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Preparatory Processes in the Task-Switching Paradigm: Evidence From the Use of Probability Cues

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The purpose of the investigations was to dissociate processes of task preparation from task execution in the task-switching paradigm. The basic assumption was that task repetitions have 2 advantages over task shifts: an activation advantage as a result of the execution of the same task type in the pretrial, and an expectation advantage, because participants, in general, implicitly expect a repetition. In Experiments 1–3, the authors explicitly manipulated expectancies by presenting cues that announced a shift and/or a repetition with probabilities of 1.00, .75, .50, or .25. Increasing latencies with decreasing probability for shifts *and* repetitions show that the expectation advantage can be equalized by preparation. However, the activation advantage represented by constant shift costs between tasks of the same probability is not penetrable by preparation. In Experiments 4 and 5, the authors found evidence that preparation involves activation of the expected task and inhibition of distracting tasks.

In everyday life, people are constantly confronted with changing demands of the environment. Consider, for example, a cognitive scientist who is sitting in her office studying an article. Suddenly the telephone rings, and as she picks up the telephone, a student appears at her door. In this example, the scientist must switch activities within seconds: She goes from reading an article to answering the phone, while simultaneously trying to remember the student's name with whom, as the scientist suddenly remembers, she has an appointment at that particular time.

What are the underlying cognitive processes that allow humans to act flexibly and adequately in a constantly changing environment? Despite changing demands, how can different cognitive actions be planned and prepared?

The Task-Switching Paradigm

The task-switching paradigm has become popular during the last few years. Originally launched 70 years ago by Jersild (1927), Allport, Styles, and Hsieh (1994) adopted this paradigm to study cognitive control. The standard procedure used to study task switching is as follows: Participants are given lists of simple cognitive tasks (e.g., judgment whether a number is odd or even or larger or smaller than a reference number) to work through. A comparison between the response latencies for "pure" lists (i.e., lists with only one task type included, such as AAA) and mixed lists (e.g., ABAB) yields significantly longer response latencies for mixed lists, or so-called switch costs.

Because of the different demands on working memory between pure and mixed lists, when more tasks have to be held active at the same time in working memory, other authors (e.g., Meiran, 1996; Rogers & Monsell, 1995) prefer calculating switch costs not between but rather within lists of tasks: Task shifts and task repetitions are presented in random or alternating (e.g., AA-BB-AA) order within a block. Switch costs are again the difference between mean latencies of task repetitions and task shifts. In the sequence AAB, the second task A would represent a task repetition and B a task shift. The main result in both paradigms is that switch costs remain stable, even after long periods of practice (Meiran, 1996; Rogers & Monsell, 1995).

Preparation Effects and the Interpretation of Switch Costs

Even though the term "switch costs" is commonly used to describe the reaction time (RT) difference between repetitions and shifts, from a theoretical perspective there is disagreement as to whether this difference actually represents the costs for the reconfiguration of the switch task or the benefit of the repeated execution of the same task. On first glance, this distinction (cost vs. benefit) might sound like two sides of the coin; however, the interpretation implies involvement of different control processes. The switch-costs view holds that slower RTs for task switches reflect the additional—and hence top-down—control requirements needed to reconfigure the system for the switch to a new task (DeJong, 2000; Goschke, 2000; Meiran, 1996; Meiran, Chorev, & Sapir, 2000; Rogers & Monsell, 1995). The common empirical finding supporting this view is that RTs for task shifts decrease

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with increasing preparation intervals, whereas those for task repetitions do not (or at least decrease less). Hence, the shift specificity of preparation—the interaction between preparation interval and task type (shift vs. repetition)—is taken as evidence that slower RTs for task switches represent the process of reconfiguration for the new task with one part possibly occurring before the actual shift task is presented (see, e.g., the distinction between endogenous and exogenous control; Rogers & Monsell, 1995).

According to the repetition-benefit view, slower RTs for shifts (faster RTs for repetitions, respectively) represent an automatic—and hence bottom-up—carryover effect from the previous task (Allport et al., 1994; Allport & Wylie, 2000; Ruthruff, Remington, & Johnston, 2001; Sohn & Carlson, 2000). This position is empirically supported by findings of additivity between preparation and task type (shift vs. repetition). For example, Sohn and Carlson (2000) compared task shifts and task repetitions with and without foreknowledge about the upcoming task. They reported strong preparation effects for both task shifts and task repetitions but no interaction between foreknowledge and task type (repetition vs. shift). With a slightly different experimental approach, Ruthruff et al. (2001) also reported additive effects between task preparation and task set reconfiguration. They presented tasks in the alternating-runs paradigm (AABBAA), and, by occasional violation of the predictable task sequence (unexpected repetition AABBAA*A* and unexpected shift AABBA*B*), they showed that task expectancy had the same effect on both task shifts and task repetitions. Consequently, they found preparation effects to the same extent either for task shifts or for task repetitions. Hence, results of both studies support the assumption that the RT difference between shifts and repetitions represents a repetition benefit, in other words, an automatic carryover effect from the previous task that cannot be overcome by advance reconfiguration. Note, however, that this assumption does not rule out a passive decay of this priming effect over time (Allport et al., 1994; Ruthruff et al., 2001).

It should be noted that, because of the contradictory assumptions concerning the appropriate interpretation of switch costs, we use this as a neutral term to describe the RT difference between task shifts and task repetition. When we present results that support either the repetition-benefit view or the switch-costs view, we will make this explicit.

To summarize, evidence for shift-specific preparation effects supporting the switch-costs view is based on the manipulation of the preparation interval, whereas evidence for additive preparation effects supporting the repetition-benefit view is based on the manipulation of the amount and validity of advance information. The manipulation of the preparation interval is, of course, the perfect method to learn more about the dynamics and the time course of preparation. However, to investigate whether preparation is indeed specific to task shifts, and thus reflects some part of the reconfiguration process, the timing manipulation seems to be problematic for at least two reasons. One obvious issue is a possible floor effect for task repetitions. Given the simplicity of the tasks in common cuing paradigms, especially in Meiran's studies (1996; Meiran et al., 2000), in which the cue basically reduces the task from a four-choice to a two-choice RT task, it is reasonable to assume that RTs for task repetition had already reached keystroke level such that they cannot benefit from longer preparation intervals. A second, and even more serious, problem of the timing manipulation arises from its implicit assumption that task shifts and task repetitions differ only with respect to the pretrial (with task repetition being preceded by the same task and task shift being preceded by a different task). In contrast to this implicit assumption, we hypothesize that task shifts and task repetitions additionally differ with respect to their subjective expectancies. More specifically, we assume that participants in task-switching experiments might implicitly expect a task repetition more than a task shift. In this case, the manipulation of the preparation interval would have less effect on the implicitly anyway-expected task repetition than on the task shift, resulting in seemingly shift-specific preparation effects.

Repetition Effects and Expectancies

Evidence for the assumed expectation advantage for task repetitions derives from simple choice reaction tasks (Bertelson, 1961, 1965). Bertelson showed that stimulus repetitions in simple choice RT tasks yield faster RTs than stimulus changes.

One explanation for this finding was that participants' expectations of a stimulus repetition would account for the repetition effect. For example, Keele and Boies (1973) found a repetition effect in a sequential choice reaction task only when stimulus repetitions were more probable than stimulus changes. If, in contrast, an event other than the stimulus repetition had a higher probability, the repetition effect disappeared. Keele and Boies concluded that "the repetition effect is primarily an anticipatory phenomenon; when Ss have reason to anticipate some event other than a repetition, repetitions lose most or all of their advantage" (p. 87).

Applied to task-switching experiments, we assume that, despite equal probabilities of task repetitions and task shifts in most of the studies on task switching, participants might implicitly expect a task repetition. This seems to be reasonable because task repetitions are easier to perform; thus, the cost of incorrectly preparing for a shift is much higher than the cost of incorrectly preparing for a task repetition. For clarification, we do not believe that participants consciously and actively always prepare for a task repetition. We rather assume an implicit expectation for a task repetition, which simply means that participants keep the just-executed task active for further use.

Therefore, we suspect that shift-specific preparation effects based on a timing manipulation are due to an implicit expectation advantage for task repetitions. Under this hypothesis, a cue announcing an implicitly anyway-expected task repetition provides less additional information than a cue announcing a task shift. This, in turn, would also result in more pronounced cuing effects over time for task shifts than for task repetitions.

However, one critical objection to our assumption might be that our expectation hypothesis cannot explain Rogers and Monsell's (1995) results. The authors used the alternating runs paradigm (AABBAA) and, by varying the response–stimulus interval (RSI), still found shift-specific preparation effects (decreasing switch costs with increasing RSI). We agree that the assumption of an expectation advantage for task repetitions is not reasonable in this case. However, using a completely predictable task order makes it difficult to decide at what point participants actually start to prepare for the upcoming task. Participants could have prepared for the (more difficult) task switch during the execution of the (easier) task repetition. This strategy would lead to slower RTs for task repetitions and faster RTs for switches and could then erroneously be interpreted in terms of shift-specific preparation effects (see also Meiran et al., 2000, for the same argument).

To summarize, the manipulation of the preparation interval in common task-switching experiments results in preparation effects that only seem to be specific to task shift but that might indeed be due to different implicit expectations for task shifts and task repetition. The main purpose of the experiments reported here, therefore, was to examine this expectation hypothesis.

