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Switching Between Tasks of Unequal Familiarity: The Role of Stimulus-Attribute and Response-Set Selection

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It has been reported that it is harder to switch to a strong, well-practiced task from a weaker, less-practiced task than vice versa. Three experiments replicated this surprising asymmetry and investigated how it is affected by a reduction in interference between tasks. Experiment 1 progressively delayed the onset of the stimulus attribute associated with the stronger task. Experiments 2 and 3 separated the response sets of the tasks. Both manipulations reduced, without eliminating, interference of the stronger with the weaker task but reversed the asymmetry of switch costs, resulting in a larger cost of switching to the weaker task. The results are interpreted in terms of a model of the interactions between control input, task strength, and task priming.

The central question addressed by task switching research is how a task set, an effective organization of the cognitive system, is imposed to ensure that the appropriate task is performed. In a typical task switching experiment, the participant is presented with a series of similar stimuli, some or all of which permit a response in two or more tasks, and is required to switch quickly and frequently between the tasks. Performance is usually slower and less accurate on *switch* trials, on which the task is changed from the previous trial, than on *nonswitch* trials, on which the task is repeated from the previous trial. The performance difference between switch and nonswitch trials may reflect additional cognitive load on switch trials (i.e., a task switching cost) and/or facilitated performance when the task is repeated (i.e., a task repetition benefit) but is typically referred to as the *switch cost.* The switch cost has been used to probe the mechanisms of cognitive control that permit the observed flexibility of responding.

Two principal sources of the switch cost have been proposed. Many theorists have attributed switch costs to the time taken by control processes to establish a changed task set, with task reconfiguration viewed as an extra processing stage (or stages) inserted prior to completion of task-specific processing (e.g., De Jong, 2000; Kieras, Meyer, Ballas, & Lauber, 2000; Rogers & Monsell, 1995; Rubinstein, Meyer, & Evans, 2001). Others have explained switch costs in terms of prolonged processing on switch trials because of interference from the prior task set (Allport, Styles, & Hsieh, 1994; Allport & Wylie, 1999). On this account, switch costs reflect positive and negative *task priming:* On a switch trial, there may be persisting suppression (negative priming) of the task now required and/or additional activation (positive priming) of the previous task, resulting in performance decrements.

Many parties to this debate now acknowledge that the switch cost reflects both task priming effects and the time taken by control processes (e.g., Allport & Wylie, 2000; Goschke, 2000; Kieras et al., 2000; Meiran, 2000; Monsell, Yeung, & Azuma, 2000; Ruthruff, Remington, & Johnston, 2001; Sohn & Anderson, 2001), and researchers have set about determining the relative contributions of these factors and the relationship between them. An issue that has proved important to this project is the effect on task switching performance of the relative strength of the two tasks switched between. In particular, a good deal of research has been stimulated by the counterintuitive findings of Allport et al. (1994) that it can be harder to switch to the stronger, better practiced of two tasks than to the weaker task. As described below, the phenomenon of a larger cost of switching to the stronger task of a pair has been taken as a signature of the contribution of task priming to task switching costs. In our lab, however, we have observed several exceptions to this rule, which might be taken to suggest that task priming may contribute to task switching performance only in certain circumstances (Monsell et al., 2000). However, to date, there have been few attempts to study systematically the conditions under which Allport et al.'s asymmetry of switch costs should be observed. The present research addresses this issue. We begin, therefore, by introducing the concepts of task strength and interference, then move to a more detailed look at Allport et al.'s findings.

Task Strength and Interference

Tasks may differ in strength because they differ in the degree of practice (MacLeod & Dunbar, 1988), in the natural compatibility of their respective stimulus-response mappings (Kornblum, Hasbroucq, & Osman, 1990), or in both. Such differences in task strength are most commonly revealed through asymmetries in between-task interference, the best known example of this being

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the Stroop effect (MacLeod, 1991; Stroop, 1935). Presented with a color word written in colored ink, the participant is required either to read the word or to name the color of the ink in which the word is written. The Stroop effect is the decrement in color naming performance that is observed when the word and ink color are incongruent (e.g., the word *green* in red ink), as compared with the case in which a neutral stimulus is presented (e.g., the letter string *XXXXX* in red ink). It seems that there is a strong tendency to make a word naming response even when color naming is required, so that production of the required response is slowed when word and color are incongruent. In contrast, word naming performance is little affected by the presence of conflicting ink color information—there is almost no reverse Stroop interference—indicating a much weaker tendency for color naming responses to be generated. By the operational criterion of this asymmetry in interference, word naming is the stronger task.

Though task strength and between-task interference are closely related concepts, they are at least partially dissociable. In particular, it is possible to take the same pair of tasks (i.e., keeping task strength constant) and manipulate the degree of interference between them by manipulating the ease of selecting the relevant stimulus attribute or response set. Regarding stimulus-attribute selection, Glaser and Glaser (1982) presented words superimposed on color patches, allowing the onsets of word and color information to be separated. They found that interference was reduced as the onset asynchrony between color and word attributes increased. Thus, interference was reduced when it was made easier to select the relevant stimulus attribute. Regarding response-set selection, Klein (1964) found that interference with color naming was large when the distracting word attribute was a potential color naming response (e.g., *green* in red ink) but was greatly reduced when the word was drawn from a set of frequency-matched, noncolor words (e.g., *friend* in red ink; see also Proctor, 1978). Redding and Gerjets (1977) found that Stroop interference is also reduced when the color task requires a manual rather than a spoken response (see also McClain, 1983). Thus, between-task interference is reduced when the distracting attribute is not associated, or is only weakly associated, with the response set of the currently required task.

Task Strength and Task Switching

Allport et al. (1994, Experiment 5) had participants switch between word naming and color naming of Stroop stimuli. Their surprising finding was of a much greater cost of switching to the strong task of word naming, with little or no cost associated with a switch to the weaker color naming task. That is, word naming was much slower and less accurate on switch trial (when the previous trial involved responding to stimulus color) than on nonswitch trials (when the previous trial also required the word naming task). In contrast, color naming performance was little slower following a word naming trial than it was following a color naming trial. Allport and Wylie's (1999, 2000) further experiments indicate that this counterintuitive asymmetry of switch costs is a robust finding, though the cost of switching to color naming is usually greater than that originally observed by Allport et al. (1994).

As originally noted by Allport et al. (1994), it is difficult to explain these findings in terms of the occurrence of an extra time-consuming control process on the switch trial. One would expect it to take less time, or at least take no more time, for control processes to organize the cognitive system into a familiar, well-established configuration than to reconfigure for a less practiced task. Allport et al. therefore suggested that the asymmetry in switch costs results from an asymmetry in control biases required to perform the two tasks and from carryover of the biases from trial to trial. A strong bias is required to perform color naming in the face of strong competition from word naming, involving extra activation of the color naming task set and/or inhibition of word naming. Hence, when one is switching to word naming, there is negative priming of this task and positive priming of the color naming task, resulting in a large switch cost. In contrast, little control bias is required to ensure performance of the stronger task of word naming, and, hence, it should be relatively easy to switch away to color naming. Thus, Allport et al. (1994) accounted for their findings by assuming that task priming effects are large when one is switching from a weak task (e.g., color naming) to a strong task (e.g., word naming) and small when one is switching in the opposite direction.

Allport et al.'s (1994) paradoxical asymmetry of switch costs has since been replicated in Stroop switching (Allport & Wylie, 1999, 2000) and in other instances of switching between pairs of tasks that differ in relative strength (De Jong, 1995; Meuter & Allport, 1999; Yeung & Monsell, 2002). However, a number of experiments in our laboratory have yielded results that are inconsistent with those of Allport and colleagues (see Monsell et al., 2000, for a summary). Of particular interest here are the unpublished results of Monsell, Williams, Wright, and Rogers (1995). If the paradoxical asymmetry in switch costs results from the inequality in control biases required to select tasks that differ in strength, then reducing interference between them ought to reduce the asymmetry. Monsell et al. found that such a manipulation might do more than this. They had participants switch between color naming and word naming and used visual noise and variations in type font and size to slow word naming latency by about 100 ms, enough to roughly equate response times (RTs) in the two tasks. With this manipulation, interference in single-task blocks remained strongly asymmetrical (with large Stroop interference but little or no reverse Stroop interference). However, in switching blocks, switch costs for the two tasks were almost identical. The present experiments follow up this unexpected dissociation between the asymmetry of interference between the tasks and the asymmetry of switch costs.

Research Aims

The primary aim of the present research is to identify boundary conditions under which Allport et al.'s (1994) asymmetry of switch costs is or is not observed and, hence, provide a greater understanding of the interaction between task strength and priming in task switching. In particular, we examine the impact of reducing between-task interference on the cost of switching between tasks of unequal strength, following up the observations of Monsell et al. (1995).

In Experiment 1, we investigated the effect on switch costs of making selection of stimulus attributes easier than in the standard Stroop task. Participants switched between color naming and word naming, but the stimuli consisted of a word written in black superimposed on a color rectangle, allowing word and color information to be presented separately. Glaser and Glaser (1982), who introduced this manipulation, showed that delaying word onset reduced the Stroop interference suffered by color naming. The question of interest here was the consequence of reduced Stroop interference for the cost of switching between the tasks.

