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Bilingual Language Switching in Naming: Asymmetrical Costs of Language Selection

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In an experimental study of language switching and selection, bilinguals named numerals in either their first or second language unpredictably. Response latencies (RTs) on switch trials (where the response language changed from the previous trial) were slower than on nonswitch trials. As predicted, the language-switching cost was consistently larger when switching to the dominant L_1 from the weaker L_2 than vice versa such that, on switch trials, L_1 responses were slower than in L_2. This "paradoxical" asymmetry in the cost of switching languages is explained in terms of differences in relative strength of the bilingual's two languages and the involuntary persistence of the previous language set across an intended switch of language. Naming in the weaker language, L_2, requires active inhibition or suppression of the stronger competitor language, L_1; the inhibition persists into the following (switch) trial in the form of "negative priming" of the L_1 lexicon as a whole. © 1999 Academic Press

A bilingual or multilingual speaker is able to switch rapidly, at will, from one spoken language to another. While switches of language sometimes occur unintentionally, particularly in moments of emotion or stress (Dornic, 1979, 1980; Grosjean, 1982), fluent bilinguals are generally efficient at language selection and at keeping their languages separate. Thus it is possible to listen to one language while speaking another (Grosjean, 1988). Indeed, skilled simultaneous interpreters temporally overlap speaking one language while listening to another language by up to 75% of the time (Gerver, 1974).

How is the intended language selected, in spoken language production, and what are the behavioral effects of switching from one language to another? Our working assumption is that bilingual language selection depends on processes that are similar in kind to those responsible for the control of task set in other monolingual and/or nonlanguage task domains. This is an assumption shared by other researchers in the field (e.g., Kirsner, Lalor, & Hird, 1992; Macnamara, Krauthammer, & Bolgar, 1968; Paradis, 1980). An alternative view, however, might stress that language is a uniquely specialized human function, with characteristics that separate it from all other cognitive processes. From this perspective, it might be argued, linguistic control mechanisms—including, specifically, the fundamental selection of which language to speak—might be supposed to be likewise *sui generis;* that is, to show properties different from those observable in shifting between other task domains. The present article proposes a means of discriminating, experimentally, between these competing views.

The experiment to be described here offers a rather demanding test of our assumption, viz. that bilingual language selection depends on the same principles of control as other nonlinguistic tasks. It does so by means of a simple but apparently counterintuitive prediction about the behavioral costs of language switching. The

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prediction is this. When switching between a stronger first language (L_1) and a relatively weaker second language (L_2), the cost of switching languages should be larger for a switch to L_1 than for a switch to L_2.

This prediction is based on our interpretation of task-switching costs observed in other task domains, none of which involve language switching (Allport, Styles, & Hsieh, 1994; Allport & Wylie, in press). In the following section, we explain how this somewhat counterintuitive prediction about language switching is motivated. Before doing so, however, there is a second, complementary objective of this experiment that should be pointed out. This is to establish whether certain behavioral properties of task-switching costs, studied hitherto in conventional, laboratory, reaction time (RT) tasks, can be extrapolated to the special case of bilingual language switching.

To date, studies of task switching have characteristically used tasks with more or less arbitrary stimulus–response (S-R) mappings, often with two-alternative key-press responses; more importantly, they have used combinations of tasks that participants normally have no experience of switching between before they enter the laboratory. In contrast, bilinguals who operate in two (or more) language contexts typically have extensive practice at switching, rapidly, from speaking in one language to speaking in the other. It seems important to establish whether the effects found in switching between relatively arbitrary pairs of RT tasks also apply to language switching in skilled bilinguals.

We now set out the background to our prediction. Early studies of task switching in simple speeded tasks were reported by Jersild (1927) and replicated by Spector and Biederman (1976). More recently, task switching among a variety of (nonlinguistic or monolingual) tasks has been studied in greater detail (e.g., Allport et al., 1994; Allport & Wylie, in press; Hsieh & Allport, 1994; De Jong, 1995; Los, 1996; Meiran, 1996; Rogers & Monsell, 1995; Styles & Allport, 1990). An excellent review is provided by Monsell (1996).

Allport et al. (1994) found RT costs associated with a wide range of shifts of task set, including when the shift of task could in principle be fully anticipated—for example, in regular alternation of two different S-R tasks—*provided* that the task stimuli themselves were not sufficient to specify uniquely which of the two (or more) tasks was to be performed (Allport et al., 1994, Experiments 3 and 4; Spector & Biederman, 1976). (The latter proviso will be assumed in all of the experiments discussed here.) Furthermore, they found that an unfilled time interval of as much as 1 or 2 s preceding the next RT trial, even with a fully predictable shift of task, was not sufficient to eliminate the RT shift cost on the next trial. This was so, even though such an interval could be an order of magnitude greater than the shift cost itself (~200 ms). Allport et al. inferred from this result that the RT shift cost *as a whole* could not simply reflect the time taken to execute an autonomous or anticipatory “shift of set” in advance of the next trial. Instead, they proposed that the persisting RT shift cost resulted in this case from a failure to effectively shift set prior to the arrival of the new task stimulus. They proposed that the task set, implemented for the preceding trial(s), persisted involuntarily into the processing of the next (switch) trial even though the participant knew that the next trial required a shift of “set” and was instructed to do her best to prepare for it. That is, under these conditions, “*active disengagement* from the task set of preceding trials . . . must wait until triggered by the imperative stimulus for the following trial” (Allport et al., 1994, p. 441, italics added). In consequence, the switch trial shows increased interference, or response conflict, between the current and the preceding task. This is demonstrated, for example, by greatly enhanced Stroop interference effects during switching.