Manipulation of Expectancies in the Task-Switching Paradigm

To test our hypothesis that shift-specific preparation effects can be explained by an implicit expectation advantage for task repetitions, we need to manipulate the specific expectancies for task repetitions and task shifts. It must be shown that the slowing of unexpected task repetitions relative to expected task repetitions is equal to the slowing of unexpected task shifts relative to expected task shifts. Hence, a paradigm must be found that allows the specific manipulation of expectancies for task repetitions and task shifts.

To manipulate the implicit expectations for repetitions and shifts, we could vary the frequencies for the different task types. However, with this approach, frequency and practice effects would get confounded (with the more frequent task types also being the most practiced tasks). Therefore, we decided to manipulate expectancies explicitly with specific probability cues. Each task will be preceded by a cue that announces, with a probability of 1.00, .75, .50, or .25, a task repetition and a shift to a specific task. With this manipulation, the global probabilities for task shifts and task repetitions can be equaled; only the local probabilities (from trial to trial) change. Because the time course of preparation is not the focus of our interest, we do not manipulate the preparation interval but instead always choose the same interval, one that is long enough to ensure that the complex probability information of the cues can be completely processed. Using this method, task repetitions and task shifts of equal probabilities (i.e., equal expectancies) can be compared. The implicit expectation advantage for task repetitions over task shifts should thus disappear.

If shift-specific preparation effects found in former experiments are indeed due to different implicit expectancies for task shifts and task repetitions, namely because of an expectation advantage for task repetitions, we should find equal preparation effects for task repetitions and task shifts under the different probability conditions: increasing latencies with decreasing announced probability. Hence, the implicit expectation advantage for task repetitions should be equalized between tasks of the same probability. Moreover, we expect that the assumed activation advantage for task repetitions is not penetrable by preparation. Hence, the remaining switch costs between task shifts and task repetitions of the same probability should be of constant size. These task shifts and task repetitions of the same probability should differ only with respect to the previous task type (same or different). These remaining switch costs should thus reflect the pure switch costs resulting from the activation advantage for task repetitions, in other words the repetition benefit.

If, alternatively, the implicit expectation hypothesis is wrong and task preparation is indeed shift specific, then we should find stronger preparation effects for task shifts than for task repetitions, as reflected by an interaction between task type (repetition vs. shift) and announced probability.

Overview of Experiments

In Experiment 1, we investigated the hypothesized expectation advantage for task repetitions by presenting probability cues that announced a task shift or a task repetition, respectively, with graded probability. Experiment 2 was a replication of the first one with new task types. In Experiment 3, we additionally investigated effects of motivation and practice on the remaining switch costs. We then focused on the cognitive processes that account for the observed preparation effects. In two additional experiments, we investigated facilitatory and inhibitory processes of task preparation.

General Method

The general method was nearly the same for all experiments. Any deviations from this approach are discussed elsewhere.

Apparatus and material. All experiments were conducted on an IBM-compatible personal computer 486 AT. Participants had to switch between four tasks: They had to decide whether a digit was odd or even or smaller or larger than a reference number. Furthermore, they had to judge whether a letter was a consonant or a vowel and whether a letter came before or after a reference letter in the alphabet. All experiments were divided into a practice phase and a test phase. In the practice blocks, the stimuli consisted of the letters *A*, *D*, *I*, and *N* and the numbers *0*, *1*, *3*, and *4*. The reference letter of the position task was *F*, and the reference number was *2*. In the test phase, the letters *E*, *H*, *R*, and *U* and the numbers *5*, *6*, *8*, and *9* were used. The reference letter then was *M*, and the reference number was *7*. The stimuli were presented in a square frame (light gray) positioned in the center of the dark gray screen. Each stimulus was presented in a specific color, with the color indicating the requested task: A blue letter signaled that a judgment concerning the type of letter had to be made, whereas a green letter required a judgment concerning the position in the alphabet. A digit printed in red indicated that the digit had to be classified as odd or even. A yellow digit indicated that a judgment related to the position had to be made. The assignment of color to the corresponding task was practiced at the beginning of the session to ensure that participants acquired strong associations between color and corresponding task type.

As response keys, we used the "<", *y*, period, and hyphen keys, which are located in the outer left and outer right positions in the second lowest row of the German PC keyboard. The outermost keys were marked with black stickers and the innermost keys with white ones. Each of the four keys was mapped to two different responses. Half of the participants had to press the left black key if the letter was a vowel or if the digit was even and the left white key if the letter came before or if the digit was smaller than a reference character. Furthermore, they had to press the right black key if the letter was a consonant or if the digit was odd and the right white key if the letter came after or if the digit was larger then a reference character. The tasks for the other half of the participants were the opposite. Table 1 summarizes the material, tasks, and corresponding keys that were used.

Probability cues. Each task, shift as well as repetition, was announced by a specific probability cue. The cue was four small colored squares at the upper edge of the task frame. Each single colored square indicated the appearance of the corresponding task with a probability of .25. Four small blue squares predicted with a probability of 1.00 that the next task would be that particular letter task. Three blue squares and one red square

Table 1
Stimuli, Tasks, and Response Keys Used in Experiments 1 and 5

Stimulus	Color	Judgment	Response key
5, 6, 8, 9 (*0, 1, 3, 4*)	Red	Odd or even?	Outer black keys
	Yellow	Larger or smaller than *7* (*2*)?	Inner white keys
E, H, R, U (*A, D, I, N*)	Blue	Consonant or vowel?	Outer black keys
	Green	Before or after *M* (*F*)?	Inner white keys

Note. The stimuli for the practice blocks are given in parentheses.

indicated a blue task with a probability of .75 and a red task with a probability of .25. Furthermore, a cue with two blue squares and two red squares announced a blue or a red task with .5 probability each. The two-colored cues—that is, all cues except for the 100% cue—were presented from left to right with the first squares indicating a repetition followed by the squares announcing a specific task shift (e.g., compare repetition with $p = .75$ and shift with $p = .75$ in Figure 1).

Procedure. Detailed instructions were presented on the screen. These instructions were followed by a practice block consisting of eight sequences of 12 or 13 tasks of each type. The stimulus (letter or digit) appeared in the center of the task frame. By pressing the response key, the stimulus disappeared but the frame remained on the screen. After 2,000 ms, the next stimulus was presented. In this first practice block, the required task type for each colored stimulus and the corresponding pair of response keys were displayed below the stimulus. Furthermore, after each response, participants received feedback about the accuracy of their response. A second practice block followed that was identical to the latter except that no supporting information was given. Again, feedback was provided after each task. At the end of each block, participants were informed about the mean error and mean RT for the entire block. After the participants had worked through the first two practice blocks, a new detailed instruction followed to introduce the probability cues. Each cue (1.00/0; .75/.25; .50/.50; .25/.75) was presented and explicitly explained cue by cue, followed by 20 exercise tasks each.

The RSI was 2,000 ms, and cues were presented for 500 ms with a cue target interval of 1,500 ms. This rather long interval was chosen to ensure that participants had sufficient time to process the complex probability cues (see also Fagot, 1994).

After the introduction of the probability cues, a final practice block with 205 tasks was presented. In this block, the tasks were presented in random order. Each task was preceded by a specific probability cue. The participants were encouraged to use the cues and to answer as quickly and accurately as possible. Participants who made more than 10% of errors in this block were given a further block of practice. In this block, participants received feedback only when an error occurred. Incorrect tasks were repeated.

Before the beginning of the test phase, the new material was introduced to prevent participants from acquiring direct stimulus–response mappings. The experimental phase consisted of four blocks that were comparable to the third practice block with 205 tasks each. After each block, participants received feedback about their mean latencies and mean percentage of error. Between blocks, participants were permitted to take a break. One session lasted about 90 min.

Experiment 1

The goal of Experiment 1 was to test whether shift-specific preparation effects can be attributed to an implicit expectation advantage for task repetitions. Each task was preceded by a specific cue that announced a shift or a repetition, or both, with graded probability (see *General Method* section). We expected increasing latencies with decreasing probability for both shifts and repetitions and correspondingly constant switch costs between tasks of the same probability. This lack of an interaction between task preparation and task type (shift vs. repetition) would then be interpreted

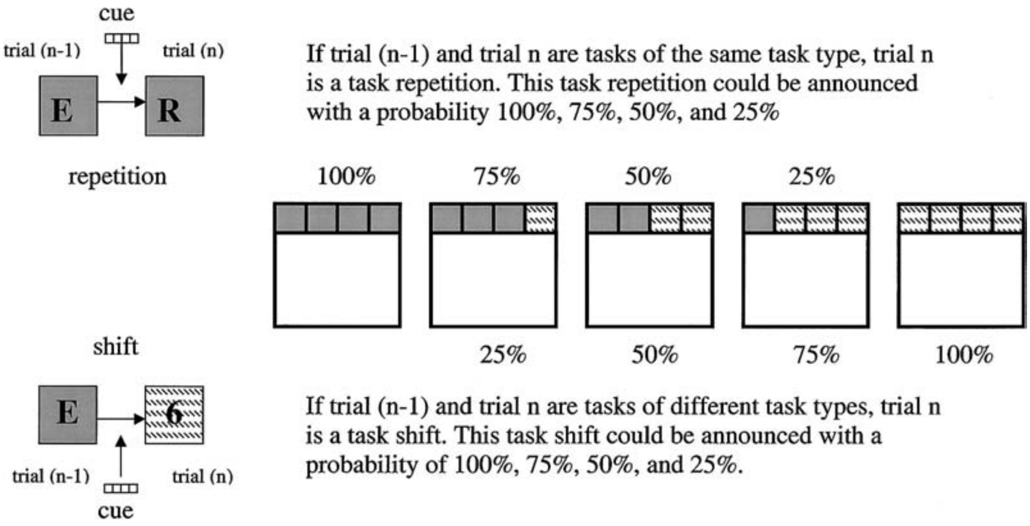


Figure 1. Probability cues: Each small colored square announces with a probability of .25 the corresponding task. For more detailed description, see text.

as evidence that preparation in the task-switching paradigm is not specific to task shifts but rather results from an expectation advantage for task repetitions. This, in turn, would provide further evidence for the interpretation of switch costs in terms of a repetition benefit, reflecting the automatic carryover effect from the previous task.