In Experiments 2 and 3, we manipulated between-task interference by varying the amount of response overlap between the two tasks. In both experiments, participants switched between digit naming (a strong overlearned task) and a weaker secondary task whose response set varied across participants. For some participants, the secondary task used the same set of responses as did the digit naming task, and for others, the response sets were different. It has been shown that participants are able to take advantage of nonoverlapping response sets to help suppress or filter out the response associated with the irrelevant task, thus reducing interference (Klein, 1964; Mayr, 2001; Redding & Gerjets, 1977). We were interested in the impact of this reduction on switch costs.

To foreshadow the results, in each experiment we found that reducing interference—whether by delaying the onset of one attribute or by separating the response sets of the two tasks—resulted not in a reduction but in a reversal of the asymmetry of switch costs. That is, we found greater costs of switching to the weaker task of the pair when interference was reduced. This was the case even though interference remained larger for the weaker task in each experiment. A second aim of our research is to provide an account of this surprising pattern of results. To this end, the General Discussion introduces a simple mathematical model of task priming effects that is capable of explaining our findings.

An interesting by-product of this modeling effort is that it answers a concern we had (Yeung, 1999) regarding Allport et al.'s (1994) explanation of their original findings. Allport et al. proposed that the extra task-set bias required to perform color naming persists over time and results in a cost when one is switching to word naming. However, this explanation appears to overlook the corollary that the same bias should produce a correspondingly greater task repetition benefit on color naming nonswitch trials. Suppose that to perform color naming entails inhibiting the word naming task, but not vice versa (Allport et al., 1994). Carry over of this inhibition of word naming following color naming should be helpful if the following trial also requires color naming, just as the inhibition is held to be harmful if the following trial requires word naming. The extra inhibition following a color naming trial should therefore reduce the RT on a following color naming nonswitch trial (extra repetition benefit), just as it increases it on a following word naming switch trial (extra switch cost). The RT difference between task repetition and switch trials (the "switch cost" or "repetition benefit") should therefore be increased not only for the stronger task (by a longer RT on the switch trial) but also for the weaker task (by a shorter RT on the nonswitch trial). Thus, there is no obvious reason why negative task priming following the performance of the weaker task should produce an asymmetry of switch costs at all. For a task priming hypothesis truly to account for Allport et al.'s findings, it must explain why the benefit of task priming when the task is repeated is much smaller than the corresponding cost when the task is switched. The model presented later offers one possible explanation of why this is the case.

Experiment 1

This experiment investigated the effect on switch costs of making selection of stimulus attributes easier than in the standard Stroop task. Participants were required to switch between naming a word superimposed on a rectangular patch and naming the color filling the rectangle. The onset delay of the word, relative to the color, was varied. For one group of participants, the onsets of color and word were simultaneous throughout the experiment, as in the experiment of Allport et al. (1994). For two other groups, the 160-ms delay and 320-ms delay groups, the word onset followed 160 ms or 320 ms after the onset of the colored rectangle. Pilot work indicated that in single-task blocks, a 160-ms delay produced roughly equivalent baseline RTs for the two tasks (measured from the color information onset) while preserving the asymmetry of interference, albeit attenuated, as had the conditions of Monsell et al. (1995) using degraded word stimuli. The 320-ms interval was chosen to extend these effects. The question of interest was what would happen to the asymmetry of switch costs.

Method

There were 24 paid participants: 15 men and 9 women, ages 19–44 years. Eight were randomly assigned to each of the three groups. The stimuli included color words presented in black on a colored background rectangle. The words and colors used were pink, green, blue, and brown. The stimuli were either incongruent (e.g., the word *pink* displayed on a blue background) or neutral. For the word naming task, the neutral background was a rectangle matching the screen color (white) and outlined in black. For color naming, the neutral attribute was a false font character string derived from one of the four color words, presented on a colored background. We constructed the false font stimuli by exchanging letter fragments in each color word to a generate a string of letterlike characters that were not identifiable as the original word but that contained the same number of screen pixels, lines, curves, and so on. Examples of the stimuli used appear in Figure 1.

For each task, there were single-task blocks in which only that task was performed. These blocks provided an estimate of interference from the irrelevant attribute when there was no requirement to switch tasks. There

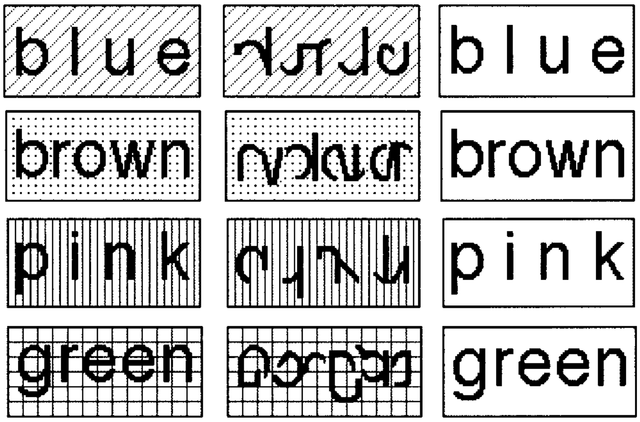

Figure 1. Examples of the stimuli used in Experiment 1, showing incongruent stimuli (leftmost column), neutral stimuli for the color-naming task (middle column), and neutral stimuli for the word-naming task (rightmost column). The word and background colors—here, simulated by patterns—were always incongruent.

were also task switching blocks in which the task changed every second trial (Rogers & Monsell, 1995), so that the participant named word, word, color, color, word, word, and so on, with task cued by location on the screen to help the participant keep track. The first trial following a task switch is referred to as the *switch trial*, and the following trial is referred to as a *nonswitch trial.* The switch cost for each task was defined as the difference in mean RT and error rate between switch and nonswitch trials.

Participants were first trained with a single block of word naming (52 trials), a single block of color naming (52 trials), and a single task switching block (68 trials), in that order. There were then four cycles, each consisting of two single-task blocks (of 52 trials each) followed by two switching blocks (of 68 trials each). The order in which the tasks were performed in the two single-task blocks was counterbalanced across cycles. The first four trials of each block were discarded as warm-up trials. Each switching block contained four randomly selected warm-up trial stimuli and then equal numbers of observations for each combination of task, trial type, congruence, and congruence on the previous trial. For these 16 conditions, each background color and word was presented once. There were no other constraints on stimulus order.

Participants spoke their answers into a microphone headset, triggering a voice key interfaced to the computer. The experimenter monitored the responses and operation of the voice key, marking response errors and trials on which the voice key failed to trigger or triggered before the participant's response. The intertrial interval was constant at 1,000 ms, measured from the onset of the participant's response on one trial to the presentation of the colored rectangle on the next. Word naming RTs for the 160-ms delay and 320-ms delay groups were measured from the onset of the word stimulus, not from the onset of the color.

At the viewing distance of approximately 80 cm, each colored rectangle subtended 1.1° vertically and 2.3° horizontally, and the word or false font string largely filled this space. In single-task blocks, the stimuli were presented in the center of the screen. In task switching blocks, each stimulus was presented 2.0° from the center of the screen in one of the quadrants defined by a large central *X* (formed from the diagonals of a square of side 6.5°); successive stimuli were displayed in successive clockwise positions. One of the diagonals of the *X* was thickened to indicate the location of the task switch. The assignment of task to quadrants and the location of the switch (NE–SW or NW–SE diagonal) were counterbalanced across participants.

Results

Analysis excluded 3.2% of the trials in single-task blocks and 4.6% of the trials in task switching blocks because they followed errors or were trials on which the voice key triggered incorrectly or followed such trials. Mean correct response times and error percentages were analyzed using an analysis of variance (ANOVA) with a between-subjects factor of delay and within-subject factors of task (word or color) and congruence (neutral or incongruent) and, in switching blocks, trial type (switch vs. nonswitch). The results reported here include trials on which stimulus attributes were repeated from the previous trial. The findings of analyses excluding trials with stimulus attribute repetitions were not materially different from those reported.

Single-task blocks. Performance in single-task blocks is summarized in Table 1. Word naming was faster, $F(1, 21) = 126.55$, $MSE = 3{,}387$, $p < .0001$, and more accurate, $F(1, 21) = 31.36$, $MSE = 1.66$, $p < .0001$, than color naming. Color naming suffered much greater interference than did word naming, as indicated by a reliable interaction between task and congruence both for RTs, $F(1, 21) = 115.45$, $MSE = 219.9$, $p < .0001$, and for errors, $F(1, 21) = 8.25$, $MSE = 1.58$, $p < .01$. The interaction between task, congruence, and delay was reliable for RTs, $F(2, 21) = 3.99$, $MSE = 219.9$, $p < .05$, but not for error rates, $F(2, 21) = 1.48$, $p > .2$, indicating that the asymmetry in interference was reduced as word onset delay increased.