In a detailed investigation of task preparation and shift costs, Rogers and Monsell (1995), and subsequently Meiran (1996), confirmed that a major component of the task-switching costs persisted, similarly, over preparatory intervals of 1 or more s. In most conditions, the shift costs showed a consistent, although *only partial* reduction with increasing preparation interval. They interpreted this as showing an additional component of the shift cost that was subject to anticipatory or "endogenous" control. We believe this interpretation to be correct. On the other hand, they found no reduction in between-task interference (incongruity) effects as a function of increasing preparation time, suggesting little, if any, effective *dis*engagement from the prior task during the preparation interval. Rogers and Monsell (1995) also noted that, in their elegant "alternating runs" paradigm, switching costs in RT were present only on the switch trial itself and not on the following nonswitch trials, consistent with a process of active disengagement during the course of the switch trial. Where switching costs are estimated in relation to uniform tasks, however, there is evidence that switching costs can persist over considerably more than one trial (e.g., Allport et al., 1994, Experiment 4; Allport & Wylie, in press, Experiments 2 and 3). Moreover, in rapid visual monitoring tasks, in which the participant does not respond overtly during the stimulus sequence, the data show a gradual recovery of performance over some five to seven successive stimuli after a shift of task has been cued (Hsieh & Allport, 1994; Hsieh, 1995).

Consistent with all of these results from task-switching studies is the idea of involuntary persistence of components of the preceding ("pre-switch") task set into the processing of the stimulus for the "switch trial" itself, which we refer to as task set inertia. It follows that the major determinant of the persisting switching costs should be the characteristics of the task set for the *preceding* trial(s) rather than those of the task to which the switch is to be made. This has some generally counterintuitive consequences for switching costs between pairs of tasks in which one task is the behaviorally dominant or stronger member of the pair. For example, when participants switched between two versions of the classic Stroop color–word task (Macleod, 1991) (that is, between naming the color and reading the word of incongruent Stroop stimuli) Allport et al. (1994) found asymmetric switching costs: much larger switching costs in switching to the dominant word-reading task than to the weaker task of color naming. This finding was replicated by Allport and Wylie (in press). The explanation, in terms of the Task Set Inertia hypothesis, is straightforward. To enable the task set for the weaker, color-naming task to be performed, the competing (normally dominant) word-reading task must be *actively suppressed* and (perhaps) the color-naming task additionally activated. Suppose that a shift of task is now required from color naming to word reading. The Task Set Inertia hypothesis predicts that the task set from the preceding trial(s) will persist; that is, active suppression of the word-reading task (and facilitation of color naming) should still be present at the start of a word-reading trial on a switch from color naming. Hence ("paradoxically") a large switching cost on a shift to the easy task of word reading. The word-reading task, on the other hand, does *not* require any comparable suppression of color naming in order for it to be performed. Task set inertia from the word-reading task thus will have a much smaller impact on a switch to color naming. Hence a much smaller (or even negligible) shift cost on a switch to color naming, as is in fact observed (Allport et al., 1994, Experiment 5; Allport & Wylie, in press).

Similar, "paradoxical," asymmetries have been reported in switching costs with other tasks. Harvey (1984, Experiment 5) used a spatial version of the Stroop task and found asymmetrical switching costs, with the dominant task showing the larger costs. De Jong (1995) used spatially compatible and incompatible tasks and found larger shift costs for the compatible task. Yeung (1997), working in our laboratory, found that differential practice at task A had the ("paradoxical") effect of greatly *increasing* the shift costs on task A and reducing them on the competing, unpracticed task B.

The motivation for our experimental prediction about language switching should now be clear. To repeat, we predict that in switching between a dominant first language (L_1) and a relatively weaker second language (L_2) we should find asymmetric switching costs: a ("paradoxically") larger cost of switching from L_2 to L_1 than from L_1 to L_2. The reason is as follows: L_1 is, by definition, acquired first and, in the case that we consider here, receives by far the greater amount of practice throughout life. Accordingly, we suggest, the weaker L_2 can win the competition with L_1 for the control of spoken production, only if L_1 is suppressed. In contrast, speech production in L_1 should normally require relatively little, if any, active inhibition of L_2. Similar proposals have been made by a number of other students of bilingualism (De Bot, 1992; De Bot & Schreuder, 1993; Green, 1986, 1993, 1998; Paradis, 1981) to the effect that the selection of a particular language entails the active suppression or inhibition of the other, potentially competing, languages(s). In bilingual word recognition it is further suggested that the stronger language (L_1) would normally exert stronger inhibition on the "language node" for L_2 than vice versa (Dijkstra & Van Heuven, 1998; Grainger & Dijkstra, 1992; Grainger, 1993).

According to the Task Set Inertia hypothesis, therefore, on an intended switch of language from L_2 to L_1, the suppression of L_1 should persist into the initial processing of the L_1 speech act, thus delaying the production of an L_1 response. For a language switch in the other direction, however, task set inertia following one or more L_1 responses may include the persisting, positive activation of L_1, but little, if any, suppression of L_2. The degree of the predicted asymmetry in language switching costs should, of course, depend on the (complementary) asymmetry in the relative strength of the speaker's two languages. (Their "absolute" level of proficiency is unimportant; what matters, for our prediction, is their *relative* strength.) We therefore refer to this prediction as the Relative Strength hypothesis. The more nearly equal in strength the two languages, the smaller the expected (reverse) asymmetry in the switching costs.

To our knowledge, no suitable data exist to enable us to test this prediction. To date, much of the research on bilingual language switching has involved the analysis of conversational speech recorded from bilingual speakers and has focused on the points in an utterance at which unintentional or "spontaneous" shifts from one language to the other occur (e.g. Berk-Seligson, 1986; Clyne, 1980, 1987; Pfaff, 1979; Poplack, 1980; Sridhar & Sridhar, 1980; Timm, 1983). The first experimental study of intentional (or experimenter-cued) switching of spoken language, in which the time costs of switching were studied, was reported by Kolers (1966, 1968). From the total reading times by bilinguals of monolingual and bilingually mixed passages, Kolers estimated the mean time-cost for a switch of spoken language when reading aloud at between 0.3 and 0.5 s. However, this measure does not distinguish between the direction of switch from L_1 to L_2 versus from L_2 to L_1. More recent studies of language switching have focused almost exclusively on the effects of a shift in the language of input; for example, on comprehension time for mixed language sentences (e.g. Chan, Chau, & Hoosain, 1983; Macnamara & Kushnir, 1971; Neufeld, 1976) and in mixed-language lexical decision (e.g. Grainger & Beauvillain, 1987; Kirsner, Smith, Lockhart, King, & Jain, 1984; Meyer & Ruddy, 1974; Von Studnitz & Green, 1997).