Method

Participants. Nine male students (age range = 21–35 years, *M* = 28.22 years, *SD* = 5.49 years) from the University of the Federal Armed Forces, Hamburg, served as participants on a voluntary basis.

Design. A 2 (task type: shift vs. repetition) × 4 (probability: 1.00, .75, .50, .25) within-subjects design was used. Response latencies and error rates served as dependent measures.

Results and Discussion

In this and all further experiments, we used the following data-analytic strategy: The first experimental block was excluded from the analysis because participants were still becoming accustomed to the new stimuli. Furthermore, we excluded incorrect responses and RTs that differed more than 2 standard deviations from the overall mean of each design cell (a weaker criterion did not alter the results). For each participant, error rates and mean RTs from Experimental Blocks 2 through 4 were computed separately for task repetitions and task shifts and for the different cue conditions.

Because error rates always were very low and did not interact with RTs, they were only treated descriptively. Mean RTs were entered into analysis of variance (ANOVA). For further analysis of single conditions, planned comparisons were conducted.

Error rates. The overall accuracy was high. Error rates for task and probability conditions ranged from 2.3% to 5.9% (*M* = 3.19%, *SD* = 2.3%).

RT data. In Figure 2, mean RTs are presented as a function of task type and probability condition. Under all probability conditions, task repetitions were executed faster than task shifts. Furthermore, we found increasing latencies with decreasing probability for both shifts and repetitions. The corresponding main effects were highly significant: for task type (shift vs. repetition), $F(1, 8) = 54.57$, $MSE = 6{,}622.53$, $p < .01$; for probability, $F(3, 24) = 140.37$, $MSE = 2{,}567.47$, $p < .01$. Switch costs between tasks of the same probability were always reliable (all *F*s > 21, all *p*s < .01). In addition, we found significant effects of linearity for shifts, $F(1, 8) = 151.26$, $MSE = 4{,}167.4$, $p < .00001$, and repetitions, $F(1, 8) = 108.31$, $MSE = 4{,}215.8$, $p < .01$. As indicated by the parallel course of the two RT gradients, the interaction between task and probability did not reach statistical significance, $F(3, 24) = 1.67$, $p = .19$.

Experiment 1 provided evidence that the probability cues affected task shifts and task repetitions in the same manner. Moreover, switch costs between tasks of equal probability remained constant. These results suggest that preparation effects in the task-switching paradigm are not specific to task shifts. Accordingly, those effects reported in the literature are mainly due to different implicit expectancies for task shifts and task repetitions. Holding expectancies explicitly constant for both task types, task repetitions lose their implicit expectation advantage, resulting in a decrease of shift-specific preparation effects.

In addition, we interpret the constant RT difference between shifts and repetitions of the same probability in terms of a repetition benefit. Shifts and repetitions of the same probability differ only with respect to the preceding task; repetitions are preceded by the same task type and shifts are preceded by a different task type. The repetition of a task obviously results in an activation advantage that cannot be overcome by preparation.

The results thus far support our assumptions of an implicit expectation and an activation advantage for task repetitions.

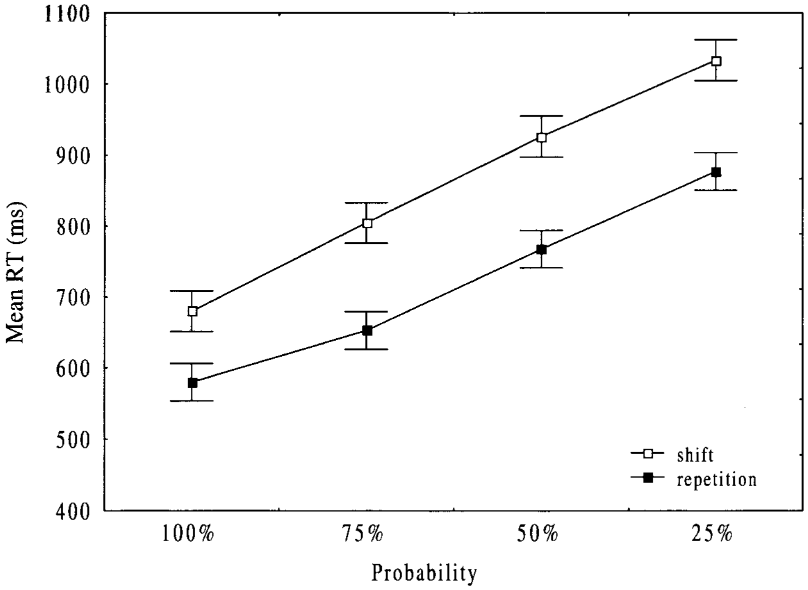


Figure 2. Mean reaction time (RT) as a function of probability and task type in Experiment 1. Error bars represent 95% within-participant confidence intervals (Loftus & Masson, 1994).

Whereas the former can be equaled during preparation, the latter does not seem to be penetrable by preparation.

However, one may argue that the remaining switch costs are created by the interference in the used material and thus do not reflect the assumed repetition benefit. Perhaps the task shift could have been completely reconfigured during the preparation interval (e.g., for the consonant–vowel shift task). However, with the onset of the stimulus letter, the conflicting task (the before–after reference letter task) might automatically have been activated. The observed shift cost would then represent the amount of time that the system needs to inhibit the irrelevant interfering task.

To test this so-called task-cuing hypothesis (see Rogers & Monsell, 1995), a further experiment was conducted. In Experiment 2, we used completely univalent (noninterfering) tasks. If the obtained switch costs were indeed due to an activation advantage for task repetitions, then using univalent stimuli should not change the general RT pattern obtained in Experiment 1.

Experiment 2

Method

Material. The experimental procedure was the same as in Experiment 1 except for the used tasks. Again, participants had to decide whether a digit was odd or even and whether a letter was a consonant or a vowel. Furthermore, they had to judge whether a figure was round or angular and whether a symbol was a punctuation mark or an arithmetic sign (Table 2). Response keys and colors for the figure task corresponded to those for the small–large task in Experiment 1, and response key and colors for the sign task corresponded to those for the before–after task (see *General Method* section). This time, the stimuli triggered unequivocally the corresponding task. Nonetheless, the stimuli were again printed in four different colors to facilitate discrimination of the particular tasks and the use of the colored probability cues. However, in this experiment, the colors of the stimuli were only useful, and not necessary, simultaneous cues (Kantowitz & Sanders, 1972; Sudevan & Taylor, 1987). We return to this issue later.

Participants. Ten male students (age range = 20–33 years, $M = 22.7$ years, $SD = 3.97$ years) from the University of the Federal Armed Forces, Hamburg, served as participants on a voluntary basis. None had participated in Experiment 1.

Design. As in Experiment 1, a 2 (task type: shift, repetition) × 4 (probability: 1.00, .75, .50, .25) within-subjects design was used. Response latencies and error rates served as dependent measures.

Results and Discussion

Error rates. The overall accuracy was high. Error rates for the task and probability conditions ranged from 1.7% to 4.5% ($M = 3.46\%$, $SD = .91\%$).

RT data. Figure 3 depicts the mean RTs as a function of task type and probability condition. The data pattern is comparable to that in Experiment 1. Again, task repetitions were executed faster than task shifts under all probability conditions. Furthermore, latencies for both shifts and repetitions increased with decreasing probability. The corresponding main effects were highly significant: for task type, $F(1, 9) = 61.33$, $MSE = 1{,}510.62$, $p < .01$; for probability, $F(3, 27) = 20.59$, $MSE = 1{,}174.52$, $p < .01$. Switch costs between tasks of the same probability were always reliable (all Fs > 20, all ps $< .01$). Furthermore, tests of linearity were significant for task shifts, $F(1, 9) = 15.81$, $MSE = 2{,}147.74$, $p < .01$, and task repetitions, $F(1, 9) = 39.57$, $MSE = 969.00$, $p < .001$. Finally, the theoretically important Task Type × Probability interaction again did not reach statistical significance, $F(3, 27) = 2.04$, $p = .13$.

The data clearly replicate the results of Experiment 1. Even with disambiguated material, switch costs remained reliable and did not vary under the different probability conditions. Hence, switch costs in Experiment 1 were created not by the interference in the used material but by the activation advantage for task repetition. The only difference between Experiments 1 and 2 was the overall speed of responses. In Experiment 1, the mean RT was 719 ms for task repetitions and 860 ms for shift tasks compared with 528 ms and 596 ms, respectively, in Experiment 2. Thus, univalent stimuli increased the overall speed of task performance but did not qualitatively change the pattern of results.