We carried out separate ANOVAs to compare the performance of each task across delay conditions. For the word naming task, a marginally reliable interaction between congruence and delay for RTs, $F(2, 21) = 2.94$, $MSE = 33.5$, $p = .07$, but not for error rates, $F(2, 21) < 1.00$, indicated that this task suffered slightly more interference from distracting color information at the 160-ms delay than in the other delay conditions. In contrast, the interference suffered by color naming was reduced as delay was increased: The interaction between congruence and delay was reliable for RTs, $F(2, 21) = 5.33$, $MSE = 346.4$, $p < .05$, but not for errors, $F(2, 21) = 1.73$, $MSE = 2.61$, $p > .2$. Despite this reduction in interference with delay, a further set of ANOVAs revealed that color naming suffered greater interference than did word naming for all three participant groups: The interaction between task and congruence was reliable in the no-delay group, for RTs, $F(1, 7) = 146.21$, $MSE = 99.1$, $p < .0001$, and marginally for errors, $F(1, 7) = 3.86$, $MSE = 2.85$, $p = .09$; in the 160-ms delay group, for RTs, $F(1, 7) = 46.73$, $MSE = 190.2$, $p < .0005$, and again marginally reliable for error rates, $F(1, 7) = 4.83$, $MSE = 1.38$, $p = .06$; and even for the 320-ms delay group, for RTs, $F(1, 7) = 10.14$, $MSE = 370.4$, $p < .05$, but not reliably so for errors ($F < 1.00$). These results are consistent with previous findings (Glaser & Glaser, 1982). Of critical interest here is the concurrent impact of delay on task switching costs.

Table 1
Performance in Single-Task Blocks of Experiment 1 and the Control Experiment 1a, Showing Mean Response Times (RTs; in Milliseconds) and Percent Error Rates

	No delay		160-ms delay		320-ms delay		Control (320 ms)	
Task	RT	% error	RT	% error	RT	% error	RT	% error
Word naming								
Neutral	462	0.1	480	0.0	468	0.0	457	0.3
Incongruent	465	0.3	492	0.0	471	0.1	459	0.4
Interference	3	0.1	12	0.0	3	0.1	2	0.1
Color naming								
Neutral	589	1.2	563	0.5	562	0.7	570	1.6
Incongruent	676	3.6	641	2.3	609	1.0	592	1.0
Interference	87	2.4	78	1.8	47	0.3	22	−0.6

Task switching blocks. As is apparent in Figure 2, there was a robust cost of task switching for both RTs, $F(1, 21) = 70.31$, $MSE = 2{,}390$, $p < .0001$, and errors, $F(1, 21) = 34.68$, $MSE = 4.12$, $p < .0001$. We are interested in the effect of delay on the difference in switch costs between the two tasks. This three-way interaction between task, trial type, and delay was reliable, both for RTs, $F(2, 21) = 13.73$, $MSE = 961.7$, $p < .0005$, and for errors, $F(2, 21) = 7.13$, $MSE = 4.45$, $p < .005$. The four-way interaction between task, trial type, congruence, and delay was also significant for RTs, $F(2, 21) = 8.97$, $MSE = 374.5$, $p < .005$, but only marginally so for errors, $F(2, 21) = 3.27$, $MSE = 4.03$, $p = .06$. The general pattern, apparent in Figure 2, is that for incongruent stimuli, a marked asymmetry in switch costs—in the direction of a higher cost for switching to the dominant task—was reversed as a delay was introduced between color and word onsets. The same interaction is visible, but greatly attenuated, for neutral stimuli.

We investigated these interactions further using separate ANOVAs for the three groups. For the no-delay group, switch costs were reliably larger for word naming than for color naming, reflected in a reliable interaction between task and trial type for RTs, $F(1, 7) = 34.76$, $MSE = 185.7$, $p < .001$, and for errors, $F(1, 7) = 9.19$, $MSE = 6.00$, $p < .05$. This difference was much greater for incongruent stimuli than for neutral stimuli, indicated by a reliable three-way interaction between task, trial type, and congruence, for RTs, $F(1, 7) = 18.25$, $MSE = 178.5$, $p < .005$, and for errors, $F(1, 7) = 3.65$, $MSE = 6.02$, $p = .10$. By contrast, switch costs in the 160-ms delay group were numerically larger for the color naming task, but the interaction between task and trial type was not reliable for RTs, $F(1, 7) = 2.92$, $MSE = 1{,}516$, $p > .1$, or for errors ($F < 1.00$), nor was there a reliable three-way interaction between task, trial type, and congruence, for RTs, $F(1, 7) = 2.41$, $MSE = 808.3$, $p > .15$, or for errors ($F < 1.00$). The 320-ms delay group showed a pattern similar to that of the 160-ms delay group, with larger switch costs for color naming. For this group, the interaction between task and trial type was reliable for RTs, $F(1, 7) = 18.10$, $MSE = 1{,}184$, $p < .005$, and, marginally, for errors, $F(1, 7) = 4.77$, $MSE = 2.59$, $p = .07$. The 320-ms delay group also showed a reliable three-way interaction between task, trial type, and congruence, for RTs, $F(1, 7) = 13.23$, $MSE = 136.6$, $p < .01$, but not for errors, $F(1, 7) = 2.33$, $MSE = 2.35$, $p > .15$, indicating that the difference in switch costs for the two tasks was particularly marked for incongruent stimuli (switch costs for neutral stimuli: word naming = 24 ms; color naming = 76 ms; switch costs for incongruent stimuli: word naming = 24 ms; color naming = 118 ms).

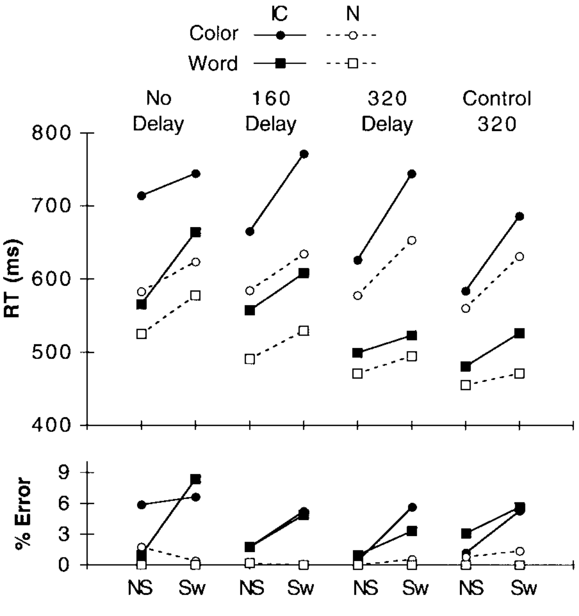

Figure 2. Mean correct response times (RTs) and error rates for neutral (N) and incongruent (IC) stimuli in the task switching blocks of Experiment 1, separately for the three participant groups (No Delay, 160 Delay, and 320 Delay) and for the control Experiment 1a (Control 320). NS = nonswitch trials; Sw = switch trials.

Experiment 1a

A possible methodological flaw in Experiment 1 is that, for both tasks, the 1,000-ms intertrial interval was measured from the response onset on one trial to the onset of the colored rectangle on the subsequent trial. Thus, the interval available for preparation for the word naming task increased with the delay separating color and word onsets. Switch costs are typically reduced as preparation interval lengthens (De Jong, 1995; Meiran, 1996; Rogers & Monsell, 1995). One second is normally adequate for asymptotic preparation, but it could nevertheless be argued that it was merely the increase in the interval available for task preparation that was responsible for the observed reduction in word naming switch costs as the onset delay of the word stimulus was increased.

To check this possibility, we ran a further group of 8 paid participants: 3 men and 5 women, ages 20–27 years. The onset difference separating the color and word attributes was 320 ms, and the 1,000-ms preparation interval was now timed from the previous response to the onset of the relevant stimulus attribute, word, or color, depending on the task. The aim was to see if we would replicate the findings in the 320-ms delay group of Experiment 1. The results, labeled *control-320* in Table 1 and Figure 2, were similar to those observed in the 320-ms delay group. A greater cost of switching to the color naming task was again apparent as a reliable interaction between task and trial type for RTs, $F(1, 7) = 9.46$, $MSE = 1{,}316$, $p < .05$, although the corresponding interaction was not reliable in the error rate data, $F(1, 7) = 1.90$, $MSE = 2.44$, $p > .2$. Comparison with the task switching performance of the 320-ms delay group revealed no significant differences between the experiments for the switch costs of the two tasks, either for the time costs or the error costs (Fs < 1.00). Thus, the confound between delay condition and intertrial interval had no material effect on the observed switch costs.

Discussion

The present experiment used a delay in the onset of the word attribute to make it easier to select the task-relevant stimulus attribute and, hence, to reduce the level of Stroop interference suffered by color naming. The manipulation was effective in reducing, although not eliminating, this interference. Reverse Stroop interference remained essentially absent at all delays. Turning to the task switching data, the no-delay group replicated the findings of Allport et al. (1994) and Wylie and Allport (2000): A much greater cost was associated with switching to the word naming task. Introducing a 160-ms delay in word onset caused only a small reduction (9 ms) in Stroop interference in single-task blocks but reversed the asymmetry of switch costs: A larger cost was observed for the color naming task. This trend continued for the 320-ms delay group (and the 320-ms control group): A reduced but still healthy Stroop effect, and no reverse Stroop effect in single-task blocks, was accompanied by a reliably larger switch cost for the color naming task, the subordinate task, in task switching blocks.