Comparatively little experimental research has been directed to intentional language switching in spoken production. Dalrymple-Alford (1985) asked bilinguals to read aloud mixed-language lists of unrelated words with varying numbers of predictable language switches per list. However, here, as in Kolers' (1966) study, the written word provides a language-specific, nonsemantic specification of the spoken response (e.g. Besner & McCann, 1987; Coltheart, 1985). That is, lexical retrieval of the spoken word form *in the appropriate language* is already directly specified by the written stimulus. Thus, aside from the selection of competing grapheme–phoneme correspondences, the task of discrete oral word reading minimizes the element of *selection* between competing languages in the bilingual lexicon.

We are aware of only one experimental study of language switching in oral naming, using language-neutral stimuli. Macnamara et al. (1968) presented Arabic numerals one at a time for naming, cueing the language of response with a circle or triangle beside each numeral. Language switching occurred either predictably (with regular alternation) or unpredictably between the two languages and was compared to monolingual naming sequences. "Switching times" were estimated simply by subtracting total list completion times for monolingual lists from those for the language-switching conditions. However, this method (like Kolers') fails to distinguish the direction of switch, from L_1 to L_2 versus from L_2 to L_1. Macnamara et al.'s pioneering study nevertheless provides a simple model for the experiment to be reported here.

In the present experiment, bilinguals were instructed to name Arabic numerals (i.e., language-neutral stimuli) in each of two languages. Surprisingly, this appears to be the first reported study of language switching in oral naming to make use of discrete RTs. In this simple naming task, bilinguals (with a dominant L_1) were asked to switch the language of production, according to the color of the background on which each numeral was displayed.

The primary purpose of the experiment was to test the "paradoxical" prediction from the Relative Strength hypothesis: Is the switching cost larger for a switch of language to the stronger L_1 than for a switch to L_2?

In discrete naming tasks, L_1 responses are typically faster and more accurate than L_2 responses. It seems intuitively plausible to suppose that this superiority of L_1 should be, if anything, even more marked when switching from one language to another. For both psychologists and nonpsychologists whom we have asked, this intuition appears to be more or less universal. Macnamara (1967) provides an example of this assumption, suggesting that it should be relatively easy to suppress the response in a weaker language (L_2), and thus easier to switch to the dominant L_1 than to switch to L_2. A third possibility to consider is that there is simply a constant time cost of switching between languages A and B, regardless of relative proficiency, and of the direction of the switch, A to B versus B to A, as earlier studies by Kolers and others would appear to assume.

There were two further questions that we hoped to explore in this experiment. These concern possible *sequential* effects of language selection over successive naming trials. Two empirical questions will be examined. First, following a successful switch from language A to language B, how rapidly does the efficiency of lexical selection for language B recover to asymptote? In their study of task switching using two-alternative, key-press responses, Rogers and Monsell (1995) reported switching costs on the first postswitch trial *only*. Is this the case also in a bilingual naming task? Second, does a series of naming trials, all in the same language (language A), lead to an increasing facilitation of language A and a consequent progressive reduction in naming RTs (a "grooved in" effect)? Complementarily, does the cost of a switch to language B vary as a function of the number of preceding trials consistently in language A?

METHOD

Participants

Sixteen bilingual participants volunteered to take part in the experiment, 11 women and 5 men. Their ages ranged from 23 to 44 years. All of the participants spoke English either as a first (L_1) or a second language (L_2), and all participants judged themselves to be reasonably proficient in their second language. The native English speakers had all studied their second language at university level and had spent at least 1 year in the relevant country. Those who spoke English as their second language had all been resident in Britain for at least 6 months at the time of testing and were occupied in full-time study or research in which English was the working language. All participants reported frequent, intentional switches of spoken language as an everyday occurrence. The languages spoken by our bilingual participants were deliberately selected from a range of European languages in order to limit possible systematic effects of cognate number names in particular language pairs (e.g., French, 6 = "six"). These languages were French ($n = 2$), German ($n = 4$), Italian ($n = 1$), Portuguese ($n = 5$), and Spanish ($n = 4$).

A self-paced numeral-naming task was used to estimate relative name retrieval efficiency in L_1 versus L_2. Participants were asked to read aloud, as fast as possible, lists of 20 numerals in each of their two languages. This task was performed twice. Over the group as a whole, mean reading time in L_2 was 20% slower than in L_1.

Materials

The stimuli were single digits ranging from 1 to 9. They were presented one at a time in random order, in short sequences (“lists”) ranging in length unpredictably from 5 to 14 items. A total of 200 such lists was presented, giving approximately 2000 trials per participant. The numerals were presented singly on a VGA color monitor interfaced with an IBM-PC compatible computer. They were 9 mm high and appeared in the center of either a blue or a yellow rectangle, measuring 11 × 4 cm. The color of the rectangle told the participant in which language to name the numeral, depending on instructions. For example, an English–French participant might be instructed that “blue” indicated “respond in English” and “yellow” indicated “respond in French.” The assignment of color cue to response language was counterbalanced across participants. A change in color of the rectangle from that of the preceding trial thus cued a switch in the language of response. The lists were designed such that on any trial there was .3 probability of a switch [$p(\text{switch}) = .3$; $p(\text{nonswitch}) = .7$]. The possible number of language switches within a list ranged from 0 to 4. A switch in the language of response could be either (1) from the weaker L_2 to the stronger L_1 (L_2 to L_1) or (2) from the stronger L_1 to the weaker L_2 (L_1 to L_2). Nonswitch trials were either in L_1 or L_2. Within each list the numerals were generated randomly for each participant, with the constraint that the same numeral could not be repeated immediately in a sequence. Each list consisted of a unique sequence of nonswitch and switch trials, and these within-list sequences were consistent across participants.

