interference at this later phase of response selection (Mayr & Kliegl, 2003; Ruthruff et al., 2001).

The assumed asymmetry of controlled and automatic factors in the task preparation versus in the task execution phase was summarized by Ruthruff and colleagues (2001): Executive control functions (top-down processes) were considered to “program” the task set, while their involvement in task-set execution was considered only limited. Instead, stimulus-driven (bottom-up) factors were seen to modulate the rate at which task execution proceeds.

The view that preparation and stimulus-driven components exert their influence independently on different phases of task switching was supported by a number of findings. For instance, in a series of experiments, Ruthruff and colleagues (2001) used a modified alternating runs paradigm in which task switches were either expected or not. The design allowed an orthogonal manipulation of task preparation and a number of stimulus-driven factors. It turned out that the effects of task-repetition, stimulus repetition, and response mapping difficulty were found to interact. Following additive factor logic (Sternberg, 1969, 1998), these factors seemed to influence the same phase of processing. It is important that all of these factors were found to possess additive effects with task expectancy, which was considered to determine task preparation. The pattern of effects was thus in congruence with the prediction that (1) both task preparation and stimulus-driven factors contribute to task-switching costs, and that (2) they operate independently at different phases of processing (see also Mayr & Kliegl, 2003).

It is likely that some executive functions are also called at response selection as a more reactive form of control when several response-plans interfere and the system has to settle on a unique solution. In fact, most multiple-components models assume that control processes are called at both phases: proactive control at the early phase should contribute to some form of advance task preparation, whereas reactive control at the later phase of response selection should help cope with interference from inertia.

Additionally, executive control may be required for the transition between the preparation phase and the response-selection
In particular, the new task needs to be activated until it is just sufficiently prepared to be used in response selection. Thus there seems to be a requirement to find the best point in time for the transition between both phases in order to optimize both speed and accuracy (cf. Mayr & Liebscher, 2001; for a related idea of trade-offs between strategic reconfiguration and phasic alertness see Meiran et al., 2000).

To summarize, task switching comprises separable phases (Gopher et al., 2000; Mayr & Kliegl, 2003; Rubinstein et al., 2001; Ruthruff et al., 2001). It is assumed that in an earlier phase some form of task preparation takes place. This process can be completed prior to the onset of the imperative stimulus. However, in the absence of advance task cues, the process has to be postponed until relevant information is available. In contrast, response selection has to wait for stimulus onset, because information from the imperative stimulus is used to select the correct response according to the currently activated task set. Inertia effects (Allport et al., 1994; Allport & Wylie, 2000) are supposed to primarily interfere at the response selection phase of processing.

A Priming Account of Explicitly Cued Performance

An alternative account of the observed performance difference between task-switch and task-repeat trials in the absence of executive control processes was offered by Logan and colleagues (Logan & Bundesen, 2003; Schneider & Logan, 2005, 2009). According to this theory, participants use a compound retrieval cue strategy in the explicit cueing paradigm. It is argued that they encode the task cue, encode the imperative stimulus, and use both as a compound retrieval cue to pull an appropriate response from long-term memory. Given that the response is selected in each trial in a stimulus-driven manner, there is no requirement to switch between tasks, and the same task set is used for all trials. Therefore it was questioned that the effect in the explicit cueing paradigm reflects the costs of controlled reconfiguration. Instead, it was suggested that the difference between task-switch and task-repeat trials reflects a benefit of cue encoding in (cue-) repetition trials. Specifically, Logan conceptualizes cue encoding as a race process in which the perceptual representation of the task cue is compared with its short-term transient representation in short-term memory and as with its persisting representation in long-term memory (Logan & Bundesen, 2003). In the case the same cue is presented as in the directly preceding trial, there should be still an active trace in short-term memory which would easily win the race process, thereby saving time. Once all information is encoded and the compound cue is formed, it is assumed that response selection takes place in form of a random walk process, in congruence with existing memory retrieval theories for binary decision tasks (e.g., Nosofsky & Palmeri, 1997; Ratcliff, 1978).

There is impressive evidence that cue-encoding benefits contribute substantially to the effect in the explicit task-cueing paradigm. In an informative study, Logan and Bundesen (2003) employed an explicit cueing paradigm with two different task cues for each task. This procedure allows one to dissociate cue-switching effects from task-switching effects. In fact, they found a substantial difference between cue-repetitions and cue-switches in task-repeat trials, but only a small difference between task switches and task repetitions when there was a cue-switch. Cue-encoding benefits were also deemed responsible for the CSI effect in the explicit cueing paradigm (Schneider & Logan, 2005)—an effect that was previously considered strong evidence for endogenous task-set preparation.

However, cue-switching costs were shown not to explain the entire task-switching costs, suggesting some additional processes (Mayr & Kliegl, 2003; Monsell & Mizon, 2006; see also Arrington, Logan, & Schneider, 2007). This objection was qualified by pointing to the finding that additional effects seem to be particularly pronounced when arbitrary task cues are employed. It was suggested that the activation of task goals or task sets in these cases is not direct but via the retrieval of a more transparent mediator cue (Logan & Schneider, 2006).

But there remain a few other critical findings. For instance, cue repetitions versus cue alternations do not moderate n-2 repetition costs, that is, the cost of executing a task previously switched away from (Allmann, 2007; Gade & Koch, 2008; see also Mayr & Keele, 2000). This finding suggests an effect of task sets independent of task cues. Another noteworthy finding is that complete switches (of task, stimulus, and response) are sometimes just as easy as complete repetitions (Hommel & Milliken, 2007)—an effect that is difficult to reconcile with repetition priming. To conclude, repetition priming may not account for all specific task-switching effects, but repetition benefits in task-repeat trials are definitely an important piece to the puzzle.

Formal Similarities of Multiple-Components Models and the Priming Account

Despite all differences discussed, multiple-components models of task switching and Logan’s account share some similarities with regard to the time course of a switching process. Precisely, in both accounts it is assumed that during a task switch at least two successive phases can be distinguished. In an early phase some sort of preparation of the new task or encoding processes (including mediator retrieval) takes place. In the second phase, the response is selected using both, information from the imperative stimulus and from the task set or the task cue, respectively.

However, there are differences as to which processes are assumed to take place at these two phases, as summarized briefly. Consider the early phase, first. According to multiple-components models, an informative task cue is used to prepare the system for the upcoming task in switching trials. Task-set reconfiguration (Rogers & Monsell, 1995) may include reprogramming of the system (Ruthruff et al., 2001), top-down biasing of the relevant task-modules (Gilbert & Shallice, 2002), or retrieving the task set or its associated S-R mapping rules from memory (Arbuthnott & Woodward, 2002; Goschke, 2000; Mayr & Kliegl, 2003). Accordingly, the observed cue-switch effect in the modified explicit cueing paradigm with two cues per task was accounted for by facilitation in retrieving the relevant mapping rules from memory when cues repeat (Mayr & Kliegl, 2003). In contrast, Logan’s account states that the first phase is used to encode the task cue and the imperative stimulus. Priming of cue encoding in cue-repetition trials is deemed responsible for the switching effect in the explicit cueing procedure (Logan & Bundesen, 2003).

There are also differences as to how response selection is assumed to take place in the later phase of the switching process. According to multiple-components models of task switching, the second phase is used for processing of stimulus
information along the constraints that were set in the first phase. Accordingly, it was argued that the later phase can only proceed when top-down biasing in the preparation interval is terminated (Gilbert & Shallice, 2002). The process of task-set execution has been considered less controlled and more stimulus-driven (Ruthruff et al., 2001). Note that a repetition benefit was also expected at this later phase (Mayr & Kliegl, 2003). This can be explained by a larger difference in activation (or biasing) between the relevant and the competing task set (or associated mapping rules), and thereby higher efficiency with which stimulus information is used to select the response. In contrast, in Logan’s account it is postulated that response selection begins when cue and stimulus are encoded and the compound cue is formed. Most likely no further information from the stimulus is used from that point on; instead, response selection is conceptualized as probing memory with the compound cue.

**Diffusion Model**

In the present series of experiments, we fitted the diffusion model (Ratcliff, 1978; Ratcliff & Rouder, 1998) to task-switching data. This was motivated by the observation that theoretical models of task switching costs, such as the multiple-components models described above, as well as Logan’s account postulate that a task switch consists of separable phases: an earlier one in which the upcoming task is prepared or in which stimuli are encoded, and a later one that corresponds to the actual process of response selection. The purpose of the present study was to decompose task-switching effects with the diffusion model, thereby drawing apart effects that would be otherwise merged in the mean latencies or errors. In the next paragraphs, we will briefly introduce the diffusion model and describe its characteristics.

The classical diffusion model (Ratcliff, 1978; Ratcliff & Rouder, 1998) is a stochastic model for two-choice decisions. The model assumes that decisions are based on an accumulation of response evidence over time until a criterion is satisfied and a response is elicited. The model decomposes the classification process into a number of theoretically meaningful parameters. Another advantage of this type of data modeling is its exhaustive use of available performance data. Parameter estimates are based on both, latencies of correct and incorrect responses as well as on the accuracy of responding. Additionally, the entire shape of the latency distributions is used.

The diffusion model decomposes a decision process into a number of meaningful components. First, it distinguishes between nondecision time and components of the actual decision process. Second, the response decision is conceptualized as a continuous sampling process in which a response counter fluctuates between two response criteria, and it elicits a response when sufficient evidence has been accumulated. As an illustration, a hypothetical sample path of a diffusion process is depicted in Figure 1 (gray line). The graph is read from left to right as a function of decision time. The ordinate represents the response related decision axis, on which two response criteria are placed at 0 and a. In this example, the upper criterion corresponds to the correct response (response A), the lower criterion to the incorrect response (response B). The counter is postulated to start at point z between the two response thresholds. It starts fluctuating as a function of both systematic as well as random information that accumulates with time. As soon as one of the two response criteria is crossed, the decision process is terminated and the respective response is initiated.

The diffusion model decomposes the processes into a set of informative parameters: The nondecision parameter (θ) subsumes a number of components outside the response selection process (encoding, preparation, execution). The drift rate (v) corresponds to the mean slope of the diffusion process across trials and quantifies the efficacy with which response-related information is accumulated. The response criterion (a) denotes the difference between the response thresholds.

Typical latency distributions obtained for correct and incorrect responses are depicted in Figure 1 above their respective response thresholds. The diffusion model can account for their positions and shapes as well as for the proportion of correct and incorrect responses. The estimation of parameters is conducted in a way so that the degree of correspondence (goodness of fit) between the observable distributions and the distributions predicted by the parameters is maximized (details are provided in the methods section of Experiment 1).

Since its introduction into psychological research in the late 1970s (Ratcliff, 1978), the diffusion model was used to analyze a wide variety of different tasks. Only most recently, was the diffusion model used to analyze task-switching data. In an informative study, Karayannis et al. (2009) employed a variant of the cueing paradigm with three different tasks and four different task cues (i.e., repeat, switch-to, switch-away, and noninformative task cues). Stimuli were letter-digit pairs presented in different colors.
that had to be classified either according to the letter, the digit or the color. All stimuli were presented in one of six wedges in a circle with position serving as a task cue. The paradigm was thus somewhat more complex than most other explicit cueing studies, but participants had enough time to prepare for the upcoming task (with a cue stimulus interval of 1000 ms), additionally there was a long inter trial interval of 1400 ms. In another relevant study, Madden et al. (2009) compared cognitive performance of young adults and elderly people using a task-switching paradigm involving word categorization (manmade vs. natural and large vs. small). In this functional magnetic resonance imaging study, the interval from cue onset to target onset was 1500 ms, and there was a variable inter trial interval ranging from 1500 ms to 7500 ms. For modeling the task-switching effect, a highly constrained diffusion model was used with only two parameters set free to capture possible effects of task switching (these were the drift rate and the nondecision parameter). Diffusion-modeling findings from both studies will be considered below.

As different task-switching paradigms are known to differ in affordances (i.e., there is substantial paradigm specificity; Yehene & Meiran, 2007), we decided to investigate task-switching effects with two frequently used paradigms: the classical alternating runs paradigm (Rogers & Monsell, 1995) as well as an explicit cueing paradigm with digit and letter classifications. In line with previous research with the diffusion model (Klauer, Voss, Schmitz, & Teige-Mocigemba, 2007; Ratcliff & Rouder, 1998; Voss, Rothermund, & Voss, 2004; see also Ravenzwaaij & Oberauer, 2009), the following parameters were considered most informative: the response criterion \( \alpha \), the drift rate \( v \), and the nondecision parameter \( \theta_0 \). Their function in the diffusion model will be described in the next paragraphs along with how they have been interpreted in previous studies with task-pure paradigms. We will also discuss which switching-related processes may be captured by the parameters.

**Nondecision Parameter (\( \theta_0 \))**

This parameter adds to response latencies of false and correct responses alike, summarizing processing time consumed by a number of factors outside the actual response selection. This may include perceptual encoding of the stimuli, task preparation, and the execution of the response. The nondecision parameter is also assumed to vary across trials with a uniform distribution of \( \sigma \). Experimental evidence has supported the notion that \( \theta_0 \) captures motor time (Voss et al., 2004; Ratcliff, Thapar, & McKoon, 2006). However, there are a number of other processes outside the response-selection phase that may contribute to the nondecision parameter. For instance, in a study using the Implicit Association Test (IAT; Greenwald, McGhee, & Schwartz, 1998), it was argued that task-switching costs are reflected in the nondecision parameter (Klauer et al., 2007). More specifically, multiple components models of task-switching would predict that additional preparatory processes take place (or are at least substantially lengthened) in task-switch trials (Ruthuff et al., 2001) —and these would contribute to processing time outside the actual response selection. To conclude, in line with conventional interpretation of the nondecision parameter, basic encoding and motor processes should contribute to \( \theta_0 \) in all trial-types. But additional preparatory processes in task-switch trials would lead to an increase in nondecision time in task-switch relative to task-repeat trials.

