Memory & Cognition
2005, 33 (2), 355-369

Language switching and the effects of orthographic specificity and response repetition

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In two experiments, Greek–English bilinguals alternated between performing a lexical decision task in Greek and in English. The cost to performance on switch trials interacted with response repetition, implying that a source of this "switch cost" is at the level of response mapping or initiation. Orthographic specificity also affected switch cost. Greek and English have partially overlapping alphabets, which enabled us to manipulate language specificity at the letter level, rather than only at the level of letter clusters. Language-nonspecific stimuli used only symbols common to both Greek and English, whereas language-specific stimuli contained letters unique to just one language. The switch cost was markedly reduced by such language-specific orthography, and this effect did not interact with the effect of response repetition, implying a separate, stimulus-sensitive source of switch costs. However, we argue that this second source is not within the word-recognition system, but at the level of task schemas, because the reduction of switch cost with language-specific stimuli was abolished when these stimuli were intermingled with language-nonspecific stimuli.

When bilinguals switch from using one language to another (in laboratory tasks), there is generally a transient cost to performance (Costa, Miozzo, & Caramazza, 1999; Hernandez, Dapretto, Mazziotta, & Bookheimer, 2001; Jackson, Swainson, Cunnington, & Jackson, 2001; Kroll, Dietz, & Green, 2000; Meuter & Allport, 1999). Such a "language switch cost" exists even when only the language of the stimulus changes and subjects respond manually so that no switch in output is required (Grainger & Beauvillain, 1987; Jackson, Swainson, Mullin, Cunnington, & Jackson, 2004; Thomas & Allport, 2000; von Studnitz & Green, 1997, 2002a). One explanation for the switch cost is that the representations for the new language are disadvantaged compared with those of the just-used language (Dijkstra & van Heuven, 1998; Thomas, 1997; van Heuven, Dijkstra, & Grainger, 1998). In other words, the cost might arise from within the word-recognition system and thus from a source specific to language switching. However, it has also been argued that the cost arises from control processes or interference external to the language system itself (e.g., Green, 1998a, 1998b). Thus, language switch costs might reflect "task switch costs," which generally arise when subjects switch between any two simple tasks (e.g., Allport, Styles, & Hsieh, 1994; Meiran, 1996; Monsell, 2003; Rogers & Monsell, 1995). In this article, we set out to test the two effects that have been the main evidence in the debate between whether language switch costs come from task control processes or from within the word-recognition system. These effects, explained below, are the interaction of switch cost with response repetition and the effect of language-specific orthography on switch cost.

Language-Specific Orthography

There is growing evidence that access to the word-recognition systems of bilinguals is not language selective (e.g., Dijkstra, Grainger, & van Heuven, 1999; Jared & Kroll, 2001; von Studnitz & Green, 2002b). That is, a word-like stimulus activates lexical possibilities in both languages even when the bilingual subject knows that only one language is currently needed. However, it is likely that depending on the current language context, the relative activation of representations in each language would differ—that use of one language facilitates subsequent word recognition in the same language in some way, perhaps by priming all lexical representations in the same language, or by activating that language's mapping of orthography to pronunciation (see Lukatela & Turvey, 1998, for the related concept of "alphabet biasing" without the ability to disable one alphabet completely). Thus the source of the language switch cost could be an exaggeration of the normal processes of competition and interference that accompany nonselective lexical activation (e.g., Dijkstra & van Heuven, 1998; Thomas, 1997; van Heuven et al., 1998). This idea would predict that stimuli that cause less or no interference between lan-

We thank Matthew H. Davis and Catherine M. Arrington for comments, and the Royal Society for financial support. Correspondence concerning this article should be addressed to P. Sumner, Division of Neuroscience, Faculty of Medicine, Imperial College London, St. Dunstan's Road, London W6 8RP, England (e-mail: p.sumner@imperial.ac.uk).

guage representations should produce a smaller language switch cost, which is exactly what Grainger and Beauvillain (1987) found: The cost of switching between English and French in the lexical decision task (LDT) was eliminated when the words contained language-specific orthographic cues.