The display of the numerals was response driven, through the activation of a voice key. The next stimulus appeared on the screen 400 ms after the voice key was triggered. For the first item in a list the background rectangle was displayed 2 s before stimulus onset. For all other stimuli, presentation of numeral and background was simultaneous.

Procedure

Participants were tested individually. They were seated at approximately 40 cm distance from the computer monitor with the voice key placed directly in front of them. Participants were told that single numerals would appear on the computer screen against a color background, in lists of varying length, and that the end of each list would be signaled by the appearance of an asterisk accompanied by a short beep. They were asked to name the numerals quickly and accurately in the appropriate language, as indicated by the color, taking care to speak sharply and clearly. Their responses were recorded by a voice key.

The experiment began with 12 practice lists, similar to those in the main experiment. Participants repeated the practice run up to three times, if necessary, to ensure consistent use of the voice key (100% accurate triggering). During the experimental session, each participant was presented with all 200 lists of numerals. The order of list presentation was randomized across participants. After every 20th list the participant was allowed a short rest.

Response latencies (RTs) were recorded by the computer and measured from stimulus onset to the triggering of the response. Tape-recorded vocal responses were checked later for errors. RTs corresponding to incorrect responses were excluded from subsequent analyses as were correct responses on trials immediately following an error.

Design and Analysis

The experimental design included three main factors: (1) Response Language (L_1 or L_2); (2) Run Length: the number of consecutive naming responses consistently in one language (i.e., “same language” responses) immediately preceding each switch or nonswitch trial (range = [1, 13]); and (3) Ordinal Position: for switch trials, the first, second, or third occurrence of a switch of response language in a list, and for nonswitch trials, those occurring (a) before the occurrence of the first switch, (b) after the first

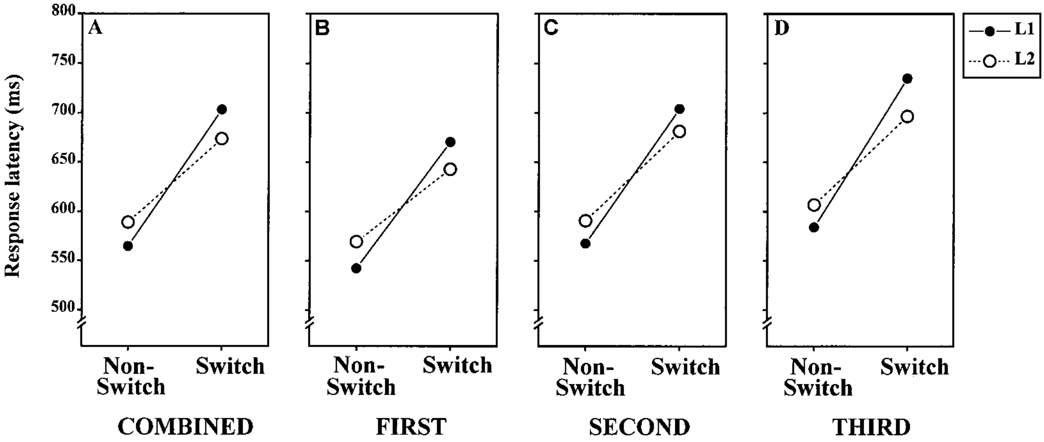

FIG. 1. (A) The overall mean RT (in milliseconds) on nonswitch and switch trials. (B, C, and D) These data are shown separately for the first, second, and third switch, respectively, in a list. In B, C, and D data for the nonswitch trials show the nonswitch RT immediately preceding the corresponding switch trial.

switch but before the second switch, and (c) after the second switch within a list. Median RTs were calculated and subjected to analyses of variance (ANOVAs). Because the full set of items was used, analyses are reported by subjects only.

RESULTS

Figure 1A shows the overall response latencies obtained when responding in L_1 and L_2 on both switch and nonswitch trials. A striking pattern can be seen, which is repeated at each of the three ordinal switch positions in a list (Figs. 1B, 1C, and 1D). This data pattern has three main features. First, for each language, switch trials are slower than nonswitch trials. Second, on nonswitch trials mean RT is faster in L_1 than in L_2; in contrast, on switch trials, mean RT is consistently slower in L_1 than in L_2. Third, RTs on both switch and nonswitch trials tend to increase with each successive switch within a list. These and other features of the data are analyzed below for the nonswitch and switch trials and in terms of the respective costs of switching between languages.

Nonswitch Trials

Nonswitch RTs were categorized in three ways. First, by language of response and second, by the number of preceding "same language" responses (Run Length). Because sample size decreases with increasing run length, this latter variable was grouped as follows: RTs preceded (a) by not more than one same language response, (b) by two to three consecutive same language responses, and (c) by four or more consecutive same language responses. Third, the data were categorized by ordinal position in the list: (a) prior to the first switch of language, (b) after the first switch, and (c) after the second switch. Figure 2 shows the nonswitch RTs grouped by ordinal position in this way (Figs. 2A, 2B, and 2C) and suggests somewhat different patterns of nonswitch RTs depending upon their occurrence relative to a switch.

A Response Language × Ordinal Position × Run Length repeated-measures analysis of variance revealed a significant and consistent advantage for L_1 [$F(1,15) = 4.98$, $MS_e = 8568.9$, $p < 0.05$]. Mean naming responses were 24 ms faster in L_1 than in L_2. The main effect of Run Length was not significant ($F < 1$) but a significant Ordinal Position main effect was observed [$F(2,30) = 49.30$, $MS_e = 775.46$, $p < 0.0001$]. Moreover, the interaction of Ordinal Position × Run Length was also significant [$F(4,60) = 14.57$, $MS_e = 475.07$, $p < 0.0001$]. There were no interactions with Response Lan-