An effect in the same direction would be also predicted by Logan’s account. According to this theory, the time prior to response selection corresponds to the encoding of the cue (including mediator-cue retrieval) and the stimulus. Priming of cue encoding in repetition trials should therefore reduce nondecision time in repetition trials. Therefore, predictions converge that the nondecision parameter is higher in switching than in repetition trials—at least when cue encoding cannot be accomplished prior to stimulus presentation.

In fact, there is first evidence that the nondecision parameter is increased in task-switch relative to task-repeat trials (Karayannis et al., 2009; Madden et al., 2009; see also Klauer et al., 2007). When describing parameters of the diffusion model to their readers, Madden and colleagues still chose a conventional interpretation of the nondecision parameter as capturing basic perceptual and motor processes, but they also noted that the parameter may be increased in difficult conditions. Karayannis and colleagues (2009) pointed out that in task-switching paradigms, reconfiguration processes may contribute to the nondecision parameter. This was supported by their finding that \( \theta_0 \) was always increased in task-switch trials that were not cued in advance by fully informative task cues.

**Drift Rate (\( v \))**

The diffusion process is thought to comprise systematic and random components. The systematic component is captured by the drift rate \( (v) \). The parameter corresponds to the mean rate of information accumulation in the response selection process, or the mean slope of the diffusion process. The sign of the parameter determines which response threshold is reached in most cases, with a positive sign indicating the upper threshold. The size of the parameter quantifies how fast the threshold is approached. High drift rates typically imply both fast and accurate responses. The speed at which information is accumulated may vary across trials as a function of S-R association, participant’s arousal, and so forth. It is assumed that drift rates are normally distributed across trials with mean \( v \) and standard deviation \( \eta \) (Ratcliff & Tuerlinckx, 2002). Mean drift rates vary between persons and experimental conditions and are interpreted as a measure of the efficacy of evidence accumulation. For instance, higher drift rates were shown for participants with higher fluid intelligence and working memory capacity (Schmiedek, Oberauer, Wilhelm, Suess, & Wittmann, 2007). At the same time drift rates reflect task difficulty with higher drift rates in easy tasks (e.g., Voss et al., 2004).

Drift rates are one of the core parameters of the response selection process, and correspond with the later phase in a task-switch trial according to multiple-components models. Note that response selection is required in all trial types (task pure, switching, repetition). Therefore a number of factors may contribute to the drift rate, including primary task difficulty, stimulus characteristics—and additionally task-switch specific factors. As to the latter, we suggest that the activation of S-R mapping rules, task-set biasing, or other factors contributing to task readiness may affect the efficiency of evidence accumulation and thus the drift rate. Note that both controlled as well as more automatic effects are considered as contributing to task readiness (Koch & Allport,
Therefore, drift rates are a complex parameter, and their slope can be predicted as reflecting effects of previous task-set preparation, stimulus-driven effects as well as carry-over effects from the previous trial. Of course, in task-switch trials, carry-over effects such as competitor priming and negative priming would impair the efficiency of response selection, leading to decreased drift rates. In contrast, in task-repeat trials, carry-over effects would rather contribute to a particular strong activation of the relevant task set, thereby increasing drift rates. Somewhat differently, according to Logan’s account, the random-walk process assumed to take place at response selection is not expected to differ in task-repeat and task-switch trials.

In support of these predictions, drift rates were found to be decreased in task-switch trials relative to task-repeat trials in the Madden et al. (2009) study in both younger and older participants. The same finding was obtained in the Karayanidis et al. (2009) study. In the latter study, it also turned out that the informativeness of the task cue (fully, partially, or noninformative) had no effect on drift rates in switching trials. This seems to suggest that drift rates primarily reflect automatic carry-over effects. Nevertheless, there was an effect of cue informativeness, but only in task-repeat trials: Drift rates were particularly high when participants knew in advance that the task would repeat in the next trial. If so, it appears to be that participants use their advance task knowledge in these trials to maintain the relevant task set maximally activated.

Response Criterion (a)

The time it takes until the diffusion process reaches one of the response thresholds also depends on the choice of response criteria. In case of a large boundary separation, it takes more time until any of the thresholds is reached. At the same time, a large boundary separation reduces the risk that random fluctuation accidently leads to the initiation of an incorrect response (see sample path in Figure 1). The choice of response criteria is considered as reflecting response caution. Participants preferring a conservative, cautious response mode would select a higher response criterion (i.e., larger threshold separation), resulting in generally slow, but highly accurate responses. Participants with a more liberal response criterion would be generally faster at the cost of an increased number of errors.

Response caution is a nonability-related personality characteristic and contributes to speed-accuracy settings, with high response caution contributing to longer latencies while reducing the likelihood of errors. For instance, in a variety of different discrimination and memory tasks, it was shown that elderly people adopt more conservative response criteria than college students, while their efficiency of stimulus processing (measured in terms of drift rate) was highly comparable in many tasks (e.g., Ratcliff et al., 2006). Moreover, the wording of instructions and performance pay-offs were shown to affect the choice of response criteria. For instance, after introducing rewards for correct responses participants were shown to adopt more conservative response thresholds (Voss et al., 2004). But even when not explicitly instructed to do so, participants tend to exercise more caution in experimental blocks with an incompatible response mapping as compared with blocks with a compatible response mapping—possibly as a function of perceived task difficulty or operant learning (i.e., increased number of errors in previous trials).

We predicted that response caution should be generally higher in task-switching blocks than in task-pure blocks, because task-switching blocks are characterized by higher working memory load, an updating requirement, and interference effects, all of which contributing to the perception of higher difficulty, promoting the choice of more conservative response criteria in these blocks (e.g., Klauer et al., 2007). Both in the Madden et al. (2009) as well as in the Karayanidis et al. (2009) study only results for switching and repetition trials from task-switching blocks are provided, therefore no comparison between the mixed-tasks and the task-pure blocks is possible. It is interesting there was a difference in response caution between trial types in the Karayanidis et al. (2009) study, suggesting that participants increase response caution in task-switch trials relative to task-repeat trials from the same block (we will return to this finding). In the Madden et al. (2009) study, response caution was constrained to be equal across trial types from the same block, as conventionally done in previous studies with task-pure paradigms. Because of this constraint, there is no evidence, neither for nor against adjustment of response caution, leaving the question unresolved as to whether participants are capable of adjusting response caution in a trial-to-trial fashion.

Overview of Experiments

Experiment 1. In the first study, we applied the diffusion model to decompose task-switching costs in the classical alternating runs paradigm (Rogers & Monsell, 1995). In this procedure, bivalent stimuli are presented in clockwise order in a 2 × 2 grid. They have to be classified according Task A in the upper fields and according to Task B in the lower fields.

Experiment 2. We manipulated task-set predictability between groups by either presenting stimuli in clockwise order or randomly in the 2 × 2 grid. Thereby, we investigated the role of task-set preparation prior to stimulus onset, and dissociated this component from task-set inertia.

Experiment 3a, 3b, and 3c. These experiments were conducted to replicate the findings obtained in Experiment 2 conceptually, avoiding confounding factors in the previous experiment. In Experiment 3a, task-set predictability was manipulated between groups within an explicit cuing paradigm, thereby removing the confounding factor of endogenous task cueing. In Experiment 3b, we used background color as task cues and presented all stimuli in the center of the screen, thereby removing the requirement to search for the next stimulus which could add to the nondecision parameter. In Experiment 3c, we separated the task cue and the stimulus in the short CSI group by a short interval, thereby removing possible effects of a bottle neck at encoding that could also contribute to the effect in the nondecision parameter.

Experiment 4. In the last study, we isolated effects of task-set inertia and investigated the decay of inertia as a function of time elapsed since the last application of the task set.

Experiment 1

In the first experiment, we investigated task-switching costs in the classical alternating runs paradigm (Rogers & Monsell, 1995). The purpose of this study was twofold: First, we investigated the
DECOMPOSING TASK-SWITCHING COSTS

applicability of the diffusion model to data from the classical alternating runs paradigm. Second, we tested our predictions that specific parameters of the diffusion model capture components of task switching costs localized at different phases of a task switch as predicted by multiple-components models of task switching (e.g., Gopher et al., 2000; Mayr & Kliegl, 2003; Rubinstein et al., 2001; Ruthruff et al., 2001). Specifically, we predicted that task-switching effects are reflected in the nondecision parameter and in the drift rate. Effects in the nondecision parameter (\( r_0 \)) are assumed to capture processes outside the actual response selection phase. In task-switch trials, preparation of the new task set can be expected to lengthen this phase, thereby leading to an increase in the nondecision parameter in these trials. Note that an effect in the same direction would also be expected according to Logan’s account: A shortening of cue-encoding in repetition trials should lead to the observed difference between task-switch and task-repeat trials, and it should be reflected in the nondecision parameter. As already mentioned, there is first evidence that the nondecision parameter is increased in task-switch trials relative to task-repeat trials (Karayanidis et al., 2009; Madden et al., 2009; see also Klauer et al., 2007). Also the drift rate was predicted to differ in task-repeat and task-switch trials. As discussed, carry-over effects from the previous trial are expected to contribute to the level of task-set activation or biasing: In task-repeat trials, this should lead to a particular strong activation of the relevant task set, and should thus improve the efficiency with which stimulus information is used to select the correct response. In contrast, reduced drift rates would be expected in task-switch trials. Also for the latter effect there is first evidence in the discussed studies.

Method

Sample. Twenty-four research volunteers (17 female) participated in the first experiment. Most of them were students of the University of Freiburg with diverse majors. Their mean (\( M \)) age was 24.2 years (standard deviation [SD] = 3.5; range 18–31). All participants reported that their vision was normal or corrected to normal. They received 7.00 Euros or partial course credit as compensation.

Alternating runs paradigm. The starting point for the present series of experiments was the classical alternating runs paradigm introduced by Rogers and Monsell (1995). In this paradigm, all stimuli are presented in one of the four fields of a 2 \( \times \) 2 grid. Digit-letter pairs were used as composite stimuli and were ambivalent with regard to the classification task, as in half of the trials the digit, and in the other half the letter was to be classified. Spatial location determined the task that had to be applied and served as a reliable external task cue. Stimuli that appeared in one of the upper two fields were to be classified with respect to their digit as odd or even (Task A); stimuli that were presented in any of the lower two fields were to be classified with respect to their letter as consonant or vowel (Task B). First, each task was practiced alone in task-homogeneous blocks, then there was a mixed block in which stimuli were presented in all four fields in clockwise fashion, resulting in a strict AABB task sequence. Note that the strict presentation order in runs of two additionally allowed for endogenous task cueing. The response-stimulus interval (RSI) was set at 400 ms. Stimuli always appeared in the center of the four fields. Centers of neighboring fields had a distance of 7.5 cm, resulting in a visual angle of ca. 7.1°. Digit-letter pairs were approximately 2 cm (2°) wide and 1.2 cm (1.2°) high. The four-field grid remained visible throughout the experimental blocks, including intertrial intervals. The same stimuli were used as in the classical Rogers and Monsell (1995) study. Consonants were randomly drawn from the set G, K, M, R and vowels from the set A, E, I, U, odd digits from the set 3, 5, 7, 9, and even digits from the set 2, 4, 6, 8. All stimuli were presented in black Courier font on a light gray screen (RGB 220,220,220). In case of an error, a red ‘X’ (RGB 250,100,100) was presented directly below the imperative stimulus and remained on screen until the correct response was given. Instructions prior to each block equally stressed speed and accuracy of responding. After each block, participants saw the mean latency and accuracy of classifications in that block as performance feedback.

Trial lists were generated for each participant prior to the experiment controlling for a number of factors. While task and task-repetition were determined by the AABB sequence, response repetition, response congruency of relevant and irrelevant stimulus component, and the affordance in a Simon task (expected response on the same vs. opposite side on which stimulus was presented) were counterbalanced for stimuli appearing in each of the four fields, for each field separately. As in the Rogers and Monsell (1995) study, there were no direct digit or letter repetitions, avoiding trial-by-trial stimulus-specific negative priming and competitor priming effects. Additionally, there were no sequences requiring more than four successive responses with the same key.

Procedure. The experiment was run in individual sessions of approximately 45 min. Participants sat about 60 cm away from a 19 in., 100 Hz CRT monitor. Responses were recorded with specific response devices connected to the parallel port of the PC (Voss, Leonhart, & Stahl, 2007). The sessions were completed by a few demographic questions that were also presented in a computerized format.

Participants started with two task-pure practice blocks (first digit classifications, second letter classifications) and then completed the task-switching block. This whole cycle was repeated four times. Restarting each block was self-paced. Each block was preceded by warm-up trials, two for each stimulus position used later in that block (i.e., four warm-up trials in the task-homogeneous blocks, and eight in the mixed blocks). Task-homogeneous blocks had 32 experimental trials each (i.e., 64 task-homogeneous trials per cycle). Mixed blocks comprised 128 experimental trials (of which 64 were task-switch trials and 64 task-repeat trials per cycle).