Experiment 2 was conducted to exclude the possibility that switch costs were created by interference of the used material. The findings again support our assumption of an expectation advantage for task repetitions that can be equaled during the preparation interval and an activation advantage for repetitions that cannot be overridden and accounts for the residual switch costs.

However, one problem in the experimental design weakens our argument: Because the stimuli in Experiment 2 unequivocally defined the corresponding task, the colors of the stimuli were only useful, and not necessary, simultaneous cues (Kantowitz & Sanders, 1972; Sudevan & Taylor, 1987). Hence, participants were not forced to learn the relation between color and task and thus might not have used the colored probability cues to prepare for the upcoming task as much as technically possible. To support our results further, we replicated the second experiment in Experiment 3. However, this time participants were encouraged to use the cues by means of the following modifications: (a) central presentation of the cues, (b) gaining points for quick and accurate responses, and (c) extended practice. To control whether the par-

Table 2
Stimuli, Tasks, and Response Keys Used in Experiments 2 and 3

Stimulus	Color	Judgment	Response key
5, *6*, *8*, *9* (*0*, *1*, *3*, *4*)	Red	Odd or even?	Outer black keys
	Yellow	Round or angular?	Inner white keys
E, *H*, *R*, *U* (*A*, *D*, *I*, *N*)	Blue	Consonant or vowel?	Outer black keys
! ? + % (, ; ≠ √)	Green	Punctuation mark or arithmetic sign?	Inner white keys

Note. The stimuli for the practice blocks are given in parentheses.

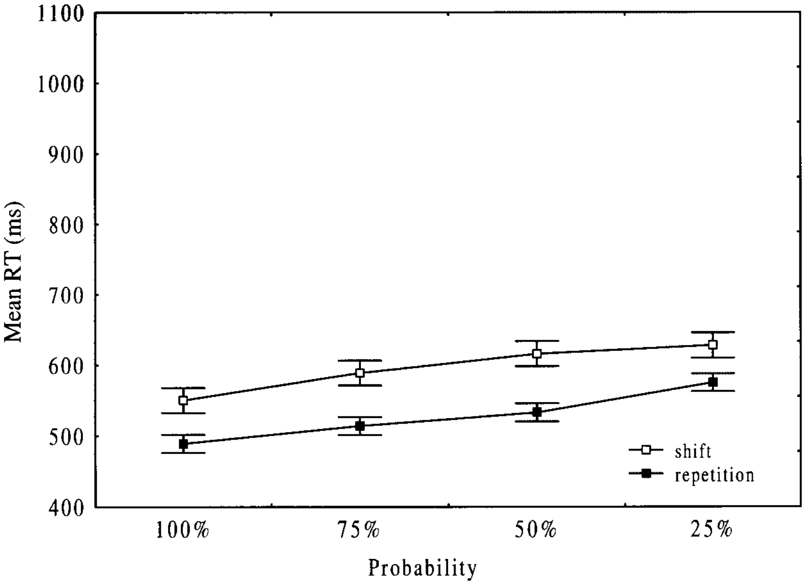


Figure 3. Mean reaction time (RT) as a function of probability and task type in Experiment 2. Error bars represent 95% within-participant confidence intervals.

ticipants actually used the cues to prepare for the upcoming task, one block was attached at the end of the experiment in which no cues were presented at all.

Experiment 3

Our goal in Experiment 3 was to show that shift tasks cannot be fully prepared for, even when participants are highly motivated and have run through a long period of practice. Further evidence for the assumed additivity of an expectation and an activation advantage of task repetitions could be provided if switch costs remained reliable under the given conditions.

Method

Stimuli and procedure. The stimuli and procedure were identical to those in Experiment 2 with three exceptions. First, to facilitate their encoding, probability cues were presented in the center of the task frame. Hence, cue—and later the imperative stimulus—appeared at the same position. Second, the experiment was extended to 12 experimental blocks divided in three sessions over 3 days. Third, in the last experimental block of each session, participants could gain points for correct responses when they reached RTs that were below a certain standard of comparison. This standard was calculated individually for each participant using the mean RT of the first experimental block of each session as standard. Participants were informed about the possibility of gaining points. However, the standard of comparison was not explained. The aim of this manipulation was to strengthen the motivation of participants at the end of each session. Results of points were not used for further analysis.

Session 1 of Experiment 3 followed the procedure of Experiment 2 except that the use of cues was emphasized repeatedly in the instruction. At the beginning of Block 4, participants were informed about the opportunity to gain points for quick and accurate responses. The individual who collected the most points by the end of the last session would win a bottle of champagne. At the end of this block, participants received feedback about their mean RT and error rate as well as about the sum of points they won. In Sessions 2 and 3, there was only one practice block, which was comparable to the third practice block of Session 1 but included only 100 tasks.

To keep participants from acquiring direct stimulus–response mappings, the material was changed between sessions. In Session 2, the material from the experimental blocks of Session 1 were used for the practice block; accordingly, the material from the practice blocks of Session 1 were now used for the experimental blocks. In Session 3, the material was assigned to practice and experimental blocks in the same way as in Session 1. Session 2 again was the same as Session 1 except for the shortened practice block and the material assigned. In Session 3, participants could already gain points within the third experimental block. The fourth experimental block served as a control to check whether the cues were used at all from participants with a high degree of practice. The reward system was maintained in this block, but no probability cues were presented. Instead, neutral cues were offered before each task. They consisted of four small gray squares and thus did not contain any information about the upcoming task, but the opportunity to predict the stimulus onset was held constant (Niemi & Näätänen, 1981).

All rewarding blocks were intentionally placed at the end of each session. We considered this to be an especially conservative way of testing whether advance reconfiguration is possible by motivating the participants at a point when they already had acquired a high level of practice.

Participants. Thirteen male students (age range = 21–30 years, $M = 24.38$ years, $SD = 2.57$ years) from the University of the Federal Armed Forces, Hamburg, served as participants on a voluntary basis. Again, none had participated in Experiment 1.

Design. A 2 (task type: shift, repetition) × 4 (probability: 1.00, .75, .50, .25) × 3 (practice: first session, second session, third session) × 2 (reward: with, without) factorial design was used. All factors were manipulated within participants. Response latencies and error rates served as dependent measures.

Results and Discussion

Error rates. Overall accuracy was high. Error rates for the task and probability conditions ranged from 1.7% to 4.5% ($M = 3.46\%$, $SD = .91\%$).

RT data. Figure 4 illustrates mean RTs as a function of task type and probability condition equalized over all three sessions. Apparently, we were able to replicate the findings from the previous experiments once again. Task repetitions were executed faster than task shifts in all probability conditions. The probability effects were also found for both shifts and repetitions. These results are supported by highly significant main effects for task type, $F(1, 12) = 90.94$, $MSE = 351.29$, $p < .01$, and probability, $F(3, 36) = 46.71$, $MSE = 730.09$, $p < .01$. Furthermore, switch costs between tasks of the same probability were always reliable (all *F*s > 26, all *p*s $< .01$). Again, tests for linearity were significant for both task shifts, $F(1, 12) = 61.77$, $MSE = 805.45$, $p < .0001$, and task repetitions $F(1, 12) = 51.50$, $MSE = 1{,}009.72$, $p < .0001$. As in the previous experiments, the Task Type × Probability interaction did not reach statistical significance, $F(3, 36) = 2.18$, $p > .10$.

Effects of practice and motivation. To investigate the effects of practice and motivation, mean RTs of each session for rewarded and unrewarded blocks were computed separately and entered into a 2 (task type: shift, repetition) × 4 (probability: 1.00, .75, .50, .25) × 3 (Session: first, second, third) × 2 (reward: with, without) ANOVA. Mean RTs for shifts and repetitions in the different cue conditions are depicted in Figure 5A (unrewarded blocks) and B (rewarded blocks).

As can be seen from Figure 5A and B, RTs decreased with each day of practice. More important, this effect is found to the same extent for shifts and repetitions. Correspondingly, the main effect for the factor practice reaches statistical significance, $F(2, 24) = 33.57$, $MSE = 4{,}456.71$, $p < .01$, whereas the interaction between task and practice is far from statistical significance ($p = .74$). RT of shifts and repetitions in the rewarded blocks, as depicted in Figure 5A, were faster than in the unrewarded blocks (see Figure 5B). These findings are confirmed by a significant main effect for the factor motivation with $F(1, 12) = 43.89$, $MSE = 840.87$, $p < .01$, whereas the Motivation × Task Type interaction and the Task × Motivation × Practice interaction did not reach statistical significance ($p = .30$ and $p > .20$, respectively). Thus, increasing practice and motivation generally led to faster RTs for repetitions and shifts, but again the activation advantage for task repetitions was not affected by this manipulation.

The main goal of Experiment 3 was to determine whether, even under optimal experimental conditions (highly practiced, highly motivated, 100% anticipation, univalent stimuli), a task shift cannot be completed without additional costs. Mean latencies for shifts and repetitions of the third (rewarded) block of Day 3 under the 100% condition were compared: The difference was 28 ms and highly significant, $F(1, 12) = 15.12$, $MSE = 338.79$, $p < .01$. Apparently, it is not possible to overcome the activation advantage for task repetitions in advance, even under these ideal conditions.