In summary, although delaying the onset of the word attribute by 160 ms or 320 ms only modestly attenuated the asymmetry of interference between the tasks, it completely reversed the asymmetry of switch costs seen in the no-delay group. Evidently, it is not a general rule that it is harder to switch to the dominant task, as Allport et al. (1994) originally suggested. This finding appears to challenge task priming accounts of the switch cost and is certainly problematic for Allport et al.'s original account. However, we argue later that our findings can be reconciled with a task priming account and illustrate our hypothesis using a formal model. But first we demonstrate that separating the response sets used by the two tasks likewise reverses the asymmetry of switch costs.

Experiments 2a and 2b

The importance of response-set overlap was suggested by a pair of simple experiments we report briefly here as an appetizer and rationale for Experiment 3. We first (Experiment 2a) had participants switch between naming digits and classifying them as odd or even. The latter task is clearly less practiced and harder than the former, yet we found that it was clearly easier to switch to the naming task than to the odd–even task—a prima facie violation of the predicted effect of task priming on switch costs. We speculated that a crucial feature of this task pair is that they use different response sets (digit names and odd–even judgments). We therefore (Experiment 2b) examined switching between digit naming and another, harder task that used the same response set: subtracting the digit from 10 and naming the difference (e.g., 3 → "seven")—we call this the *tens-complement* task. If overlap of response sets is critical, we would now expect to find larger switch costs for the stronger task of digit naming, in line with the predictions of the task priming hypothesis.

Method

In Experiment 2a, participants were presented with a digit stimulus between 2 and 9 and were required either to name the digit or to classify it as odd or even. Participants were given brief practice at the two tasks and then performed 14 task switching blocks of 68 trials. The first 4 trials of each block were regarded as warm-up trials and were discarded from the analysis. As in the task switching blocks of Experiment 1, the task changed every 2nd trial. Here, it was redundantly cued by a colored background shape on which the digit stimulus was presented. For example, a participant might see as background cues the repeating sequence pink square, pink diamond, blue diamond, blue square, pink square, and so forth, with the requirement to perform digit naming when the background shape was pink and the odd–even task when it was blue. The assignment of cue, dimensions, and values (pink vs. blue, or diamond vs. square) to each task was counterbalanced across participants, and the cue dimension was changed halfway through the experiment (with no significant impact on switch costs).

The design of Experiment 2b was similar, except that participants switched between digit naming and subtracting the digit presented from 10. Because the stimuli were the numbers from 1 to 9 (excluding 5, for which the answer for the two tasks is the same), the response sets for the tasks overlap completely. Participants performed 16 task switching blocks of 36 trials each, with the task redundantly cued by a colored background shape (which in this experiment did not change during the session).

In both experiments, participants spoke their answer into a microphone headset, triggering a voice key interfaced to the computer. At this point, the screen cleared and the colored shape cue for the next trial was presented 50 ms later. After a further 1,150 ms, the next digit stimulus appeared. Participants were instructed to respond as quickly as possible while avoiding errors. The responses and operation of the voice key were monitored by the experimenter, but no feedback was given. There were no immediate stimulus or response repetitions. There were 8 paid participants in each experiment: 5 men and 3 women, ages 18–27 years, in Experiment 2a; and 8 men, ages 20–26 years, in Experiment 2b.

Results

In Experiment 2a, digit naming responses were significantly faster than odd–even task responses, $F(1, 7) = 45.93$, $MSE = 40{,}269$, $p < .0005$, and were more accurate, $F(1, 7) = 19.46$, $MSE = 63.5$, $p < .005$. In Experiment 2b, digit naming was performed faster, $F(1, 7) = 107.8$, $MSE = 1{,}551$, $p < .0001$, and slightly more accurately ($F < 1.00$), than the tens-complement task. These results are consistent with digit naming being the stronger of the two tasks in both experiments. As shown in Figure 3, however, the pattern of switch costs differed substantially between experiments. In Experiment 2a, the switch cost was larger for the odd–even task than for digit naming: The interaction

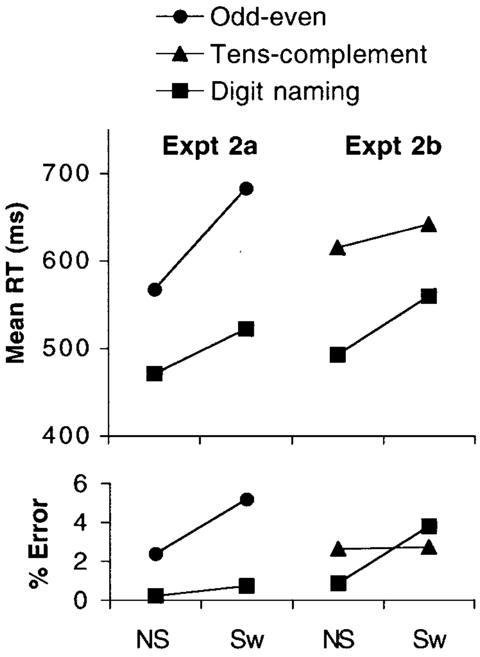

Figure 3. Mean response times (RTs) and error rates in Experiments (Expts) 2a and 2b. NS = nonswitch trials; Sw = switch trials.

between task and trial type was reliable both in the RT data, $F(1, 7) = 10.33$, $MSE = 11{,}262$, $p < .05$, and in the error data, $F(1, 7) = 7.79$, $MSE = 18.8$, $p < .05$. This pattern was reversed in Experiment 2b, in which digit naming suffered larger switch costs than did the tens-complement task, both in RTs, $F(1, 7) = 14.44$, $MSE = 460.6$, $p < .01$, and in error rates, $F(1, 7) = 5.50$, $MSE = 5.84$, $p = .05$.

Discussion

Although both are perfectly easy to perform, the odd–even and the tens-complement tasks are clearly less familiar and harder than the naming task, arguably by about the same amount: On nonswitch trials, latencies for both were about 100 ms longer than for the naming task, and the differences in error rates were also similar. Yet in Experiment 2b (with a shared response set) it was clearly easier to switch to the harder task, whereas in Experiment 2a (with separate response sets) the opposite was the case. Of course, we cannot claim that the odd–even task and the tens-complement task are identical in all respects other than their response sets. However, the results certainly license the working hypothesis that shared versus different response sets may be another critical determinant of the two patterns of switch costs observed. We pursue this hypothesis further in Experiment 3.

Experiment 3

Experiment 3 was intended to provide a more systematic analysis of the effects of response-set overlap on task switching. We manipulated whether the tasks switched between shared a response category and/or a response modality. *Response category* refers to the abstract response decision being made—that is, the meaning of the response (e.g., digit names or odd–even judgment), irrespective of the way it is expressed (e.g., hands or voice). As described above, Klein (1964) has shown that between-task interference is greater when responses for the two tasks are drawn from the same category than when the responses differ. *Response modality* means hands versus voice. Redding and Gerjets (1977) have shown that the interference suffered by the weaker of two tasks is reduced when the response modality used in this task is not highly associated with the stronger task. Thus, there is evidence that separating the response sets of the two tasks by category or modality allows participants to effectively *gate out* responses for the currently irrelevant task, reducing the interference suffered when incongruent stimuli are presented. We were interested how manipulations of response-set overlap would affect task switching performance.

The stimulus was a digit (*1*, *2*, *3*, or *4*) displayed on a colored background square. Four groups of participants each switched between naming the digit and responding to the color with an arbitrary response. Each group used a different set of responses for the color task, as shown in Table 2. For the full overlap group, participants responded to colors with spoken digit names, so there was overlap with the digit naming task in both response category and response modality (as in Experiment 2b). The modality overlap group responded to colors with a spoken direction word (*up*, *down*, *left*, and *right*), so there was overlap in response modality but not in response category (as in Experiment 2a). The *directions* response set was chosen to be as arbitrarily associated with colors as the digit response set and to allow roughly equivalent vocal and key-press expressions. The response category overlap group responded to colors using a left-to-right array of keys marked *1*, *2*, *3*, and *4*, so the tasks shared a response category but differed in response modality.[1] Finally, the no-overlap group responded to background color by pressing one of a set of keys compatibly corresponding to the directions up, down, left, and right, so that the tasks used different response categories and these responses were expressed in different modalities.

Table 2
Specification of the Color Task for Each of the Four Participant Groups in Experiment 3

Group	Full overlap	Modality overlap	Res cat overlap	No overlap
Red	“One”	“Up”	*1* key	↑ key
Yellow	“Two”	“Right”	*2* key	→ key
Green	“Three”	“Down”	*3* key	↓ key
Blue	“Four”	“Left”	*4* key	← key

Note. Res cat = response category.

Method

There were 32 paid participants: 13 men and 19 women, ages 18–32 years. Eight were assigned to each of four groups, defined by the nature of the color task responses, as described above. Half of the stimuli used were incongruent, permitting different responses in each of the two tasks. The other half of the stimuli were neutral, affording a response in only one task. Incongruent stimuli consisted of a digit (*1*, *2*, *3*, or *4*) presented on a background colored red, yellow, green, or blue. In place of a digit, neutral stimuli for the color task had a false font character that we made by exchanging fragments of the digit stimuli to generate digitlike hieroglyphs not recognizable as the original digits but with the same number of screen pixels, lines, curves, and so forth. Neutral stimuli for the digit task were digits presented against an empty background square. Digit and color information were presented simultaneously throughout.

