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Reconfiguration of Processing Mode Prior to Task Performance

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Participants performed choice reaction time (RT) tasks on 2-dimensional stimuli such that each task was based on 1 stimulus dimension. A cue preceded the target stimulus and instructed the participant about which (randomly selected) task to perform. Shifting between tasks was associated with an RT cost, which was larger when the (randomly varying) cue–target interval was short as opposed to when it was long. Cue–target interval was not confounded with the remoteness from the previous trial. Hence, it affected the task-shift cost through preparation rather than by allowing carryover effects to dissipate. Similar results were obtained for 2 location tasks and for the object-based tasks (color and shape discrimination). They indicate a time–effort consuming process that operates after a task shift, precedes task execution, and presumably reflects the advance reconfiguration of processing mode.

Cognitive psychologists have developed complex models to describe how participants perform on individual tasks. However, much less attention has been devoted to understanding the processes by which participants shift between tasks. There are at least three reasons why task shifting is important. First, patients with lesions in the frontal cortex show impairment in their ability to shift between tasks (Shallice & Burgess, 1991) and in the ability to switch attention between perceptual dimensions (Owen, Roberts, Polkey, Sahakian, & Robbins, 1991; see Shallice, 1994; Stuss & Benson, 1984; Stuss, Eskes, & Foster, 1994, for reviews). Second, normal participants show a substantial reaction time (RT) cost when shifting between tasks (e.g., Allport, Styles, & Hsieh, 1994; Biederman, 1972; Jersild, 1927). Finally, and this is the focus of the present study, task shifting is perhaps one of the best laboratory preparations by which "executive control" (Logan, 1985; Rogers & Monsell, 1995) can be studied.

The executive control has been described within a model by Norman and Shallice (1986; Shallice, 1988, 1994). According to these authors, behavior in familiar tasks is controlled by schemata, which select and coordinate the elementary processes that take place in task execution. For behavior to take place, a schema must first be selected or activated. The schema selection process is performed in two qualitatively different modes. Schemata are triggered automatically by external environmental cues. Because more than one schema can be activated, the model assumes a process of *contention scheduling,* in which schemata compete with one another and only one schema emerges from the competition as dominant. Schemata can be centrally selected, however, when the *supervisory attentional system* biases contention scheduling through top-down activation of a preferred candidate. Biasing is required when the environmental cues are novel and fail to trigger the appropriate schema. Another role of the supervisory attentional system is to prevent automatic selection of an inappropriate schema.

It is difficult to isolate executive functions because they affect behavior indirectly through the elementary processes that take part in task execution. In the case of a task shift, behavior is jointly determined by the control processes, which execute the shift act, and the lower level processes, which execute the task proper. Therefore, some a priori criteria are needed that will help in distinguishing between control processes and task-execution processes. Two necessary (but possibly insufficient) criteria are offered. The first criterion is that a control process must be specifically related to task shifting. By using the terms, *executive* and *control,* researchers draw an analogy between the mind and an organization. An organization can function without an executive under routine conditions. An executive involvement becomes necessary when the organization is faced with changing demands, such as after a task shift. The second criterion is proactivity. The hallmark of control functions is that they "command" elementary processes (as the terms *executive* and *control* imply), and commands precede the resultant performance. After a task shift, executive control processes reconfigure the system in advance of task performance. *Reconfiguration* (the term is borrowed from Rogers & Monsell, 1995) involves the selection, ordering, and coordination of a set of elementary processes that perform the task. In Norman and Shallice's (1986) model, advance reconfiguration is achieved through schema selection.

Previous research on task shifting has led to two important findings (as detailed below). It is argued that, at present, the findings cannot be taken as reflecting advance reconfiguration (and therefore executive control) because each of them meets only one of the two criteria that were listed above.

Task Shifting Is Associated With a Cost

Task shifting is associated with a decrement in performance called the *task-shift cost.* Most of the research (Allport et al.,

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1994; Bernstein, 1924, Biederman, 1972; Botwinick & Brinley, 1962; Botwinick, Brinley, & Rubin, 1958; Brinley, 1965; Dashiell, 1937; Hartley, Kieley, & Slabach, 1990; Jersild, 1927; Jones, 1915 cited by Bernstein, 1924; Kleemeier & Dudek, 1950; Meiran, Israeli, Levi, & Grafi, 1994; Pinard, 1932; Rubinstein, Meyer, & Evans, 1994; Schaie, 1955, 1958) failed even in showing that the effect is specific to task shifting. This is because, in the paradigm that was used, the task-shift cost was estimated by comparing two separate blocks of trials: one in which the participants shifted between tasks, and another that involved a single task. This leads to a confounding of the task-shift variable, because the two conditions differ in working-memory demands (Rogers & Monsell, 1995), division of attention between perceptual dimensions (Ward, 1982), and so on. Nevertheless, the cost has been shown to be specific to task shifting by other investigators who manipulated task shift within a block of trials (Biederman, 1972; Rogers & Monsell, 1995; Shaffer, 1965, 1966, 1967; Stablum, Leonardi, Mazzoldi, Umilta, & Morra, 1994). What still needs to be shown is that the cost reflects a process that precedes task execution.

If the cost is interpreted as reflecting a control process, it is presumably related to reconfiguration that is required after a task shift in order to set up the system to meet changing demands. In contrast, reconfiguration is not required when repeating the task from the previous trial because the system has already been set up. Evidence that supports this interpretation was reported by Proctor and Fisicaro (1977), whose findings suggest the involvement of central processes within the first few hundreds of milliseconds after shifting attention between perceptual attributes. Shifting attention between perceptual attributes is probably a part of the reconfiguration process, especially when participants shift between two tasks that involve the same set of stimuli. An example is the experimental paradigm that was used here and is described later in the article.

However, it is unclear if the cause of the task-shift cost on Trial *N* reflects the process of advance reconfiguration that took place prior to responding on that trial. An alternative explanation attributes the task-shift cost to a carryover from Trial $N - 1$ (Allport et al., 1994). An example is retroactive adjustment (e.g., Ward, 1982). The difference between advance reconfiguration and retroactive adjustment is highlighted in the following analogy. Imagine that the two tasks were swimming and running. The advance reconfiguration model assumes that before swimming, one puts on flippers and before running, one puts on running shoes. Shift costs, according to the advance reconfiguration model, reflect the added processing requirement associated with putting on the running shoes or the flippers prior to running or swimming. In contrast, the retroactive adjustment model suggests that, after a few strokes in the pool, one puts on the flippers to make the swimming more efficient. When shifting to a running task, the person performs inefficiently because they wear flippers rather than running shoes. Note that the alternative account does not imply that participants cannot prepare themselves in advance of performing the task, only that advance preparation is not reflected in the task-shift cost. The broader implication is that if the task-shift cost reflects retroactive adjustment, it should not be taken as an index of executive functioning. Retroactive adjustment may be considered a form of learning, and learning can be automatic and not involve the executive. For example, Strayer and Kramer (1994) suggested that the retroactive adjustment of a strategy can take place within processes that are unaffected by central decisions, which are "cognitively impenetrable."

Foreperiod Effects in Task Switching

In some studies, the tasks were ordered randomly and the participants were given an instructional cue regarding which task to perform on the upcoming target stimulus. For example, LaBerge, Petersen, and Norden (1977) asked participants to perform either one of two tasks on target stimuli, which were pairs of digits: Decide if the digits match or decide if the value of the digits increased from left to right. LaBerge et al. found that advance warning regarding which task to perform facilitated performance relative to a condition in which the instructional cues were given as a part of the target. An analogous finding refers to performance facilitation as a result of an increase in the foreperiod between the instructional cue and the target stimulus (Bernstein & Segal, 1968; Biederman, 1973; Davis, 1967; Davis & Taylor, 1967; Sudevan & Taylor, 1987). Foreperiod effects may be interpreted as showing that the participants use the instructional cues to perform advance reconfiguration. Consistent with this interpretation is the finding that participants can use instructional cues to change their processing strategy in advance of task performance (Gratton, Coles, & Donchin, 1992; Logan & Zbrodoff, 1982, Logan, Zbrodoff, & Fostey, 1983, but see Strayer & Kramer, 1994, for a discrepant finding). Nonetheless, the findings regarding strategy preparation are only suggestive because a change in strategy is not identical to task shifting.

Foreperiod effects should certainly be taken as evidence for proactive processing. However, it is less clear whether they are specific to task shifting. This question arises in light of the following result. It was found that foreperiod affects RT not only when the instructional cue precedes the target stimulus but also has a similar effect on RT when the target stimulus precedes the instructional cue (e.g., Bernstein & Segal, 1968; Biederman, 1973; Davis, 1967; Davis & Taylor, 1967; LeMay & Simon, 1969). There are a number of alternative explanations that can easily account for the similarity between the cue-then-target condition and the target-then-cue condition. One explanation holds that participants use the instructional cue to predict target onset rather than perform advance reconfiguration. It is widely believed (see Niemi & Näätänen, 1981, for a review) that when the foreperiod changes randomly, its effect on RT is mediated by the prediction of target onset. This criticism holds for the experiments on task shifting in which foreperiod varied randomly (e.g., Davis, 1967; Davis & Taylor, 1967; Sudevan & Taylor, 1987).

A second alternative explanation is based on the literature on the number of alternatives in choice RT (e.g., Hick, 1952). According to this explanation, participants treat a task-shift experiment as involving a single task in spite of the instructions. To do so, participants do not consider the instructional cues as being distinct from the target stimulus but rather they respond to the combination of the cue and the target. This may be illustrated as follows: In the present study, there were two instructional cues, each made up of a pair of arrows (see Figure 1) and four target stimuli, corresponding to four quadrants of a square. It is conceivable that participants responded to eight complex stimuli, each being a combination of a given pair of arrows and a given quadrant. This explanation holds that the present paradigm involved an eight-choice RT task. Supplying the arrows in advance of the target stimulus narrows down the number of alternatives from eight to four, which explains why foreperiod affects RT (but see Biederman, 1972, for results that do not support this interpretation).

A third alternative explanation is based on the notion of perceptual interference. Presumably, presenting the instructional cue and the target stimulus simultaneously or in close temporal proximity interferes with their perceptual encoding. When either one of the elements, the cue or the target, precedes the other element, perceptual encoding is facilitated. This is because their separation in time prevents the perceptual interference.

Research Goals and Predictions

It was offered that the process of executive control in task shifting be identified according to two criteria: specificity to task shifting and proactivity. The literature review revealed that the task-shift cost has been shown to have only one characteristic, namely that it is specific to task shifting. However, it is still unclear if the cost reflects proactive processing. Foreperiod effects clearly reflect proactive processing, but it is yet unclear whether they are specific to task shifting. The goal of the present study was to demonstrate that the two effects reflect the same process, which therefore has both characteristics, that is, it is specific to task shifting and reflects proactive processing. This is shown later in the article by an interaction between foreperiod (cue–target interval) and task shift. In the present experiments, the tasks were ordered randomly and each trial was preceded by an instructional cue. In other words, the design was similar to that used in studies on foreperiod effects in task shifting (e.g., Sudevan & Taylor, 1987). In this design, task shift is defined as a sequential effect (e.g., Biederman, 1972; Shaffer, 1965). Trial N belonged to the *task-shift condition* if Trial $N - 1$ involved a different task than Trial N. If the two trials involved the same task, Trial N belonged to the *task-repeat condition.* It was predicted that compared with short foreperiods, long foreperiods would be associated with smaller task-shift costs. Stated differently, greater effects of foreperiod on RT were predicted for the task-shift condition as compared with the task-repeat condition.

Evidence for an Interaction Between Foreperiod and Task Shift

Hartley et al. (1990) manipulated the interval between an instructional cue and the target and found that an increase in the cue–target interval was associated with a decrease in the task-shift cost. However, this result is equivocal because task shift was manipulated between blocks of trials and this manipulation is confounded (see the full argument in the earlier section, *Task Shifting Is Associated With a Cost*). Shaffer (1965), who manipulated task shift within a block of trials, found that presenting an instructional cue one third of a second prior to the target stimulus led to a small reduction in shift costs (from 37 to 23 ms), as compared with a condition in which the instructional cue and the target were presented simultaneously. Unfortunately, these results (reported in Table 1 of Shaffer's article) were not accompanied by a formal statistical test. Another problem with Shaffer's design was a confound between cue–target interval and the remoteness from the previous trial (see Experiments 2 and 3 of the present article for details).