Control block (no cue condition). To control whether, after 3 days of practice, the cues were used at all to prepare for the upcoming task, we presented in the last block of the last session no probability cues. Instead, to hold the predictability of the stimulus onset constant, only neutral warning signals in the form of four small gray squares were given. In this block, mean RT for task shifts made 562 ms, which is slower than the slowest RT for shifts under the .25 condition in the preceding block. Task repetitions reached a mean RT of 497 ms, which corresponds to the time taken for the .50 repetitions in Block 11.

Thus, apparently the cues were used even after 3 days of practice. The higher switch cost in this control block fits our initially stated assumption that generally, switch costs consist of at least two additive components: the activation advantage and the implicit expectation advantage for task repetitions. Because the expectation advantage is not equaled in this control block, task repetitions enjoy both advantages (or task shifts suffer from both disadvantages, respectively), thus resulting in higher switch costs.

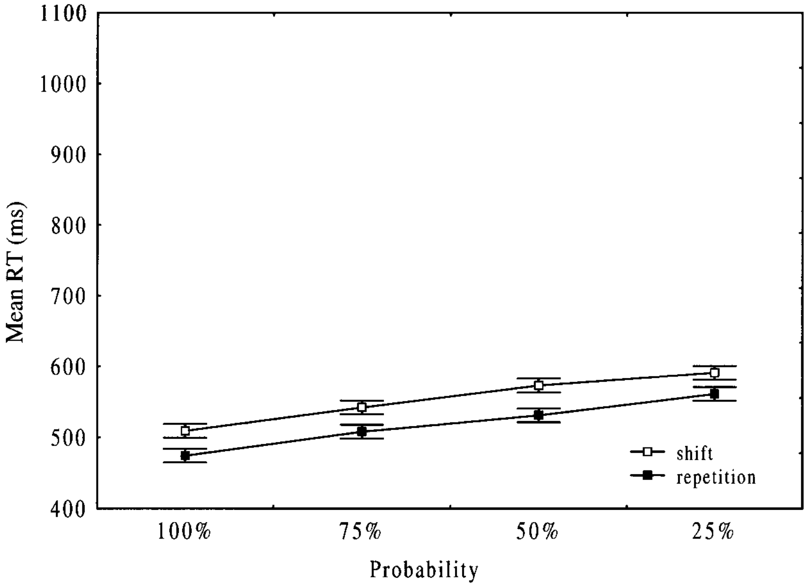
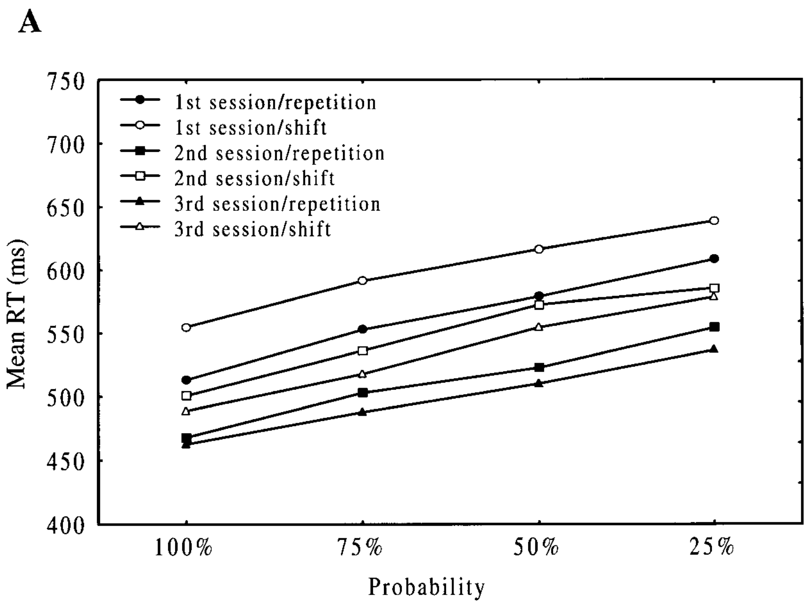
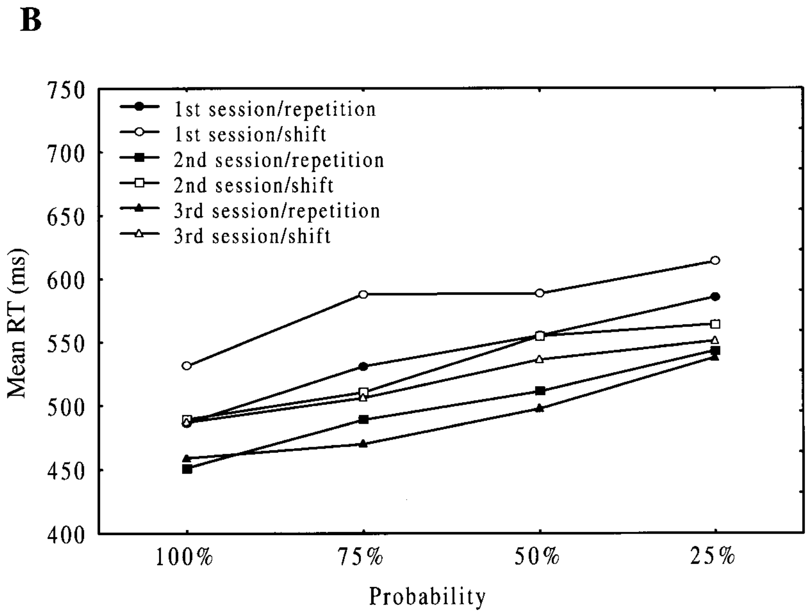


Figure 4. Mean reaction time (RT) as a function of probability and task type matched over all three sessions in Experiment 3. Error bars represent 95% within-participant confidence intervals.



unrewarded blocks



rewarded blocks

Figure 5. Mean reaction time (RT) as a function of probability, task type, and session of (A) unrewarded blocks and (B) rewarded blocks in Experiment 3.

The results of Experiment 3 again replicate those from Experiments 1 and 2. Generally, task repetitions have an implicit expectation advantage and an activation advantage over task shifts. Whereas the expectation advantage can be compensated by providing explicit probability information in advance, the activation advantage is not penetrable by preparation. Moreover, we showed that switch costs remain significant after 3 days of practice and motivational instruction. Practice and motivation mainly affect general task performance for task repetitions as well as for task shifts. Hence, related to overall response speed, residual switch costs represent a constant phenomenon that cannot be overcome in advance.

General Discussion of Experiments 1–3

Experiments 1, 2, and 3 provided evidence that task preparation is not limited to task shifts. None of the experiments yielded a significant Task Type × Probability interaction, indicating that the probability cues affected task repetitions and task shifts to the same extent. Even though arguing with a null effect might seem to be problematic, given that we could replicate the results in three experiments, we interpret these results as clear evidence for the additivity of task preparation (represented by the nearly identical probability effect for shifts and repetitions) and task set reconfiguration (represented by a constant switch costs between tasks of the same announced probability). This result supports our assumption that shift-specific preparation effects based on a timing manipulation are due to an expectation advantage for task repetitions. The remaining RT difference between tasks of equal probability can be attributed to an activation advantage for task repetitions. That this RT difference remained reliable even with univalent stimuli further supports the interpretation of switch costs in terms of a repetition benefit: The slower RTs for task shifts obviously do not represent the costs to overcome the task interference from the previous task; in Experiments 2 and 3, tasks did not interfere (see also Ruthruff et al., 2001). In turn, the faster RTs for task repetitions represent the facilitation resulting from repeated task performance, or the repetition benefit.

The question arises as to what kind of processes account for the observed preparation effects. Imagine, for example, the cue that indicates at .75 a task repetition and at .25 a task shift. It seems intuitively plausible to assume that the probable .75 task repetition is activated during the preparation interval. However, what happens to the improbable .25 task shift for which we always obtained the longest latencies? Is it only the activation of the probable task that "occupies" the cognitive resources and thus necessarily leads to slower RTs for the improbable task? Alternatively, are there two processes at work: activation of the probable task and inhibition of the improbable task? We refer to the assumption implied in the first question as the "limited-capacity hypothesis" and to the second one as the "inhibition hypothesis."

Limited-Capacity Hypothesis

In the 1970s, several priming studies varied the probability of the match between prime and target (e.g., Posner & Boies, 1971; Posner & Klein, 1973; Posner & Snyder, 1975). The general finding was that the higher the probability of the correspondence between prime and target, the faster were the RTs compared with a neutral prime. Correspondingly, in the few (improbable) cases of a mismatch between prime and target, RTs were higher compared with the neutral prime. Moreover, the following dependency between priming effects and foreperiod was reported: Given short preparation intervals, only facilitatory effects were found, whereas additional inhibitory effects could be identified only with long intervals. Posner argued, with reference to Keele and Boies (1973), that with the onset of the prime an automatic, resource-independent activation starts, and that only in the further course of preparation does a conscious, attention-demanding processing of the prime occur. This conscious processing of the prime, given long preparation intervals, then leads to slowed ("inhibitory") responses for improbable stimuli because less processing capacity is available.

Applied to our results so far, this would mean that the preparation of the probable .75 task necessarily would lead to slowed RTs for the improbable .25 task because of limited cognitive resources.

Inhibition Hypothesis

It is also reasonable that two distinctive processes occur during the preparation interval: the preparation (i.e., activation) of the probable task and the inhibition of the improbable task.