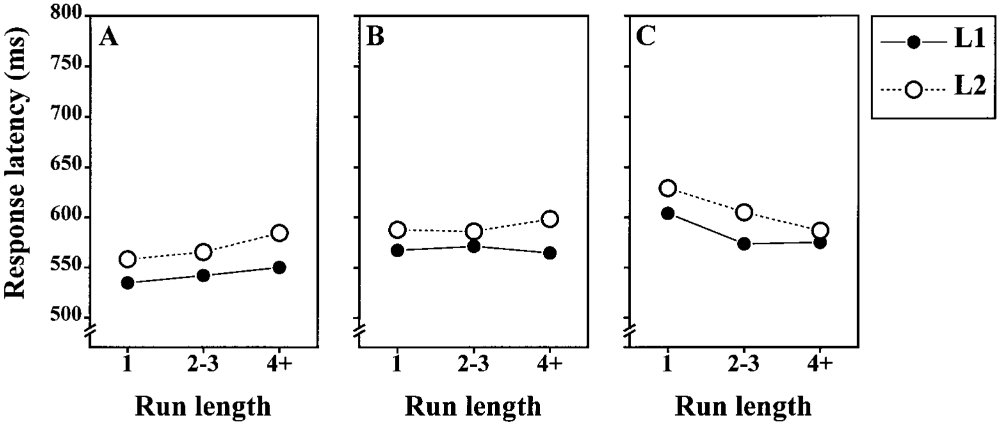

FIG. 2. Mean RT (in milliseconds) on nonswitch trials for L_1 and L_2 naming responses as a function of preceding run length. (Run length is the number of consecutive responses in the *same* language, without a switch, immediately preceding a given nonswitch trial. Run length is divided into: no more than 1, 2–3, and 4 or more same-language responses.) (A) Nonswitch RTs on trials prior to any language switch in a list; (B) trials following the first switch; (C) trials after a second language switch.

guage (Fs < 1) and there was no three-way interaction [$F(4,60) = 1.65$, $MS_e = 537.21$, ns].

A simple effects analysis of the Ordinal Position × Run Length interaction showed the following. (1) For nonswitch trials prior to any language switch (see Fig. 2A), there was a significant simple main effect of Response Language [$F(1,15) = 4.69$, $MS_e = 3741.75$, $p < .05$] and of Run Length [$F(2,30) = 10.12$, $MS_e = 353.58$, $p < .005$]. That is, nonswitch RTs unexpectedly *increased* (slightly but consistently) with increasing number of consecutive preceding "same language" responses. The two factors did not interact ($F < 1$). (2) For nonswitch trials occurring after the first switch but before the second (see Fig. 2B) there was again a significant simple main effect of Response Language [$F(1,15) = 6.3$, $MS_e = 2047.70$, $p < .025$]. However, no effect of Run Length was observed ($F < 1$) and there was no interaction [$F(2,30) = 2.27$, $MS_e = 335.38$, ns]. (3) For nonswitch trials occurring after the second switch the effect of Response Language failed to reach significance [$F(1,15) = 2.86$, $MS_e = 4368.82$, ns]. There was, however, a significant effect of Run Length [$F(2,30) = 7.39$, $MS_e = 1497.56$, $p < .005$]. As is clear from Fig. 2C, naming latency on the trial immediately following a second language switch (Run Length = 1) was slower than those with longer run lengths (2–3, 4+). No interaction was observed [$F(2,30) = 1.08$, $MS_e = 746.11$, ns].

Switch Trials

Figure 3 shows switch trial RTs for L_1 and L_2 responses as a function of preceding run length (i.e., the number of consecutive preceding nonswitch responses in the *other* language immediately preceding the switch). Switch RTs appear completely unaffected by this variable. As already noted, switch RTs from L_2 to L_1 are consistently slower than switches from L_1 to L_2.

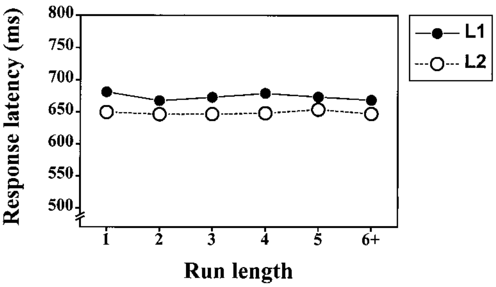

FIG. 3. The figure shows RTs (in milliseconds) on switch trials for L_1 and L_2 responses as a function of preceding run length. (Run length is here the number of consecutive, preceding, nonswitch responses in the *other* language.) The data shown are for the first language switch in each sequence of trials.

These observations were confirmed by a Response Language × Run Length repeated-measures ANOVA. For the switch trials, Run Length was analyzed over a range of 1 to 6+ consecutive preceding responses in the other language. The only significant effect was that for Response Language [$F(1,15) = 15.02$, $MS_e = 2002.34$, $p < 0.0025$]. A switch from the weaker L_2 into the dominant L_1 showed consistently slower RTs than a switch from L_1 to L_2 (a mean difference of 28 ms). There was no effect of Run Length and no interaction between the two factors (Fs < 1).

Figure 1 shows that mean RTs on language switch trials (as on nonswitch trials) apparently increased with each successive switch in a list. This was tested using a Response Language × Ordinal Position × Run Length repeated-measures ANOVA. For this analysis, preceding Run Length was divided into two categories, three or fewer and four or more immediately preceding responses in the other language. There were significant main effects of Response Language [$F(1,15) = 13.76$, $MS_e = 2477.49$, $p < 0.0025$] and of Ordinal Position [$F(2,30) = 8.19$, $MS_e = 7490.71$, $p < 0.0025$]. Mean RTs on the first, second, and third language switch within a list were 657, 693, and 716 ms, respectively. Again, there was no effect of Run Length ($F < 1$). No interactions were found.

Language-Switching Cost

The RT language-switching cost is defined as the difference between switch and nonswitch RTs. Given that nonswitch RTs increased significantly with ordinal position in a list, the language-switching cost was calculated by taking as a baseline the set of nonswitch responses immediately preceding each switch. (That is, for the first language switch, the nonswitch trials occurring before any switch of language were taken as the baseline; for the second switch, the baseline consisted of the nonswitch trials occurring after the first switch but before the second switch; for the third switch, the baseline was the nonswitch trials occurring after the second switch but before the third.) Median switch and nonswitch RTs were computed for each participant, separately at each ordinal position, for both L_1 and L_2. Language-switching costs in each case were derived by subtracting the median nonswitch RTs from the median switch RTs for each participant. The results are summarized in Table 1 (see also Fig. 1).