Participants were first trained on three blocks of 52 trials of the color task, followed by a 52-trial block of the digit task, and, finally, a single 68-trial task switching block. In the main experiment, there were two cycles, each consisting of two single-task blocks (52 trials) followed by two switching blocks (68 trials). The order in which the tasks were performed in the two single-task blocks was counterbalanced across cycles. The first four trials of each block were discarded as warm ups. The constraints imposed on stimulus order were as in Experiment 1.

Vocal responses were detected by a voice key driven by a head-worn microphone and were monitored by the experimenter. Key-press responses

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[1] It may be objected that participants in the response category overlap group need not pay attention to the numbers on the keys when performing the color task, questioning whether there is really any overlap in response category for this group. However, as will become apparent, significant between-task interference was observed in the response category overlap condition—indeed, this interference was greater than that seen in the response modality overlap group—suggesting that participants were using the category labels on the response keys. This finding is consistent with previous research showing that, in versions of the Stroop task that require manual responses, the size of the Stroop effect is modulated by whether the keys are marked with color words or color patches (McClain, 1983; Pritchatt, 1968).

were made on a standard computer keyboard. Participants in the response category overlap group made color task responses by pressing, with the index and middle fingers of the two hands, four keys in a row marked with the digits *1*, *2*, *3*, and *4* (the *v*, *b*, *n*, and *m* keys). For the no-overlap group, the *r*, *g*, *c*, and *d* keys were marked with up-, right-, down-, and left-pointing arrows, respectively. The left and right arrow keys were operated by the middle and index fingers of the left hand, and the up and down keys were operated by index and middle fingers of the right hand.

At the viewing distance of approximately 80 cm, the background square subtended 1.1° of visual angle, and the number or false font characters were 1° high. During single-task blocks, the stimuli were presented in the center of the screen. In task switching blocks, stimuli were presented in one of the quadrants defined by a large central cross as in Experiment 1. The intertrial interval was 1,000 ms.

Results

Prior to analysis, we removed 3.5% of the data set from single-task blocks and 5.1% of trials from task switching blocks according to the exclusion criteria used in the previous experiments. The analyses also excluded trials on which stimulus features and responses were repeated. Response repetition is known to interact with switch costs—response repetitions are facilitatory only on nonswitch trials (Rogers & Monsell, 1995). However, exact response repetitions on switch trials are possible only in the full overlap condition and, hence, their presence was confounded with group. In addition, the color tasks, with arbitrary mappings of background color to digit or direction responses, showed very large response repetition benefits on nonswitch trials ($M = 195$ ms). Hence, including stimulus repetitions tended to inflate switch costs for the weaker task. To avoid these confounds in the comparison of switch costs across tasks and participant groups, we excluded trials with repeated stimulus features or responses. Mean correct RTs and error percentages were analyzed using mixed ANOVAs with between-subjects factors of color task modality (vocal/key press) and color task response category (numbers/directions), and within-subject factors of task (digit/color), congruence (neutral/incongruent) and, in switching blocks, trial type (switch/nonswitch).

Single-task blocks. Results from single-task blocks are given in Table 3. Digit naming responses were faster, $F(1, 28) = 146.26$, $MSE = 16{,}068$, $p < .0001$, and more accurate, $F(1, 28) = 46.17$, $MSE = 6.97$, $p < .0001$, than color task responses. The asymmetry in task strength was also revealed by the presence of larger interference effects for the color task: The interaction between task and congruence was reliable for RTs, $F(1, 28) = 30.32$, $MSE = 382.8$, $p < .0001$, but was not significant for errors ($F < 1.00$). The color task suffered greater interference when it shared a response category with the digit naming task, indicated by a reliable interaction between task, congruence, and response category for the RT data, $F(1, 28) = 5.76$, $MSE = 382.8$, $p < .05$, although not reliably so for error rates, $F(1, 28) = 1.64$, $MSE = 5.05$, $p > .2$. Between-task interference was not reliably modulated according to the response modality of the color task; the four-way interaction was not significant in the RT or the error rate data (Fs < 1.00).

Overall, it appears that the color task suffered more interference in the full overlap and response category overlap groups than in the modality overlap and no-overlap groups. Separate ANOVAs on the data for each group confirmed these impressions. The color task suffered much greater interference than did the digit naming task in the full overlap group, indicated by a reliable interaction between task and congruence for RTs, $F(1, 7) = 90.01$, $MSE = 72.74$, $p < .0001$, although not for errors, $F(1, 7) = 1.06$, $MSE = 2.23$, $p > .3$. A similar interaction was apparent for the response category overlap group, for RTs, $F(1, 7) = 9.11$, $MSE = 597.7$, $p < .05$, but again not for errors ($F < 1.00$). Interference did not differ reliably for the two tasks in the modality overlap group—the Task × Congruence interaction was not reliable either for RTs, $F(1, 7) = 2.69$, $MSE = 688.3$, $p > .1$, or for errors, $F(1, 7) = 1.96$, $MSE = 3.60$, $p > .2$—or for the two tasks in the no-overlap group, either for RTs, $F(1, 7) = 1.83$, $MSE = 172.3$, $p > .2$, or for errors ($F < 1.00$). As expected, therefore, interference between the tasks was marked in the full overlap group and was much less apparent in the modality overlap and no-overlap groups, suggesting that participants in the latter two groups were able to gate out the irrelevant digit naming response when performing the color task. Large between-task interference effects were observed in the response category overlap group, even though the tasks used different response modalities. However, as we show, performance in task switching blocks suggest that participants in this group were able to use response gating, albeit less effectively than were those participants in the modality and no-overlap groups.

Table 3
Performance in Single-Task Blocks of Experiment 3, Showing Mean Response Times (RTs; in Milliseconds) and Percent Error Rates

	Full overlap		Modality overlap		Res cat overlap		No overlap	
Task	RT	% error	RT	% error	RT	% error	RT	% error
Digit naming								
Neutral	486	0.0	481	0.0	469	0.0	461	0.0
Incongruent	490	0.3	490	0.0	480	0.3	466	0.0
Interference	4	0.3	9	0.0	11	0.3	5	0.0
Color								
Neutral	677	1.3	736	3.4	692	3.4	632	1.8
Incongruent	740	2.9	767	1.6	747	5.7	648	2.6
Interference	63	1.6	31	−1.8	55	2.3	16	0.8

Note. Res cat = response category.

Task switching blocks. Performance in task switching blocks is shown in Figure 4. There were significant time costs, $F(1, 28) = 83.92$, $MSE = 3{,}573$, $p < .0001$, and error costs, $F(1, 28) = 6.22$, $MSE = 4.45$, $p < .05$, of task switching. There was a reliable four-way interaction between task, trial type, color task response category, and color task response modality, both for RTs, $F(1, 28) = 9.14$, $MSE = 1{,}656$, $p < .01$, and for errors, $F(1, 28) = 9.20$, $MSE = 6.39$, $p < .01$. As shown in Figure 4, this interaction indicates that switch costs were larger for digit naming than for the color task in the full overlap group but showed the opposite pattern for the other three groups. A marginally reliable five-way interaction, for RTs, $F(1, 28) = 3.86$, $MSE = 2{,}064$, $p = .06$, and for errors, $F(1, 28) = 3.07$, $MSE = 8.30$, $p = .09$, indicated that these between-groups differences in switch costs were especially marked for incongruent trials.

Separate ANOVAs for each participant group confirm these impressions. For the full overlap group, switch costs were larger for the stronger task of digit naming, although the interaction between task and trial type was reliable only for the error rates; for RTs, $F(1, 7) = 1.03$, $MSE = 2{,}446$, $p > .3$; for errors, $F(1, 7) = 7.49$, $MSE = 12.39$, $p < .05$. An interaction between task, trial type, and congruence indicated that between-task differences in switch costs were particularly marked for incongruent stimuli; for RTs, $\mathrm{F}(1, 7) = 2.32$, $MSE = 1{,}527$, $p > .15$, and reliably so for errors, $F(1, 7) = 6.13$, $MSE = 8.57$, $p < .05$. For the other three groups, switch costs were smaller for digit naming than for the color task. For the modality overlap group, the interaction between task and trial type was reliable, indicating larger switch costs for the color task than for digit naming, for RTs, $F(1, 7) = 16.77$, $MSE = 1{,}153$, $p < .005$, but not for error rates ($F < 1.00$). The same interaction was apparent for the response category overlap group, for RTs, $F(1, 7) = 10.31$, $MSE = 1{,}828$, $p < .05$, and for errors, $F(1, 7) = 2.58$, $MSE = 8.77$, $p > .15$. In the no-overlap group, color task switch costs were only slightly larger than those for digit naming, but, again, the interaction between task and trial type was reliable for RTs, $F(1, 7) = 5.41$, $MSE = 1{,}197$, $p = .05$, but not for the error data ($F < 1.00$). In the latter three groups, there was no further interaction between task, trial type, and congruence—for RTs and for errors in all three groups, $F\mathrm{s} < 1.00$.