Inhibitory mechanisms in the course of selective attention have been discussed for a long time and especially attracted attention in the last few years (e.g., Houghton & Tipper, 1994; Milliken & Tipper, 1998; Neill, 1977; Tipper & Cranston, 1985; Tipper, Weaver, & Houghton, 1994). The main assumption is that activating a target within a number of distracting stimuli is not enough to ensure a correct response. Additionally, the inhibition of possibly disturbing stimuli is assumed. Evidence for this inhibition hypothesis comes from slowed response latencies for targets that represented the distractor in the preceding trial (e.g., Neill, 1977).

In this context, a study from Mayr and Keele (2000) is worth mentioning because the authors provided evidence for inhibitory processes during task switching. They showed that response latencies were slowed as a result of persisting inhibition when people had to switch to a task that had been abandoned two tasks before. For example, they compared task sequences like *ABC* with *CBC* and found slower latencies for the second *C* in the second sequence as compared with the *C* in the first sequence. This backward inhibition—and thus the inhibition of the previous task—was found only when participants were given the opportunity to prepare for the upcoming task. This result was taken as evidence that backward inhibition is a top-down, controlled executive process.

With regard to our own results, this means that the certain announcement (1.00) of a task shift should trigger the inhibition of the previous, and now no longer relevant, task. The experimental paradigm applied in our studies is not appropriate to examine this effect. However, the slowed latencies for improbable (.25) task repetitions fit perfectly to the assumption of backward inhibition: During the preparation of the probable .75 task shift, the improbable .25 task repetition is inhibited. So far, as the expression "backward inhibition" suggests, inhibitory processes during task switching were assumed to act exclusively on the preceding, and now no longer relevant, task. To us it seems plausible to assume that this inhibition might also act "forward" on other possibly interfering tasks. This could explain the RT data for the improbable .25 shift tasks in Experiments 1, 2, and 3: The improbable shift task might have been inhibited during the simultaneous activation of the probable task repetition.

Experiment 4

The purpose of Experiment 4 was to investigate whether the slowed latencies for the improbable .25 tasks were due to the inhibition of the specific improbable task or to limited processing capacity in the course of the simultaneous preparation of the probable .75 task. For that purpose, in Experiment 4, we only presented cues of the .75/.25 and .25/.75 types. Furthermore, the cues differed with respect to the specificity of the information they carried. In one half of the experiment, the cues were as informative as in Experiment 1; in the other half, the cues were semispecific (i.e., they provided full probability information for task shifts and task repetitions but did not announce which particular task type would appear in the case of a shift). The critical condition is the cue that indicates a task repetition with a probability of .75 and a task shift with a probability of .25. If the capacity hypothesis proves correct, we should again find slowed latencies for the improbable task shifts for both semispecific and specific cues because the (capacity-demanding) preparation for the probable task repetition should not differ between cue conditions. In contrast, if the inhibition hypothesis applies, then the improbable shift task should be executed faster in the semispecific condition than in the specific condition. The reasoning is that, in the case of semispecific cues, the inhibition of the specific improbable task is not possible because participants do not know which shift task will follow in the improbable case.

Method

Stimuli and procedure. The experimental approach was the same as in Experiment 1 with Stroop-like tasks. In contrast to the previous experiments, the stimuli consisted of eight different characters for both letter and digit tasks: the letters *D*, *E*, *H*, *I*, *O*, *P*, *T*, and *U* (reference letter *M*) and the digits *1*, *2*, *3*, *4*, *6*, *7*, *8*, and *9* (reference digit *5*; see Table 3). The enlarged stimulus pool allowed us to use the same stimuli for practice and experimental trials because, in this experiment, participants should not be able to acquire direct stimulus–response mappings.

We presented only two different probability cues: the .75/.25 and the .25/.75 cue. One block consisted of 125 items; the first five trials served as warm-ups. Of the remaining items, 60 were preceded by a .25/.75 cue (followed by 15 repetitions and 45 shifts) and 60 by a .75/.25 cue (followed by 45 repetitions and 15 shifts correspondingly). Task types as well as the shifts from and to different task types were again counterbalanced; tasks and probability cues were presented in random order.

To ensure that participants would not make use of the semispecific cues given their less informative value compared with the specific cues, we presented the cue types (specific vs. semispecific) in a blocked design: Half of the participants had to work through three blocks with specific cues and then three blocks with semispecific cues. The order was reversed for the other half.

Participants. Seventeen male students (age range = 21–28 years, $M = 23.3$ years, $SD = 1.79$ years) from the University of the Federal Armed Forces, Hamburg, served as participants on a voluntary basis.

Design. A 2 (cue: specific, semispecific) × 2 (task type: shift, repetition) × 2 (probability: .75, .25) within-subjects design was used. Response latencies and error rates served as dependent measures.

Results and Discussion

Because the stimuli did not change between practice and experimental trials in this experiment, the first experimental block was included, resulting in three blocks for each cue condition.

Error rates. Mean error rates for task, cue, and probability conditions ranged from 2.06% to 4.8% ($M = 3.47\%$, $SD = .97\%$).

RT data. A 2 (block order: specific first vs. semispecific first) × 2 (cue type: specific vs. semispecific) × 2 (task type: shift vs. repetition) × 2 (probability: .75 vs. .25) ANOVA showed that block order did not affect the general data pattern and did not interact with any other factor either (all $p\text{s} > .60$). Therefore, we collapsed data over the block order conditions and only report these collapsed data. Figure 6 presents mean RTs as a function of cue type, task type, and probability condition. We first report the results separately for each cue condition and then provide a comparison based on cue conditions.

Specific cues. The left panel of Figure 6 displays the results for the specific cue condition. Again, we found equal probability effects for task shifts and task repetitions; task repetitions reached faster RTs than task shifts. A 2 (task type: shift vs. repetition) × 2 (probability: .75 vs. .25) ANOVA confirms this observation with significant main effects for task type, $F(1, 16) = 50.06$, $MSE = 8{,}298.49$, $p < .01$, and probability, $F(1, 16) = 37.19$, $MSE = 9{,}724.10$, $p < .01$. The Task × Probability interaction was far from statistical significance ($p > .6$). The results thus confirm those of Experiments 1 to 3.

Semispecific cues. The right panel of Figure 6 displays the RTs for the semispecific cue condition. Task repetitions were again faster under the .75 condition compared with the .25 condition. However, task shifts did not show any probability effects. In fact, although the overall RTs were slower compared with task repetitions, the announced probability did not have any effect. A 2 (task type) × 2 (probability) ANOVA was conducted, yielding significant main effects for task type, $F(1, 16) = 114.54$, $MSE = 7{,}461.71$, $p < .01$, probability, $F(1, 16) = 9.85$, $MSE = 2{,}098.79$, $p < .01$, and a significant Task Type × Probability interaction, $F(1, 16) = 8.26$, $MSE = 1{,}651.53$, $p < .02$. The interaction is justified by a significant difference between the task repetitions under the different probability conditions, $F(1, 16) = 15.26$, $MSE = 2{,}225.37$, $p < .01$, and the small difference of 7 ms between the .75 and .25 task shifts ($p > .60$). Apparently, the information about the probability of an upcoming task shift alone could not be used for preparation. We discuss this point later.

Comparison between specific and semispecific cue conditions. We conducted planned comparisons of single data points between the different cue conditions. Figure 6 shows that the improbable shift task (.25 probability) was executed about 42 ms slower under the specific cue condition compared with the semispecific cue condition. This difference is statistically significant, $F(1, 16) = 6.82$, $MSE = 2{,}242.86$, $p < .02$. Thus, even though participants under the specific condition did know which specific task would be presented in case of an improbable task shift, they were slower than under the semispecific condition, in which they did not know which specific task in case of a task shift would follow. It cannot be argued that participants did not use the semispecific cues as a result of their lower utility because the corresponding probable .75 task repetitions did not differ between cue conditions ($p >$

Table 3
Stimuli, Tasks, and Response Keys Used in Experiment 4

Stimulus	Color	Judgment	Response key
1, *2*, *3*, *4*, *6*, *7*, *8*, *9*	Red	Odd or even?	Outer black keys
	Yellow	Larger or smaller than *5*?	Inner white keys
D, *E*, *H*, *I*, *O*, *P*, *T*, *U*	Blue	Consonant or vowel?	Outer black keys
	Green	Before or after *M*?	Inner white keys