A related finding was reported recently by Allport et al. (1994, Experiment 5). These researchers did not use instructional cues. Instead, the participants were given pairs of trials and were either told to perform the same task on both trials (an AA or BB sequence of the tasks A and B) or shift tasks (an AB or BA sequence). Of interest are the results in the second trial in each pair. Allport et al. found a marginally significant reduction in the task-shift cost (which was observed only in the easier task) when the response–stimulus interval increased from 20 to 1,100 ms. This finding suggests that advance reconfiguration was involved. Nevertheless, Allport et al. explained the reduction in task-shift costs by assuming that the task-shift cost reflects a proactive inhibition-like process—a carryover effect. According to the authors, the carryover effect partly dissipated during the intertrial interval.

Unlike all the alternative explanations that were listed until now, the dissipating carryover effect (Allport et al., 1994) accounts for the interaction between foreperiod and task shift. The reason is that in all of the four relevant studies (Allport et al., 1994; Hartley et al., 1990; Rogers & Monsell, 1995; Shaffer, 1965), foreperiod was confounded with the remoteness from the previous trial. Prolonging the foreperiod not only allows more time for advance reconfiguration but also enables the dissipation of the carryover effect. Consequently, the results of the studies are equivocal and may not be taken as evidence for advance reconfiguration. In the present study, the explanation by Allport et al. was rejected because the predicted interaction between task shift and foreperiod was obtained under conditions in which foreperiod was not confounded with the remoteness from the previous trial (see Experiments 2 and 3).

Some evidence that suggests that Allport et al.'s (1994) dissipation explanation is wrong comes from the study by Rogers and Monsell (1995). These researchers used a technique that involves runs of trials of predictable length on a given task. In all but one experiment, the runs were of two trials in length, which means an AABBAABB . . . ordering of tasks (see also Stablum et al., 1994, Experiment 2). Task shift was defined as a sequential effect, just as in the paradigms that were based on instructional cues (e.g., Biederman, 1972; Shaffer, 1965). It was found that prolonging the response–stimulus interval from 150 ms to 600 ms led to a reduction in the task-shift cost, but when the interval was extended further to 1,200 ms, no additional significant reduction in the RT cost was observed. This suggested to Rogers and Monsell that advance reconfiguration was completed within 600 ms. Rogers and Monsell argued that Allport et al.'s explanation is unlikely to be correct because, presumably, the dissipation of the carryover effect is passive. Being passive, the process is predicted to operate regardless of the way in which foreperiod is manipulated. In contrast to this prediction, Rogers and Monsell found that prolonging the foreperiod led to a reduction in the task-shift cost when foreperiod was blocked but not when it varied randomly. The authors suggested a model that is based on a railroad metaphor:

> It is obvious that a sensible signalperson [the executive] would not pull the switch lever [reconfigure] if there was any possibility of a train's [task execution] running through the switch while the switching operation was still in progress, because the train would derail! (Rogers & Monsell, 1995, p. 218)

A problematic aspect regarding the metaphor is the assumption that presenting the target stimulus while advance reconfiguration is in progress will lead to the disruption of the latter process ("the train would derail"). However, according to the authors, "If a train needing to go down line B arrives before one has had the time to move the lever [reconfigure], it [the train] will have to wait. That is the origin of the switch cost" (Rogers & Monsell, 1995, p. 216). Hence, the authors assume that, in most cases, task performance awaits advance reconfiguration. It is unclear why presenting the target stimulus when reconfiguration is in progress causes a problem. In these instances, the executive could halt task execution and complete the reconfiguration process. Another reason why I do not find the railroad metaphor compelling is that blocking foreperiod is believed to affect a number of processes, such as predicting target onset (see Niemi & Näätänen, 1981, for a review).

The Paradigm and an Overview of the Experiments

The paradigm used in the present experiments overcomes most of the shortcomings that were mentioned above. First, the task-shift variable was manipulated within a block of trials so that the possible confounding with strategy differences, memory load, and so forth was overcome. In that respect, the present paradigm is as good as those used by Biederman (1972), Rogers and Monsell (1995), Shaffer (1965), and Stablum et al. (1994). Second, the use of instructional cues teases apart the process of advance reconfiguration from that of fast dissipation of carryover effects (Allport et al., 1994; see the present Experiments 2 and 3 for details). In this respect, the present paradigm has an advantage over that used by Rogers and Monsell (1995) and Allport et al. (1994). Shaffer (1965) used a paradigm that was conceptually similar to the present one but did not make use of this advantage. It should be noted that, as in most of the studies on task cueing (e.g., Sudevan & Taylor, 1987), the instructional cues were uninformative with respect to the location of the target stimulus, implying that the cues did not direct spatial attention to the position of the target stimulus (see Posner, 1980, for a review). A number of additional attributes of the present paradigm are discussed below.

Description of the Paradigm

The participants responded according to the position of a target stimulus, which was presented in one of the four quadrants of a 2 × 2 grid. The position of the target stimulus could thus be classified along both the vertical dimension and the horizontal dimension. These dimensions defined the two choice RT tasks that the participants performed: An up–down discrimination and a right–left discrimination. The two tasks were ordered randomly within a block of trials so that the participant must have been given an instructional cue in each trial in order to know which task to perform. The series of events within a trial consisted of (a) the presentation of an empty grid (for fixation), (b) an instructional cue that was presented for either a short or a long cue–target interval, and (c) the target stimulus. In all of the experiments, the cue–target interval varied randomly within a block of trials. The instructional cue was a pair of arrows. They remained on the screen until the response was made, to ensure that participants would not forget which task was required (see Jersild, 1927, Table 5; Rubinstein et al., 1994; Spector & Biederman, 1976, for demonstrations regarding the importance of a task reminder).

Same Responses in the Two Tasks and Compatibility

As the notion of advance reconfiguration suggests, task-specific preparation involves more than just the characterization (DeJong, Wierda, Mulder, & Mulder, 1988; Hendrikx, 1986; Reeve & Proctor, 1984, 1985) or the preparation (Miller, 1982, 1983, 1987; Rosenbaum, 1980) of a motor response. Reconfiguration probably involves earlier processing stages, including the relative importance of perceptual dimensions (Proctor & Fisicaro, 1977; Ward, 1982) and changing response-selection criteria (e.g., Shaffer, 1965). In support of this argument, task-shift costs are observed when participants switch between tasks that require the same motor responses (e.g., Rogers & Monsell, 1995; Shaffer, 1965). Because participants use the same motor responses, preparing for a task shift cannot be based on the preparation of a motor response. In contrast, when each task is associated with different motor responses, task-specific preparation may involve the selection of an effector. My colleagues and I (Moulden, Picton, Stuss, Meiran, Lins, et al., 1996) used such a preparation and found that the instructional cue elicited an increased negativity in the event-related potential recorded above the motor cortex on the side contralateral to the responding hand. This wave pattern is taken as evidence for hand selection (e.g., DeJong et al., 1988, and Gratton et al., 1992, for the reasoning). On the basis of Moulden et al.'s results, the decision was to use the same key presses in the two tasks. Each keypress was mapped to two interpretations of the target stimulus, one from each task. The two interpretations, up and down, were mapped to the 7 key (located on the upper part of the keypad) and the 3 key (located on the bottom of the keypad). The same keys were mapped to right (the 3 key, positioned on the right side of the keypad) and left (the 7 key, positioned on the left side of the keypad; see Figure 1). Half of the participants were assigned to use this pair of keys, the other half were assigned to use the 1 and 9 keys (not shown in Figure 1). The 1 and 9 keys were mapped to stimulus interpretations in an analogous manner: 1 indicated down and left, and 9 indicated up and right.

The price for using the same keypresses in the two tasks was an additional independent variable. On half of the trials, the participants could respond correctly even when considering the wrong task. For example, the correct response for the upper-left target was 7 regardless of whether the task was up–down (where 7 meant up) or right–left (where 7 meant left). The distinction here is between *compatible targets,* which produce the same correct overt response in the two tasks, and *incompatible targets,* which produce different responses depending on the task. This type of compatibility (see Sudevan & Taylor, 1987) may be termed *task compatibility.*

There is another type of compatibility, *spatial compatibility* (see Lu & Proctor, 1995; Simon 1990, Umilta & Nicoletti, 1990, for reviews), referring to the fact that some targets occupied the same relative position as the response key, whereas other targets did not. For example, participants pressed the upper-left key (7) in response to an upper-left target or the lower-right key (3) in response to a lower-right target. In these instances, the relative position of the response key was the same as that of the target stimulus. In contrast, the participants pressed either the lower-right key or the upper-left key (depending on the task) in response to an upper-right target stimulus. In these instances, the relative position of the response key was different than that of the target stimulus. It turns out that target stimuli, which were compatible with respect to the task, were also spatially compatible, whereas targets, which were incompatible with respect to the task, were spatially incompatible. Hence, the two forms of compatibility were perfectly confounded. Nevertheless, compatibility did not enter into significant third-order interactions involving task shift and cue–target interval, which suggests that the critical interaction, involving task shift and cue–target interval, was not a function of compatibility (see the General Discussion).

Task Shift Versus Stimulus Repetition

The task-shift cost, in the present paradigm, may be explained in terms of a stimulus-repetition effect (see Campbell & Proctor, 1993; Pashler & Baylis, 1991; Soetens, 1990; Soetens, Boer, & Heuting, 1985, for background on intertrial stimulus-repetition effects), that is, if the task repeated from the previous trial, so did the instructional cue. For example, if Trial $N - 1$ involved a right–left task, the instructional cue was the pair of arrows pointing to the sides (Figure 1). In the task-repeat condition, Trial N also involved the right–left task, which implies that the same set of arrows was repeated. In contrast, the set of arrows was changed from Trial N to Trial $N - 1$ in the task-shift condition. Biederman (1972) has shown that the same physical attribute of a stimulus resulted in a larger repetition effect when it served as an instructional cue as compared with when it served as a target stimulus. This result suggests that the task-shift effect is not a stimulus repetition effect. Nevertheless, an attempt was made to minimize the stimulus-repetition aspect in the task-shift manipulation. To achieve that goal, the instructional cues preceded the target stimulus by a minimal duration, to allow for sufficient time to encode them. Furthermore, the results suggest that in the present paradigm, the task-shift effect is qualitatively different from the stimulus-repetition effect (see Experiment 4).

Two Components of the Task-Shift Cost

The present paradigm yields separate (plausible) estimates of two components of the task-shift cost: one component that is related to advance reconfiguration, and a residual component that is unrelated to advance reconfiguration. This was achieved by using two cue–target intervals: short and long. The short interval was long enough to permit the encoding of the instructional cue but was probably too short for the completion of the advance reconfiguration process. Consequently, the task-shift cost in the short cue–target interval was predicted to reflect the added processing associated with advance reconfiguration. The long cue–target interval was always considerably longer than the average RT in the slowest condition. It is, therefore, reasonable to assume that the task-shift cost in the long cue–target interval does not reflect advance reconfiguration—it is a residue. The results of Experiment 4 show a dissociation between the two components, which tentatively supports the distinction between them.

Overview

Five experiments are reported. All tested the prediction regarding an interaction between cue–target interval and task shift. Although this prediction was confirmed in Experiment 1, the results of that experiment could be attributed to the dissipation of a carryover effect because cue–target interval was confounded with the remoteness from the previous trial. In Experiments 2 and 3, this possibility was ruled out because cue–target interval was manipulated while keeping constant the interval between the response on Trial $N - 1$ and the target stimulus on Trial N. Experiment 4 was considerably longer than the other experiments, which resulted in more data per participant. This made it possible to conduct fine-grained analyses that addressed the issues of task differences and practice effects and compared between task-shift effects and stimulus-repetition effects. The paradigm in Experiment 5 involved tasks of shape and color discrimination, and the results were analogous to those obtained with tasks that required location.