However, Thomas and Allport (2000), also using English and French, found no effect of orthography on switch cost and suggested (we believe correctly) that Grainger and Beauvillain's (1987) results were due to a missing control condition (there were no nonwords with language-specific orthography, so subjects could have used the specific orthography as a direct cue to respond *word* without the need for lexical recognition). Thomas and Allport concluded that the switch cost arises from a process free from the influence of orthographic features: that of switching between task schemas. This is in accordance with other researchers who have also explicitly postulated influences of task schemas, or language schemas, in language processing, especially in the context of bilinguals (e.g., Dijkstra & van Heuven, 2002; Green, 1998a). A *task schema* or *task set* "is an organization of mental resources that will accomplish a particular cognitive task, given appropriate input" (Monsell, Sumner, & Waters, 2003). Likewise, a *language schema* is a particular organization of language-processing resources and their output. Grosjean's (2001) "language mode" can be seen as the current relative state of activation of each language schema (von Studnitz & Green, 2002b). Thus, Thomas and Allport argued that the processes of language switching should be interpreted in the wider context of cognitive control of switching between any two tasks, rather than as a topic relevant only to bilingualism.

Although Thomas and Allport's (2000) results have been used to argue for a task schema account of language switching, they are not fully in accord with results from the task switching literature. Stimuli that unambiguously tell a subject which task to perform (*single affordance* or *univalent* stimuli) generally produce much smaller switch costs than do stimuli that could be associated with either task (*dual affordance* or *bivalent* stimuli; note that affordance or valency refers here to task associations, not responses: A bivalent stimulus may or may not produce the same response in either task; e.g., Allport et al., 1994; Dreisbach, Haider, & Kluwe, 2002; Jersild, 1927; Rogers & Monsell, 1995; Spector & Biederman, 1976; Sumner & Ahmed, in press). In the context of language switching, words and nonwords with language-nonspecific orthography are bivalent stimuli because they do not themselves tell the reader which language is relevant, whereas words and nonwords with language-specific orthography can be called univalent. Thus, the latter would be expected to reduce switch costs even if the source of the cost lies solely in the domain of task schemas rather than word recognition.

Why did Thomas and Allport (2000) not find such a reduction? One possibility is that language switching and task switching are not equivalent after all. Alternatively, it may be because the stimuli and experimental design used by Thomas and Allport differed in two potentially important ways from those used in studies of task switching with univalent stimuli. First, Thomas and Allport's language-specific orthography did not make the stimuli entirely task exclusive: A French word with language-specific orthography, such as NEUF, could still be read by an English monolingual and categorized as a "nonword." That is, although French and English have unique letter clusters that can supply language information at the bigram and trigram level, they use the same alphabets, and thus language information cannot be supplied by individual letters. However, information at the letter level is thought to have a much stronger influence on word recognition than do bigram and trigram information (e.g., Dijkstra & van Heuven, 2002; McClelland & Rumelhart, 1981; Westbury & Buchanan, 2002). To address this, we took advantage of the fact that Greek and English have different, but partially overlapping alphabets, which enabled us to manipulate language specificity at the letter level. Thus we produced language-specific stimuli that could not be read at all in the wrong language, as well as nonspecific stimuli that could be read in either language (though they may be words only in one language; Lukatela & Turvey, 1998, pioneered the use of partially overlapping alphabets while studying word recognition in Serbo-Croatian.)

Second, in Thomas and Allport's (2000) study, language-specific and nonspecific trials were interspersed in the same block, whereas in the task switching literature single affordance stimuli have generally not been intermingled with dual-affordance stimuli (see Meiran, 2000, for an exception). Intermingling or blocking the stimuli may encourage different strategies and thus alter the task schema. To test whether this was the case, in Experiment 1 we placed language-specific stimuli in different blocks from nonspecific stimuli, and in Experiment 2 we intermingled the two stimulus types in the same blocks.