Data analyses. Because of large training effects, data from the first cycle were discarded. Trials from the remaining three cycles (excluding warm-up trials) were aggregated across tasks and response categories (response sides), so that there were originally 192 trials for the task pure, task-switching and task-repetition condition, from which post error trials and RT outliers were excluded for the reasons given below (details concerning proportions of excluded trials are provided in the results section of each experiment).

We removed post error trials for two reasons: First, there may be error-related cognitive processing unrelated to the demands of the actual task. Second, task-confusion errors invert the type of trial transition on the next trial (i.e., task-switch trials become task-
repeat trials, and vice versa; see Steinhauser & Hübner, 2006). Both mechanisms can contribute to noise when aggregating data.

Additionally, we also removed extreme RT outliers—which is a bit delicate, as outliers may bias the estimation of diffusion model parameters, while at the same time, extreme values may contain valuable information (see Ratcliff & Tuerlinckx, 2002, for a discussion). In the present study, parameter estimation was based on the Kolmogorov–Smirnov (KS) statistic that is relatively robust against distorting effects of outliers (Voss & Voss, 2008). Therefore, only the most extreme trials based on individual exclusion criteria (Weinstein, 1999) were excluded: Responses were removed when they were slower than the 75 percentile plus 3 times the interquartile range and when they were faster than the 25 percentile minus 3 times the interquartile range or below 200 ms.

Parameters of the diffusion model were estimated with fast-dm (Voss & Voss, 2007, 2008). The degree of correspondence between observed cumulative distribution and predicted cumulative distribution is quantified by the KS statistic (Kolmogorov, 1941) and minimized during parameter estimation. A graphical test of the model fit is given in Appendix A. Fast-dm yields estimates of all six parameters of the Ratcliff diffusion model. Other programs currently available for diffusion modeling are DMAT (Vandekerckhove & Tuerlinckx, 2007) and EZ (Wagenmakers, van der Maas, & Grasman, 2007; Wagenmakers, van der Maas, Dolan, & Grasman, 2008). DMAT also estimates the full set of parameters, but the estimation is based on bins of the distributions of correct and erroneous response times, thereby requiring a sufficiently high numbers of correct and erroneous trials (the latter was not met in the current study). EZ uses a simplified procedure in which three observable parameters (M and SD of correct trials, and percent accuracy) are transformed into three scores corresponding with the main parameters of the diffusion model (a, v, and t0). In the previous studies by Madden et al. (2009) and Karayanidis et al. (2009), versions of the EZ program were used to obtain the parameters of the diffusion model.

We used fast-dm to estimate independent diffusion models for each participant and each of the three trial types (task pure, switching, repetition). No parameter constraints were imposed between conditions. All parameters were set free with the exception of the starting point which was fixed in the middle between the two response barriers. Also variances of the parameters were free to vary across conditions.2 Throughout this article we will present conventional performance data (RT and accuracy) as well as the main parameters of the diffusion model (a, v, and t0). Findings for all three trial types (task pure, switching, repetition) will be presented in the figures, which allows for a quick comparison of their effects across all trial types. In the text we will focus on the analyses of trials from the mixed task block, in line with our primary goal to decompose the local task-switch effect.3

Results and Discussion

Latencies and errors. Prior to data analysis, 7.7% of all trials were excluded according to the above described criteria (5.3% as post error trials; additionally 2.4% as RT outliers). Performance data for Experiment 1 are displayed in Figure 2: latency data in the upper panel, error data in the lower panel. Latency and error data from trials of the task-switching block were independently submitted to a one-factorial analysis of variance with the task-switching factor (switching vs. repetition) as a within-participants factor. As expected, task-switch trials were characterized by longer latencies, $F(1, 23) = 35.28, p < .001$ and more errors, $F(1, 23) = 38.35, p < .001$ than task-repeat trials.

Parameters of the diffusion model. Results for the parameters of the diffusion model are depicted in Figure 3: response criterion ($a$) in the upper panel, drift rates ($v$) in the middle panel, and the nondecision parameter ($t_0$) in the lower panel. For each parameter, a separate analysis of variance was conducted with task-switching (switching vs. repetition) as a within participants factor.

Consider first the nondecision parameter. As can be seen, parameter values did not differ much between conditions, which was confirmed by the analyses of variance, $F(1, 23) = 2.06, p = .17$. The absence of an effect could imply that the nondecision parameter is generally not sensitive to task preparation or any other component of local switching costs, which would contradict the above theorizing as well as first empirical evidence. So an alternative interpretation could be that the descriptively higher nondecision time in task-switch trials might have missed significance because task preparation was already largely completed prior to stimulus onset at an RSI of 400 ms. In fact, task preparation prior to stimulus onset was possible in principle, because the perfectly predictable task-sequence in the alternating runs paradigm would have allowed endogenous task cueing. Note that previous studies are not conclusive in this respect. Karayanidis and colleagues (2009) used a substantially longer CSI of 1000 ms, which was

![Figure 2. Experiment 1—Latencies and Errors. Performance data are displayed separately for trials from the task-pure blocks as well as for task-switching and task-repeat trials from the mixed blocks. Error bars indicate standard errors.](image-url)
that a more cautious response criterion was chosen in task-switch than in task-repeat trials, \( F(1, 23) = 29.66, p < .001 \). In fact, previous research has ruled likely that trial-to-trial adjustment of response caution can take place even within one block. But this was usually considered a reactive mechanism following the detection of response-incongruency (e.g., Botvinick et al., 2004; Jones, Cho, Nystrom, Cohen, & Braver, 2002). The present findings suggest that an adjustment of response caution also takes place in anticipation of the next trial transition (i.e., task switch vs. task repetition; see also Karayanidis et al., 2009). However, it remains unresolved whether such an adjustment takes place largely automatically or in a controlled and strategic way. In fact, characteristics of the classical alternating runs paradigm would have allowed automatic adjustment: The task sequence is extremely regular (i.e., AABB) and task switches and repetitions always occur in the same spatial positions. Thereby errors are more likely in some positions than in others and operant learning mechanisms may lead to an increase of response caution whenever a stimulus is presented in these positions. A similar effect could contribute as a function of endogenous updating (e.g., increasing response caution every second trial). As mentioned, the observed effect can be reconciled as well with more controlled adjustment of response caution, and previous findings using the explicit cueing paradigm (Karayanidis et al., 2009) support this interpretation—we will return to this point in the discussion of Experiment 3.

In summary, the diffusion model was applied to the alternating runs paradigm. Contrary to our expectation, there was no increase in the nondecision parameter in task-switch trials relative to task-repeat trials. The finding might imply that task-set preparation can be largely completed within 400 ms when the nature of the next task can be predicted by design. However, there was the predicted effect in the drift rate parameter, with higher drift rates in task-repeat than in task-switch trials. Additionally, there was also an effect in the response criterion, suggesting that participants adjust response caution within one block as a function of anticipated task switches versus task repetitions.

**Experiment 2**

We were surprised that there was no effect in the nondecision parameter in Experiment 1, although the other parameters reflected effects of the task-switching factor in a plausible way. This could either imply that the nondecision parameter in the diffusion model is generally not sensitive for task-switching processes (e.g., preparatory processes or repetition priming; but see Madden et al., 2009; Karayanidis et al., 2009) or that the processes captured by the nondecision parameter were largely completed within the RSI of 400 ms (as shown for a CSI of 1000 ms in the study by Karayanidis et al., 2009). Note that preparation could be accomplished in the classical alternating runs procedure, because task switches and task repetitions are reliably predictable, allowing for preparation prior to stimulus onset. Additionally the extreme regularity of the task sequence (AABB) would allow automatic adjustment: The task sequence is extremely regular (i.e., AABB) and task switches and repetitions always occur in the same spatial positions. Thereby errors are more likely in some positions than in others and operant learning mechanisms may lead to an increase of response caution whenever a stimulus is presented in these positions. A similar effect could contribute as a function of endogenous updating (e.g., increasing response caution every second trial). As mentioned, the observed effect can be reconciled as well with more controlled adjustment of response caution, and previous findings using the explicit cueing paradigm (Karayanidis et al., 2009) support this interpretation—we will return to this point in the discussion of Experiment 3.

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group of participants we replicated the design of Experiment 1 without modification. Second, we wanted to test whether the nondecision parameter captures task-set preparation at all—given there is any. Therefore, we removed task predictability while staying as close as possible to the presentation format in the other group (keeping other factors constant). Stimuli were presented in an identical 2 × 2 grid, with stimulus position functioning as the task cue. But stimulus position was randomly selected in the mixed block. As the relevant task was only cued with the onset of the stimulus, advance preparation of the new task was not possible.

The RSI was constant at 400 ms in both groups. As this time was apparently sufficient to accomplish task-set preparation (given this is captured by τ0, at all) in Experiment 1, we predicted that the effect would be absent in the predictable sequence group in the current experiment, too. Instead, we predicted an increase in τ0 in switching trials in the random sequence group. This pattern of findings would confirm that the nondecision parameter is sensitive to preparatory processes prior to response selection (e.g., Rogers & Monsell, 1995; Ruthruff et al., 2001) or cue-encoding benefits (Logan & Bundesen, 2003; Logan & Schneider, 2006). However, inertia effects can be ruled out as an explanation, because the RSI was constant in both experimental groups (see also Mayr & Kliegl, 2000; Meiran, 1996).

Another aim of the current study was to follow-up on the finding that participants apparently set different levels of response caution for task-switch and task-repeat trials in the alternating runs paradigm. We assumed that response criteria are adjusted prior to the response selection phase given time and opportunity, so that a more cautious response style is chosen when expecting a difficult trial (as in Experiment 1; see also Karayanidis et al., 2009). As task predictability was removed in the random task sequence group, we expected that also the difference in response caution should disappear in that group.

Method

Sample. There were 36 research volunteers (24 female). All of them were students at the University of Freiburg. Their mean age was 23.2 years (SD = 3.1, range 19–31). Their vision was normal or corrected to normal, and they received 7.00 Euros or partial course credit.

Procedure. Task predictability was manipulated between participants. One group of participants underwent the same procedure as in the previous experiments. Stimuli were presented in a clockwise fashion and hence the next task was perfectly predictable. For the other group of participants, the visual display, the stimuli and classification tasks were identical. The digit-letter pairs again appeared in one of the four fields of the 2 × 2 grid. But the position where the next imperative stimulus would appear was randomly selected in the task-switching block, with an equal probability that the next trial would be a task-switch or a task-repeat (with the constraint that a stimulus never appeared in the same position as in the directly preceding trial). RSI was constant at 400 ms in both groups.

Results and Discussion

Latencies and errors. 8.4% of all trials were removed prior to analyses (6.4% as post error trials; 2.1% as RT outliers). Data from the task-switching blocks were entered into an analysis of variance with task predictability (fixed sequence vs. random sequence) as a between participants factor and task-switching (switching vs. repetition) as a within participants factor. As can be seen in Figure 4 (upper panel), latencies were longer in task-switch than in task-repeat trials. Participants in the random sequence group had larger latencies than participants in the fixed sequence group. This difference was more pronounced in the task-switching condition. Results from the analysis of variance confirmed that there was a main effect of task predictability, $F(1, 34) = 22.55$, $p < .001$, a main effect of the task-switching factor, $F(1, 34) = 175.54, p < .001$, and a significant interaction of both factors, $F(1, 34) = 4.40, p < .05$. Post hoc test were significant for all pairs of means (all $p < .001$).

An analogous pattern was obtained for the error data (see Figure 4, lower panel). Participants generally committed more errors in task-switch than in task-repeat trials. Only in task-switch trials did the number of errors in the random sequence group exceed those made in the fixed sequence group. There was no main effect of task predictability, $F(1, 34) = 1.08, p = .31$, there was a main effect of the task-switching factor, $F(1, 34) = 53.19, p < .001$ and a significant interaction of task-switching and task predictability, $F(1, 34) = 4.29, p < .05$. Post hoc tests confirmed that there was no difference between the fixed sequence group and the random sequence group in task-repeat trials, $t(34) = -0.25, p = .80$; if anything, the difference between both experimental groups approached a marginal effect in the task-switching condition, $t(34) = 1.56, p = .13$. The main effect of the task-switching factor was present in both experimental groups (both $p < .01$).

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Figure 4. Experiment 2—Latencies and Errors. Participants were either presented stimuli in a strictly clock-wise fashion (fixed, predictable task sequence) or randomly in any of the quadrants (nonpredictable task sequence). Performance data are displayed for each group, separately for trials from the task-pure blocks as well as for task-switching and task-repeat trials from the mixed blocks. Error bars indicate standard errors.
Parameters of the diffusion model. Each parameter was submitted to a two-factorial analysis of variance with the between-participants factor task predictability (fixed task-sequence vs. random task-sequence) and the within-participants factor task switching (switching vs. repetition).