General Method and Analytic Procedures

Participants

The participants were tested individually and were undergraduate students from Erindale College, University of Toronto, who participated for partial course credit. In the experiments in which the grid paradigm was used (Figure 1), half of the participants were assigned to use the 3 key (indicating right or down, depending on the task) and the 7 key (up or left). The other half of the participants used the keys 1 (indicating down or left) and 9 (up or right). In every case, the participants used their two index fingers to respond.

Apparatus and Stimuli

All testing was performed in front of an IBM 286 clone that was controlled by software written in Micro Experimental Laboratory (MEL) 1.0 (Schneider, 1988). Two display formats were used with the grid paradigm (Figure 1), but in both formats the stimuli were drawn in white on black and included a 2 × 2 grid that was presented in the center of the screen. In Experiment 1, the grid subtended approximately 3.6° × 3.6°. The instructional cues were schematic arrowheads, taken from the font file supplied by MEL, which subtended approximately 0.9° × 0.9°. The arrows for the up–down task were positioned approximately 1.3° from the upper or lower ends of the grid, whereas the arrows for the right–left task were positioned approximately 0.6° from the left and right ends of the grid (see Figure 1). A circle with a diameter of approximately 1.5° was used as a target stimulus. In Experiments 2–4, the stimuli were drawn on the screen by using the graphic symbols that are part of the extended ASCII code. This presentation mode allows for flexible control of the display timing. The smiling-face character (ASCII Code 1), which subtended approximately 0.3° (width) × 0.5° (height), was used as a target stimulus instead of a circle. The arrowheads (ASCII Codes 16, 17, 30, and 31) subtended approximately 0.3° (width) × 0.3° (height) and were positioned 0.7° from the end of the grid. The grid subtended approximately 3.4° (width) × 2.9° (height).

Design and Analysis

The major dependent measure was RT, measured to within 1 ms from the flashing of the target stimulus on the screen until the keypress. Three independent variables were considered in all of the experiments, including task shift (task-shift–task-repeat), cue–target interval (short–long), and compatibility (compatible–incompatible). In Experiments 1–3 and 5, the first 20 trials were considered a warm-up and were not analyzed. In Experiment 4, there were separate blocks of warm-up trials and these data were not analyzed. RTs that were associated with errors or that were longer than 2,000 ms were also excluded from the analyses. Because of the random selection of tasks, cue–target intervals, and targets, the number of observations in each of the eight experimental conditions was not exactly the same. Prior to each analysis, the mean number of correct responses with RT shorter than 2,000 ms is reported.

A natural logarithm transformation was applied to RTs prior to all analyses. For each participant, the mean $\log_{\mathrm{RT}}$ for each experimental condition was computed and served as the basic datum in subsequent analyses of variance (ANOVAs). The figures report the antilogs of mean $\log_{\mathrm{RT}}$s (which are geometric means). The logarithmic transformation was used for two reasons. The first was to minimize the influence of outliers (see Ratcliff, 1993, for a review). Secondly, in this method, the task-shift cost is expressed as a difference between logarithms, which is equivalent to a ratio. If raw or trimmed RTs had been used, the task-shift cost would have been expressed as an absolute difference. One advantage of expressing task-shift costs as proportions rather than as absolute differences is that proportions are relatively insensitive to changes in baseline. Such changes were predicted because it was found that prolonging the (randomly varied) foreperiod led to a facilitation in performance even without task shifting (see Niemi & Näätänen, 1981, for a review). Choosing to analyze $\mathrm{Log}_{\mathrm{RT}}$s rather than raw or trimmed RTs was conservative because it made it more difficult to support the predictions. The cost of this decision is that it was impossible to apply the additive-factors logic (Sternberg, 1969). This should not be considered as a serious shortcoming, because the results are incompatible with assumptions regarding serial ordering of processes (see the General Discussion).

Experiment 1

I predicted that the task-shift cost would be larger when the cue–target interval was short than when it was long. Such an interaction would suggest a common cause for the two major findings in the task-shift literature: the task-shift cost and the foreperiod effects. As explained in the introduction, a significant interaction would suggest executive involvement according to the two criteria that were offered: specificity to task shifting and proactivity.

Method

Procedure. A session began with instructions that were presented on the monitor and followed immediately by 140 experimental trials. Each trial consisted of the cue (see Figure 1), which was presented for either a short (203 ms) or a long (1,423 ms) cue–target interval, after which the target was presented along with the cue for an additional 1,500 ms. Participants responded by pressing keys on the keypad on a standard extended keyboard that was shifted to the left to align it with the screen. The intertrial interval was 1,138 ms, and during that time, the task, target, and cue–target interval were selected randomly with equal probabilities.

Participants. 16 participants were involved in this experiment.

Results

The results (see Figure 2) of Experiment 1 supported the predictions by showing that the task-shift cost was reduced by prolonging the cue–target interval. In the rare instances (there were three) when the participants failed to respond within the given time (1,500 ms), their responses were regarded as errors. The mean number of correct within-deadline responses ranged between 12.2 and 15.4 per condition.

An ANOVA showed that all of the three main effects were significant: task shift, $F(1, 15) = 36.42, p < .0001, MSE = 0.0098$; cue–target interval, $F(1, 15) = 25.39, p < .0001, MSE = 0.0184$; compatibility, $F(1, 15) = 42.25, p < .0001, MSE = 0.0133$. The only significant interaction involved cue–target interval and task shift, $F(1, 15) = 8.62, p < .05, MSE = 0.0091$. RT was longer in the incompatible condition than in the compatible condition. Most important, the task-shift cost was larger when the cue–target interval was short than when it was long. Planned contrasts showed that the task-shift cost was significant at both cue–target intervals, $Fs(1, 15) = 38.82$, and $5.59, ps < .0001$ and $< .05, MSEs = 0.0099$ and 0.0089, for short and long cue–target intervals, respectively.

A similar ANOVA on errors revealed that all of the main effects were significant: task shift, $F(1, 15) = 14.13, p < .005, MSE = 0.0041$; cue–target interval, $F(1, 15) = 14.76, p < .005, MSE = 0.0051$; compatibility, $F(1, 15) = 13.73, p < .005, MSE = 0.0599$. In addition, the Compatibility × Cue–Target Interval interaction was also significant, $F(1, 15) = 11.36, p < .005, MSE = 0.0031$. Errors were more frequent in the task-shift condition than in the task-repeat condition. When the cue–target interval was short, compatible trials were associated with 4% errors, but incompatible trials were associated with 22% errors. When that interval was long, the effect was smaller: 2% versus 15%. Planned contrasts revealed that both of these effects were significant, $Fs(1, 15) = 15.81$ and $10.28, ps < .005$ and $.01, MSEs = 0.0381$ and 0.0251, for the short and the long cue–target intervals respectively.

Experiment 2

The results of Experiment 1 support the predictions because the task-shift cost was smaller when the cue–target interval was long compared with when the interval was short. Nevertheless,

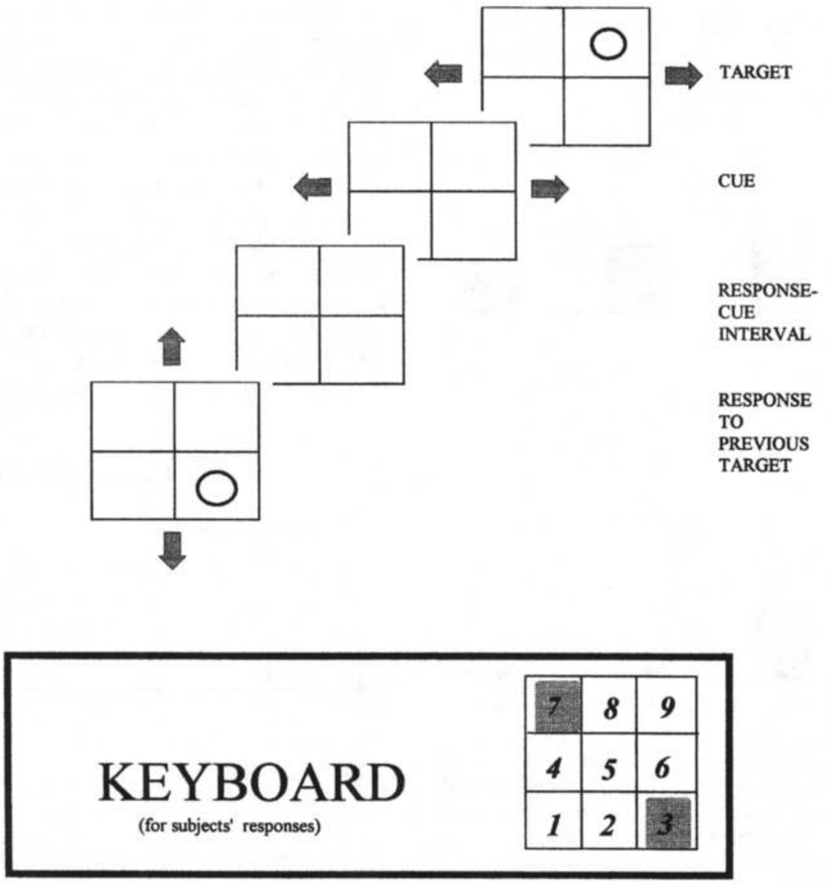

Figure 1. Schematic description of the paradigm used in Experiments 1–4. The figure presents a trial sequence proceeding from the bottom to the top. The bottom display corresponds to the last event in the previous trial, which required an up versus down discrimination. The response should have been down. The current trial began with an empty grid (second display), followed by a cue for a right versus left task (third display), followed by the target stimulus (fourth display). Hence, the response given to the fourth display is considered as belonging to the task-shift condition because the previous trial involved a different task than the current trial. Participants responded by pressing keys on the keypad part of the keyboard: 3 (for down or right) and 7 (for up or left) in this example.

the results can still be explained by Allport et al.'s (1994) model, which assumes that the task-shift cost reflects a carryover effect that dissipates during the intertrial interval. This is an acceptable explanation because, in Experiment 1, the cue–target interval was confounded with the remoteness from the previous trial. In Experiment 2, the confounding was removed by keeping constant the interval between the previous response and the current target. As seen in Figure 3, manipulating the cue–target interval was done by placing the instructional cue either far from the previous response and close to the current target stimulus (short cue–target interval) or close to the previous response and close to the current target stimulus (short cue–target interval), or close to the previous response but far from the target stimulus (long cue–target interval). A replication of the results in Experiment 1 would suggest that an increase in cue processing time (advance reconfiguration) is sufficient to cause a reduction in the task-shift cost. It would suggest that carryover effects are not the sole reason for the task-shift cost.

Method

Procedure. The experimental session began with detailed instructions that were followed by only 100 trials. A trial consisted of (a) a response–cue interval (132 ms or 1,632 ms), (b) the instructional cue for either a short (216 ms) or a long (1,716 ms) cue–target interval, and (c) the instructional cue and the target stimulus presented together until the response (see Figure 3). In every case, the response–target interval (which consisted of the response–cue interval and the cue–target interval) summed up to 1,848 ms.

Participants. Twenty-eight participants were involved in this experiment.

Results and Discussion

There were, on average, between 7.6 and 10.0 correct responses per condition with RT shorter than 2,000 ms. Summary statistics are presented in Figure 4.