Response Repetition Effect

More recently, a different measure has been taken as evidence for the schema account of language switch costs. von Studnitz and Green (2002a) argued that if switch costs interact with response features (such as repetition of the same response), such costs are not caused by word recognition, but "arise in the course of mapping decisions onto responses." They found such an interaction in a study in which bilinguals switched between English and German while categorizing words as animate or inanimate: Responses were slowed on switch trials only if subjects had to make the same response as on the previous trial. A similar interaction occurred in the language switching study of Thomas and Allport (2000), and this parallels results for task switching—although the usual effect of repeating a response is to reduce reaction time (RT; Rabbitt, 1968), on a switch trial it seems to prolong RT (Rogers & Monsell, 1995).

This interaction between switch cost and response repetition gives us the opportunity to test the locus of other influences on switch costs. Most models of task switching now incorporate more than one source for task switch costs, and we can therefore expect that there may be more than one source for language switching costs. As discussed above, both the task schema and the word-recognition accounts of the language switch cost can predict a reduction in switch cost with language-specific stimuli. But we can use the response repetition effect to try to distinguish the accounts: If the effect of orthographic specificity interacts with the effect of response repetition, we can conclude that the two effects are not isolated from each other, and that both result from processes external to the word-recognition system. If, on the other hand, the specificity effect does not interact with the effect of response repetition, we might conclude that the two effects arise from different processes. This would be consistent with one source arising from within word recognition, and one from the process of mapping decisions onto responses. Alternatively, there could be separable sources within the task schema system, neither of which is entirely within word recognition. This last possibility was investigated in Experiment 2, but first, in Experiment 1, we tested (1) whether language-specific orthography does reduce switch costs at all, (2) whether we would replicate the interaction of response repetition with switch cost, and (3) if both these effects are found, whether they interact with each other (indicating whether the effects come from the same or separate sources).

EXPERIMENT 1

Crucially, Greek and English have different but partially overlapping alphabets. Of the 26 letters in English and the 24 letters in Greek, 14 symbols are used in both languages (see Figure 1). Therefore, we could generate *language-nonspecific* words and nonwords containing only symbols used in both languages, in legally pronounceable sequences (e.g., MATE, ENOPIA, BOTH). We could also generate *language-specific* words and nonwords containing some letters unique to one language (e.g., CURE, BRAGE, ΔΙΨΑ, ΣΩΜΕ), which therefore could not possibly be read in the wrong language.

The participants alternated between performing an LDT in English and in Greek. In separate blocks, the stimuli were language specific or language nonspecific.

Method

Participants. Twenty-four Greek–English bilinguals, 20–26 years of age, volunteered to take part in the study. All were native speakers of Greek but had studied English for a minimum of 8 years (most began learning English at elementary school, at age 8). At the time of the study, all the participants were occupied in full-time study or research in which English was the working language. All were free of any known language disorder and had normal or corrected-to-normal visual acuity and normal color vision (21 were right-handed, 3 were left-handed). For counterbalancing purposes, the participants were divided into two groups of 12 on the basis of the time spent living in the UK. The "highly proficient" group (6 male, 6 female) had been living in the UK for 1–5 years. The "less proficient" group (5 male, 7 female) had been living in the UK for 9 months or less.

Procedure. Each trial consisted of the presentation of a cue to indicate which language was currently relevant, followed after 800 msec by a *word* or *nonword* letter string, requiring a left or right button-press, respectively. Both the cue and letter string remained on the screen until a response was made. The next trial's cue was presented immediately after the response was made, so that the time between the word/nonword stimuli of one trial and the next was 800 msec plus the RT. On each trial, only the current language was relevant, and the participants were instructed to ignore whether or not the stimulus was a word in the other language. On Greek trials, for example, a letter string that was an English word, but not a Greek word, had to be classified as a nonword, and vice versa (this is known as a *language-exclusive LDT*). If an error was made, feedback was given in the form of a 400-Hz auditory tone for 20 msec.