TABLE 1

Mean Language-Switching Costs (in milliseconds) for Switches from L_2 to L_1 and from L_1 to L_2 Presented Separately for Each Ordinal Position in a List

	Language-switching cost	
Ordinal position	L_2 to L_1	L_1 to L_2
First switch	140	74
Second switch	137	91
Third switch	151	90
Mean	143	85

The language-switching costs were subjected to a Response Language × Ordinal Position repeated-measures ANOVA. A significant main effect of Response Language was found [$F(1,15) = 19.29$, $MS_e = 4132.52$, $p < .001$], confirming that language-switching costs were consistently greater when switching from the weaker L_2 into the stronger L_1. However, there was no main effect of Ordinal Position and there was no interaction (Fs < 1). The analysis confirms that the size of the language-switching cost depends only on the direction of the switch (L_1 to L_2 versus L_2 to L_1) and remains essentially constant over successive switches within a list.

Our principal prediction, the Relative Strength hypothesis, is thus strongly confirmed. As predicted by the Task Set Inertia model of task switching, the cost of switching language to the relatively stronger L_1 is greater than the cost of switching in the opposite direction, to L_2.

Further Tests of the Relative Strength Hypothesis

Practice effects. The data permit two further tests of this hypothesis. The first of these considers the differential effects of practice on numeral naming in L_1 versus L_2, within the course

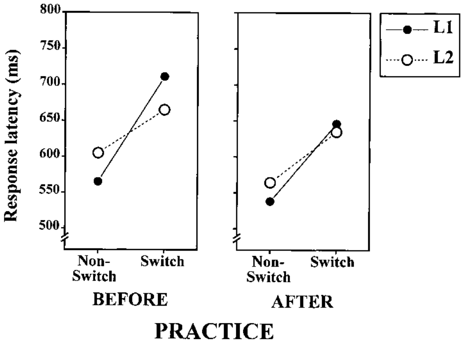

FIG. 4. The figure shows the effect of practice on the language-switching costs over some 2000 naming responses. Mean RTs (milliseconds) for the first language switch and for the immediately preceding nonswitch trial are shown. (A) The results early in practice, on the first 15 correct trials of the experiment; (B) results from the last 15 correct trials.

of the experiment. The participants all had a lifetime of practice at this simple task in L_1, but in many cases only a year or so of intensive use of L_2. During the course of the experiment, each participant performed some 2000 trials of speeded numeral naming, 1000 trials in each of L_1 and L_2. It seems plausible that this amount of practice should have benefited the weaker L_2 more than it should L_1. (Notoriously, bilinguals often continue to perform numerical tasks in L_1, even when operating otherwise principally in L_2.) Accordingly, the relative strength of L_1 and L_2 for numeral naming should differ less by the end of the experiment than they did at the start. We would therefore predict that the asymmetry in the language-switching costs should likewise be less at the end of the experiment than at the beginning.

In order to test this prediction, the first 15 correct trials and the last 15 correct trials in each condition (over the 200 numeral lists) were selected for further analysis (the Practice factor), using RTs for the first language switch and for the immediately preceding nonswitch trial in a list as the critical data (see Fig. 4). Evidently, on nonswitch trials, both L_1 and L_2 benefited from the intervening practice, L_2 RTs showing, as expected, a proportionally larger practice effect than L_1 (41 ms versus 27 ms for L_1). Conversely, on the switch trials, again as predicted, it is the L_1 RTs that show the largest reduction with practice (65 ms) compared to L_2 (30 ms). A repeated-measures ANOVA on the language-switching costs showed a significant main effect of Response Language [$F(1,15) = 12.43$, $MS_e = 4911.50$, $p < .005$], but, on account of the crossover effect just described, no Practice main effect was observed [$F(1,15) = 1.78$, $MS_e = 1665.57$, ns]. However, the predicted Response Language × Practice interaction was just short of significance [$F(1,15) = 4.17$, $MS_e = 2202.35$, $p = .059$]. Post hoc Newman–Keuls analyses showed significant reduction with practice in the language-switching cost for L_1 but not for L_2. Over the first 15 trials the language-switching cost was larger for L_1 than L_2 by 86 ms ($\alpha = .01$); by the last 15 trials this difference was reduced to 38 ms, though this remaining asymmetry in the switching costs was still significant ($\alpha = .05$).

Relative proficiency. As a further test of the Relative Strength hypothesis, participants were divided into two groups, according to their relative proficiency in the two languages. For this purpose, we used the simplest but also the most directly relevant index of relative language proficiency within our experimental task, namely the speed of numeral naming in L_1 versus L_2. For each participant the mean of median naming RTs, in L_1 and L_2, for all nonswitch trials prior to the first language switch in a list were computed. Based on the difference between these two values (mean L_1 minus L_2 nonswitch RT), two equal-sized groups resulted: Group A ($n = 8$), with a mean L_1-L_2 difference in RT of 90 ms ($SD = 58$), and group B ($n = 8$) with a much smaller mean difference ($M = 15$ ms, $SD = 13$).

The data for these two groups are shown in Fig. 5 (Figs. 5A and 5B, respectively) and suggest a reduction in the switching cost asymmetry for Group B, where the difference in relative proficiency was comparatively small. An ANOVA with Relative Proficiency as the between-subjects factor and Trial Type (nonswitch versus switch) and Response Language as the within-subjects factors, however, failed to show a reliable three-way interaction ($F < 1$). Simple effects analyses were carried out nonetheless to evaluate the Trial Type × Response

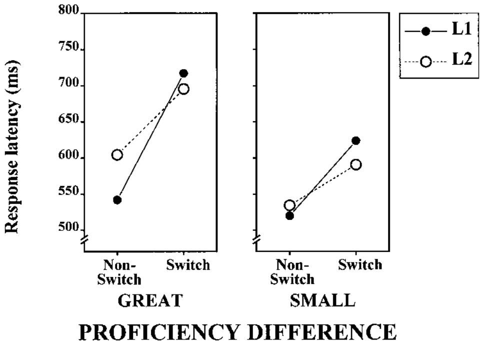

FIG. 5. The effect of relative proficiency in L_1 versus L_2 on the language-switching cost. (A) The data (in milliseconds) for participants with a larger difference in relative L_1/L_2 proficiency. (B) The data for the participants with little difference in relative proficiency between the two languages.