RT. The three main effects came out as significant in the ANOVA: task shift, $F(1, 27) = 25.42$, $p < .0001$, $MSE =$

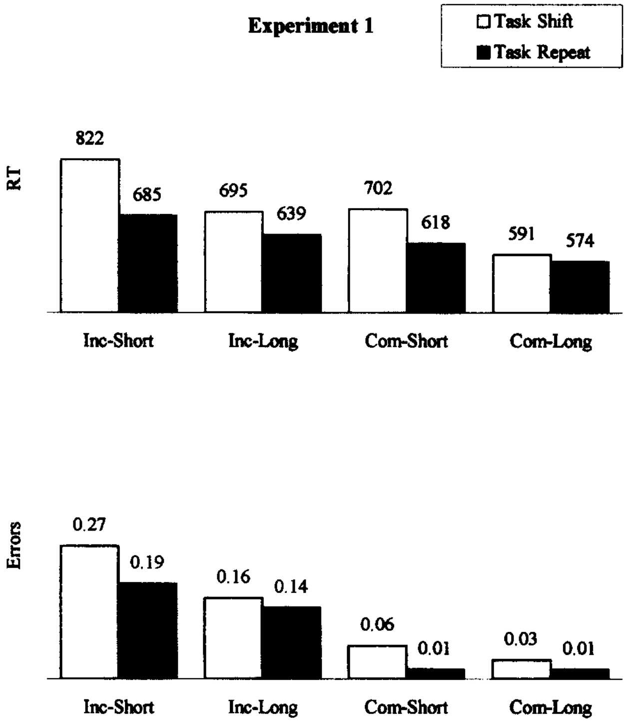

Figure 2. Geometric mean reaction time ([RT], in milliseconds) and proportion of errors according to compatibility, cue–target interval, and task shift in Experiment 1. Com = compatible; Inc = incompatible. Short and long refer to the length of the cue–target interval.

0.0276; compatibility, $F(1, 27) = 17.57, p < .0005, MSE = 0.0168$; and cue–target interval, $F(1, 27) = 30.87, p < .0001, MSE = 0.0284$. The predicted Task Shift × Cue–Target Interval interaction was marginal, $F(1, 27) = 3.12, p = .089, MSE = 0.0159$, and so was the third-order interaction, $F(1, 27) = 2.83, p = .104, MSE = 0.0130$. The marginal third-order interaction reflects that the interaction between cue–target interval and task shift was practically zero in the compatible condition ($F = 0.04$) but was significant in the incompatible condition, $F(1, 27) = 5.22, p < .05, MSE = 0.0165$.

Errors. An ANOVA revealed a main effect of compatibility (more errors in the incompatible condition than in the compatible condition), $F(1, 27) = 13.35, p < .005, MSE = 0.0399$, and a marginally significant main effect of task shift, $F(1, 27) = 3.79, p = .062, MSE = 0.0132$. No other source of variance approached significance.

Errors were more frequent in this experiment than in Experiment 1. A potential consequence of the increased error rate was an increase in the rate of undetected errors, associated with the failure to shift task. It should be remembered here that in the compatible condition, participants could respond correctly even if they considered the wrong task. Nominally, these responses are correct and are included in the RT analysis. As a consequence, the task-shift variable becomes contaminated, which resulted in a failure to obtain a significant Task Shift × Cue–Target Interval interaction. To support this interpretation, I ran an additional ANOVA on the RT data. In this analysis I did not include the data of participants who tended to respond according to the wrong task when required to shift tasks. This tendency affects accuracy only in the incompatible condition, where correct responding depends on the consideration of the task. However, participants can respond correctly in the compatible condition even when considering the wrong task. For this reason, the difference in error rate between the shift-incompatible and the shift-compatible conditions served as an index for the tendency to fail to comply with the instructions to shift task. A high score indicates that the participant responded according to the wrong task when required to shift. An ANOVA was performed after omitting the data of half of the participants who showed the highest tendency to fail in task shifting. Despite the fact that the number of participants was greatly reduced, the

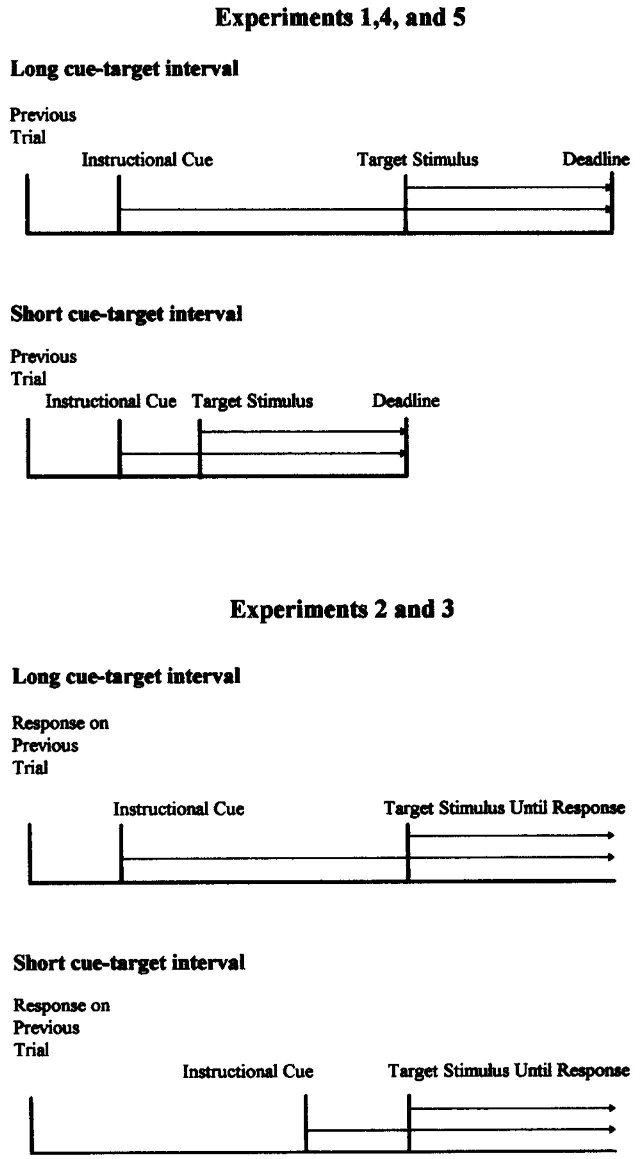

Figure 3. The timing of the displays in Experiments 1–5. Note that cue–target interval is confounded with the remoteness from the previous trial in Experiments 1, 4, and 5 but not in Experiments 2 and 3.

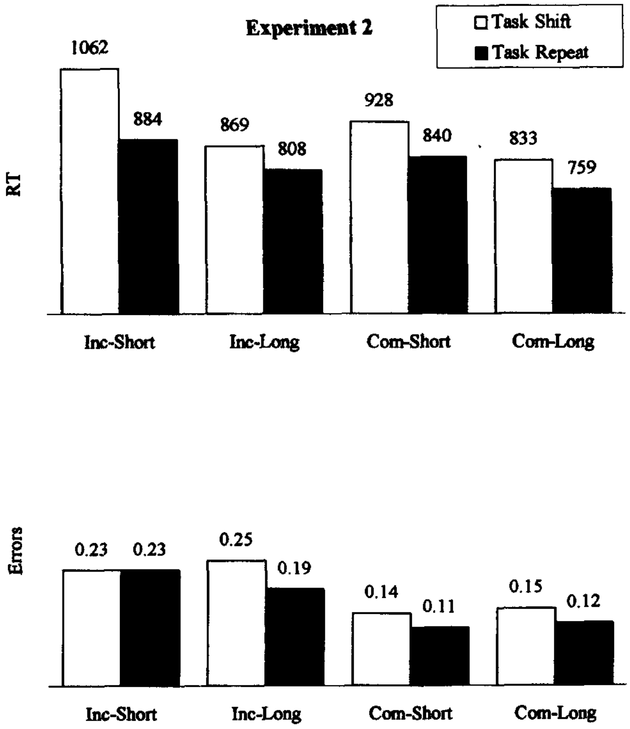

Figure 4. Geometric mean reaction time ([RT], in milliseconds) and proportion of errors according to compatibility, cue–target interval, and task shift in Experiment 2. Com = compatible; Inc = incompatible. Short and long refer to the length of the cue–target interval.

interaction between task shift and cue–target interval increased and reached statistical significance, $F(1, 13) = 5.46$, $p < .05$, $MSE = 0.0137$. Furthermore, the third-order interaction, which was marginally significant in the previous analysis, was far from significance ($F < 0.15$). The two additional significant sources of variability were cue–target interval and task shift, Fs(1, 13) = 50.88 and 9.91, ps < .0001 and .01, MSEs = 0.0146 and 0.0405. In conclusion, the last analysis strongly suggests that failing to comply with the task instructions resulted in a contamination of the results. When the contaminated data were omitted, the results were exactly as predicted. They indicated that when foreperiod is not confounded with the remoteness from the previous trial, it still interacts with task shift. Hence, the results suggest that carryover is not the only reason for the task-shift effect.

Experiment 3

In Experiment 2, the predicted interaction between cue–target interval and task shift failed to reach significance. This failure was attributed to the fact that some participants did not follow the instructions regarding a task shift, and a post hoc analysis supported this interpretation. However, the post hoc nature of the analysis made it necessary to run an additional experiment in which the participants were forced by experimental means to shift task on (almost) every trial. This was done by manipulating the proportion of compatible and incompatible targets (in all the experiments except for Experiment 3, there was an equal number of compatible and incompatible targets). When most of the targets are incompatible (and require that participants take the task into account to respond correctly), participants must follow the task-shift instructions to maintain a reasonable level of accuracy. I predicted that in this condition, task shift and cue–target interval would interact significantly. An additional difference between the two experiments was length. Experiment 3 included about three times as many trials compared with Experiment 2, to increase reliability. The two experiments were otherwise similar to one another.

Method

Participants. For 12 of the participants, 80% of the targets were incompatible and 20% were compatible (Group 80/20), whereas another group of 12 participants received the opposite combination of targets (Group 20/80). The assignment to keys was counterbalanced within groups as in the previous experiments.

Stimuli and procedure. These were exactly the same as in Experiment 3, except for two differences: (a) two targets (either compatible or incompatible, depending on the group) were presented more frequently than the other two targets and (b) there were 350 trials, with a short break after 175 trials.

Results and Discussion

The mean number of correct responses with RTs shorter than 2,000 ms ranged between 23.4 and 54.6 per cell (the large gap reflects the manipulation of proportion). Geometric mean RTs and error rates are presented in Figure 5.

RT. The ANOVA included proportion (80/20 vs. 20/80) as an additional (between-subjects) independent variable. Three main effects were significant, including compatibility, $F(1, 22) = 20.17$, $p < .0005$, $MSE = 0.0111$; cue–target interval, $F(1, 22) = 24.16$, $p < .0001$, $MSE = 0.0147$; and task shift, $F(1, 22) = 38.78$, $p < .0001$, $MSE = 0.0175$, but were accompanied by a significant Task Shift × Cue–Target Interval interaction, $F(1, 22) = 42.22$, $p < .0001$, $MSE = 0.0046$, and a significant fourth-order interaction, $F(1, 22) = 6.87$, $p < .05$, $MSE = 0.0017$. As before, the simple effect of task shift was still significant when the cue–target interval was long, $F(1, 22) = 6.47$, $p < .05$, $MSE = 0.0113$. The fourth-order interaction resulted from the fact that there was one condition in which the residual task-shift cost (long cue–target interval) was exceptionally large but still much smaller than the cost in the short cue–target interval. This was the incompatible condition in the 80/20 group, for whom most of the targets were incompatible, $F(1, 11) = 35.52$, $p < .0001$, $MSE = 0.0042$.