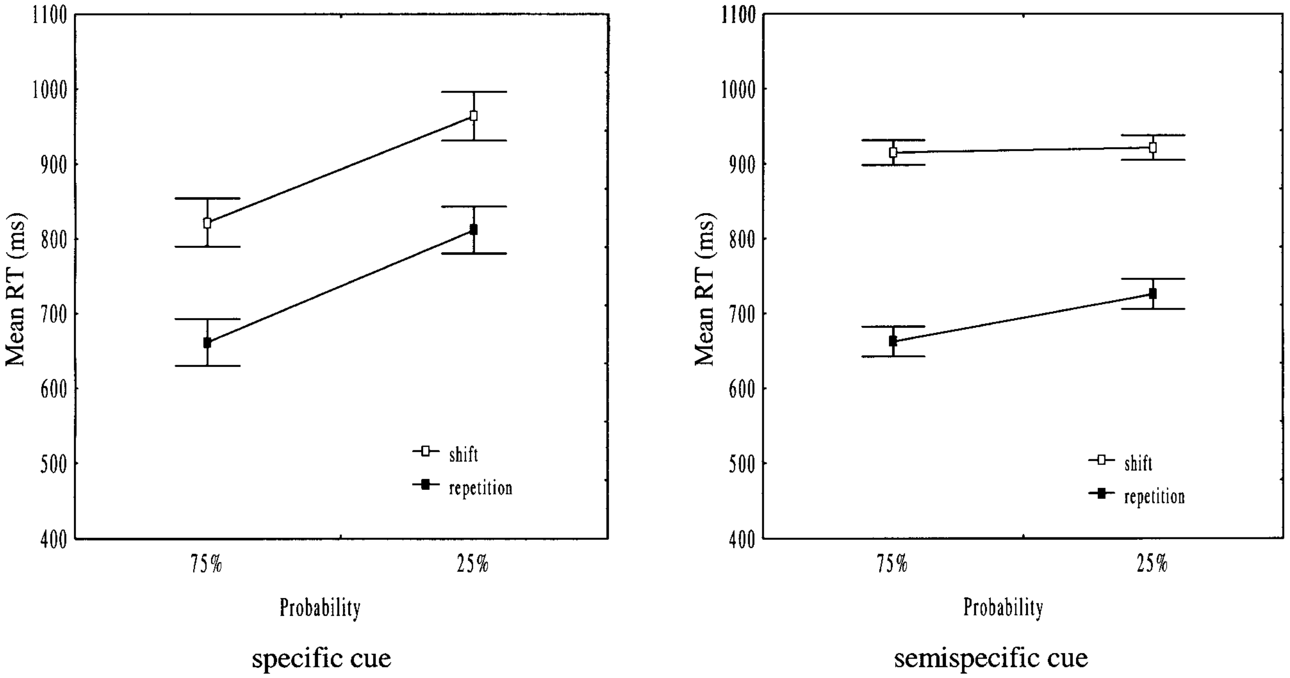


Figure 6. Mean reaction time (RT) as a function of probability and task type in Experiment 4. Error bars represent 95% within-participant confidence intervals.

.90). It follows that the preparation of the probable task repetition does not necessarily result in slowed RTs for the improbable task shift, as the limited-capacity hypothesis suggests. In contrast, the obtained results support the inhibition hypothesis: The slower RTs for improbable shift tasks after specific cues are due to the preparatory inhibition of the specific shift task.

The next analysis compares the .25/.75 cue (.25 repetition and .75 shift) under the different cue conditions. The probable shift task could be prepared under the specific cue condition but not under the semispecific cue condition, in which participants did not know which specific task would probably follow. Correspondingly, RTs for task shifts are about 93 ms faster in the specific compared with the semispecific cue, $F(1, 16) = 14.05$, $MSE = 5{,}202.06$, $p < .01$. The corresponding improbable task repetitions also differ significantly, $F(1, 16) = 9.67$, $MSE = 6{,}519.31$, $p < .01$; task repetitions under the specific condition yielded about 87 ms slower RTs compared with the semispecific cues. Thus, it seems that the inhibition of the improbable task repetition depends on the simultaneous activation of the probable task. This finding fits perfectly with those obtained by Mayr and Keele (2000), who found evidence for backward inhibition only when people were given the chance to prepare for the upcoming task shift.

Our goal in Experiment 4 was to investigate the cognitive processes during the preparation interval. The question was whether preparation after the presentation of a specific .75/.25 or .25/.75 cue involves only one process (the activation of the probable task, resulting in less capacity and hence slowed latencies for any other task) or two processes (the activation of the probable and the inhibition of the specific improbable task). The results suggest that the improbable specific shift task is inhibited during preparation of the probable task repetition. Otherwise, one cannot explain why participants are faster in executing the improbable shift task under the semispecific condition: Even though participants obtain no information about the specific improbable shift task, RTs are faster than with cues providing full information about the specific task. To us, the most plausible reason for this data pattern is that a specific shift task cannot be inhibited in the semispecific condition because participants do not know which specific task will follow in case of a shift. One alternative explanation might be a combination of both hypotheses. So far, we cannot exclude the possibility that, in the case of the semispecific cue, all three (remaining) improbable shift tasks were inhibited but to a weaker extent as a result of limited capacity. However, the important point is that the results can be explained without the assumption of limited capacity but not without the assumption of additional inhibitory processes.

For the .25/.75 cue (.25 repetition, .75 shift), in both cue conditions, it should be possible to inhibit the specific improbable task repetition. Nonetheless, the improbable task repetitions in the specific cue conditions were slower, and hence more inhibited, than in the semispecific condition. If we assume that the inhibition of the improbable task repetitions is an appearance of backward inhibition (Mayr & Keele, 2000), we should have expected this result. Mayr and Keele showed that backward inhibition appears only when participants are given the opportunity to prepare for the upcoming task (Experiment 3). Applied to our experiment, the improbable task repetition should be inhibited only when a cue at the same time provides information about the specific probable shift task. This is exactly what we found.

Taken together, the results of Experiment 4 emphasize the importance of inhibitory processes during task switching. We showed that inhibition affects not only the preceding and now no longer relevant task but also other, possibly interfering tasks. Further, the findings of Mayr and Keele (2000) can be extended. In their final discussion, the authors raise the question of whether backward inhibition occurs during the preparation interval or with the stimulus onset of the new task. Our results suggest the first alternative; an inhibition that only starts with the stimulus onset should not have shown different effects in specific and semispecific cue conditions. An inhibition occurring with the stimulus onset should have evoked slowed latencies in any improbable case regardless of whether participants knew in advance which kind of task type would follow.

A second interesting finding of Experiment 4 is the lacking probability effect for task shifts under the semispecific cue condition. Apparently, the information that a task shift is more or less probable cannot be used to prepare for the upcoming shift. This finding is of theoretical interest because it again emphasizes our assumption derived from Experiments 1 to 3 that preparation in the task-switching paradigm is not shift specific but rather task specific and, accordingly, depends on specific information about the upcoming task. An unspecific preparation for a task shift does not seem to be possible.

To investigate whether this holds true even for the case of a certain (1.00) announcement of a task shift, a further experiment was performed. In Experiment 5, we presented solely semispecific cues but included the 1.00 and .50/.50 cues. A lack of preparation effects for task shifts under the four different probability conditions would provide further evidence that preparation—activation as well as inhibition—is dependent on specific information about the particular task.

Experiment 5

Experiment 5 investigated whether even the announcement of a certain task shift (1.00), without further specifying the particular task, can be used for preparation. In Experiment 4 we showed that task preparation includes not only the activation of probable tasks but also the inhibition of improbable tasks. Furthermore, we found that inhibition depends on the simultaneous activation of another, more probable task. If this result proves reliable, we should find no preparation gain for task shifts, even in the case of the 1.00 shift cue.

Method

Stimuli and procedure. Except for the cues, the experimental approach was the same as in Experiment 1 with Stroop-like tasks. The semispecific probability cues appeared as four small colored squares at the upper edge of the task square. The small squares that announced a task shift were presented in dark gray, thus providing information only about the probability of the task shift, not about the particular task. The small squares that indicated a task repetition were again presented in the corresponding color. For example, a .50/.50 cue presented as two small squares in the color of the preceding task and two small gray squares. Correspondingly, four small gray squares indicated a 100% probability for a task shift without giving any information about the particular task type that would follow. We expected a probability effect for task repetitions but not for task shifts, because specific task preparation should not be possible.

Participants. Ten male students (age range = 20–27 years, M = 24.0 years, SD = 2.7 years) from the University of the Federal Armed Forces, Hamburg, served as participants on a voluntary basis.

Design. A 2 (task type: shift, repetition) × 4 (probability: 1.00, .75, .50, .25) within-subjects design was used. Response latencies and error rates served as dependent measures.

Results and Discussion

One participant was excluded from the analysis because of an overall error rate greater than 10%, which was taken as indicator that he did not follow the instructions. Data of the remaining 9 participants were analyzed analogous to the previous experiments.

Error rates. Overall accuracy was high. Error rates for the task and probability conditions ranged from 2.2% to 4.7% (M = 3.77%, SD = 1.59%).

RT data. Figure 7 illustrates mean RTs as a function of task type and probability condition. Two conclusions are apparent: Task repetitions can be executed faster than task shifts under all probability conditions. Furthermore, RTs for task shifts are nearly the same in all four probability conditions, whereas for task repetitions we again found increasing latencies with decreasing probability.

These results are supported by significant main effects for task type, $F(1, 8) = 65.05$, $MSE = 15{,}442.54$, $p < .0001$, and probability, $F(3, 24) = 4.02$, $MSE = 1{,}534.11$, $p < .02$. Tests for linearity were significant for task repetitions with $F(1, 8) = 32.07$, $MSE = 963.94$, $p < .001$, but not so for task shifts ($p > .70$). Correspondingly, the interaction between task and probability reaches statistical significance, $F(1, 8) = 6.51$, $MSE = 728.70$, $p < .01$. This result confirms that the cues could be used only to prepare for the specifically announced task repetitions but not an unspecific task shift.

In Experiment 5 we replicated the finding of Experiment 4 that preparation depends on specific information about the upcoming task. The probability information alone cannot be used to prepare for a task shift. Even in the case of a 100% shift cue, RTs are not faster than under the other probability conditions. Hence, the certain information that at least the preceding task will not be repeated cannot be used to inhibit this particular task. This result fits well with those of Experiment 4, which showed that inhibition occurs only in the course of the simultaneous activation of another task. As stated previously, this result also corresponds with the findings of Mayr and Keele (2000), who found patterns of backward inhibition only when specific cues were provided.