Consider the nondecision parameter first (Figure 5, lower panel). There was a distinct pattern of findings: The only condition in which the nondecision parameter was substantially increased was in task-switch trials in the random task-sequence group. In all other conditions, the parameter was far lower and of comparable magnitude. In fact, there was a main effect of task predictability, $F(1, 34) = 135.00$, $p < .001$, a main effect of the task-switching factor, $F(1, 34) = 84.14$, $p < .001$, and a significant interaction between both experimental factors, $F(1, 34) = 79.81$, $p < .001$. Post hoc tests revealed that the main effect of the task-switching factor was present in the random-sequence group, $t(17) = 11.27$, $p < .001$, whereas effects were virtually absent in the fixed-sequence group, $t(17) = 0.20$, $p = .84$, replicating Experiment 1. The difference between both groups was significant in both trial types, but the difference was by far larger in the task-switch trials, $t(34) = 11.64$, $p < .001$ as compared with task-repeat trials, $t(34) = 2.42$, $p < .05$. The pattern of effects thus supports the interpretation that the nondecision parameter captures task-set preparation: The parameter is elevated only in trials in which a new task needs to be prepared (switching trials) and when this process cannot be accomplished prior to stimulus onset (unpredictable task sequence). At the same time, the findings suggest that the components of task-set preparation captured by $\alpha$ can be successfully completed prior to stimulus onset when the nature of the next task is known in advance. Note that the pattern of findings can be also reconciled with predictions derived from Logan’s account (e.g., Logan & Bundesen, 2003), as previously discussed.

Findings for the drift rates are displayed in the middle panel of Figure 5. The pattern is described by two main effects: Higher drift rates occurred in task-repeat trials as compared with task-switch trials. Furthermore, drift rates were higher in the group with fixed task-sequence than in the group with random task-sequence. In fact, there was a main effect of task predictability, $F(1, 34) = 23.51$, $p < .001$ and a main effect of the task-switching factor, $F(1, 34) = 84.93$, $p < .001$. The interaction of both factors was not significant, $F(1, 34) = 1.70$, $p = .20$. Post hoc tests revealed significant differences between all four conditions (all $p < .05$).

Findings in the drift rate support an account in terms of task readiness: Replicating the findings obtained in Experiment 1, drift rates were generally higher in task-repeat trials relative to task-switch trials, which may reflect more automatic carry-over effects from the previous trial. Additionally, there was also a main effect of task predictability in this experiment. This supports the notion that controlled task-set activation also contributes to task readiness. How and by which means task expectancy may contribute to task readiness will be addressed in detail in the discussion of Experiment 3.

Findings for response caution (Figure 5, upper panel) were also informative. Participants generally chose a more cautious response criterion in the task-switching blocks than in the task-homogenous blocks. Replicating Experiment 1, participants adjusted their response criterion on a trial-by-trial basis, but only in the group with fixed task-sequence. Participants in the unpredictable task-sequence group chose an equally cautious response criterion for all trials. The analysis of variance conducted across trials of the task-switching blocks revealed no main effect of task predictability, $F(1, 34) = 1.47$, $p = .23$, a significant main effect of the task-switching factor, $F(1, 34) = 23.34$, $p < .001$, and a pronounced interaction of task switching and task predictability, $F(1, 34) = 21.81$, $p < .001$. Post hoc tests confirmed that the main effect of the task-switching factor was completely driven by the fixed task-sequence group, $t(17) = 5.63$, $p < .001$. In contrast, participants in the random-sequence group did not adjust response criteria between task-switch and task-repeat trials, $t(17) = 0.15$, $p = .88$. Participants in the fixed-sequence group only chose more liberal (i.e., less cautious) response criteria than in the random sequence group in task-repeat trials, $t(34) = 3.08$, $p < .01$, but not in task-switch trials, $t(34) = -0.44$, $p = .66$.

The pattern of findings seems to corroborate that participants can adjust response criteria on a trial-by-trial basis, suggesting that a higher degree of response caution is exercised in error-prone task-switch trials relative to task-repeat trials (see also Karayanidis et al., 2009). Note that automatic as well as controlled factors may have contributed to this difference between both groups: In the
explicit cueing paradigm, stimulus positions were not confounded with task switches and repetitions, thereby ruling out an effect of an automatically elicited adjustment of response caution triggered by stimulus position (which could have contributed to the effect in the alternating-runs paradigm). Controlled adjustment in the interval prior to stimulus onset was not possible in the explicit cueing paradigm, either, as task cues were presented simultaneously. In contrast, in the alternating runs paradigm, the time prior to stimulus onset could have been used for adjusting response caution. Additionally, the extreme regularity of the alternating runs procedure would have theoretically allowed one to use undulating response criteria (e.g., in form of a regular a sinus wave), possibly influenced by endogenous updating and control processes. Therefore results of the present experiment cannot answer the question whether adjustment in response caution only takes place in the alternating-runs paradigm or whether trial-to-trial adjustment is generally possible—when given the opportunity (see Karayanidis et al., 2009).

The results of Experiment 2 can be summarized as follows: In the alternating runs group, the nondecision parameter was comparable in task-switch and task-repeat trials, replicating the absence of an effect in this paradigm. In contrast, there was a marked increase in the nondecision time parameter in switching trials in the group who performed the explicit cueing paradigm without advance task cues. The pattern can be reconciled with the suggested interpretation that the nondecision time parameter captures time for task-set preparation when preparation is not possible prior to stimulus onset.

Additionally, the focusing requirement could have contributed to lower drift rates in the random sequence group. Given that response selection can begin even before the stimulus is perfectly focused, it can be expected that at least the efficiency of information uptake would be reduced. Evidence in this direction comes from research in visual perception, where it has been shown that lexical processing of stimuli presented in the periphery of the visual field is possible in principle, but at a far lower rate (Lee, Legge, & Ortiz, 2003; see also Rayner & Bertera, 1979).

Experiments 3a–3c

The purpose of Experiments 3a–3c was to replicate the effects obtained in Experiment 2 while avoiding some of the confounding factors. Particularly, we wanted address the marked effect in the nondecision time parameter, which was interpreted as reflecting preparatory processes at an earlier phase of a task-switch. In Experiments 3a to 3c, we used explicit cueing paradigms with random task sequences in all groups and manipulated task predictability by varying the CSI between groups.

Experiment 3a was a close replication of Experiment 2. But this time, the digit-letter pairs were randomly presented in one of the four fields of the $2 \times 2$ grid in both groups. Task repetitions and task switches were equiprobable in all trials (avoiding location repetitions). The random task sequences in both groups would help remove a possible confound of endogenous task cueing and long-term strategies (which could have played a role in the fixed AABB sequence group in Experiment 2). Task predictability was manipulated between groups by indicating the location of the next stimulus (and thus the nature of the upcoming task) either at the beginning of the RSI or simultaneously with the onset of the imperative stimulus.

In Experiment 3b, the focusing requirement was removed by presenting all stimuli in the center of the screen and using background colors as task cues. Again, task switches and task repetitions were equiprobable (and so were color-cue repetitions and switches). The primary motive of this experiment was to investigate whether the marked increase in the nondecision parameter in unpredictable task switches would still occur after removing the focusing requirement.
Experiment 3c, was to address the question whether encoding of the task cue could have contributed to the effect in the nondecision parameter. For instance it was argued that there is a bottleneck for encoding percepts to working memory (e.g., Jolicoeur, 1999). Given that task cue and stimulus are encoded sequentially, stimulus processing would have to wait until the task cue is encoded, thereby increasing 0% in the group with a CSI of 0 ms. However, cue-encoding time would be greatly reduced in task-repetition trials in which also cues repeat (because of persisting object files, Kahneman, Treisman & Gibbs, 1992; or repetition priming, Logan & Bundesen, 2003), thereby contributing to differences in the nondecision parameter between switching and repetition trials. In an attempt to reduce possible effects of an encoding bottleneck we increased the time between task cue and imperative stimulus in the short CSI group to 100 ms. This should give (at least basic processes of) cue encoding plenty of time; but we hoped that 100 ms would not suffice to accomplish full preparation of the upcoming task.

Another aim of the current experiments was to follow-up the finding that people seem to adopt different response criteria when they can predict whether the next task will be a task switch or a task repetition. Note however, that this effect was only observed in the previous studies when participants performed an alternating runs paradigm. Therefore, evidence of an intentional trial-to-trial adjustment is not unequivocal, and more automatic effects cannot be ruled out: First, there may have been operant learning of which stimulus positions are associated with higher (respectively lower) task difficulty or errors. Second, the extreme regularity of the task sequence in the alternating runs paradigm (AABB) could have been task repetition. Note however, that this effect was only observed in one experimental group, the background color of the field was changed to that of the next task directly after giving a response (i.e., 600 ms prior to stimulus onset). In the other group the background of the field turned to neutral gray (RGB 220, 220, 220; the same tone as the rest of the screen) directly after a response and the new background color only appeared simultaneously with the imperative stimulus.

Experiment 3c procedure. The procedure was identical to that of Experiment 3b with the one exception that in the short cue-stimulus interval group the time between task cue and imperative stimulus was set to 100 ms (in the long CSI group the interval remained unchanged at 600 ms).

Methods

Samples. Twenty-four research volunteers (18 female) participated in Experiment 3a. Most of them were students with different majors. On average they were 21.4 years old (SD = 1.5, range 19–24). Thirty-four participants (23 female) volunteered in Experiment 3b. All of them were students with different majors. Their mean age was 22.9 years (SD = 3.2, range 19–33). In Experiment 3c there were 34 participants (24 female). Their mean age was 22.3 (SD = 3.4); all were students with different majors. In all three experiments, participants reported that their vision was normal or corrected to normal. Participants received 7.00 Euros or partial course credit as compensation.

Experiment 3a procedure. Experiment 3a was a close replication of Experiment 2, with the exception that participants in both groups had to complete random task lists with a fixed response stimulus interval of 600 ms. The only difference between both groups was the time available for task preparation: One group was presented task cues in advance, whereas the other group received task cues simultaneously with the imperative stimulus. A task cue was used that did not perceptually interfere with the digit-letter stimulus, even under simultaneous presentation: the background color of the field where the next stimulus would appear was changed from light gray to light yellow (RGB 255, 255, 130). The background of the field returned to gray after the correct response had been entered.

Experiment 3b procedure. All stimuli were presented in the center of a rectangle in the middle of the screen, identical in shape and size to one of the four fields of the 2 × 2 grid in the previous experiments. This field remained visible throughout the experimental blocks to facilitate focusing the position where the next stimulus would appear. The background color of the rectangle served as a task cue: A light yellow tone (RGB 255, 255, 130) indicated that the digit was relevant, whereas a light blue tone (RGB 130, 255, 255) indicated that the letter was relevant. Task predictability was manipulated between participants. In one experimental group, the background color of the field was changed to that of the next task directly after giving a response (i.e., 600 ms prior to stimulus onset). In the other group the background of the field turned to neutral gray (RGB 220, 220, 220; the same tone as the rest of the screen) directly after a response and the new background color only appeared simultaneously with the imperative stimulus.

Results and Discussion

Latencies and errors. Latency and error data are depicted in Figure 6 for Experiments 3a, 3b, and 3c in the left, middle and right column, respectively. In Experiment 3a 9.3% of all trials were excluded (7.4 as post error trials; 1.9% as RT outliers); in Experiment 3b 8.2 (5.9; 2.3) percent trials were excluded; and in Experiment 3c 10.4 (7.5; 2.8) percent were removed prior to analysis. Latency and error data from the task-switching blocks were each submitted to a mixed analysis of variance with the between participants factor CSI (the long CSI was 600 ms in all three Experiments; the short CSI was 0 ms in Exp. 3a and 3b, but 100 ms in Exp. 3c) and the within participants factor task switching (task-switch vs. task-repeat trials).

The latency data are displayed in the upper part of Figure 6. As can be seen, latencies were increased in switch trials, and this effect was more pronounced in the short CSI group. Note however, that this effect was more pronounced in Experiments 3a and 3b, whereas differences between groups were reduced in Experiment 3c. The latter was accounted for by both, faster responses in the short CSI group (recall that CSI was 100 ms now), but to a lesser extent also by the longer latencies in the long CSI group (which were not predicted, as CSI was identical as in the previous experiment). In Experiment 3a, there was a main effect of CSI group, \( F(1, 22) = 20.67, p < .001 \), an even more pronounced main effect of the task-switching factor, \( F(1, 22) = 139.51, p < .001 \), and an interaction effect of both factors, \( F(1, 22) = 14.45, p < .01 \). The effect of CSI group was larger in task-switch trials than in task-repeat trials, but all four means differed significantly from each other as found in separate t-tests (all \( p < .01 \)). In Experiment 3b, there was also a main effect of CSI group, \( F(1, 32) = 32.80, p < .001 \). We thank one of the reviewers for pointing out this potential problem.

4 We thank one of the reviewers for pointing out this potential problem.

5 One of our subjects in Experiment 3b reported to have been diagnosed with an impairment in color vision, but she did not experience difficulty in distinguishing blue and yellow color tones.
.001, a main effect of the task-switching factor, $F(1, 32) = 96.90$, $p < .001$, and an interaction effect, $F(1, 32) = 8.78$, $p < .01$. Again, all four means were significantly different from each other (all $p < .001$). In Experiment 3c, there was no reliable effect of CSI group, $F(1, 32) = 2.17$, $p = .15$, there was an effect of the task-switching factor, $F(1, 32) = 75.70$, $p < .001$, but the interaction of both factors did not reach significance, $F(1, 32) = 2.50$, $p = .12$.