The language cue was a pink or blue rectangle or oval of approximately $2^\circ \times 4^\circ$. On every trial, either the shape or the color changed. For half of each group of participants, the shape indicated the language and the color was irrelevant, and for the other participants this was reversed. This was done to ensure overall stimulus change on every trial, not just on switch trials. The task stimuli were letter strings 0.5° in height and presented in the center of this cue shape. These letter strings came from one of eight categories: Greek/English × word/nonword × language-specific orthography/language-nonspecific orthography (see below).

The language changed predictably every two or four trials. This manipulation of "run length" was an extra control and its justification is explained alongside the relevant results below. The run length was constant within a block, and the participants were explicitly informed which run length to expect. To help them keep track of when to expect the language switch, a "clock hand" was presented that moved around the cue shape. The language changed when the clock hand reached the 12 o'clock position. For run lengths of 4 trials, the clock hand moved 90° each trial, and for run lengths of two trials, it moved 180° each trial. The language switch was thus indicated in two ways: the cue shape or color and the clock hand. The language-specific stimuli also indicated the language.

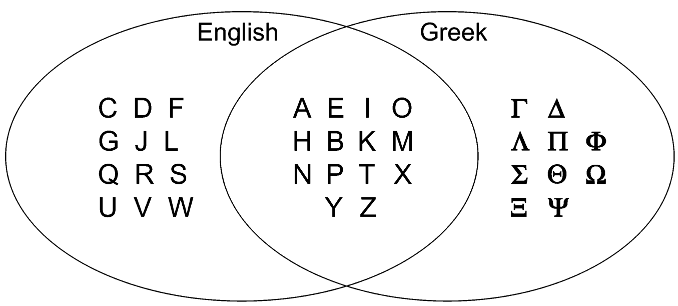

Figure 1. Language-specific and nonspecific symbols in English and Greek alphabets.

Stimulus presentation and response collection were performed by a Cambridge Research Systems (CRS) VSG 5 board (housed in a PC host) connected to a Sony GDM-F520 Trinitron monitor and a CRS CB3 response box. Stimulus presentation was synchronized with a screen refresh rate of 100 Hz, and all timings were controlled and measured by the CRS clock and thus not subject to errors produced by normal PC operating systems.

Stimuli. Seventy-two test stimuli were produced for each of the eight needed categories (Greek/English × word/nonword × specific/nonspecific orthography), making a total of 576 test stimuli (additional stimuli were produced in each category for practice blocks). Specific stimuli contained letters unique to one language, whereas nonspecific stimuli were those that included only letters common to both languages, in common legal arrangements (see Figure 1; all letters were presented in uppercase). The total number of stimuli produced in each category was limited by the paucity of nonspecific words in either language (i.e., words in Greek that could plausibly be English words, and vice versa). Words were matched across languages as far as possible for length, letters, and letter clusters. Specific words were approximately matched to their nonspecific counterparts in length (by definition, they had different letter compositions). Nonspecific words had higher frequencies than did specific words, so that if a larger switch cost was found for nonspecific words it could not possibly be explained by their frequency (see Table 1). Some Greek words had to be included in conjugated or superlative forms (e.g., TATE) in order to make them nonspecific (i.e., with no specific letters and in a form that could be a plausible word in English with common arrangements of letters). Similarly formed words were therefore included in all categories, and the participants were informed of this. The paucity of nonspecific words made it impossible to match the stimuli in all possible and ideal ways, but this is not critical since the specific and nonspecific categories are, by definition, required to be recognizably different, and our participants were unbalanced bilinguals, anyway. However, it was important that Greek and English nonspecific stimuli did not contain certain orthographic patterns that might become associated with one language within the experimental setting, which would in effect make them more language specific.