General Discussion

We wanted to show that preparation is not specific to task shifts but, rather, is additive to the actual switching process. The assumed additivity should support the interpretation of switch costs in terms of a repetition benefit, the activation advantage for task repetitions that cannot be overcome in advance. Finally, we further examined cognitive processes during task preparation.

Our basic assumption was that task repetitions and task shifts differ with respect not only to the pretrial (with task repetitions being preceded by the same task type and task shifts being preceded by a different task type, resulting in an activation advantage for task repetitions) but also to the subjective implicit expectancy for the upcoming task type, with an implicit expectation advantage for task repetitions. This expectation advantage leads to seemingly shift-specific preparation effects in common cuing paradigms

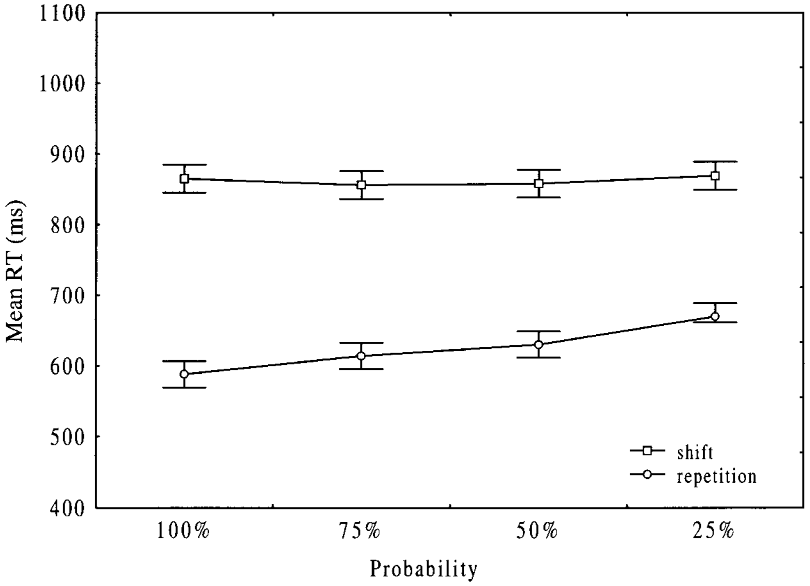


Figure 7. Mean reaction time (RT) as a function of probability and task type in Experiment 5. Error bars represent 95% within-participant confidence intervals.

based on a timing manipulation, because a cue that indicates a subjectively unexpected task shift provides more information than a cue that indicates an anyway-expected task repetition.

In accordance with these assumptions, the results of Experiments 1, 2, and 3 provide clear evidence that manipulating explicitly the specific expectancies for task repetitions and task shifts affects both task types to the same extent. Thus, task preparation is by no means shift specific but, rather, is additive to the actual switching process. The additivity between task preparation and task activation is confirmed by our finding that the RT difference between shifts and repetitions remained constant between tasks of the same probability. These remaining switch costs cannot be overcome in advance, even after long periods of practice, and thus reflect the activation advantage for task repetitions: the repetition benefit.

In Experiments 4 and 5, we examined the preparatory processes in greater detail. We showed that the observed preparation effects can be attributed to the activation of probable tasks and the inhibition of improbable tasks. Furthermore, it was shown that inhibition occurs only when a specific other task can be activated simultaneously. By preventing the activation of a probable task (e.g., by withholding specific task information as was the case in the semispecific cue condition), the corresponding specific improbable task is not inhibited.

Expectancies

Our basic assumption was that shift-specific preparation effects based on the manipulation of the preparation interval are mainly due to an implicit expectation advantage for task repetitions. Of course, it is critical to argue with subjective expectations because they always lie beyond our direct observation. However, by presenting probability cues that explicitly varied the expectancies for the upcoming tasks, we were able to emphasize the impact that expectations can have on task performance. We showed that an expected task shift can be executed faster than an unexpected task repetition: In the case of the .25/.75 cue (.25 repetition and .75 shift) in Experiment 1, the induced expectation advantage for the task shift even covered the activation advantage for the task repetition, resulting in negative switch costs of 73 ms! Hence, we showed that expectancies can be altered in advance, and thus underlie strategic control, whereas the activation advantage for task repetitions represents an automatic carryover effect that is by no means penetrable.

Taken together, our results suggest that expectancies play an important role in the task-switching paradigm and, if not equaled between repetitions and shifts, will always distort the measurement of switch costs: An expectation advantage for task repetitions will result in an overestimation of the actual switch costs (see, e.g., RTs after the .75/.25 cue). An expectation advantage for task shifts will result in an underestimation of the actual switch costs (e.g., RTs after the .25/.75 cue).

Activation and Inhibition

In Experiments 4 and 5, we showed that the preparatory processes resulting from different expectancies for task repetitions and task shifts involve activation and inhibition. So far, we have argued that preparatory processes can intentionally be controlled. Apparently, however, this does not hold for both activation and inhibition. In Experiment 5, the unspecific 1.00 announcement of a task shift could not be used to prepare for the upcoming shift task even though one possible strategy would have been the inhibition of the previous task. However, this is not what the results suggest. Obviously, the inhibition of one specific task without the simultaneous activation of another task is not possible. This mechanism might be comparable to the phenomenon that we are unable to intentionally not think of, for example, a specific word or a specific event unless we think of some other issue (Wegener, 1997). Hence, intentionally, we only can replace one thought by another, which then, as a result, might be inhibited. Applied to our paradigm, this means that the inhibition of a specific task is a kind of an epiphenomenon of the activation of another specific task.

The conceptualization of inhibition as a by-product of activation implicitly suggests the involvement of different control mechanisms: Whereas the activation of a specific task underlies intentional control, the inhibition represents an automatic process that lies beyond our strategic influence. From this, it follows that inhibition not only cannot be initiated intentionally but it cannot be prevented either. Actually, there is empirical evidence for this failure to prevent inhibition. Mayr and Keele (2000, Experiment 5) found backward inhibition even when participants had prior knowledge of the whole task sequence and, accordingly, knew in advance that they would have to switch back to the inhibited task.

To summarize, the results suggest that inhibition during task switching is automatically triggered by the preparation of the upcoming task. It is effective not only on the previous task but also on other tasks that might interfere with the task at hand. Hence, comparable to the mechanism of lateral inhibition (Walley & Weiden, 1973), the function of this inhibition might be the protection of the system against any event that will conflict with the current goal.

Sources of Residual Switch Costs

By definition, task repetitions differ from task shifts with respect to the pretrial: Task repetitions are preceded by the same task, whereas task shifts are preceded by a different task, resulting in an activation advantage for task repetitions that apparently cannot be overcome in advance. Hence, we agree with others (Allport et al., 1994; Ruthruff et al., 2001; Sohn & Carlson, 2000) that switch costs can be attributed to a carryover effect from the pretrial. Therefore, the term "repetition benefit" would describe the phenomenon more appropriately. This interpretation gains further support from the fact that, even with univalent stimuli, task repetitions are faster than task switches (Experiment 2 and 3; Ruthruff et al., 2001). Hence, it is not the interference between different tasks that drives the cost to execute a task switch; rather, it is the repeated performance of the same task that facilitates the task repetition.

The results of Experiments 1, 2, and 3 as well as those of Sohn and Carlson (2000) and Ruthruff et al. (2001) provide strong evidence for the dissociation of preparation and switch costs. That is, the activation during preparation is completely independent from the activation advantage for task repetitions resulting from execution. Additionally, this dissociation gains further support by studies using event-related functional magnetic resonance imaging. These studies showed that different brain areas are active during task preparation and task execution (MacDonald, Cohen, Stenger, & Carter, 2000; Sohn, Ursu, Anderson, Stenger, & Carter, 2000). For example, MacDonald et al. (2000) investigated processes of preparation in a color versus word-naming Stroop task. Actually, they also reported a dissociation of preparation and execution: During preparation, the left dorsolateral prefrontal cortex (DLPFC) was active; during task execution, the anterior cingulate cortex (ACC) was active. Furthermore, the DLPFC activity during preparation was higher for the color-naming task compared with the word-naming task, suggesting that DLPFC might play a critical role in the implementation of control; the ACC activity during execution was higher for incongruent stimuli compared with congruent stimuli, suggesting a role in conflict monitoring.

To summarize the results of MacDonald et al. (2000), the engagement of different brain areas during task preparation and task execution implies not only different control demands during both stages, namely the implementation of control during preparation and the monitoring of task performance during execution. But the anatomical dissociation itself between task preparation and task execution might also explain why, during task switching, the preparatory activity never reaches the activation level of a just-executed task: Task preparation simply does not affect structures that are responsible for the execution of a task switch.

Conclusion

The experiments presented in this article suggest that the human ability to prepare for future events is supported by the activation of expected events and the simultaneous inhibition of possibly distracting events. Whereas the activation can be initiated intentionally, and thus underlies strategic control, the inhibition represents a process that automatically goes along with the activation of the expected task. However, the constant switch costs between equally expected (prepared) tasks show that the execution of a task will always result in an activation advantage for this particular task. This activation advantage can never be reached by preparation alone. Hence, the best preparation is task execution.

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