Error data are displayed in the lower part of Figure 6; they follow a similar pattern as the latencies. In Experiment 3a, there was a main effect of task switching, $F(1, 22) = 48.14$, $p < .001$, no main effect of CSI ($F < 1$), and an interaction effect of both factors, $F(1, 22) = 4.70$, $p < .05$. In Experiment 3b, there was a main effect of task switching, $F(1, 32) = 38.62$, $p < .001$, no main effect of CSI ($F < 1$), and a marginal interaction effect of CSI and task switching ($F(1, 32) = 4.07$, $p < .05$, one-tailed). In Experiment 3, there was no main effect of the CSI factor, $F(1, 32) = 0.03$, $p = .87$, but there was an effect of the task-switching factor, $F(1, 32) = 29.58$, $p < .001$, the interaction of both factors was not significant, $F(1, 32) = 2.82$, $p = .10$.

**Parameters of the diffusion model.** Results for the parameters of the diffusion model are graphically depicted in Figure 7. Separate analyses of variance were computed for each parameter of the diffusion model. CSI was entered as a between participants factor, and task switching (switching vs. repetition) was entered as a within participants factor.

**Nondecision parameter ($d_0$).** In the nondecision parameter, the characteristic pattern obtained in Experiment 2 was replicated: Throughout Experiments 3a to 3c, there was a marked increase in the nondecision parameter only in task-switch trials in the short CSI group. In Experiment 3a, there was a main effect of CSI group, $F(1, 22) = 59.38$, $p < .001$, a main effect of task switching, $F(1, 22) = 44.22$, $p < .001$, and an interaction of both factors, $F(1, 22) = 42.55$, $p < .001$. Post hoc tests confirmed that the interaction effect was driven by a selective increase in unpredictable switch trials. There was a clear effect of CSI in task-switch trials, $t(22) = 8.87$, $p < .001$ but not in task-repeat trials, $t(22) = 1.30$, $p = .21$ Only in the short CSI group was there an increase in the task-switch trials as compared with task-repeat trials ($t(11) = 7.21$, $p < .001$), but not in the long CSI group, $t(11) = 0.15$, $p = .88$.

Similarly, in Experiment 3b, there was a main effect of CSI group, $F(1, 32) = 32.22$, $p < .001$, a main effect of task switching, $F(1, 32) = 45.69$, $p < .001$, and an interaction of both experimental factors, $F(1, 32) = 74.99$, $p < .001$. Post hoc tests confirmed that there was an effect of CSI group only in the task-switch trials, $t(32) = 8.04$, $p < .001$ but not in task-repeat trials, $t(32) = 0.42$, $p = .68$. There was a substantial increase from task-repeat trials to task-switch trials only in the short CSI group, $t(16) = 9.95$, $p < .001$, but not in the long CSI group, $t(16) = -1.50$, $p = .15$; note that the effect points in the unexpected direction.

In Experiment 3c, there was a main effect of CSI, $F(1, 32) = 11.32$, $p < .001$, a main effect of the task-switching factor, $F(1, 32) = 15.09$, $p < .001$, and an interaction of both factors, $F(1, 32) = 16.01$, $p < .001$. Post hoc tests revealed that the nondecision parameter was only increased in the short CSI group in switching trials, $t(32) = 4.05$, $p < .001$, whereas no effect of CSI was found in the task-repeat condition, $t(32) = 0.42$, $p = .68$. In the long CSI group, estimates of the nondecision parameter were comparable in task-switch and task-repeat trials, $t(16) = 0.14$, $p = .89$. Only in the short CSI group was there a significant increase in switching trials as compared with task-repeat trials, $t(16) = 4.36$, $p < .001$.

Additionally, we submitted data from Experiment 3a–3c to a joint ANOVA with the previous two factors, CSI group and task switching, and additionally experiment (Experiment 3a vs. 3b vs. 3c) as a possible moderating factor. There was neither an interaction of the experiment factor with task switching, $F(2, 86) = 1.10$, $p = .29$. 

*Figure 6. Experiment 3a, 3b, and 3c–Latencies and Errors. Tasks were presented in random sequence in the mixed blocks and with a fixed RSI of 600 ms. Task cues were either spatial (Exp. 3a) or background colors (Exp. 3b and 3c). In all experiments, the group with long CSI saw the task cue 600 ms prior to onset of the imperative stimulus. In the groups with short cue stimulus interval the CSI was 0 ms (Exp. 3a and 3b) or 100 ms (Exp. 3c). Performance data are displayed for each group, separately for each trial type (task pure, switching, repetition). Error bars indicate standard errors.*
p = .34, nor with CSI group, $F(2, 86) = 1.23, p = .30$, nor was there a three-way interaction of all three factors, $F(2, 86) = 1.82, p = .17$.

To summarize, results in the nondecision parameter were clear cut. There was always an increase in the nondecision parameter in unpredictable switching trials. This supports the previously suggested interpretation of the nondecision parameter as reflecting preparatory processes additionally inserted (or at least lengthened) on task-switch trials (see also De Jong, 2000; Rogers & Monsell, 1995; Rubinstein et al., 2001). As the effect occurred in Experiments 3a throughout 3c, the selective increase in the nondecision parameter cannot be caused solely either a focusing requirement or sequential encoding of task cue and stimulus. But it can be noted that the effect slightly decreased from Experiment 3a to Experiment 3c (see Figure 7; although the decrease was not significant), which can be interpreted as evidence for at least a partial effect of the latter factors. Note however, that reduced effects in Experiment 3c can also reflect at least partial preparation of the new task set prior to stimulus presentation in the 100 ms CSI. The latter is not unlikely, given that effects in the nondecision parameter were completely removed when participants had 400 ms time to prepare the upcoming task (as in Experiment 2). Note that the choice of a CSI of 100 ms in the short CSI group in Experiment 3c should have ruled out possible effects of a bottleneck at early stimulus encoding. However, some sort of “higher cue encoding” (including the retrieval of mediator cues; Logan & Schneider, 2006) may not have been finished by that time, thereby offering an alternative explanation of the group difference in the nondecision parameter.

**Drift rates ($\nu$).** In the drift rates, there was an effect of the task-switching factor in all Experiments, indicating higher drift rates in task-repeat trials. Additionally, there was an effect of the CSI, with generally higher drift rates when a long CSI left more time to prepare for the upcoming task. In Experiment 3a, there was a main effect of CSI group, $F(1, 22) = 7.24, p < .05$, a main effect of the task-switching factor, $F(1, 22) = 33.80, p < .001$, but there was no interaction effect ($F(1, 22) < 1$). Post hoc tests confirmed significant differences between all means (all $p < .05$). Similarly, in Experiment 3b, there was a main effect of CSI group, $F(1, 32) = 10.47, p < .01$, a main effect of the task-switching factor, $F(1, 32) = 32.68, p < .001$, but no interaction effect ($F(1, 32) < 1$). Again, all four pair-wise post hoc tests were significant (all $p < .05$). In Experiment 3c, there was a main effect of the task-switching factor, $F(1, 32) = 96.60, p < .001$, there was no main effect of the CSI factor, $F(1, 32) = 0.50, p < .47$, but there was an interaction of both factors, $F(1, 32) = 5.99, p < .05$. The latter was accounted for by a significant effect of CSI in task switching trials, $t(32) = 2.11, p < .05$, but the absence of an effect in task-repeat trials, $t(32) = 0.41, p = .69$. 

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**Figure 7.** Experiment 3a, 3b, and 3c–Parameters of the Diffusion Model. Tasks were presented in random sequence in the mixed blocks with a constant RSI of 600 ms. Spatial task cues (Exp. 3a) or color task cues (Exp. 3b and 3c) were presented 600 ms prior to the imperative stimulus in the group with long CSI, and simultaneously (CSI = 0 ms; Exp. 3a and 3b) or 100 ms before stimulus onset (Exp. 3c) in the group with short CSI. Estimates of response caution ($\alpha$), drift rate ($\nu$), and the nondecision parameter ($\theta$) are displayed in separate panels. Separate estimates are displayed for both groups and for each trial type (task pure, switching, repetition). Error bars indicate standard errors.
Findings obtained in Experiments 3a to 3c seem to support the previously suggested interpretation that task readiness contributes to the drift rate (among other factors, such as stimulus characteristics, which contribute to task difficulty in task-switch and task-repeat trials alike). Task readiness was considered as jointly determined by automatic and controlled processes (e.g., Koch & Allport, 2006; Yeung & Monsell, 2003a). As previously argued, the effect of trial type (switching vs. repetition) can be reconciled with automatic carry-over effects from the previous trial. For instance, higher drift rates in task-repeat trials may reflect a particular strong activation or biasing of the relevant task set. Conversely, lower drift rates in case of task switches may result from an incomplete task-set activation or a distraction from a competing task set that may impair the efficiency with which stimulus information can be used to select the response. We also argued that the effect of task predictability indicates that controlled factors contribute to task readiness and thus influence drift rates. However, we have not discussed how task predictability might affect task readiness—and why task predictability can have a beneficial effect on both, task switch and task repeat trials. This will be addressed next.

Advance task knowledge permits preparatory processes prior to stimulus onset like the activation of the relevant task set and the inhibition of the irrelevant task set (Mayr & Keele, 2000; Karayanidis et al., 2009). But the level of task-set preparation (biasing) may depend on circumstances. For instance, it was argued that preparation is adaptive and used to overcome the effects of task-set inertia (see Koch & Allport, 2006; Yeung & Monsell, 2003a). Additionally, it was reasoned that a task set may only just be activated to the point at which the appropriate task is performed rather than the competing one on most occasions (Monsell et al., 2003; see also Gilbert & Shallice, 2002; Yeung & Monsell, 2003b; for related ideas that participants terminate preparation at some point in time in order to proceed with response selection see also Mayr & Liebscher, 2001; Meiran et al., 2000). Such a satisfying strategy could help save time and cognitive effort—the question is only when it would be used.

Consider task switches first. Participants in our experimental group with advance task knowledge could use the entire CSI of 600 ms for preparation of the next task. It is reasonable to assume that this is plenty of time which allows a relatively complete task-set preparation. In contrast participants in the group without task knowledge could only prepare the upcoming task set after stimulus onset (which corresponds to the increase in nondecision time in that condition, as discussed). In this case, participants may use a satisfying strategy and only prepare the relevant task set until some liberal criterion is met. Reduced task readiness may therefore contribute to the lower drift rates in this group.

But task predictability was also shown to influence drift rates in task-repeat trials. In this condition, participants in the group who could predict the next task set in advance may have simply maintained the last task set highly activated. In contrast, participants in the group who could not predict the next task set may have refrained from such a strategy as it would maximize inertia costs in task-switch trials. Again, the relevant tasks set would be less prepared in this group which would explain lower drift rates (see Experiment 3a and 3b).

Related to the previous mechanisms, task preparation may be moderated by a feeling of perceived control and confidence. Given sufficient time for preparation, advance task knowledge may encourage participants to strongly activate task sets in all trials, thereby contributing to high drift rates in task switches and repetitions. However, in case of task uncertainty, participants may refrain from activating cognitive sets too strongly. In fact, it was argued that excessive control input can be maladaptive when situational demands change in an unpredicted fashion (Goschke, 2000). This would contribute to the lower drift rates in task switches as well as task repetitions.

Note however, that in Experiment 3c, drift rates in the group with a CSI of 100 ms were only reduced in switching trials but not in repetition trials. A cautious interpretation of this effect could be that it reflects different levels of difficulty in task preparation: whereas 100 ms may not suffice to accomplish preparation of a new task set in switching-trials (there was also still the effect in the nondecision parameter), it may just have been sufficient time to reactivate the previous task set again, so that sampling could proceed at maximum efficiency.

Response criterion (a). Findings for the response criterion were comparable across experiments and conceptually replicated the pattern obtained in Experiment 2. Response criteria were generally higher in the task-switching blocks as compared with the task-homogeneous blocks. But participants who could predict the nature of the next trial adjusted their response criteria on a trial-by-trial basis. In contrast, participants who received task cues only simultaneously with stimulus onset chose equally high response thresholds in all trials.

In Experiment 3a, there was neither a main effect of CSI group, $F(1, 22) = 2.07, p = .17$ nor a main effect of the switching factor, $F(1, 22) = 1.23, p = .28$. But there was a significant interaction between both factors, $F(1, 22) = 7.35, p < .05$. Post hoc test confirmed that more liberal response criteria were only chosen in case of predictable task repetitions: CSI group only had an effect in task-repeat trials, $t(22) = 2.53, p < .05$, but not in task-switch trials, $t(22) = 0.07, p = .95$. Only the long CSI group adjusted their response criteria between task-repeat and task-switch trials, $t(11) = 2.35, p < .05$, but not the short CSI group, $t(11) = -1.37, p = .20$.

In Experiment 3b, there was a main effect of CSI group, $F(1, 32) = 6.35, p < .05$, a main effect of the task-switching factor, $F(1, 32) = 7.95, p < .01$, and a significant interaction of both factors, $F(1, 32) = 13.29, p < .01$. Post hoc test revealed that both main effects were driven by the pronounced selective decrease in the response criterion as CSI increased in predictable task-repeat trials. In fact, there was a significant decrease in response criterion only in task-repeat trials, $t(32) = 3.64, p < .01$, but not in task-switch trials, $t(32) = 1.09, p = .29$. Additionally, only the long CSI group adjusted response criteria on a trial-by-trial basis, $t(32) = 4.62, p < .001$, but not the short CSI group, $t(32) = -0.58, p = .57$.