Errors. The ANOVA showed that the main effect of task shift was significant, $F(1, 22) = 32.49$, $p < .0001$, $MSE = 0.0007$, as well as the interaction between task shift and compatibility, $F(1, 22) = 9.89$, $p < .005$, $MSE = 0.0006$. The effect of task shift on accuracy was larger for incompatible targets than for compatible targets, probably reflecting a ceiling effect in the compatible condition. Furthermore, the fact that task shift and compatibility interacted significantly on errors is consistent with the interpretation that some of the accurate responses in the compatible condition resulted from the wrong application of the previous task when a shift was required. The 20/80 group showed a larger numerical differ-

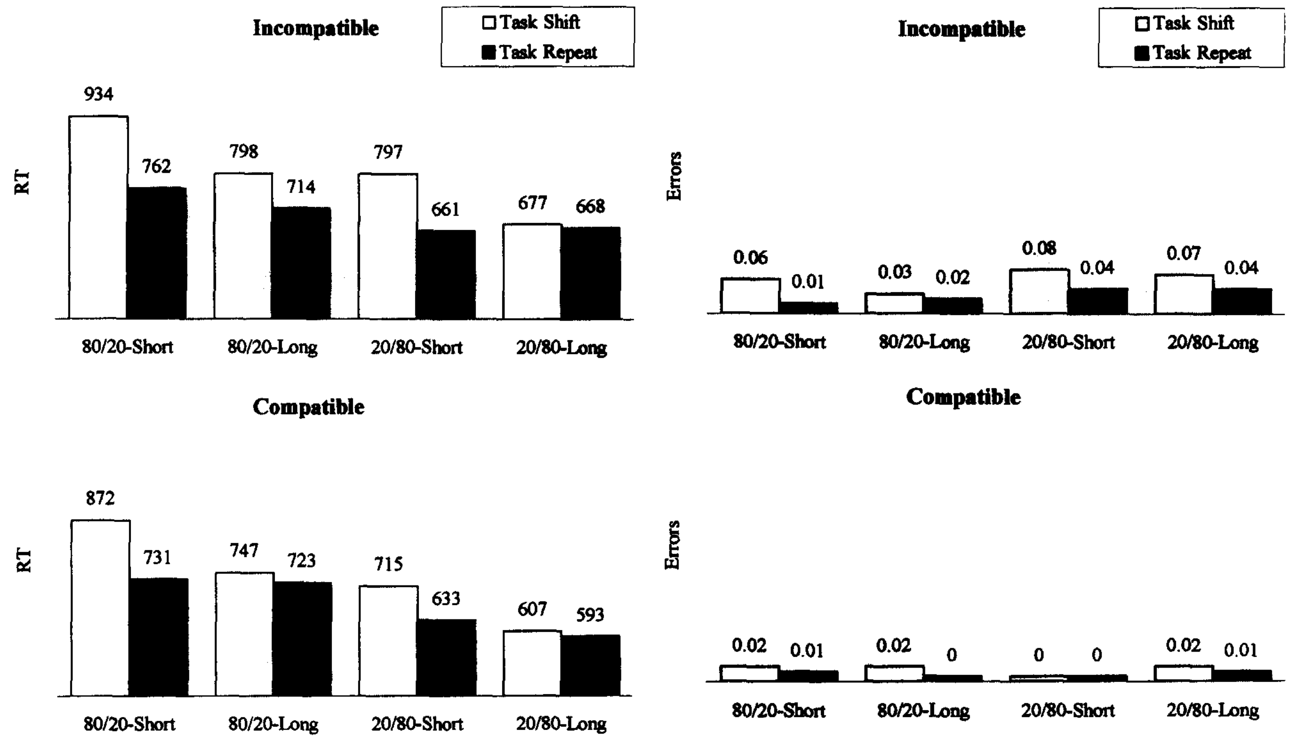

Figure 5. Geometric mean reaction time ([RT], in milliseconds) and proportion of errors according to proportion, compatibility, cue–target interval, and task shift in Experiment 3. Short and long refer to the length of the cue–target interval. 80/20 = results for the group for whom 80% of the targets were incompatible and 20% were compatible. 20/80 = results from the group for whom 80% of the targets were compatible and 20% were incompatible.

ence in error rate between the incompatible and compatible conditions than the 80/20 group. This trend was far from significant ($F < 1$), probably because accuracy approached ceiling. Nevertheless, it shows that participants who were presented with 80% compatible targets were less likely to shift tasks than were those for whom 80% of the targets were incompatible. This explains why the former group tended to react more quickly than the latter group.

In Experiments 2 and 3, cue–target interval was manipulated while keeping the response–target interval constant. This is precisely the advantage of a cueing technique over that used by Allport et al. (1994) or Rogers and Monsell (1995). Keeping the response–target interval constant equates the conditions in terms of the dissipation of carryover effects (including retroactive adjustment). Therefore, this is the first unequivocal demonstration for a reduction in the task-shift cost by an increase in foreperiod. Previous results were equivocal because foreperiod was confounded with the remoteness from the previous trial. The fact that the task-shift cost decreased with increasing foreperiod implies that the cost was not entirely due to a carryover effect from the previous trial. A minor issue that came up is the importance of a low error rate, especially in the incompatible condition. A high error rate suggests that participants do not follow the task-shift instructions, which leads to the confounding of the task-shift variable.

Experiment 4

All of the experiments, except for the present one, were very short. This reflected the belief that central processes show lessened involvement after practice (e.g., Logan, 1988; Navon & Gopher, 1979). The price paid for running short experiments was having only a small amount of data per participant. Experiment 4 was relatively long and yielded more data per participant. This made it possible to conduct fine-grained analyses. These analyses addressed the following questions: Do the same patterns of results emerge in the two tasks, in what way does (limited) practice affect the task-shift cost, and in what way is the task-shift cost different from a stimulus-repetition effect? The questions are explained below.

Comparison Between Tasks

According to Norman and Shallice (1986), advance reconfiguration is executed by schema selection, and schema selection is believed to take place at a level that is functionally distinct from task performance proper. Therefore, the model predicts that an increase in cue–target interval will lead to a reduction in task-shift costs irrespective of the task. This prediction was addressed in the present experiment by comparing the up–down and the right–left tasks.

Practice

According to Norman and Shallice (1986), schemata are activated by environmental cues. In the present paradigm, these were the instructional cues. Therefore, practice was predicted to strengthen the links between the instructional cues and the corresponding schemata and, consequently, lead to the automatization of the schema-selection process. Therefore, practice was predicted to affect the task-shift cost, especially in the short cue–target interval, where reconfiguration was required. In other words, the prediction was for an interaction between the block variable (reflecting practice) and task shift, which will be larger when the cue–target interval is short than when it is long. Such an interaction would suggest that the schema-selection process had become partly automatized. Given the theoretical considerations, the predicted result would imply that practice had led to decreased involvement of the supervisory attentional system and to an increased reliance on contention scheduling.

Target Versus Task Repetition

In the present design, when the task repeated so did the instructional cues. For example, when Trial $N-1$ and Trial N involved the right–left task, Trial N was considered as task repeat, but the trial involved the repetition of the arrows pointing to the sides. Hence, it could be argued that the task-shift cost reflects an intertrial stimulus-repetition effect (see Campbell & Proctor, 1993; Pashler & Baylis, 1991; Soetens, 1990, for background on stimulus-repetition effects). To rule out this possibility, I ran the following analyses. One analysis made a comparison between two forms of repetition: that of the instructional cues and that of the target stimuli. Biederman (1972) found that the repetition of a stimulus attribute led to a larger effect if the attribute served as an instructional cue as compared with when it served as a target stimulus. On the basis of Biederman's results, the prediction was that the effect of task-shift would be larger than that of target-stimulus repetition. This result suggests a qualitative difference between the two forms of repetition, namely, that the task-shift cost is not merely a stimulus-repetition effect.

In another analysis, the condition in which only the task repeated and the target did not repeat was subdivided into two conditions. The first condition included trials in which two aspects repeated: the interpretation (e.g., up, right) and the overt response (e.g., pressing the 7 key). The other condition included trials in which neither the interpretation nor the response repeated. For an example of the first condition, consider the case where, in Trial $N-1$, the participant pressed 7 to indicate up in response to an upper-right target. On Trial N, the participant pressed 7 again to indicate up, but this time in response to an upper-left target. For an example of the second condition, consider a case when in Trial $N-1$ the participant responded up (7) and on Trial N the participant responded down (3). This comparison addresses the issue of response repetition.

A few procedural changes were made on the basis of the results of the previous experiments, which suggested that a high level of accuracy is important. Basically, the attempt was to emphasize accuracy through the design of the warm-up trials. Because of this aspect, there were two blocks of warm-up trials instead of just one. One block was in the beginning of the experiment and the other was in the middle of the experiment. In the warm-up trials only, 80% of the targets were incompatible, meaning that participants were forced to take the task into account to respond correctly. This was accompanied by an emphasis in the instructions regarding a very high level of accuracy and by warning beeps from the computer in response to errors. Furthermore, in the warm-up trials only, the correct interpretation of the target as up, down, right, or left was presented on the screen after each response. There were four experimental blocks in which the proportion of compatible and incompatible targets was 50/50, and the correct interpretation was not displayed after the response.

Method

Stimuli and procedure. Participants went through two warm-up sessions (50 trials each) and four experimental blocks (150 trials each) in the following order: warm-up, Block 1, Block 2, warm-up, Block 3, and Block 4. The instructions were the same as those used before, except that a highlighted message was added that emphasized the need to reach an extremely high level of accuracy. Each trial consisted of (a) a constant response–cue interval of 1,416 ms, (b) the instructional cue for either a short (132 ms) or long (1,632 ms) cue–target interval, and (c) the instructional cue and the target stimulus presented simultaneously until the response. Errors were followed by a 400-Hz tone for 50 ms. During the warm-up blocks, 80% of the targets were incompatible and each response was followed by a message on the screen indicating the correct interpretation of target position as right, left, up, or down. This message stayed on the screen for 700 ms. In the experimental blocks, the targets, tasks, and cue–target intervals all had equal probabilities of getting selected. Given the lengthy procedure, participants were encouraged to get up from the chair and stretch for a few minutes between blocks of trials.

Participants. Ten participants were involved in this experiment.

Results

The two tasks produced similar results. Practice caused the task-shift cost to decrease but only when the cue–target interval was short, and all the indications were that the task-shift cost was not mediated by the same processes as the stimulus-repetition effect.

Main analysis and task comparison. The ANOVA included task as an additional (within-subjects) independent variable. The mean number of correct responses with RT less than 2,000 ms ranged from 33.5 to 41.7 per condition (see Figure 6 for summary statistics).

All the independent variables, except for Task ($F < 1$), had significant main effects, Fs(1, 9) = 35.13, 56.89, and 120.40; ps < .0001; MSEs = 0.0149, 0.0338, and 0.0034, for compatibility, cue–target interval, and task shift, respectively. There were also three two-way interactions between compatibility and cue target interval, $F(1, 9) = 7.06$, $p < .05$, $MSE = 0.0025$; compatibility and task shift, $F(1, 9) = 5.98$, $p < .05$, $MSE = 0.0001$, and critically, between cue–target interval and task shift, $F(1, 9) = 61.10$, $p < .0001$, $MSE = 0.0021$. Compatibility exerted a stronger influence after a task shift (80 ms) than after repeating the task (60 ms) and when the cue–target interval

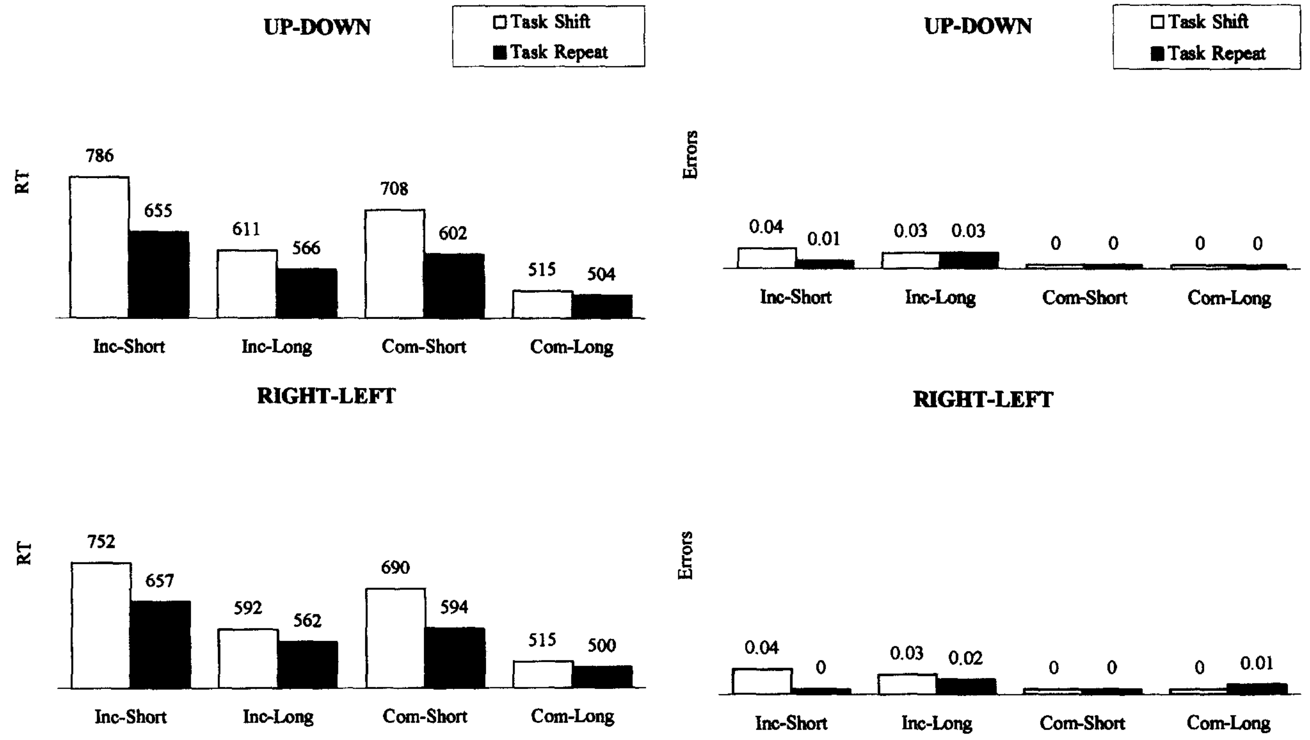

Figure 6. Geometric mean reaction time ([RT], in milliseconds) and proportion of errors according to task, compatibility, cue–target interval, and task shift in Experiment 4. Com = compatible; Inc = incompatible. Short and long refer to the length of the cue–target interval.