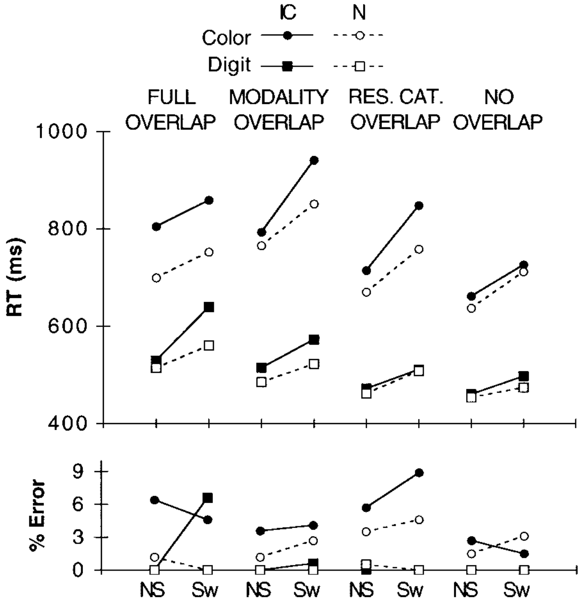

Figure 4. Mean response times (RTs) and error rates for neutral (N) and incongruent (IC) stimuli in the task switching blocks of Experiment 3, separately for the four participant groups. RES. CAT. = response category; NS = nonswitch trials; Sw = switch trials.

It is interesting that switch costs were larger for the color task than for the digit task in the response category overlap group, given that the color task suffered a high level of interference in single-task blocks. With regard to this issue, we draw attention to the pattern of interference effects on nonswitch trials, which provide the performance baseline for switching blocks. For these trials, color task interference effects in the response category overlap group (45 ms) were closer to those of the modality overlap and no-overlap groups (29 ms and 24 ms, respectively) than to those observed in the full overlap group (104 ms). This result suggests that participants in the response category overlap group were able to effectively gate out irrelevant digit naming responses. It remains a puzzle, therefore, why they did not appear to do so in single-task blocks. One speculative hypothesis is that there is less need to use a response gate in single-task blocks, so perhaps the benefit to performance of gating responses was outweighed by the cost of enforcing the gate. Alternatively, it could just be that the added demands of the task switching blocks accentuated performance differences between the conditions that were present but less apparent in single-task blocks. Whatever the true explanation of the data, the critical point is that interference was larger in the full overlap group than in the other groups, and only in this group were switch costs larger for the stronger task of the pair.

Discussion

This experiment examines the impact of separating the response sets of two tasks on the cost of switching between them. We conjectured that separating response sets in this way would allow participants to gate out responses of the irrelevant task set, thus reducing the interference they suffer. In the full overlap group, for which response gating should have been impossible because the tasks shared a response set, switch costs were larger for the stronger task of digit naming, and particularly so for incongruent stimuli. For the three groups in which response category and/or response modality differed for the tasks, the time costs of switching were larger for the weaker color task. Thus, the pattern of switch costs observed by Allport et al. (1994), with a greater switch cost for the stronger task, was only apparent when the tasks shared a response set. In all three other cases, in which the tasks differed in the modality or category of responses, switch costs were reliably larger for the weaker color task.

However, it does not seem that Allport's asymmetry of costs is always reversed when two tasks of different strength use different response sets. Elsewhere, we have found a greater cost of switching to a more recently practiced task—further evidence of Allport's asymmetry of costs—using tasks with separate response sets (Yeung & Monsell, 2002). Similarly, in a study of digit naming in bilinguals by Meuter and Allport (1999), a larger cost was observed for switching to participants' stronger language, even though the responses for the two tasks used different languages. It could be that response gating cannot be established effectively at short preparation intervals. In the experiments reported here, the preparation interval was at least 1,000 ms, whereas our selective practice experiments used a 600-ms interval, and Meuter and Allport presented the task cue and digit simultaneously, leaving no time for effective preparation. Alternatively, response gating may not prove an effective strategy in some cases. In our selective practice experiments, the stimulus set was changed regularly and the tasks were subjectively very demanding. In the language switching experiment of Meuter and Allport (1999), the meaning of responses was the same for both tasks (i.e., number values), and all that differed was the language in which these concepts were communicated. Such factors may contribute to the participant's decision about what control strategy to use. Further work is needed to understand the temporal properties and the limitations of the response gating process.

General Discussion

The experiments reported here are concerned with identifying boundary conditions for the phenomenon of a larger performance cost of switching from a weaker task to a stronger task than vice versa. In all three experiments, we had a base condition with simultaneous onset of the two stimulus attributes and shared response sets (as in standard Stroop experiments). In each experiment, we duly observed a larger cost of switching to the stronger task. In the other conditions of Experiment 1, we delayed presentation of the stimulus attribute associated with the stronger task; in Experiments 2 and 3, we separated the response sets for the two tasks. We expected both manipulations to reduce the level of interference between the tasks, and, generally, they did so, without eliminating the asymmetry of interference. However, both manipulations did more than reduce the asymmetry of switch costs; they completely reversed it. When the onset of one stimulus attribute was delayed or the response sets for the two tasks separated, a larger cost was associated with switching to the weaker task of the pair. This reversal in the pattern of switch costs was found to result from both a decreased cost of switching to the stronger task and a greatly increased cost of switching to the weaker task. Our findings suggest that Allport et al.'s (1994) asymmetry of switch costs will consistently be observed only in conditions that maximize the degree of interference between tasks—for example, where there is both (a) simultaneous onset of stimulus attributes, making selection of the relevant attribute difficult, and (b) overlap in the response sets of the two tasks, making selection of the relevant response difficult.

The present findings place important constraints on theories of task switching performance. In particular, no existing theory predicts our finding that reducing the interference suffered by a weak task should greatly increase the cost of switching to that task: Task priming when switching from the stronger task should be no larger because, even with delayed word onset or separated response sets, the strong task suffered little interference from the weaker task. Similarly, the time taken for control processes to establish the weaker task set should take no longer (and should perhaps even be faster) as interference with this task is reduced. As neither of the putative components of the switch cost should be increased when interference is reduced, existing theories of the switch cost cannot explain the present findings. However, it may be possible to explain our findings in terms of task priming effects if we add a crucial assumption about how an appropriate level of control input is determined (Yeung, 1999). In the following section, we outline this hypothesis and use a simple mathematical model to illustrate our ideas.

Developing Task Priming Theory

The present model (Yeung, 1999) provides an analytical demonstration of how the interaction we observed between switching, interference, and relative task strength can emerge from an interaction between task priming and control input. In this model, as in previous task priming accounts of the switch cost (e.g., Allport et al., 1994), task sets are held to compete according to their degree of activation, with competition between task sets dependent on task strength, control input, and task priming effects. Two further assumptions form the core of our theory. These assumptions are as follows.

Asymmetrical priming. Like Allport et al. (1994), we assume that task priming effects are particularly large following performance of a weak task. We ground this assumption in a simple diminishing-returns principle linking the factors that influence task-set activation to the activation achieved.

Minimization of control. We assume that top-down control is effortful; hence participants typically apply the minimum control bias required to perform the required task with a reasonable degree of accuracy (cf. Goschke, 2000; Mozer, Colagrosso, & Huber, 2002). The aim of the simulations is to investigate how these simple assumptions may be used to explain the complex pattern of findings we report.

Simulation Details

Our theory is formalized as a set of simple equations. These equations represent an analytic tool for investigating the interaction between task priming and control rather than a complete mechanism for response selection in task switching. Although the quantitative performance of the model varied with the precise parameters chosen, qualitatively comparable patterns of results were found using a wide range of parameter values. That is, the simulation results followed from the processing principles incorporated into the model rather than the particular parameters used. The values given here were chosen to provide a reasonable quantitative fit to the empirical data.

Implementation of core assumptions. The core assumptions of the model are implemented in terms of two equations that govern the activation levels of competing tasks. First, for each task i, we calculate the inputs to an activation function for the task set as follows:

$$\text{input}_i = \text{strength}_i + \text{priming}_i + \text{control}_i + \text{noise}. \qquad (1)$$

Task strength effects are implemented by assigning a higher baseline level of activation to a strong, well-practiced task (e.g., word naming) than to a weaker task (e.g., color naming). We model task priming as a transient increase in the activation of the most recently performed task set (i.e., positive priming). Endogenous control input is simulated as increasing the activation of the currently relevant task set, and the level of this input is determined as described below. Finally, Gaussian noise ($M = 0.0$, $SD = 0.1$)

is added to the input of each task set. The activation of the task set is then given by

$$\text{activation}_i = 1 - e^{(-c*\text{input}[i])}, \quad (2)$$

where c is a constant equal to 1.5 in the present simulations.

The use of a negatively accelerated function means that task priming has a larger impact on the activation of a weak task than on the activation of a strong task (see Figure 5)—the first central assumption of our model. To implement the second critical assumption—that top-down control is effortful and, hence, minimized where possible—control inputs are determined by iteration during a training phase to be at the minimum level required to keep error rates low in each condition (<5%). In this training phase, control input to each task set was initialized at a minimum value (0.15), and then the performance of the model was assessed over several trials for each task and trial type (nonswitch and switch). The control input for the relevant task set and trial type was incremented by 0.05 units each time the model made an error and was reduced slightly (by 0.001 units) for each correct response. In this way, control input was set to the minimum level required to produced generally accurate performance, capturing our assumption that levels of control input reflect a trade-off between accuracy and effort. Performance quickly stabilized to a level at which responding was accurate on most trials, and the output of the model was assessed once this stable level of performance was reached.