In Experiment 3c, there was no main effect of CSI group, $F(1, 32) = 0.32, p = .65$, a main effect of the task-switching factor, $F(1, 32) = 23.09, p < .001$, and a significant interaction of both factors, $F(1, 32) = 6.24, p < .05$, indicating that participants in the long CSI group reduced their response caution more strongly in repetition trials. In fact, the long CSI group reduced their response caution in repetition trials relative to switching trials substantially, $t(16) = 4.93, p < .001$, but also participants in the short CSI group tended to decrease response caution in switching trials, $t(16) = $
1.72, \( p = .11 \), possibly indicating that adjusting response caution can be accomplished within 100 ms in some cases. This had the effect that the difference between groups did not reach significance in repetition trials, \( t(32) = 1.44, p = .16 \). Response caution was not different between groups in switching trials, \( t(32) = 0.23, p = .82 \).

To conclude, the pattern in response caution was consistent across experiments: Response caution was generally high in the mixed task blocks. But participants lowered response caution when they knew in advance that the next task would be a less difficult repetition trial. It is important that results from Experiments 3a through 3c indicate that the trial-to-trial adjustment takes place (a) intentionally and (b) during the limited time of the CSL. (a) This can be inferred from the finding that the effect occurred even though task cues (positions in Experiment 3a, colors in Experiment 3b and 3c) were not confounded with task switches and repetitions, thereby excluding possible effects of an operant learning of response caution. (b) Additionally, task sequence was random now, ruling out any endogenous form of long-term or “strategic” effects of regulating response caution (e.g., in form of a sinus-wave). Note that trial-to-trial adjustment of response caution was also found in the study by Karayanidis and colleagues (2009). But in their study, response caution was only increased in predictable task switches—not in task switches that followed noninformative cues (in which task repetitions could follow with equal probability). This suggests that participants increased response caution only in the study by Karayanidis and colleagues (2009) when they were certain that a difficult task switch would follow. Results obtained in the current study imply that participants generally exercise high levels of response caution in the task-switching blocks relative to task-pure blocks. However, when participants are certain that the next trial will be an easy task-repetition, they chose somewhat more lenient response criteria. A possible reason for these differences may result from procedural specificities of the respective switching paradigms (Yehene & Meiran, 2007).

**Summary.** Experiments 3a–3c replicated findings obtained in Experiment 2 avoiding confounding factors such as formal affordances, including endogenous task-cueing, extreme predictability, long-term-strategies, the focusing requirement and possible effects of sequential encoding of task cue and stimulus. But there was still a selective increase of the nondecision parameter in unpredictable task-switch trials throughout this series of experiments. This corroborated the notion that some form of task preparation is captured by the nondecision parameter, like the retrieval of that task set from long-term memory (Arbuthnot & Woodward, 2002; Goschke, 2000; Mayr & Kliegl, 2000, 2003) or the biasing of relevant task-demand units (Gilbert & Shallice, 2002). At the same time, the virtual absence of an effect when switches are predictable also supports the view that this component of task-set preparation can be completed prior to stimulus onset when the nature of the next task is known in advance. Note that priming of cue-encoding (e.g., Logan & Bundesen, 2003) would also contribute to the observed pattern of effects.

Drift rates were influenced by both, the type of task transitions (switching vs. repetitions) and advance task knowledge. The first was interpreted as reflecting more automatic carry-over effects and the latter a more strategic process of task-set activation. Both should affect the level of task readiness which determines the efficiency with which information from the stimulus is used to select the response. Additionally, the effect in the response criterion confirms that participants can adjust response caution on a trial-to-trial basis (as observed in Experiments 1–3; Karayanidis et al., 2009), but only when the type of task transition can be predicted.

**Experiment 4**

Carry-over effects from previous trials can be described by two fundamental characteristics: their congruency with constraints of the actual task set as well as their strength. Congruency effects were addressed in the previous series of experiments: Drift rates clearly reflected the type of task transitions. We argued that higher drift rates in case of task repetitions (and lower drift rates in case of task switches) reflect, in part, carry-over effects from the previous task set that may be beneficial (or adverse) depending on congruency with constraints of the current task set or its associated mapping rules (e.g., Gilbert & Shallice, 2002; Mayr & Kliegl, 2003). But it was also stated that the activation of a task set decays with time since its last application (Altmann & Gray, 2008; Yeung & Monsell, 2003b). This implies that stronger carry-over effects (both beneficial and adverse) from the previous task set are predicted when the time since its last application is short. The notion of a passive inertia decay was supported in studies in which the response stimulus interval was manipulated while keeping the time for active task-set preparation constant (Meiran et al., 2000; Ruthruff et al., 2001). Also the form of the decay process was investigated. Sohn and Anderson (2001) successfully fitted models of task-switching, assuming that task-set activation follows an exponential decay. Relatedly, Ruthruff and colleagues (2001) found that latencies increased linearly with the log of trials since the actual task set was performed last. The present experiment was conducted to investigate the effects of passive inertia decay on the parameters of the diffusion model. To this end, we manipulated the RSI between participants in two groups (400 ms vs. 1000 ms) while not allowing for advance preparation in either of the groups.

We predict that the RSI manipulation should affect the strength of proactive interference from the previous task set. In case of task repetitions, there should be a particularly strong activation of the relevant task set (or mapping rules) when the CSI is short, resulting in high drift rates. Conversely, in case of task switches, strong proactive interference from the competing task set (or mapping rules) in case of a short RSI should result in a situation where the relevant task set is less clearly biased, thereby leading to particularly low drift rates. Effects in the same direction would be expected when the RSI is increased, but their magnitude would be reduced because of inertia decay.

As the previous experiments suggest that the drift rate is also influenced by task expectancy, we presented task cues simultaneously with stimuli in both groups, thereby not allowing for advanced preparation. This would also remove the problem that active task preparation may be used to overcome the effects of passive task-set inertia (Koch & Allport, 2006; Yeung & Monsell, 2003a), thereby reducing the effects of interest in the present study. Stimuli were again presented in a 2 × 2 grid and stimulus location determined which classification to perform. As task expectancy was removed in both groups, spatial focusing could not contribute to differences between experimental groups. Similarly, the unpredictability of the next task could...
lead to a generally more cautious response strategy (Mayr, 2001; Mayr & Liebscher, 2001); but this factor was not expected to differ between groups, either.

The following predictions for the parameters of the diffusion model result: There should be neither a difference in the nondecision parameter nor in the response criterion between both RSI groups: As neither group knew the next task set in advance, neither group could prepare the new task set in switching trials. Therefore an increase in the nondecision parameter in task-switch trials was expected in both groups. The absence of task predictability should also lead to a comparable level of response caution in switching and repetition trials in both groups. But we predicted a difference in the drift rate parameter, as already argued: As carry-over effects should be generally larger in the short RSI group than in the long RSI group, the previously observed effects of type of trial transition (switching vs. repetition) should be more pronounced in the short RSI group than in the long RSI group.

Method

Sample. There were 60 participants (48 female) in this study. Most of them were students. On average they were 22.2 years old (SD = 3.3, range 18–38). They reported normal or corrected-to-normal vision. They received 7.00 Euros or partial course credit as compensation.

Procedure. The dissipation of task-set inertia was investigated by manipulating the length of the response-stimulus-interval between two groups (400 ms vs. 1000 ms). Apart from that, the procedure was identical with the simultaneous task cueing group in Experiment 3a. The background color of the field in which the stimulus occurred turned yellow simultaneously with the onset of the imperative task stimulus.

Results and Discussion

Latencies and errors. A total of 10.9% of all trials were excluded (8.5 as posterror trial; 2.4 as RT outlier). Data from the task-switching blocks were entered into an analysis of variance with RSI (400 ms vs. 1000 ms) as a between-participants factor and task-switching (switching vs. repetition) as a within-participants factor.

Results for the latency and error data are displayed in Figure 8. As can be seen, there was a general increase in latencies in the task-switch trials (as compared with task repeat trials), but the increase was more pronounced in the short RSI group. This pattern was confirmed by a significant main effect of the task-switching factor, $F(1,58) = 328.25, p < .001$ and a significant interaction of task-switching and RSI, $F(1, 58) = 4.55, p < .05$. The main effect of RSI was not significant ($F(1, 58) < 1$). Post hoc tests confirmed the main effect of the task-switching factor in both RSI groups (both $p < .001$). The effect of RSI tested separately within task-switching and task-repeat trials missed significance; however, in line with predictions the effect was larger in the task-switching condition, $t(58) = 1.33, p = .09$, one-tailed than in the task-repetition condition, $t(58) = 0.28, p = .78$. An analogous pattern of results was obtained for the errors, but the only main effect of the task-switching factor was significant, $F(1, 58) = 94.53, p < .001$. Neither the main effect of RSI group, $F(1, 58) = 1.18, p = .28$, nor the interaction of both factors ($F < 1$) reached significance.

Parameters of the diffusion model. Parameters of the diffusion model were entered into analyses of variance with between-participants factor RSI (400 ms vs. 1000 ms) and within-participants factor task-switching (switching vs. repetition). Mean parameter values are depicted in Figure 9.

For the nondecision parameter (lower panel of Figure 9), a highly comparable pattern was obtained for both experimental groups: There was a selective increase in the task-switching condition that was not affected by RSI. This was confirmed by a significant main effect of the switching factor, $F(1, 58) = 154.73, p < .001$. Neither the main effect of RSI group ($F(1, 58) < 1$) nor the interaction of RSI and the task-switching factor were significant ($F(1, 58) < 1$). Post hoc tests confirmed the effect of the task-switching factor in both RSI groups (both $p < .001$); but neither in task-switch trials nor in task-repeat trials was there an effect of RSI group (both $p > .12$). The pattern of effects was in line with predictions: Neither of the groups could predict the nature of the next task, necessitating task-set preparation after stimulus onset in case of task switches. This accounts for the increase in the nondecision parameter in task-switch trials in both groups. Moreover, it was shown that a manipulation of RSI (supposed to affect inertia) did not influence the parameter. Along with findings obtained in the previous experiments, this supports the idea that the nondecision parameter is specifically sensitive for task-set preparation.

The pattern in the response criterion also confirmed predictions (see upper panel of Figure 9). Response caution was generally higher in trials of the task-switching blocks than in task-pure blocks, but neither group adjusted response criteria on a trial-by-trial basis. The analysis of variance conducted across trials of the task-switching block did not reveal any significant effect. There was neither a main effects of trial type nor of RSI group (both $F(1, 58) < 1$), and the interaction of both factors was not significant.
To conclude, in the absence of advance task cues, both experimental groups chose equally high response caution (1), and a significant interaction of both factors, $F(1, 58) = 110.23, p < .001$, no main effect of RSI group ($F < 1$), and a significant interaction of both factors, $F(1, 58) = 16.84, p < .001$. The interaction effect reflected that the short RSI group had increased drift rates on task-repeat trials, $t(58) = -1.71, p < .05$, one-tailed and decreased drift rates in task-switch trials, $t(58) = 1.86, p < .05$, one-tailed relative to the long RSI group. The task-switching factor was significant in both experimental groups, but in line with predictions, the effect was larger in the short RSI group, $t(29) = 11.1, p < .001$, Cohen’s $d = 4.1$ as compared with the long RSI group, $t(29) = 4.2, p < .001$, Cohen’s $d = 1.6$.

To conclude, the results confirmed the predictions for the drift rate parameter. In particular, the pattern of the interaction effect of task-switching and RSI seemed to indicate that proactive interference from the preceding trial is generally more pronounced when the RSI is short than when the RSI is long, and that its impact is beneficial in task-repeat trials but detrimental in task-switch trials.

Results of Experiment 4 can be summarized as follows: In the absence of advance task cues, there was evidence neither for advance task preparation nor for adjustment of response criteria on a trial-by-trial basis. This was reflected by a marked increase of the nondecision parameter in task-switch trials in both experimental groups, and the absence of an effect in the response criterion. Removing task predictability eliminated effects of controlled task-set activation on drift rates. The pattern observed in the drift rates supported the notion of task-set inertia and its decay across time: The previously observed beneficial effects in task-repeat trials and the adverse effects in task-switch trials (Experiment 1–3; Karayannis et al., 2009; Madden et al., 2009) occurred in both RSI groups. But in line with predicted decay of task-set inertia, they were reduced when more time had elapsed since the last task set was applied.

### General Discussion

In the present study we applied the diffusion model for binary decisions (Ratcliff, 1978; Ratcliff & Rouder, 1998) to different task-switching paradigms: the classical alternating-runs paradigm (Rogers & Monsell, 1995) and variants of the explicit cueing paradigm (e.g., Mayr & Kliegl, 2000; Meiran, 1996), demonstrating the usefulness of the diffusion-model analyses to tease apart mechanisms that contribute to the task-switching effect at different phases in a task-switch trial. Theoretically, the modeling exercise was motivated by current multiple-components models of task switching (Gopher et al., 2000; Mayr & Kliegl, 2003; Rubinstein et al., 2001; Ruthruff et al., 2001). According to these, a number of controlled and automatic factors contribute to the task-switching effect as briefly recapitulated next.