was long (580 vs. 508 ms, or 72 ms) as compared with when it was short (710 vs. 647 ms, or 63 ms). Most important, the effect of task shift was much larger when the cue–target interval was short (733 vs. 626 ms, or 107 ms) than when it was long (556 vs. 506 ms, or 50 ms) but was still significant even then, $F(1, 9) = 15.65, p < .005, MSE = 0.0022$.

Errors. The only significant sources of variance were a main effect of compatibility, $F(1, 9) = 14.88, p < .005, MSE = 0.0013$, and an interaction between cue–target interval and task shift, $F(1, 9) = 5.63$, $p < .05$, $MSE = 0.0004$. The interaction had a pattern similar to that found in the RT analysis.

In summary, the results confirm the prediction regarding a significant reduction in the task-shift cost as a result of increasing the cue–target interval. There were no indications for task differences, as predicted.

Practice. For brevity, only the results regarding block are mentioned. In this ANOVA, task was omitted as an independent variable and was replaced by block (1–4). The mean number per condition of RTs shorter than 2,000 ms and corresponding to correct response ranged between 17.1 and 21.1. Block had a significant main effect, $F(3, 27) = 16.82, p < .0001$, $MSE = 0.0351$; interacted significantly with cue–target interval, $F(3, 27) = 6.62, p < .005, MSE = 0.0044$, and with task shift, $F(3, 27) = 3.74, p < .05, MSE = 0.0027$; and entered into a significant third-order interaction (see Figure 7) involving both of these variables, $F(3, 27) = 5.41, p < .005, MSE = 0.0023$.

Practice reduced the task-shift cost when the cue–target interval was short, as indicated by a significant interaction between block and task shift in that condition, $F(3, 27) = 6.86$, $p < .005, MSE = 0.0040$. The decrease in task-shift cost over blocks was monotonic, as shown by a significant linear component of the interaction, $F(1, 9) = 18.22, p < .005, MSE = 0.0131$, and insignificant quadratic and cubic components ($F < 0.10$). When the cue–target interval was long, block and task shift did not interact significantly ($F < 1.00$). Analysis on

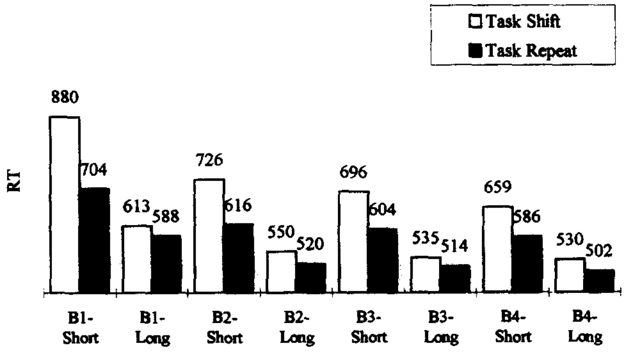

Figure 7. Geometric mean reaction time ([RT], in milliseconds) according to experimental block, cue–target interval, and task shift in Experiment 4. B1–B4 = Block 1–Block 4; Short and long refer to the length of the cue–target interval.

errors did not reveal any significant sources of variation involving block. Interestingly, the Block × Compatibility interaction was far from significant ($F < 0.15$), suggesting that practice had a similar effect in the compatible condition and in the incompatible condition. As argued in the *Discussion* of this experiment, this finding suggests that practice did not lead participants to base their responses on stimulus–response associations.

Target repetition versus task repetition. Eight conditions were created according to task shift, cue–target interval, and target repetition. The mean number of RTs per condition ranged between 34.4 and 109.2. A condition in which neither the task nor the target repeated served as the baseline. The task-shift effect was computed as the difference between the baseline and a condition in which the task repeated but not the target. The target-repetition effect was computed as the difference between the baseline and a condition in which the target repeated but not the task. Finally, the effect of repeating both the task and the target was computed by subtracting the baseline from the corresponding condition (see Figure 8). An ANOVA was performed on these effects. The two main effects were significant: effect type, $F(2, 18) = 68.07$, $p < .0001$, $MSE = 0.0023$, and cue–target interval, $F(1, 9) = 39.80$, $p < .0001$, $MSE = 0.0013$, but were accompanied by a significant interaction, $F(2, 18) = 27.08$, $p < .0001$, $MSE = 0.0014$. The effect of target repetition was not significant, but all other effects were significant ($p < .005$). Furthermore, the reduction in effect size by cue–target interval was similar regardless of whether only the task repeated or both the task and the target repeated, as indicated by an insignificant planned contrast ($F = 2.17$). Nevertheless, the magnitude of the effect was larger if both the task and the target repeated, as compared with when only the task repeated, $F(1, 9) = 33.35$, $p < .0005$, $MSE = 0.0104$.

As can be seen, the "pure" target-repetition effect was negligible. Hence, the present results replicate those reported by Biederman (1972) in the sense that a stimulus element, which serves as an instructional cue, produces a much stronger repetition effect compared with a stimulus that serves as a target. The fact that the task-shift effect was larger if the target repeated suggests that the estimates of the cost in the remaining experiments were slightly inflated. This is because the task-repeat condition, in the corresponding analyses, included trials in which both the task and the target repeated. Nevertheless, the bias is minimal because only one fourth of the task-repeat trials included a target repetition. Furthermore, despite the slight inflation in estimates, the critical interaction involving task shift and cue–target interval was not biased.

An additional ANOVA compared two conditions that involved task repetition but not target repetition. In one condition, both the interpretation (e.g., up) and the response (e.g., 7) repeated and in another condition neither the interpretation nor the response repeated. The repetition of the interpretation and the response was associated with a significant slowing of 34 ms (574 vs. 608 ms), $F(1, 9) = 9.16$, $p < .05$, $MSE = 0.0033$. In addition, the effect was similar for the two cue–target intervals, as shown by an insignificant interaction involving this variable. Hence, there was no evidence for a facilitation that was due to the repetition of the response or stimulus interpretation.

Discussion

Task differences. There was no evidence for differences between the two tasks. The fact that task did not enter into a third-order interaction involving task shift and cue–target interval shows that the time course of advance reconfiguration was similar for the up–down and right–left tasks, as predicted. This result suggests that advance reconfiguration takes place at a level that is functionally distinct from task execution. The fact that task did not have a significant main effect implies that up–down and right–left were of similar difficulty. When participants shift between tasks that differ in difficulty, some effects become asymmetric. For example, Allport et al. (1994, Experiment 6) found a task-shift cost after switching from a difficult task to an easy task but not vice versa (see also Sudevan & Taylor, 1987). Also of interest is the insignificant interaction between compatibility and task. This result suggests that the situation created under task-shift conditions is different than that obtained when participants are engaged in a single task. For example, Nicoletti and Umilta (e.g., 1984, 1985), who studied each task separately, found that when there were cues for both the horizontal and the vertical dimension, compatibility effects were larger for the right–left dimension regardless of whether the participants made right–left or up–down decisions.

Practice. In the short cue–target interval, the task-shift cost declined monotonically over blocks of trials. This suggests that practice resulted in strengthening the association between the instructional cues and the appropriate tasks, which led to

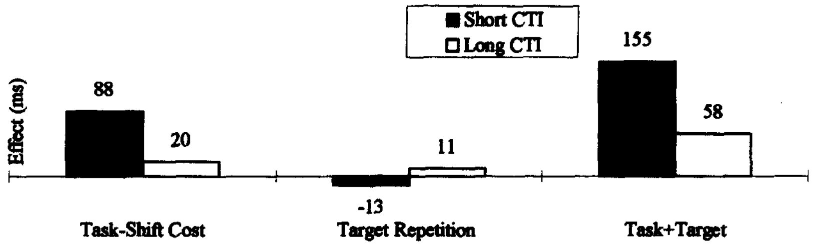

Figure 8. Task shift and target-repetition effects (in milliseconds) according to cue–target interval in Experiment 4. The baseline that was used to compute the effects was the condition in which neither the task nor the target repeated. CTI = cue–target interval.

faster reconfiguration. The fact that practice did not affect the task-shift cost in the long cue–target interval supports the distinction between two components of the task-shift cost. One component reflects advance reconfiguration and the other is a residue, possibly reflecting retroactive adjustment. Because retroactive adjustment is a one-trial learning phenomenon, it is not affected by long-term practice.

An alternative interpretation is that the (limited) practice resulted in faster encoding of the cues rather than in faster reconfiguration. This, I believe, is unlikely to be true, given the simple cues that were used, the fact that they were presented for at least 132 ms before the target, and the fact that practice still affected shift costs towards the end of the experiment. For example, shift costs in the short cue–target interval were reduced by 19 ms during the fourth block of 150 trials. A simple process such as the encoding of the arrows is expected to reach asymptotic performance after a few trials.

The effects of practice point to a possible difference between the present study and Rogers and Monsell's (1995). Their experiments were of an order of a magnitude longer than the present experiments. This difference, I believe, partly explains why Rogers and Monsell found that increasing the prewarning interval led to a relatively small reduction, about a third, in the task-shift cost. The present experiment suggests that practice reduces the reconfiguration-related component of the task-shift cost and leaves intact the residual component, which is unaffected by cueing. Prolonging the cue–target interval led to a reduction of task-shift costs from 176 ms (25%) to 25 ms (4%) in the first block, a reduction of 86%. However, in the fourth block the reduction was only from 73 ms (12%) to 28 ms (6%) or only 62%.

Does practice result in learning stimulus–response associations? Rogers and Monsell (1995) raised the possibility that, after practice, participants will "learn an association between each combination of attributes, task cue, and response" (p. 211). To avoid this possibility, their paradigm was designed so that "any combination [will be] . . . experienced with low enough frequency" (p. 211). The results of the present experiment suggest that, although there were only eight possible combinations of cues and targets, practice did not result in the learning of specific stimulus–response associations. Stimulus–response associations could potentially be formed if the targets were compatible, where the same response was correct regardless of the task. For example, participants could form an association between the upper left target and pressing the 7 key. However, the same was not true, or true to a lesser extent, with regard to incompatible targets. Any association between a given stimulus (e.g., the upper-left position) and a response (e.g., 7) would have been wiped out by an association between the same stimulus and the opposite response (e.g., 3). On the basis of this analysis, the prediction is that a stimulus–response-based behavior would have resulted in greater effects of practice in the compatible condition than in the incompatible condition. Nevertheless, compatibility did not interact significantly with block, suggesting similar effects of practice in the two conditions.