Language separately for each group. Group A, with the larger difference in language proficiency between L_1 and L_2, showed the expected main effects of nonswitch versus switch [$F(1,14) = 28.37$, $MS_e = 5004.51$, $p < .0005$] and of Response Language [$F(1,14) = 5.03$, $MS_e = 658.59$, $p < .05$]. In addition, there was a significant Trial Type × Response Language interaction [$F(1,14) = 6.64$, $MS_e = 2134.96$, $p < .025$]. In contrast, for the more balanced bilingual group, group B, the only significant effect was for nonswitch versus switch [$F(1,14) = 10.23$, $MS_e = 5004.51$, $p < .01$]. There was no main effect of Response Language in this group [$F(1,14) = 1.08$, $MS_e = 658.59$, ns] and, more importantly, no significant interaction of Trial Type (switch vs nonswitch) with Response Language [$F(1,14) = 2.12$, $MS_e = 2134.96$, ns].

Errors

Very few errors were made. The percentage of errors in the four main categories of response were as follows: nonswitch $L_1 = 0.6\%$; nonswitch $L_2 = 0.5\%$; switch L_2 to $L_1 = 0.6\%$; switch L_1 to $L_2 = 0.3\%$. These were too few to enable meaningful analysis, but it should be noted that twice as many errors occurred when switching from L_2 to L_1 than vice versa. It is also worth noting that the errors included a number of phonological blends between the two languages. The exclusion of correct responses, on trials immediately following an error, resulted in the loss of 1.1% of correct responses. Incorrect triggering of the voice key resulted in a further loss of 0.9% of responses.

DISCUSSION

As predicted, the language-switching cost was larger in switching from the weaker L_2 to the stronger L_1 (mean switching cost = 143 ms) than in switching from L_1 to L_2 (mean switching cost = 85 ms). This asymmetric pattern was also remarkably consistent. It was further strengthened by analyses of differential practice effects in L_1 versus L_2 and of individual differences in the relative proficiency of L_1 and L_2. The asymmetry in the size of the language switching cost resulted in a crossover effect, on switch trials, such that numeral naming in L_1 was actually slower than it was in L_2 (see Figs. 1 and 3). We are not aware of any previously reported RT data in which a *more* highly practiced RT task (or a task with earlier age of acquisition) shows a slower RT than a corresponding, but *less* highly practiced task.

We believe this result—which appears to run counter to common sense intuition on the subject—provides substantial support for the Task Set Inertia interpretation of task-switching costs, as a form of “negative priming” of task set. The “negative priming” arises from the active inhibition of one of two mutually competing tasks (or languages), A, which then persists involuntarily into the processing of the next task (or language), B. Thus, for language production in a weaker L_2, active suppression of the competitor language (L_1) is needed, as well as potentiation of L_2. On a subsequent switch trial, this L_2 language set thus generates powerful interference with the intended L_1 response. For production in L_1, in contrast, little suppression of any competitor language(s) may be needed. Hence the reverse asymmetry in the language-switching costs.

On the contrary, if the language-switching costs are taken to represent the time needed for a control operation to prepare or engage the *new* intended language set, the asymmetric effect appears paradoxical. If this were the origin of the language-switching costs, why should the language set for the better learned, “stronger,” L_1 be more difficult and take longer to engage than the weaker L_2? On the other hand, if the switching cost reflects primarily the difficulty of *disengagement* from the preceding language set, as the Task Set Inertia model assumes, this pattern of results is exactly what should be expected.

As proposed in the Introduction, the predicted pattern of language-switching costs lends support also to the assumption that bilingual language switching reflects processes that are fundamentally similar to task switching in other domains. Furthermore, even very extensive, extraexperimental practice at switching between their two languages, which our participants undoubtedly had, did not eliminate the switching cost.[1]

A clear-cut prediction from the Task Set Inertia account of the switching costs is that the (reverse) asymmetry should diminish, as the relative proficiency of the two languages approaches equality. Post hoc analyses of the current data supported this prediction. A more demanding test would be provided by truly balanced bilinguals. In a study of bilinguals specially selected for their matched performance in both languages, Meuter (1994) found *identical* costs of switching to either language in a category naming task.

An alternative possible explanation for the asymmetry of the language-switching costs was put forward by an anonymous referee. This is that a difficult task on trial N may carry over to produce a slow response also on the next trial. Thus, the relatively difficult task of naming in L_2 would be followed by a further slow response on a switch to L_1. (Let us refer to this as the “carry over” hypothesis.) There are two rather straightforward reasons why this attractively simple account can be ruled out. The first is this, and is, we believe, conclusive. The merit of the “carry over” hypothesis is its simple generality. If the “carry over” hypothesis were correct, it would, of course, have to apply to all cases of task switching. That is, the same asymmetry of switching costs should be observed between *all* pairs of competing tasks that differ in RT. However, this prediction is strongly disconfirmed by the available evidence. By no means all manipulations of task “difficulty” (and hence RT) result in reverse asymmetry in switching costs. For example, Azuma and Monsell (1998) reported a series of task-switching studies with pairs of tasks, each pair consisting of one “easy” task and one more “difficult” task, by manipulating S-R compatibility. They found no asymmetry of switching costs. Similarly, to give another example, we manipulated the intrinsic difficulty of a number comparison task by varying the symbolic distance between the numbers. This manipulation had a substantial effect on RT, but left the shift cost unchanged and symmetrical for both “easy” and “difficult” comparisons (Allport et al., 1994, Experiment 2). To our knowledge, only mutually competing tasks (or languages) in which the stronger task (or language) must be actively suppressed to enable the relatively weaker competitor to be performed result in (reverse) asymmetry in the switching costs. There is indeed a “carry over” effect, in switching set, but it concerns specifically the carry over of suppression of the previously competing language or task. This issue is addressed at length, with extensive further evidence, in Allport and Wylie (in press).