The most important matching was between words and nonwords, in order for the LDT to be based on the retrieval of lexical information alone, rather than on other cues in the letter strings. Words and nonwords were matched for length, letters, and letter clusters (with special attention to initial and final letters/clusters), and care was taken to ensure that nonwords were legal, pronounceable, and plausible letter strings in the appropriate language. Nonwords were created by changing a vowel, a consonant, or a consonant cluster of words of each language (that were not being used as word stimuli). Most Greek nonwords of both categories (specific and nonspecific) were not English words either, and vice versa, but we did include eight words of one language as nonwords in the other language (necessarily in the nonspecific category) in order to check that participants were performing a language-exclusive LDT as directed (i.e., that they had not set themselves an inclusive task set to look for words in either language, in which case switch trials with nonwords would not require any task switch).

Because of the paucity of truly nonspecific word forms that do not occur with much higher frequency in one language than the other, our nonspecific sets of words and nonwords could not reach the ideal of being equally likely to be words in either language. However, they were all possible legal words in either language, whereas our specific words and nonwords could not be read in the other language at all. In this way, we believe we created a larger separation of specific and nonspecific stimuli (within the same pair of languages) than has been achieved in bilingual studies before. See the Appendix for lists of the stimuli and Table 1 for mean frequency and length for each category.

Design. The language-specific and language-nonspecific stimuli were presented in separate blocks (and the participants were informed of this difference). Also in different blocks, the language alternated every 2 or 4 trials. Thus, there were four types of block, and the participants performed two of each (eight blocks in total), in a counterbalanced order within and between subjects. The starting language was also counterbalanced within and between subjects. The eight blocks were divided into two sessions (of about 30 min each, with a break of several minutes between them), with one block of each type per session. Blocks with run length 2 had 96 trials, and blocks with run length 4 had 192 trials, because in each block there were 48 switches of language, making 24 switch trials in each language, 12 of which had word stimuli and 12 had nonword stimuli (in a random order). Likewise, there were 12 trials of each type of repeat trial (with the order randomly shuffled). Subsets (12 items) of the 72 stimuli of each category were allocated to each run position of each block, counterbalanced across subjects, and such that no stimulus was repeated within the same session. Because of the limit on the number of stimuli created, it was necessary to allow stimuli to occur in both sessions, but always in a different type of block. The participants were given a 40-trial practice block of each type, making four practice blocks in total, administered in a counterbalanced order across subjects. The practice stimuli were all different from the test stimuli.

The participants were instructed to perform each individual trial as fast as possible while minimizing errors. RT and error rate were monitored during the practice blocks, and the participants were given feedback and encouraged to try to be more careful or to respond faster, as appropriate.

Analysis. Separate parallel analyses were performed for RT and percentage error rates. In addition, mean RT and median RT for each cell were analyzed separately, and their patterns of effects were found to agree, except where highlighted below (the means are reported, unless otherwise stated). All error trials, and trials immediately following an error, were excluded from the RT analysis (13%). Also excluded (as warm-up trials) were trials before the second switch in each block. RTs of less than 200 msec or more than 2,000 msec were also automatically excluded (<1%).

Table 1
Mean Frequency (Per Million) and Length of the Word Stimuli

	Greek		English	
	Specific	Nonspecific	Specific	Nonspecific
Lemma frequency	133	298	17	44
Word form frequency	58	145	9	16
Length	5.7	5.2	5.4	5.3

Note—English frequencies are from Kučera & Francis (1967); Greek frequencies are from ILSP (1999–2002) Hellenic National Corpus Web Version 1.1, available at http://corpus.ilsp.gr/statistics.asp.

Results

Our main interest lay in the interactions of switch (switch or nonswitch trial) with orthography (specific or nonspecific) and previous response (same or different). These are shown in Figures 2 and 3, respectively. However, first we describe preliminary analyses on the effects of language, bilingual proficiency, and run length.