There is evidence that the switching effect reflects both, switching costs in task-switch trials as well as repetition benefits in task-repetition trials. Task-switch costs were classically accounted for by either the time it takes to reconfigure the mental system for the new task (Rogers & Monsell, 1995) or inertia effects (Allport et al., 1994; Allport & Wylie, 2000), including competitor priming and negative priming; also effects of stimulus task-set bindings were shown (Waszak et al., 2003, 2005). Additionally, a benefit was shown when task cues repeat in task-repeat trials (Logan & Bundesen, 2003). However, these mechanisms are assumed to operate at different phases in a task-switch trial: It was assumed that in an earlier phase some sort of task-set preparation takes place. According to reconfiguration theories controlled processes set constraints according to which stimulus processing and response selection take place in a later phase (Gilbert & Shallice, 2002; Mayr & Kliegl, 2000, 2003). In turn, the phase of response selection was considered less controlled (Ruthruff et al., 2001),
although it is likely that some sort of monitoring takes place which would allow late control processes. Also in Logan’s account two phases are postulated: The first comprises cue and stimulus encoding, the second phase starts when task cue and stimulus have formed a compound cue that is used to probe memory in order to retrieve the correct response (Logan & Bundesen, 2003; Schneider & Logan, 2005, 2009).

As theories converge that there are at least two separable phases in a task-switch trial, one corresponding to preparation or encoding, the other to response selection, the diffusion model seems well suited to decompose some of the processes taking place at these phases. In the present study we employed well-established paradigms of task switching that have been previously used to decompose theoretically postulated components of the task-switching effect by means of an experimental manipulation. In the following we will summarize how parameters of the diffusion model capture the effects of the manipulations—thereby revealing which processes may be captured by the parameters: Effects on the nondecision parameter ($\theta$) were consistent with the assumption that it reflects task-set preparation (additionally to basic encoding and motor processes necessary in all trial types). But higher-level cue encoding may offer an alternative explanation. Effects on the drift rate ($\nu$) are generally interpreted as reflecting response selection difficulty. Findings in the present study suggest that the parameter additionally captures task readiness which is jointly determined by controlled as well as automatic factors and the decay of task-set activation across time. Finally, there was also evidence that response caution ($a$) contributes to both global as well as local task-switching effects.

**Non-Decision Parameter ($\theta$)**

Previous studies have corroborated the view that the nondecision parameter ($\theta$) captures basic stimulus encoding and motor activity outside the actual decision process (e.g., Voss et al., 2004; Ratcliff et al., 2006). It is reasonable to assume that these processes have contributed to all trial types (task pure, switching, repetition) in the present study in a similar way. More important, the parameter was selectively increased in task-switch trials in which task-set preparation was not possible prior to stimulus onset. This suggests that task set preparation is reflected in the nondecision parameter (see also Karayanidis, 2009), which implies that the parameter captures as well some sort of higher cognitive processes. Given that the nondecision parameter reflects task-set reconfiguration (Rogers & Monsell, 1995) or reprogramming of the system (Ruthruff et al., 2001), the parameter may index the time required for top-down biasing of the relevant task-modules (Gilbert & Shallice, 2002) or retrieving the new task set or its associated S-R mapping rules from memory (Arbuthnott & Woodward, 2002; Goschke, 2000; Mayr & Kliegl, 2003). The absence of an increase in the nondecision parameter in switching trials when task preparation was possible by design or by advance task-cuing also implies that preparatory processes can be largely completed prior to stimulus onset, or at least those components of it that can be completed without the imperative stimulus. However, cue encoding (Logan & Bundesen, 2003) would also contribute to the nondecision parameter and could account for the differences between task-switch and task-repeat trials. If so, only some sort of higher-level cue encoding, such as mediator cue retrieval (Logan & Schneider, 2006), would offer a satisfactory explanation, whereas a bottleneck at basic encoding processes was ruled out in Experiment 3c.

As previously noted, findings concerning the nondecision parameter were inconclusive in the other recent task-switching studies using the diffusion model, which will be discussed here briefly. In the study conducted by Madden and colleagues (2009), the $\theta$ parameter was increased in task-switch trials relative to task-repeat trials in old and young participants—even though their cue stimulus interval of 1500 ms should have allowed accomplishing task-set preparation prior to stimulus onset. This might indicate that their semantic switching task was simply too complex to complete preparation in time. The task used by Karayanidis and colleagues (2009) was more comparable with ours, and they also found that the nondecision parameter was not increased in task switches that were cued by fully informative task cues. It is interesting they found that nondecision time was the more increased the less informative the task cue was (switch-to, switch-away, noninformative). The effect of variable cue informativeness at a constant CSI complements the evidence offered in the present study obtained with perfectly reliable task cues presented at variable CSI, and thus provides independent corroboration of the interpretation of the nondecision parameter as indicative of task-set preparation.

**Drift Rate ($\nu$)**

In the diffusion model, the drift rate ($\nu$) reflects the speed with which response evidence is accumulated. We predicted that the parameter relates to the later phase of the switching process in which stimulus information is used to select the correct response according to the currently activated task set (Mayr & Kliegl, 2003; Ruthruff et al., 2001). In previous research with task-pure paradigms, the drift rate has been interpreted as reflecting task easiness, task dominance, stimulus characteristics, or ability-related performance (e.g., Ratcliff et al., 2006; Voss et al., 2004). In the present experiments, we were more concerned with the impact of task-switching specific processes that may additionally contribute to the efficacy with which a response is selected. Specifically we argued that task-set biasing, or the activation of the relevant task set relative to the activation of the competing one (e.g., Gilbert & Shallice, 2002; Ruthruff et al., 2001) affects the efficiency with which stimulus information is sampled and the response is selected. In other words, task readiness was predicted to contribute to the drift rate. However, task readiness was considered the product of a number of factors, including controlled as well as more automatic ones (Koch & Allport, 2006; Rubinstein et al., 2001; Waszak et al., 2003, 2004, 2005; Yeung & Monsell, 2003a).

In the present series of experiments, we found drift rates affected by both, task sequence and task predictability. Such effects would not be predicted by Logan’s account (e.g., Logan & Bundesen, 2003) according to which cue-encoding would contribute to preparation time, but not to the random-walk process used for response selection.

Consider effects of task-sequence first. Drift rates were high in case of task-repeat trials, which was interpreted as evidence for beneficial carry-over effects from the previous trial that should result in a particular strong activation of the relevant task set or its associated mapping rules. In contrast drift rates were low in
task-switch trials in which adverse carry-over effects such as competitor priming and negative priming would contribute to a relatively low readiness for the currently relevant task set. Results thus support predictions derived from inertia accounts (e.g., Allport et al., 1994).

Consider next effects of task-set predictability. Note that drift rates describe the efficiency at the response selection phase, whereas task preparation was considered to take place at an earlier phase. Nevertheless, we argue in line with multiple-components models of task-switching that a more complete preparation or biasing of the relevant task will lead to superior task readiness, thereby facilitating response selection as soon as the stimulus appears. A number of mechanisms may contribute to the observed effect, in (1) task-switch as well as (2) task-repeat trials:

(1) In task-switch trials, participants who could predict the next task could have used the entire time prior to stimulus onset for task preparation; this would result in high task readiness and an efficient response sampling process. In contrast, participants who could not predict the new task set in advance had to engage in preparation after stimulus onset and might have used a satisfying strategy then (see Mayer & Liebscher, 2001; Meiran et al., 2000; Monsell et al., 2003). This would have saved time and effort—at the cost of a reduced task readiness.

(2) In task-repeat trials, participants who could predict that they would have to repeat the same task again could have actively maintained the task set in highly activated state. Participants who could not predict the task set would reasonably have refrained from this strategy, as it would maximize inertia costs in case of switching trials. Note that this may also account for the effect in the Karayanidis et al. (2009) study, in which drift rates were particularly strong in validly cued repetition trials as compared with repetition trials that were preceded by noninformative cues. In addition to the above described mechanisms, advance task knowledge may have led to an enhanced feeling of controllability which may have encouraged participants to activate task sets more completely. However, despite relatively consistent evidence for a role of task predictability in most of our studies, occasionally task unpredictability was not found to contribute to drift rates where it might have. In particular, in the Karayanidis et al. (2009) study, there was no effect of cue informativeness in switching trials, and in our Experiment 3c, the cue-stimulus interval had no effect in repetition trials. We argued that the latter may reflect that the slightly increased CSI in the short CSI group now sufficed to completely reactivate the old task set (but not to prepare a new task set in switching trials), thereby allowing evidence accumulation at comparably high efficiency in task-repeat trials even in the short CSI group. In any case, the composition of drift rates is complex, a multitude of factors was expected to contribute to the efficiency of evidence accumulation—and task expectancy would only have an indirect effect by strengthening task preparation in an earlier phase that would lead to increased task readiness at the phase of response selection.

Finally, we also tested for the effects of inertia decay on drift rates while not allowing for active task-set preparation (in the absence of advance task cues). As predicted, carry-over effects had the same signature as in the previous experiments, with higher drift rates in task-repeat relative to task-switch trials. But the effects were stronger when less time had elapsed since the last response.

**Response Criterion (a)**

Effects of response caution have been argued to contribute to the generally slower response times observed in task-switching blocks as compared with task-homogeneous blocks as a function of global control strategies (Mayr, 2001; Mayr & Liebscher, 2001; see also Fagot, 1994; De Jong, 2000). However, response caution has been largely disregarded in theorizing on local switching costs between task-switching and task-repeat trials within the same block. In the present study, we found evidence of both. Participants were generally more cautious in the task-switching blocks as compared with the task-pure blocks. But they also adjusted their response criteria within the task-switching blocks on a trial-by-trial basis whenever they could predict the nature of the next trial: More specifically, the pattern in the response criterion suggested that participants exercise generally high levels of response caution in the task-switching blocks relative to the single task blocks. But when they can predict that the next trial will be a comparatively easy task-repeat trial they reduce response caution relative to task-switch trials. In case participants cannot predict the type of task transition, they use high response caution in all trials that is comparable to that used in predictable task switches. Note that trial-to-trial adjustment in response caution was also found in the study by Karayanidis and colleagues (2009), but the pattern was slightly different. On the one hand, findings converge that response caution was generally higher in task-switch relative to task-repeat trials, but in case of noninformative task cues it was always as low as in task-repeat trials with fully informative task cues. This suggests that participants in the Karayanidis et al. (2009) study only increased response caution when there was certainty of a task-switch, whereas participants in our study seemed to be cautious by default and only slightly reduced response caution when there was certainty of an easy task-repetition. In the Madden et al. (2009) study, response caution was constrained across trials of the task-switching block, so that the reported results are nondiagnostic with respect to possible trial-to-trial adjustment of response caution.

In any case, the convergent finding of an anticipatory trial-to-trial adjustment of response caution is informative and contributes to our understanding of regulatory processes of speed-accuracy settings. Note that there is previous evidence of trial-to-trial adjustment in response caution in the response-incongruency/error processing literature (Botvinick et al., 2004; Jones et al., 2002). But according to these models, adjustment takes place in a post hoc fashion, following trials with response conflict: a focusing of attention, more top-down control, but also an increase in response caution. In a recent version, the connectionist model of conflict-control mechanisms was adapted to task-switching (Brown, Reynolds, & Braver, 2007). In this model it is assumed that two conflict-control loops, termed change detection and incongruency detection, exist that are distinct from the mechanisms that reconfigure task set. Especially change detection may be relevant here, as it biases performance toward accuracy (rather than speed) and exploration (rather than exploitation).

The conflict-control model can contribute to an explanation of trial-to-trial adjustment of response caution. However, the findings of the present study impose a number of constraints on it. First, the response criterion is adjusted only given time and opportunity to do so (i.e., given a CSI of sufficient length). This could imply that either change detection or the adjustment of response criteria...
requires time to be accomplished. Second, what is actually adjusted are response criteria in predictable task-repeat trials, whereas unpredictable task-repeat trials (where there is actually no change in task at all) are processed as cautiously as task-switch trials. This is difficult to explain in terms of speed-accuracy trade-offs sensitive to change detection. To conclude, there is evidence of an adjustment of response caution in the present data. But this does not seem to be a fast reflex-like mechanism. Instead, findings can be reconciled with the view of a controlled adjustment that builds on the perceived risk of being presented with an error-prone trial.

The finding of an adjustment in response caution may be especially relevant when task-switching effects address populations that are known to adopt more extreme response criteria, such as the elderly (e.g., Ratcliff et al., 2006). Particularly, if they increase response caution more strongly in task-switch trials, this may bias estimates of the task-switching effect. Analyzing the data with the diffusion model, as done in the present study, would help tease apart a bias of response caution.

**Additional Analyses**

**Exclusion of trials.** In the reported analyses we excluded all trials that met criteria for extreme latencies and additionally those trials following an error, which is common practice in many task-switching studies (see method section of Experiment 1). This led to the exclusion of about 9.0% of all trials across the five experiments (range 7.7–10.9%). In an additional series of analyses, we only excluded trials with extreme latencies, but left all trials following an error in the dataset. This lead to the exclusion of only 2.3% of all trials across the five experiments (range 1.9–2.4%), but yielded the same pattern of significant and nonsignificant effects across experiments.

**Effects in the variability estimates.** We used the program fast-dm (Voss & Voss, 2007, 2008) for the parameter estimation. So additionally to the usually reported main parameters of the diffusion model, that is, response criterion (a), drift rate (v), and nondecision parameter (θ0), also the variabilities of the parameters are estimated, that is, variability of the drift rate (η), variability in the starting point (s0) and variability in the nondecision parameter (sθ0). Variability estimates are usually not very reliable, so they should be interpreted with caution.