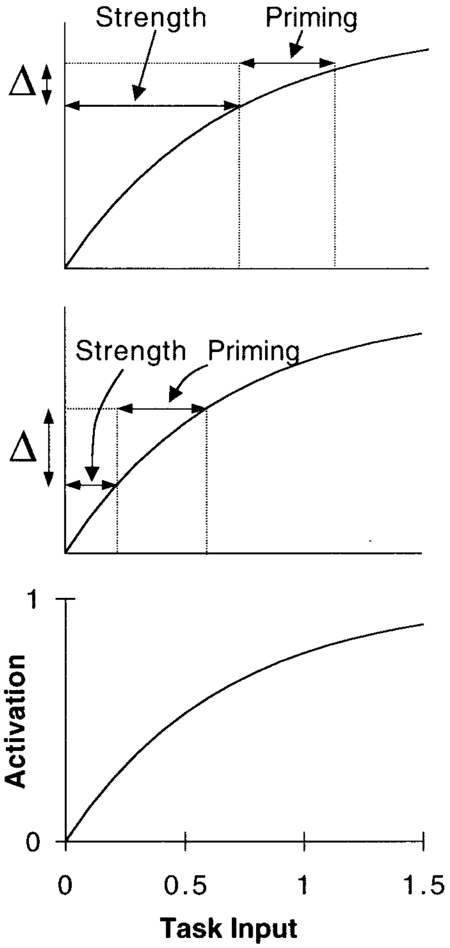

Figure 5. Illustration of the negatively accelerated function determining task activation. The bottom panel shows the quantitative features of the function; the upper panels show the effect of task priming on task activation. Task priming results in a small change (Δ) in activation of the strong task (upper panel) but a large change in activation of the weak task (middle panel).

Auxiliary details. Equations 1 and 2 and the assumption of minimization of control input, adjusted from trial to trial, represent the core of the model. However, we need further equations to specify how task-set activation affects the time taken by response selection. To this end, we use three equations that implement two critical properties of response selection: that its duration is affected by the activation level of the task set, and that more interference is observed if a competing task set has a level of activation close to that of the relevant task. As noted by Hillstrom and Logan (1997), at an abstract level, all models of response selection designed to account for between-task interference share a common design: Task-specific processes generate responses, which then converge at a shared response resolution stage. For simplicity, we implement response selection using separate equations for the response generation and response resolution stages. The time taken by response generation is given by two equations:

$$\text{generation rate}_i = \text{activation}_i / \Sigma \text{ activation} \quad (3)$$

and

$$\text{generation time}_i = \text{THRESHOLD}/\text{generation rate}_i. \quad (4)$$

THRESHOLD takes a value of 100 in the model. Thus, response codes are generated and transferred asynchronously to the resolution process, with a transfer-time difference dependent on the relative activation of the competing task sets. The time taken for response resolution depends on the relative time at which response codes for competing tasks are generated and is determined by the following equation:

$$\text{resolution time} = r + f\,[r - (\text{generation time}_j - \text{generation time}_i)], \quad (5)$$

where r is drawn on each trial from an ex-Gaussian distribution, the convolution of a normal distribution ($M = 140$, $SD = 10$) and an exponential distribution ($M = 40$); and i and j refer to the two competing tasks. For simplicity, the first response generated is always produced by the model, but interference or facilitation is observed to the extent that another response code is generated before the resolution process is finished. The function, f, indicates that the magnitude of interference or facilitation depends on the proximity in time with which the competing responses are generated. The appearance of the term r within function f indicates that interference and facilitation will be observed only if the second response is generated before the resolution process is finished. For the sake of simplicity, f is taken to be a linear multiplier of the time difference, with a minimum value of zero. The value of f is set to equal 0 for neutral stimuli and 0.5 for incongruent stimuli. Thus, Equation 5 captures the notion that the resolution process takes longer if there is competing response information than if there is no competition. Additionally, to simulate the effects of response gating, *f* is set to 0 for all stimuli when response gating is in place.

With the parameters varying as described, the output of the model gives the simulated RT in milliseconds according to the following equation:

$$\text{RT} = \text{P} + (\text{generation time} + \text{resolution time}) + \text{R}, \quad (6)$$

where P and R are constants representing the time taken for perceptual and response-production processes, respectively. P + R took a value of 150 ms in the simulations reported. The simulation results reported are each based on 50 simulations of 600 trials each. Multiple simulations were run to ensure that the results obtained were robust to slight variations in levels of control input dependent on training phase performance.

Modeling neutral stimuli. There is a range of evidence that stimuli activate not just responses but task sets associated with them (Monsell, Taylor, & Murphy, 2001) and that competitive activation of the irrelevant task set modulates switch costs (e.g., Rogers & Monsell, 1995; Waszak, Hommel, & Allport, in press). Neutral stimuli are not associated with the irrelevant task set, except by weak generalization from their attributes (e.g., letter fragments in the case of false font strings). To capture this in the simulations, we reduced the "intrinsic" or uncontrolled component of the input to the irrelevant task set on trials with neutral stimuli by modifying Equation 1:

$$\text{input}_i = k(\text{strength}_i + \text{priming}_i) + \text{control}_i + \text{noise}. \quad (7)$$

Preliminary simulations indicated that setting k to 0.6 adequately simulates the interaction between stimulus congruence and other variables. This feature of the model is not needed to produce the basic effect that reduced interference reverses the asymmetry of switch costs; it contributes to the attenuation of these effects for neutral stimuli.

Simulation Results

We first describe how the model accounts for the finding of larger switch costs for word naming than for color naming in the no-delay condition of Experiment 1. Table 4 gives the activations of the color naming and word naming tasks, and Figure 6 (no-delay condition) shows the simulated RT for switch and nonswitch trials of each task. Evidently, the model captures qualitative features of the data, with a larger cost associated with switching to word naming than with switching to color naming. The large cost of switching to word naming results from the large effect of task priming on the activation of the weaker color naming task set. On switch trials, priming of the color naming task set allows it to provide strong competition for word naming. On nonswitch word naming trials, on the other hand, the only input to the color naming task is from its intrinsic task strength—which, by definition, is low—so word naming suffers little competition. Therefore, only on switch trials does word naming suffer interference from color naming, and the result is a large cost of switching to this task.

Now consider color naming trials. A high level of control input is required to perform this task correctly in the face of competition from word naming (Table 4). Although this is true for both switch and nonswitch trials, the required level of control input is less on nonswitch trials because the required task is primed on these trials. The net effect is that activation of the color task differs little across switch and nonswitch trials. Activation of the word naming task also changes little from switch to nonswitch trials of color naming, as priming has relatively little effect on the activation level of a strong task. Thus, both tasks are active to a similar degree on switch and nonswitch trials, so the color task suffers interference from word naming for both trial types. Because there is almost as much between-task interference on switch and nonswitch trials, the switch cost is small. Thus, the model replicates the basic finding of greater costs of switching to word naming than to color naming.

Table 4
Parameters and Partial Results of a Simulation of Switching Between Color Naming and Word Naming

Trial type and task set	Strength	Control	Priming	Total input	Activation
Word switch					
Color	0.10	0.00	0.30	0.40	0.45
Word	0.50	0.20	0.00	0.70	0.65
Word nonswitch					
Color	0.10	0.00	0.00	0.10	0.14
Word	0.50	0.15	0.30	0.95	0.76
Color switch					
Color	0.10	0.97	0.00	1.07	0.80
Word	0.50	0.00	0.30	0.80	0.70
Color nonswitch					
Color	0.10	0.38	0.30	0.78	0.69
Word	0.50	0.00	0.00	0.50	0.53

Note. The table shows for each task set in each condition three inputs to the task-set activation function—task strength, task priming, and the minimum control parameters required to achieve correct performance as determined in a training phase—and the resulting activation of the color- and word-naming task sets for each trial type. Task-set activation is a negatively accelerated function of task strength, control input, and task priming.

The simulation results address the question raised in the introduction of why strong priming of the color naming task does not result in an extra repetition benefit for this task equal to the extra cost observed for switching to word naming. In our model, the repetition benefit for color naming is offset by a reduced level of control input on nonswitch trials. It follows that these repetition benefits should be more marked in situations in which repetition priming cannot be offset by changes in control input in this way. One such situation is when control input is low on all trials (so that it cannot be reduced on nonswitch trials relative to switch trials). Crucially, this should be the case when between-task interference is reduced, such that much less control input is needed to perform the weaker task. This observation forms the basis for our simulation of our findings concerning Stroop switching with delayed word onset. Our data show that introducing a delay reduced Stroop interference without creating any reverse Stroop effect. The model naturally reproduces this basic finding, as interference depends on the relative rate at which response tendencies are generated. Of interest is the impact of these changes on simulated switch costs, and this is shown in Table 5 and Figure 6 (simulation of 160-ms delay condition). Evidently, the primary effect of delaying word onset is to increase the cost of switching to the color naming task. Indeed, replicating our empirical findings, the simulated switch cost is larger for color naming than word naming when the word attribute is delayed. This is the case even though interference remains clearly asymmetrical.