Second, we are not aware of any empirical data, even within uniform tasks, adequate to motivate the “carry over” hypothesis as a potential explanation for our findings. Clearly the RT carry over effect would have to be massive to account for the very large asymmetry in the data reported here. In general, autocorrelation on sequential RT data does not reveal gross patterns of speeding or slowing following “easy” versus “difficult” responses, although small fluctuations in speed–accuracy criterion, affecting errors, have been reported (e.g. Fearnley, 1978).

The effect of the number of preceding, consecutive responses in language A (or task A) on the cost of switching to a competing language

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[1] This latter point, however, should be qualified by noting that switching between languages, in response to an arbitrary color cue, is unlikely to have formed a major part of their prior experience.

(task), B, has not previously been investigated. The data give no indication of cumulatively increasing engagement of one particular language set, nor an increasing difficulty of switching to a different language, over successive same-language responses. This pattern of results is of some theoretical importance. It suggests that the lexicon of language A versus language B is selectively activated *as a whole unit*, rather than as a function of the cumulative activity of individual lexical items, piecemeal. The data are consistent with the construct of functional supralexical "language nodes," which activate and/or suppress a language-specific lexicon for spoken language production *as a whole*, as proposed for bilingual lexical representation in the BIA model (Dijkstra & Van Heuven, 1998; Grainger, 1993; for similar proposals, see also De Bot & Schreuder, 1993; Green, 1986, 1993, 1998; Paradis, 1981). This conclusion, however, is qualified in two ways. First, it is qualified by our use of small, closed vocabularies (numeral names) in this experiment. It would be valuable to confirm whether it also holds well with much larger vocabularies, without item repetition. Second, it is possible that participants increasingly expect alternations the longer a run gets, despite the objectively constant probability of a switch ($p = 0.3$) and despite the fact that lists with zero switches occurred. If so, this effect might mask a small effect, in the opposite direction, of increasing facilitation at the repeated language. On the other hand, for the first occurrence of a language switch in a list, the switching cost was observed only on the switch trial itself. By the next trial, naming RT was as fast as on any subsequent trials, consistent with the "language node" idea. (However, following a second language switch in a list, there was a significant tendency for the next (nonswitch) trial to show a slower RT.) The issue is an important one for bilingual language selection. On the whole, our data strongly suggest that all-or-none language selection (for spoken naming) can and does indeed occur.

Further evidence in favor of all-or-none language selection is provided in a recent study of language switching (Loasby, 1998). Proficient bilinguals named line drawings of objects with predictable, cued switches of the language of response. Prior to the actual experiment, participants were given intensive practice at speeded naming of a selected subset of the stimulus items, some items in one language, a different subset in the other language. Another, larger set of items remained unpracticed. In the switching task, stimuli that had received intensive prior naming practice could be presented for naming, either in the *same* language in which they had been practiced ("target-primed" items) or in the *other*, competing language ("competitor-primed" items). The purpose of this manipulation was thus to simulate the L_1/L_2 difference in relative language strength, exploited in the main study reported in this article, but now on a trial-by-trial (item-by-item) basis. That is, target-primed trials are designed to elicit naming responses in the relatively stronger language (*for that item*); competitor-primed trials elicit responses in the weaker language (for that item). The latter should therefore require greater suppression of the competing language response *on that particular trial*. It should be no surprise that naming RT on target-primed trials was about 100 ms faster than on competitor-primed trials. As expected, also, this difference remained about the same on both language switch and nonswitch trials. The critical question concerns the efficiency of naming on the trial immediately *following* a primed trial. According to the account of language-switching costs presented here, active suppression of the competitor language may persist, involuntarily, despite an intention to switch to the other language, resulting in "negative priming" of the intended language on the switch trial. (Hence larger switching costs in switching from L_2 to L_1 than from L_1 to L_2.) However, with *item-by-item* manipulation of relative language strength, a similar effect should occur *only* if suppression of the primed competitor item results in suppression, not just of that item, but of the competing language vocabulary *as a whole*. The results were dramatic and clear-cut. On trials following a target-primed item, when little if any suppression of the competing response would have been needed, the mean cost of a language switch was less than 40 ms. In contrast, following a competitor-primed item, the switching cost was over 180 ms. The item-by-item manipulation of relative language strength thus resulted in the same asymmetry in language switching costs as in our global L_1/L_2 manipulation, reported in this paper. The item-by-item manipulation, however, permits two further inferences. First, switching costs are determined primarily by the control requirements of the language (or task) *from* which the switch is made, rather than the control requirements of the upcoming language (or task). (For further evidence supporting this proposition, see Allport and Wylie, in press.) Second, activation of a particular lexical item in language A, and/or suppression of a competing lexical item in language B, apparently results in facilitation of the language A vocabulary and "negative priming" of the language B vocabulary, *as a whole.*

The question of active preparation of a specific language set, *in advance of* the naming stimulus, was not addressed in this experiment (but see Meuter & Powell, 1997, for a preliminary study). The extent of anticipatory control of task set, in other task-switching domains, and the conditions under which it may or may not occur, is the subject of active current research (De Jong, in press; Meiran, 1996; Rogers & Monsell, 1995). We note here the earlier finding by Macnamara et al. (1968) that, in their experiment, an intertrial interval of 2.0 s, even with fully predictable alternation between languages, did not reduce the observed switching cost. It would be interesting to establish whether the reverse asymmetry of language-switching costs, reported here, would be observed even with precueing of the language for naming. What kinds of stimuli act as effective cues for language selection, and to what extent they can reduce or even eliminate language-switching costs, are questions for future research.

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