Another association that the participants could learn in the course of practice was between the cue–target combination and the response. This implies that participants could have learned to treat the situation as involving a single eight-choice RT task instead of involving two tasks (see the introduction for details). If this were true, we would not expect a task-shift cost, because a paradigm with a single task does not involve task shifts. Therefore, a gradual shift to an association-based strategy is predicted to result in the shrinkage of the task-shift cost. This prediction was not supported by the results because the task-shift cost at the long cue–target interval was not affected by practice.

Stimulus-repetition effects. How can one be sure that the task-shift cost is not caused by the repetition of the cue in the task-repeat condition? This explanation is unlikely to be true, on the basis of both theoretical grounds and empirical findings. On the theoretical side, studies on the intertrial repetition effect show that it is either reduced or eliminated in the absence of response repetition (Pashler & Baylis, 1991, Experiments 6 and 7) or the repetition of salient response features (Campbell & Proctor, 1993). In the present paradigm, the repetition of the instructional cue was not systematically associated with a repetition of the response and therefore, task repetition was not expected to produce a stimulus-repetition effect.

In addition, the results suggest that task shift and stimulus repetition are dissociable; that is, the task-shift cost was large, whereas the target repetition effect was either negative or negligible. Such a dissociation is dramatic because it was obtained under conditions that favor target repetition. This is because the instructional cues were presented before the target stimulus. Hence, the instructional cue that accompanied the target stimulus was primed both in the task-shift condition and in the task-repeat condition. Another form of repetition was that of the interpretation (e.g., up) and the response (e.g., 7). This repetition led to a significant response slowing rather than facilitation. Finally, the literature suggests that stimulus-repetition effects are larger if the stimulus and response are compatible than if they are incompatible (see Soetens, 1990, for a review). There was no evidence for a similar interaction between compatibility and task shift in the present results.

Experiment 5

The similar pattern of results obtained for the two tasks in Experiment 4 supports Norman and Shallice's (1986) model, in which reconfiguration is performed at a level that is distinct from task execution. Therefore, the model predicts that there will be a task-shift cost that is reduced by increasing foreperiod, regardless of the task. Nevertheless, the up–down and right–left tasks were very similar to one another, which limits the generalizability of the findings. In fact, one could argue that these were not separate tasks but two aspects of a single task. For this reason, it was important to test the predictions in a set of tasks that do not involve target location and are not as similar to one another as up–down and right–left. Neuropsychological data suggest that visual tasks are associated with two brain systems. Localization tasks are subserved by dorsal brain regions, especially the parietal lobes. In contrast, tasks that require object identification are mainly subserved by ventral regions, including the temporal lobes (e.g., Ungerleider & Mishkin, 1982). Presumably, the tasks in the previous experiments predominantly involved the dorsal system. In the present experiment, the two tasks presumably involved the ventral system to a greater extent than the dorsal system. The participants were presented with schematic drawings of moons or stars that were either green or red. The tasks involved discrimination according to either color or shape (see Figure 9). Each trial consisted of (a) an empty frame for fixation, (b) instructional cues for either a short or long cue–target interval, and (c) the target stimulus accompanied by the instructional cues that remained on the screen for a constant duration. Hence, the present paradigm was logically identical to that in Experiments 1 and 4.

Method

Display and procedure. Each target stimulus consisted of three colored objects that were positioned horizontally within a rectangular frame subtending approximately 2.9° (width) × 6.6° (height) painted in white on black (see Figure 9). Stars (taken from the font file supplied by MEL, ASCII Code 56, subtending 1.2° in width by 1.2° in height) and moons (Code 55, subtending 1.0° in width by 1.7° in height) were presented in either red or green on a black background. The instructional cues informed the participants about how to indicate their decisions by keypresses. The instructional cue for the color-discrimination task was two colored 1.7° × 1.7° squares positioned 0.5° to the right or 0.5° to the left side of the rectangular frame. The bottom part of the instructions was approximately 2.0° above the bottom part of the rectangle. The red square was on the right side of the frame, indicating to the participants to press the right key (/) when red objects are presented, and the green square appeared on the left side, indicating to the participants to press the left key (Z) when green objects appear. The instructional cue for the object discrimination task was similar, with a white star on the right side of the frame and a white moon on its left side.

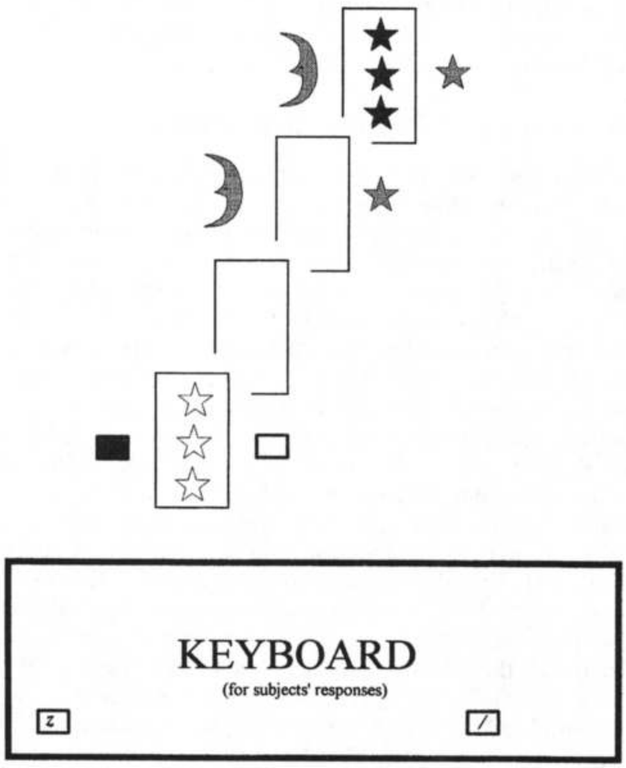

Figure 9. Schematic description of the paradigm used in Experiment 5. The figure displays a trial sequence proceeding from bottom to top. The first display is the last event of the previous trial, which required color discrimination. The two colored squares served as cues that instructed the participant to respond by pressing the left key (Z) when the target was green (in the figure green is represented by shading) and pressing the right key (/) when it was red (represented by no shading). The second display stayed for the intertrial interval and was replaced by the cue for the current trial, which signaled that the task was shape discrimination and that the participant should press the left key for stars and the right key for moons.

The participants were given written instructions that also included examples of the displays. After the instructions, they received separate warm-ups on the two tasks (10 trials per task, with order of tasks counterbalanced across subjects). The actual testing consisted of 120 trials in which the task, target, and cue–target interval were selected at random with equal probabilities during the intertrial interval. The sequence of events in a trial is depicted in Figure 9 and consisted of (a) an empty frame during the intertrial interval; (b) an instructional cue, presented for either a short (659 ms) or a long (1,908 ms) cue–target interval; and (c) the instructional cue and the target stimulus presented simultaneously for 2,000 ms, during which the participant responded. A slight variation in the intertrial interval (1,137 vs. 1,173 ms) was caused by differences in the times required to draw the two instructional cues in the background: 20 ms were required to draw the color cue and 56 ms were required to draw the shape cue.

Participants. Twenty participants were involved in this experiment.

Results and Discussion

The predictions were supported, as evidenced by a pattern of results in this experiment, where the tasks involved object identification, that was similar to the pattern found in the previous experiments, when the tasks involved object location (see Figure 10).

The compatibility variable was defined in relation to the current task so that green moons (for which the correct response was always to press the left key) and red stars (right key) were considered compatible, whereas red moons and green stars were considered incompatible. The mean number of RTs per condition, corresponding to correct within-deadline responses, ranged from 11.3 to 13.1. An ANOVA found two main effects to be significant: task shift, $F(1, 19) = 23.24, p < .0001, MSE = 0.0098$, and compatibility, $F(1, 19) = 10.01, p < .01, MSE = 0.0108$. Faster RTs were recorded in the compatible condition (637 ms) than in the incompatible condition (671 ms). Most important, the Task Shift × Cue–Target Interval interaction was also significant, $F(1, 19) = 8.63, p < .01, MSE = 0.0053$. Planned contrasts found that the task-shift cost was significant at both cue–target intervals, $Fs(1, 19) = 20.79$ and 9.51, $ps < .0005$ and $< .01$, $MSEs = 0.0115$ and 0.0036, for short and long cue–target intervals, respectively. A similar ANOVA on errors found that only the main effect of compatibility was significant, $F(1, 19) = 7.24, p < .05$, $MSE = 0.0438$, reflecting that there were more errors in the incompatible condition as compared with the compatible condition.

The fact that the proportional task-shift costs were smaller in the present experiment than in the previous experiments could be accounted for by the longer cue–target intervals that

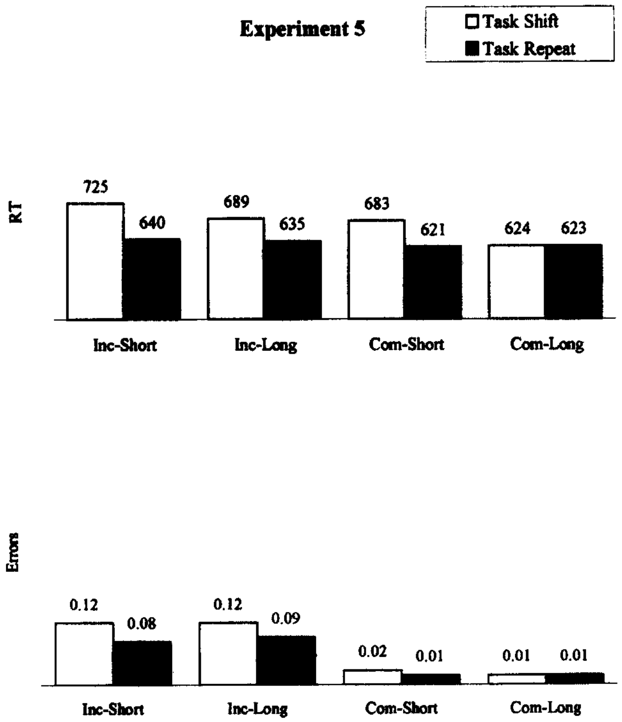

Figure 10. Geometric mean reaction time ([RT], in milliseconds) and proportion of errors according to compatibility, cue–target interval, and task shift in Experiment 5. Com = compatible; Inc = incompatible. Short and long refer to the length of the cue–target interval.

were used. To make this argument more convincing, I predicted, on the basis of interpolation of the results of Experiment 1, that in the short cue–target interval in Experiment 5, the shift cost would be 13%. This prediction contrasts with a very similar observed value of 11%.[1]

In summary, the results of the present experiment show that in spite of the change in tasks, there was a task-shift cost. It was significantly reduced by prolonging the cue–target interval, but was not eliminated even when the cue–target interval was substantially longer than the longest mean RT. The fact that the same pattern of results generalized across tasks supports Norman and Shallice's (1986) model, in which the task-selection process (indicated by the reduction in task-shift cost over the cue–target interval) is functionally distinct from task execution.

General Discussion

Two criteria can help in identifying executive–control functions in task-shifting paradigms: specificity to task shifting and proactivity. Previous research revealed two findings associated with task shifting. One finding, the task-shift cost, (e.g., Allpont et al., 1994; Biederman, 1972; Jersild, 1927) has been shown to be specific to task shifting, but demonstrations regarding proactivity were equivocal. The foreperiod effect, (e.g., Biederman, 1973; Davis, 1967; Sudevan & Taylor, 1987) which is the second finding, reflects proactive processing but this is the first time in which it was shown unequivocally that it is specifically related to task shifting. In the experiments reported here, I found that compared with short cue-target intervals, long intervals were associated with a smaller task-shift cost. In other words, the effect of cue-target interval on RT was larger after a task shift than after repeating the task from the previous trial. Hence, the present experiments constitute the first unequivocal demonstration that task shifting involves specific processes that operate prior to task execution. The existence of such a process is compatible with the notion of advance reconfiguration and maps to what control functions are believed to be. The main findings are summarized and discussed in turn.

A Task-Shift Cost

Participants took longer to respond when the task in the previous trial was different than the task in the current trial, as compared with when the two trials involved the same task. This result constitutes a replication of the many experiments reviewed in the introduction. Most important, in the present experiments, the task-shift variable was manipulated within a block of trials and, therefore, was shown to be specifically related to the task-shift situation, like in the experiments by Biederman (1972), Rogers and Monsell (1995), Shaffer (1965), and Stablum et al. (1994).