Variability in the nondecision parameter (sθ0) clearly mirrored the effect in the mean estimates of this parameter: Variability in the nondecision parameter was selectively increased in switching trials in each group for which conditions did not permit to predict the new task set in advance. The interaction effect was significant in Experiments 2, 3a, and 3b (all p < .05); in Experiment 3c the pattern looked very similar, but missed significance (p = .26). As was the case for the mean estimates of the nondecision parameter, also both main effects of experimental group and the switching factor were significant, but this was caused again by the selective increase in unpredictable task-switches. In Experiment 4, in which neither group knew the nature of the next task in advance, there was an increase in variability in switching trials in both groups (p < .001), there was no main effect of group (p = .30), but the increase in variability in the long CSI group was more pronounced (p < .05).

Given that the nondecision parameter reflects preparatory processes, the selective and pronounced increase in its variability in unprepared task switches implies that the preparation process varies in duration between trials. This is worthy of note, as it suggests that the reconfiguration phase additionally inserted in these trials (Rogers & Monsell, 1995; Rubinstein et al., 2001) may considerably vary in length (or its duration is variably increased). This may either imply that task-set preparation is sometimes easier and sometimes more difficult, in line with the notion that reconfiguration is used adaptively to overcome inertia effects which may also vary in strength (cf. Koch & Allport, 2006; Yeung & Monsell, 2003a). Alternatively, it may mean that task-set preparation is hardly ever complete and only takes place until a satisfying criterion is met (e.g., Monsell et al., 2003) that additionally varies from trial to trial. This variability would reflect the difficulty of determining the exact point when preparation should end and when response-selection should begin (see also Mayr & Liebscher, 2001). Findings for the less reliable variability estimates of the drift rate and of the starting point are considered in Appendix B.

**Tests for a moderating effect of task and congruency.** Next we will address a potentially moderating role of the task (digit vs. letter classification) and response-congruency. These factors were investigated in previous task-switching research, but findings did not allow any strong predictions for the present experiments. For instance, it has been reported that in a Stroop-type switching task it is more difficult to switch from a weaker, less practiced task (e.g., color naming) to a stronger, more practiced task (e.g., word reading) than the other way round (Allport et al., 1994; Yeung & Monsell, 2003b), but we did not expect that in the present study, digit or letter classifications should differ in this respect. Findings are also inconclusive to what extent response-congruency contributes to the switching effect. Some studies have supported a role of response congruency (e.g., Kiesel et al., 2007), whereas others did not find an effect of response-congruency (e.g., Koch & Allport, 2006; Waszak & Hommel, 2007). In the other studies in which the diffusion model was applied to task-switching data (Krayanidis et al., 2009; Madden et al., 2009), no analyses are reported in which a possible moderating effect of the task-factor or response-congruency on the parameters was tested. In the following, we will briefly describe findings of the tests for both factors in our data.

Consider the task factor first. Across experiments and conditions, digit and letter classifications did not differ systematically in response time or accuracy. In repetition trials, digit classifications were descriptively slower than letter classifications (on average 20 ms across the six experiments; mean p = .35), but the difference was virtually absent in task-switch trials (across experiments −1 ms, p = .36). The latency difference between both tasks was thus 21 ms smaller in switching trials than in repetition trials (mean p = .36). In repetition trials participants made 0.9% more errors in the digit task than in the letter task (mean p = .36). In repetition trials participants made 0.9% more errors in switching trials (mean p = .30). Thus, the difference between tasks was increased by one percent (mean p = .45) in task-switch trials relative to task-repeat trials. To summarize, there was no effect of the task factor, neither in the response times nor in the errors.

Next we tested whether the task-factor moderated any of the effects in the parameter estimates. To this end, we further subdivided the available trials into groups of digit and letter trials,

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6 We thank one of the reviewers for pointing this out.
resulting in 96 trials for each condition (task pure, switching, repetition) before applying the same exclusion criteria as in the previous analyses. Separate diffusion models were estimated for each trial group, and parameters were simultaneously entered into an ANOVA with task as a moderator factor. There were 48 possible interaction effects of the task factor across the three parameters and the six experiments, and we did not have any a priori hypotheses. This necessitated a correction for multiple testing. We used the Holm-Bonferroni method (Holm, 1979) with a lenient correction factor of 3 for Experiment 2–4, because there were three interaction terms in each (resulting in a critical \( p = .016 \)). In neither of the ANOVAs for each parameter in each experiment was there a significant interaction term with the trial-group factor.

A formal test of parameter equivalence across both tasks is not possible using fast-dm. In an attempt to obtain a fit statistic that allows a direct comparison of two models, we tried DMAT using the quantile maximum likelihood (QML) method as described by Vandekerckhove and Tuerlinckx (2007, 2008).\(^7\) Separate diffusion models were estimated for each participant, and all parameters were either allowed to differ between tasks or they were constrained to be equal across tasks. Conventional model selection indices favored the model assuming identity of the parameters across tasks (corrected Akaike information criterion [AICc] in 95% of all cases; Bayesian information criterion [BIC] in 100% of all cases).

We repeated the same analyses for response congruency. Somewhat to our surprise, the factor did not have a gross effect in the latency data. Congruency tended to have a more pronounced effect in task-switch trials (average latency difference 34 ms; mean \( p = .25 \)) than in task-repeat trials (average latency difference 19 ms; mean \( p = .23 \)), and the difference between congruent and incongruent trials was not significantly different in switching and repetition trials (\( p = .47 \)). However, there were effects in the accuracy domain. Incongruent trials were less accurate in repetition trials (3.0% points, mean \( p = .04 \)) as well as in switching trials (8.2% points, mean \( p = .004 \)), and this difference just reached significance (\( p = .04 \)) between repetition and switching trials.

Again separate diffusion models were estimated for response congruent and response incongruent trials of all three trial types (task pure, switching, repetition), and parameters were submitted to an ANOVA with congruency as a moderator factor. Applying the same criterion as in the previous study, none of the interaction effects with the congruency factor reached significance. DMAT was used again to formally test for a moderating effect of congruency in the parameters of the diffusion model in the way described above. According to the information indices, the model assuming identical parameters was favored again (AICc in 95% of all cases; BIC in 100% of all cases).

Models and modeling in task-switching research. Task switching has become one of the most active research domains in psychological research—and a number of different statistical modeling approaches were developed in this field (e.g., Altmann & Gray, 2008; Gilbert & Shallice, 2002; Karayanidis et al., 2009; Kieras et al., 2000; Logan & Gordon, 2001; Meiran, 2000; Meiran & Daichman, 2005; Meiran, Kessler, & Adi–Japha, 2008; Ruthruff et al., 2001; Schneider & Logan, 2005, 2009; Sohn & Anderson, 2001; Yeung & Monsell, 2003b). It is beyond the scope of the present paper to review these accounts in detail. But it is important to note that task switching data have been described using quite dissimilar statistical approaches, including parallel distributed models (Gilbert & Shallice, 2002; Brown et al., 2007), multinomial processing tree models (Meiran & Daichman, 2005), modeling approaches that use computer analogies (Kieras et al., 2000), and models that comprise continuous evidence accumulation processes (Logan & Gordon, 2001; Logan & Bundesen, 2003; and our own). Some of these models differ substantially in their architecture, the number and meaning of parameters, their complexity and explanatory power. This implies that not all of these approaches can be “the true model,” in the sense that the model specifies all the cognitive process taking place at a task switch. In fact, it would be naïve to assume that any model meets this criterion. Nevertheless, a model can be useful, when it helps approximate the true processes and allows for meaningful predictions, even when it is known that the model is “wrong” (Brown & Heathcote, 2008). In this sense, a model can be useful when it helps tease apart at least some of the processes of interest. An additional advantage of simplified models is that they are usually more user friendly and may thus stimulate research (Wagenmakers et al., 2008).

Another point that deserves discussion is the translation of theoretical models into statistical models (and vice versa). Consider the approach proposed by Gilbert and Shallice (2002) that was frequently mentioned in this paper. Their parallel distributed processing (PDP) model implements ideas of some of the previously discussed multiple-components models (e.g., Ruthruff et al., 2001) by assuming both, top-down biasing via task-demand units as well as stimulus-driven activation of responses (and task sets). The account also postulates two phases: During a preparation interval, top-down input is fed into the task-demand units; whereas stimulus-input units are set to zero and output-units are not updated at that time. After the preparation interval, stimulus-input units are activated so that stimulus information can now enter the decision process which is terminated when the response-unit most strongly activated reaches a predefined difference to the response unit that is next highest activated (in other words, when a criterion is met). Note that their PDP model is different from the diffusion model used in the present paper in its statistical predictions, but both approaches can be reconciled with broader theoretical predictions derived from multiple components models of task switching. Thus, we suggested that top-down biasing (as per the PDP terminology) may be one way of how task-set preparation can be conceptualized which in turn was interpreted as contributing to the increase in the nondecision parameter in nonpredictable task switches. But there are also approaches which are statistically close, but nevertheless interpret effects in different theoretical terms. For instance, Logan’s account is, at least technically speaking, very similar to the diffusion model used in this study (one difference is that it only assumes discrete evidence accumulation with a few steps). But from a theoretical perspective, Logan’s theory differs substantially from the multiple-components framework of task-switching.

\(^7\) The QML method would usually require more trials, especially error trials, than were available in our datasets—results should be interpreted with caution.
Conclusion

The diffusion model (Ratcliff, 1978) was applied to different versions of classical task-switching paradigms, the alternating-runs paradigm and the explicit cueing paradigm. This parsimonious model was found to correspond reasonably well with important aspects of current multiple-components models of task switching (e.g., Gopher et al., 2000; Mayr & Kliegl, 2003; Rubinstein et al., 2001; Ruthruff et al., 2001) and predictions derived from them. Multiple-components models posit that task-set preparation, as well as carry-over effects, contribute to the switching effect at different phases in a task switch. The parameters of the diffusion model were used in an attempt to dissociate components of task-switching at different phases. Findings can be reconciled with the view that the nondecision parameter captures processes at an earlier phase of a task switch, such as task-set reconfiguration (Rogers & Monsell, 1995) or higher level cue encoding (e.g., mediator retrieval; Logan & Schneider, 2006). Findings also suggest that at a later phase, task readiness contributes to response selection, thereby affecting the drift rate. This parameter was also shown to be influenced by task-set inertia (Allport et al., 1994; Allport & Wylie, 2000). Additionally, there was evidence that response caution can contribute to local as well as global switching effects.

References


DECOMPOSING TASK-SWITCHING COSTS


Appendix A

Graphical Display of Model fit for the Data From Experiments 1–4

The 11 pictures show overlaid predicted (parameter based) and empirical cumulative distribution functions (cdfs) for all experiments and experimental groups. Predicted and empirical cdfs were first computed for each participant and then averaged across participants. Separate cdfs are given for the three trial types (task pure, switching, repetition). The plotted functions are joint cdfs of correct and erroneous responses, with latencies of erroneous responses plotted on the left side of the horizontal axis (with negative scale values) and latencies of correct trials plotted on the right side (with positive scale values). The intercept of the cumulative distribution function indicates the percentage of erroneous responses.

(Appendices continue)
Appendix B

Effects in the Variability of the Drift Rate and of the Starting Point

In the variability of drift rates (η) there was an interaction of task-predictability and the task-switching factor ($p < .05$ in Experiments 2, 3a, 3c; and descriptively [$p = .11$] in Experiment 3b). In Experiment 4, in which task switches were unpredictable for both groups, there was only a main effect of the task switching factor ($p < .001$) and the pattern in both groups was comparable (no interaction effect $p = .36$) and followed the pattern observed with participants who could not predict the next task in the other experiments. Throughout all experiments and experimental groups, variability of the drift rate was low in the task pure condition. Variability was increased in task-switches in the group who could predict the next task, but it was low for the group who could not predict it. However, on task-repeat trials, variability in the group who could not predict the next task increased to about the level of the group who could predict it. This pattern of findings can be reconciled with the notion that the drift rate reflects task readiness and its variability therefore fluctuations in task readiness. For instance, task readiness should be maximal and thus invariable in all trials of the task pure blocks in both groups. The increase in variability in predictable task switches could indicate that preparation fails in some of the trials, thereby leading to variability in task readiness. Given this interpretation is accurate, the low variability in task switches and the higher variability in task repetitions in the group who could not predict the next task, could indicate that for these participants, task-preparation is required to a similar extent in all switching trials, but that it is facilitated in some of the task-repeat trials.

In the variability of the starting point ($\tau_s$) there was a general increase in switching trials in both groups across all experiments ($p < .05$; only in Experiment 3b was there a more pronounced increase in the group who could not predict the new task). The increase in starting point variability in switching trials might reflect, we speculate, that carry-over effects contribute to a bias in starting point generally penetrate more easily in conditions when attention and control functions are depleted because of the task-switching demand. Additionally, switch failures (task confusion) in a limited number of trials may contribute to an artificial increase in starting point variability: Task-switch neglect will only lead to an erroneous response in response-incongruent trials. So while participants would give the response according to the competing task set (possibly even without noticing their task-failure), the diffusion model would try to account for these errors in part by estimating low starting point in a portion of all trials. This would increase variability in starting point in switching trials, as the mean starting point was fixed in the middle between both response criteria. This problem cannot be easily circumvented by splitting congruent and incongruent trials, because it only affects a supposedly small subset of incongruent trials.

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