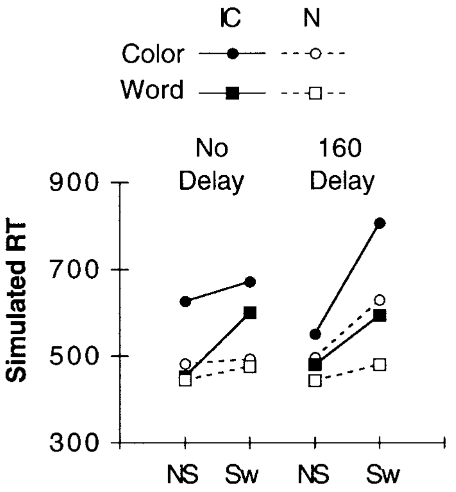

Figure 6. Simulation of the results of Experiment 1, showing simulated mean response times (RTs) on switch (Sw) and nonswitch (NS) trials of each task, separately for neutral (N) and incongruent (IC) trials.

The model behaves in this way because reduced interference from the delayed word stimulus means that a reduced level of control input is required to perform the color naming task: Because word onset is delayed, color naming responses can be generated more quickly than word naming responses even when the color naming task set is less activated than the word naming task set. However, because of low control input to the color naming task, task priming now has a major impact on color naming performance: Performance is poor on color naming switch trials—as priming here favors the word naming task, leaving color naming relatively inactive. Performance is relatively good on nonswitch trials, on which the color naming task benefits from positive priming. In effect, there is a large repetition benefit (rather than switch cost) when the color naming task is performed. The model therefore explains the results of Experiment 1 through the effect of delaying word onset on the control inputs required to perform color naming: As control input is decreased, task priming effects come to have a large effect on color naming performance, resulting in a large repetition benefit (or, equivalently, switch cost) for this task.

Table 5
Results of the Simulation of the 160-ms Delay Condition of Experiment 1

Trial type and task set	Strength	Control	Priming	Total input	Activation
Word switch					
Color	0.10	0.00	0.30	0.40	0.45
Word	0.50	0.15	0.00	0.70	0.60
Word nonswitch					
Color	0.10	0.00	0.00	0.10	0.14
Word	0.50	0.15	0.30	0.95	0.76
Color switch					
Color	0.10	0.15	0.00	0.25	0.31
Word	0.50	0.00	0.30	0.80	0.70
Color nonswitch					
Color	0.10	0.15	0.30	0.55	0.56
Word	0.50	0.00	0.00	0.50	0.53

A similar logic applies to our explanation of the results of Experiments 2 and 3. We model these findings by assuming that response gating can prevent responses outside the currently relevant set from entering the resolution process. For simplicity, let us assume that gating is completely effective. The simulation results are shown in Figure 7. Separating response sets reduces interference (although there remains a small amount of task-level interference, as described above). The consequence of this reduction in interference is the same as in the previous simulation: A lower control input is supplied to the weaker task set. The reduction in control input, in turn, leads to an increased contribution of task priming to the performance of a weaker task on nonswitch trials and, hence, a larger observed cost of switching to this task.

In this way, the model explains why switch costs are reversed when interference between tasks is reduced: Changes in required control inputs alter the way task priming affects performance. Of course, this explanation requires the assumption that participants are able to adjust the control input for the particular trial type anticipated in a close-to-optimal way from trial to trial. This assumption merits examination of circumstances in which participants can be misled or uncertain about the type of trial that is upcoming. This would be the case, for example, if the delay conditions of Experiment 1 were varied within a block of trials. Testing predictions of the model such as this represents an important avenue for future research.

Relation to Existing Theories

We implement task priming effects in terms of a transient increase in the activation of a recently performed task. This differs somewhat from the task-set inertia theory initially put forward by Allport et al. (1994), which places more emphasis on inhibition and negative task priming effects (e.g., Meuter & Allport, 1999). However, our assumption that task-set inertia is positive rather than negative is more consonant with the recent proposal of

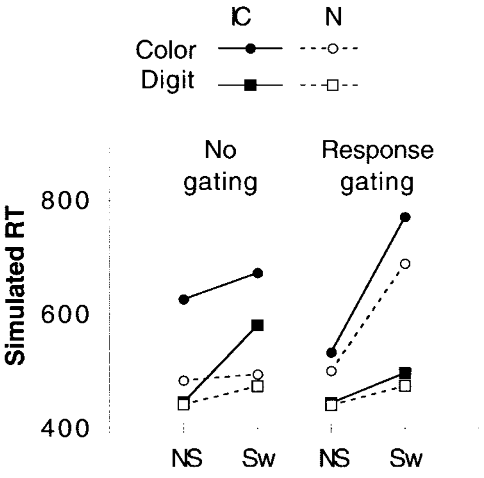

Figure 7. Simulation of the results of Experiment 3, showing simulated mean response times (RTs) on switch (Sw) and nonswitch (NS) trials of each task, separately for neutral (N) and incongruent (IC) trials.

Allport and Wylie (1999, 2000) that performing a task increases the strength of associations between aspects of the task set such as stimulus attributes, response attributes, and representations of task goals. Positive task priming effects have also been incorporated into the formal models of task switching proposed by Meiran (2000), Sohn and Anderson (2001), and Gilbert and Shallice (2002): Meiran (2000) interpreted the task priming effect in terms of strengthening stimulus-response mappings related to a recently performed task, Sohn and Anderson (2001) proposed that retrieval of the cue-task mappings or stimulus-response mappings is faster the more recently a task was performed, and Gilbert and Shallice (2002) suggested that there is carry over of activation of control input across trials. All of these suggestions are broadly compatible with the present claim that there is a transient increase in the activation of a recently performed task. The model of Gilbert and Shallice (2002) is particularly interesting in the present context. Although very different in implementation from the present model, their model likewise predicts that Allport et al.'s (1994) surprising asymmetry of switch costs, with a larger cost of switching to the stronger task of a pair, should only be observed when strong control biases are applied when performing the weaker task.

Our model is able to account for the present findings without including time-consuming control processes (cf. Rogers & Monsell, 1995; Rubinstein et al., 2001) or inhibitory control (cf. Mayr & Keele, 2000), both of which have been proposed to contribute to the switch cost. However, we do not wish to deny the importance of these contributions to task switching performance. Regarding time-consuming control processes, we note that the model presently does not specify how long it takes to set control inputs anew on each trial. It is entirely possible that the time required for this process contributes to the switch cost if there is insufficient time to complete this process prior to the stimulus or if the participant fails to make the adjustment on a proportion of trials (cf. De Jong, 2000). Such a process might correspond to the preparatory executive processes envisioned, for example, by Rogers and Monsell (1995) and Meiran (1996) and in more recent quantitative models of task switching performance (Logan & Gordon, 2001; Meiran, 2000; Rubinstein et al., 2001). On this view, time-consuming reconfiguration and task priming effects make separate contributions to the switch cost, consistent with the suggestions of a number of recent authors (e.g., Allport & Wylie, 2000; Meiran, 2000; Ruthruff et al., 2001; Sohn & Anderson, 2001).

In common with other models of task-set control (e.g., Cohen, Dunbar, & McClelland, 1990; Kimberg & Farah, 1993), we have modeled top-down input as increasing the activation level of the relevant task, with no direct inhibition of the irrelevant task. However, it is likely that a complete model of task switching will need to incorporate inhibitory effects. Mayr and Keele (2000), for example, provided evidence that task switching involves inhibiting the switched-from task. This *backward inhibition* is evident as a slowing of the next performance of the inhibited task relative to a third task. Although backward inhibition appears to contribute to the switch cost, it is unlikely that strategic modulation of backward inhibition can explain the present findings. In general, manipulations that reduce the difficulty of switching to Task A should reduce the need for backward inhibition of the previous task (Task B) and, hence, should also reduce the cost of switching back to Task B. That is, switch costs for the two tasks should covary. However, we found that reducing between-task interference reduces the difficulty of switching to the stronger task of a pair but increases the cost of switching to the weaker task. This finding cannot be explained in terms of backward inhibition. Nonetheless, an important topic for future research is the relationship between backward inhibition and the task priming effects studied here. For example, it would be interesting to determine whether backward inhibition is sensitive to the stimulus- and response-set manipulations studied in the present experiments.

Conclusion

In three experiments, we replicated Allport et al.'s (1994) finding that it is harder to switch from the weaker to the stronger of two tasks than vice versa, confirming the robustness of this finding. However, we found that this asymmetry of switch costs occurs only in limited circumstances: We observed the opposite pattern of results, with larger switch costs for the weaker task, when the stimulus attributes relevant to the two tasks were presented asynchronously and when the tasks used different sets of responses. We interpret these findings in terms of the interaction between task priming effects and top-down control input and implement this idea in a simple formal model. An important implication of the modeling work is that use of the term *switch cost* should not be allowed to obscure the fact that task priming effects benefit performance when a task is repeated as well as disrupting performance when task requirements change. Our simulations suggest that the expression of these positive and negative effects of task priming will be influenced by the strength of top-down control biases applied to ensure that the appropriate task is performed.

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