Reduction in Task-Shift Costs by Foreperiod

When the participants were allowed sufficient prewarning regarding the nature of the upcoming task, the shift cost was nearly eliminated. This is probably the most important finding of the present experiments. Similar results were obtained by previous investigators (Allport et al., 1994; Hartley et al., 1990; Rogers & Monsell, 1995; Shaffer, 1965). Nevertheless, in the first two of these studies, task shift was manipulated between runs or between blocks of trials. Therefore, it was potentially confounded with working-memory demands, division of attention between perceptual dimensions, and so forth. In addition, in all four studies, foreperiod was confounded with the remoteness from the previous trial. Hence, in these experiments, the interaction between foreperiod and task shift could be explained in terms of a dissipating carryover effect (Allport et al., 1994). Experiments 2 and 3 constitute the first demonstration in which the interaction between foreperiod and task shift could not be explained in terms of Allport et al.'s model. Therefore, the present experiments show that a large portion of the task-shift effect meets both criteria of control functions: It is specifically related to task shifting and reflects proactive processing. Presumably, the task-shift cost reflects, among other things, the added processing requirement associated with advance reconfiguration. In the absence of a neutral condition, it is difficult to determine whether the effect of task shifting reflects a cost or a benefit. Nonetheless, an interpretation in terms of cost is more plausible because it suggests that task shifting is associated with extra processing (and therefore

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[1] I could not estimate the shift cost for the long cue–target interval condition because when the cue–target interval is long it is probably not linearly related to shift cost (i.e., Rogers & Monsell, 1995).

with a cost) that can be performed in response to an instructional cue. In contrast, a benefit interpretation suggests that task repetition results in processing facilitation (or a benefit). It is difficult to see how the latter interpretation explains why foreperiod had a greater effect in the task-shift condition than in the task-repeat condition.

Rogers and Monsell's (1995) railroad metaphor (see the introduction for details) suggests that two conditions must be met for advance reconfiguration to take place: (a) participants must be able to predict target onset and (b) target onset must be remote, to allow sufficient time for advance reconfiguration to be completed. These assumptions explain why Rogers and Monsell found a significant interaction between task shift and foreperiod when foreperiod was constant for a block of trials, but not when it varied randomly. In the present experiments, foreperiod varied randomly within a block of trials. Nevertheless, there was a significant interaction between task shift and foreperiod, indicating that a process of advance reconfiguration took place. Furthermore, the interaction between foreperiod and task-shift was larger compared with that obtained by Rogers and Monsell. In the experiments conducted by Rogers and Monsell, a relatively long foreperiod led to a reduction of only a third of the task-shift cost. Here, a shorter foreperiod nearly eliminated the task-shift effect. Clearly, the two conditions specified by Rogers and Monsell are wrong. Unfortunately, the metaphor was used to rule out a carryover-based explanation (Allport et al., 1994; see the introduction for details). If the railroad metaphor is wrong, so is the argument that it supports. Hence, Allport et al.'s model may still apply to Rogers and Monsell's results. The present results clearly demonstrate that Allport et al.'s explanation cannot entirely account for the task-shift cost.

It remains unclear why the present paradigm and that used by Rogers and Monsell (1995) produced discrepant results. Rogers and Monsell observed a task-shift cost that was not affected by a randomly varying foreperiod, whereas here the task-shift cost was nearly eliminated. It is impossible to compare the paradigms directly because of the many procedural differences and, therefore, my explanation is speculative. The most noteworthy difference is that the present paradigm is based on explicit instructional cues, whereas in Rogers and Monsell's paradigm, the task shift was induced by means of a predictable ordering of tasks. A possible account (which I believe to be wrong) holds that a randomly changing foreperiod interferes with advance reconfiguration. According to that account, the reconfiguration process is robust when triggered by an environmental cue (the present experiments) but fragile when triggered by prediction (Rogers & Monsell's, 1995, experiments). This distinction maps exactly to that between schema selection by contention scheduling versus the supervisory attentional system. Nevertheless, I suggest that the true distinction is between two forms of environmental cueing that differ in potency. Rogers and Monsell cued the tasks by the relative position of the target stimulus within a grid. The stimuli were letter–digit pairs (e.g., *G7*) whose position within a grid was rotated clockwise across trials. When, for example, the stimuli were in the upper quadrants, the task involved digits. When they were in the lower quadrants, the task involved letters. This method of cueing is similar to that used by Jersild (1927). In the experiment, each column of word stimuli was associated with a different task, either opposites (e.g., *BLACK–white*) or verb–noun (e.g., *ANSWER–question*). Presenting the words in adjacent lists led to near elimination of the task-shift cost because, according to Jersild, it supplied the participants with an "aid of spatial associations" (p. 23). Jersild's results suggest that spatial position is an efficient environmental cue. Nevertheless, the position of the target is probably a less-potent cue than the cues that were used in the present experiments. A reconfiguration process that is triggered by a potent cue is unlikely to be disrupted by a randomly varied foreperiod, whereas a process triggered by weak cues, like task order and spatial position (Rogers & Monsell, 1995), is easily disrupted.

The Effects of Foreperiod and Task Shift Involve More Than Advance Reconfiguration

The results indicate a certain degree of independence between foreperiod and task shift. First, the task-shift cost was rarely eliminated even when the foreperiod was very long. The residue of the task-shift cost probably reflects retroactive adjustment. Rogers and Monsell (1995, Experiment 6) tried to determine what processes are reflected in the residual component of the task-shift cost. They discarded a variant of the retroactive adjustment model, *micro-learning,* on the basis of a finding that RT in the task-repeat condition was not affected by the number of task repetitions. In an experiment not reported here, I found that RT was affected by the number of task repetitions. This result leads me to interpret the residual task shift (at least in the present paradigm) as reflecting retroactive adjustment.

The present findings suggest that foreperiod effects were mediated by advance reconfiguration and by an additional process. This was shown by foreperiod effects in the task-repeat condition, where reconfiguration was not required. Possibly, the underlying process is the prediction of target onset (see Niemi & Näätänen, 1981, for a review). Support for this interpretation comes from a set of experiments in which we manipulated the relative proportion of cue–target intervals. On the basis of the literature on "aging" foreperiods, it was hypothesized that when most of the cue–target intervals are long and a few are short, participants can predict target onset. Under these conditions, we found a large effect of foreperiod. When most of the cue–target intervals were short and a few were long, participants could not predict target onset, and the effect of foreperiod was *smaller.*

A minor point I would like to discuss here refers to a potential artifact in the present study. Frequently, when there was a reduction in task-shift cost, it was accompanied by a reduction in RT in the task-repeat condition. This may suggest that the reduction in the cost resulted from a floor effect. Nevertheless, in a few conditions, the task shift cost was reduced in spite of the fact that foreperiod did not affect RT in the task-repeat condition (e.g., Experiment 5 and one condition in Experiment 3). Second, in the experiments described in the previous paragraph, I found a dissociation between two advance preparation processes: reconfiguration (the reduction in task-shift cost by foreperiod) and the prediction of target onset (the reduction in RT by foreperiod). Manipulating the proportion of foreperiods (mostly short foreperiods vs. mostly long foreperiods) affected the prediction of target onset, as commonly found (e.g., Niemi & Näätänen, 1981), but did not affect advance reconfiguration. Finally, the present analyses were conservative in the sense that task-shift costs were expressed as proportions (differences in $\log_{RT}$s) rather than as absolute differences in RT.

Does Reconfiguration Finish Before Task Execution Begins?

The exact temporal relationship of advance reconfiguration and task execution was not addressed in the present study. A possible model is serial (in Sternberg's, 1969, sense) and suggests that reconfiguration must complete before task execution begins (Biederman, 1972, but probably Rogers & Monsell, 1995, as well; see the introduction). Although the present experiments were not designed to distinguish between the alternative explanations, the results suggest that the serial model is wrong. The reduction in task-shift costs or in RT was much smaller than the associated increase in cue–target interval. For example, the short cue–target interval was between 203 and 216 ms in Experiments 1–3, which contrasts with 132 ms in Experiment 4. This 71–84 ms difference in cue–target interval was associated with a reduction in the task-shift cost of only a half: 43–65 ms. These results contrast with the prediction of the serial model, which is that increasing the cue–target interval will lead to an equivalent reduction in task-shift costs or RT. Other authors reported similar findings regarding foreperiod effects on RT (as opposed to a task-shift cost, see Bernstein & Segal, 1968; Shaffer, 1965, 1966).

Compatibility Effects

Task compatibility and spatial compatibility were perfectly confounded in the present paradigm, mainly because neither one of them was the focus of the study. For this reason, it is difficult to interpret results that are associated with this variable. It is sufficient at present to note that in three out of five experiments, compatibility did not enter into a significant interaction involving both task shift and cue–target interval. The two exceptions are Experiment 2, where the marginal three-way interaction was completely eliminated when the data for participants who failed to comply with the task-shift instructions were omitted from the analysis, and Experiment 3. However, the fourth-order interaction in the letter experiment reflected differences in the residual task-shift cost that were observed when the cue–target interval was long. Compatibility did not affect the task-shift cost in the short cue–target interval, where it presumably reflected advance reconfiguration.

Implications for Norman and Shallice's (1986) Model

The results are, in general, favorable to Norman and Shallice's (1986) model. First, it follows that the model predicts that participants will not be able to execute a task unless the proper schema has first been activated. The present demonstration of a proactive process that is associated with task shifting is consistent with this prediction. Second, the similarity between tasks (Experiments 4 and 5) is consistent with the distinction in the model between control processes (that are insensitive to task differences) and task execution. Finally, Norman and Shallice's model is not serial and, therefore, is better equipped than are serial models (e.g., Biederman, 1972) to explain the results.

Some aspects of the results were not predicted by Norman and Shallice's (1986) model but are still consistent with it. As argued above, the residual task-shift cost suggests that retroactive adjustment was involved. Norman and Shallice's (1986) model is compatible with this assumption. It is conceivable, for example, that the component processes that were recruited by a schema were tuned by retroactive adjustment. Some processes were shared between the tasks (this is especially true regarding the right–left and up–down tasks). Consequently, retroactive adjustment resulted in a process that was better tuned for the task on Trial $N - 1$ than for the task on Trial N and led to a decrement in performance. Note that a cost is expected only as long as the process that is adjusted retroactively is used in both tasks.

Although Norman and Shallice's (1986) model served as a theoretical framework, it is by no means the only relevant model. The notion of executive functions that choose, construct, maintain, and disable cognitive strategies was elaborated by Logan (1985). Cohen, Dunbar, and McClelland (1990) suggested that participants who respond to Stroop stimuli activate a *task demand node,* analogous in some aspects to Norman and Shallice's schema. According to Cohen et al., the interpretation of task instructions leads to the activation of a task-demand node that, in turn, leads to an increased sensitivity to the appropriate stimulus dimension. Shaffer (1965) and Duncan (1977) suggested that choice RT tasks are specified as stimulus–response mappings rather than as individual stimulus–response pairs. Stimulus–response mappings are analogous to schemata in that a single mapping carries with it the information regarding a task as a whole. The stimulus–response mapping perspective is limited, however, because it is only applicable to experiments in which the stimuli vary along a single relevant dimension as in Shaffer's and Duncan's studies, where the single dimension involved location. However, in most of the studies that looked into task shifting, the relevant mapping concerned stimulus interpretation rather than the physical stimulus. The present study is an example because, when participants shifted between tasks, they needed to reinterpret the same physical stimulus according to the requirements of the new task. For example, the upper-left stimulus was supposed to be interpreted as up in one task but as left in another task.

Conclusions

In this study, it was shown that shifting between tasks was associated with a task-shift cost. Most important, when participants were given sufficient time to prepare for the task shift, the task-shift cost was nearly eliminated. These results indicate that task shifting is associated with a process that operates prior to task execution. The existence of this process is compatible with the notion of advance reconfiguration and therefore with the idea of executive control processing.

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