Language asymmetry? Meuter and Allport (1999) found counterintuitively larger switch costs for the dominant language when bilinguals switched language in a naming task. However, in our LDT experiment there was no such effect. If anything, there was a smaller switch cost for the dominant language, especially for the less proficient bilingual group (Greek, 30 msec; English, 46 msec), but analyses of variance (ANOVAs) showed this not to be a reliable effect (no interaction between factors of language and switch [$F(1,22) = 0.4$] or between language, switch, and bilingual proficiency [$F(1,22) = 1.5$]. However, there was also no main effect of language [$F(1,22) = 1.0$] and only a small interaction of language and proficiency (the highly proficient group was 23 msec faster in English than in Greek, whereas the less proficient group was 10 msec slower in English [$F(1,22) = 6.1$, $MS_e = 8{,}554$, $p < .05$]. None of these effects were significant in the error rates. It seems that even our less proficient participants did not find the task much harder in English, removing any strong prediction of an asymmetry of switch cost. Since neither the language of the task nor the bilingual proficiency of the participant interacted with any other factor, our main analyses were simplified by not including the factors of language and proficiency.

Run length. In different blocks, the participants switched trial either every two trials or every four trials. This manipulation served two related purposes. In task switching, when the switch is unpredictable there tends to be further improvement in RT from the second to the third trials of one task, but for predictable switches all nonswitch trials are normally alike (Monsell et al., 2003; Sumner & Ahmed, in press). Thus, when language switches are predictable we would expect all nonswitch trials to have similar RTs. If they do not, first we would have evidence that language switching and task switching are not equivalent. Second, we would know that the nonswitch trials for run lengths of two trials (the usual run length in previous language switching experiments) would not be representative of all nonswitch trials, and thus the calculated switch cost would not be secure. Fortunately, we found that performance was alike for the second, third, and fourth trials after a switch [RT, $F(2,46) = 1.1$, $MS_e = 3{,}270$; linear trend, $F(1,23) = 0.6$, $MS_e = 4{,}117$; errors, $F(2,46) = 2.3$, $MS_e = 28$; linear trend, $F(1,23) = 0.1$, $MS_e = 33$]. In addition, nonswitch trial performance was alike for both run lengths, as was switch trial performance, so there was no main effect of run length [$F(1,23) = 0.3$] and no interaction of run length switch cost [$F(1,23) = 1.6$]. Since there were no other interactions involving run length either, data were combined for run lengths two and four, and we do not discuss this factor further.

Main analyses. The main ANOVAs, performed on both RT and error rate, had factors of switch (switch or nonswitch trial), orthography (specific or nonspecific), previous response (same or different), and lexicality (word or nonword). The main effects were not our primary interest, but as expected, the main effect of switch (the overall switch cost) was robust in RT [830 msec for switch trials vs. 788 msec for nonswitch trials; $F(1,23) = 29$, $MS_e = 12{,}100$, $p < .001$] and error rate [9.0% vs. 7.7%; $F(1,23) = 9.7$, $MS_e = 31$, $p < .01$]. Also as expected, overall performance was better on the specific stimuli than on nonspecific stimuli, both in RT [719 vs. 899 msec; $F(1,23) = 81$, $MS_e = 76{,}762$, $p < .001$] and error rate [5% vs. 12%; $F(1,23) = 140$, $MS_e = 67$, $p < .001$]. Finally, mean performance on words was faster than on nonwords [749 vs. 869 msec; $F(1,23) = 35$, $MS_e = 78{,}445$, $p < .001$], but it was less accurate [10.2% error vs. 7.0% error; $F(1,23) = 31$, $MS_e = 64$, $p < .001$]. The main effect of response repetition was not significant in either RT or error rate [$F(1,23) = 0.2$; $F(1,23) = 0.5$].

Interactions of primary interest. Our primary interests in this study were the interactions of language switch with language specificity and response repetition. These are plotted in Figures 2 and 3, respectively. As Figure 2 shows, RT was much less raised on switch trials for specific stimuli than for nonspecific stimuli [19 vs. 66 msec; interaction of orthography and switch, $F(1,23) = 16$, $MS_e = 6{,}920$, $p = .001$]. The error switch cost was also slightly larger for the nonspecific stimuli (1.6% vs. 0.9%), but this interaction was not reliable [$F(1,23) = 1.5$, $MS_e = 18$]. Figure 2 also shows that this effect of orthographic specificity was not different for words and nonwords [interaction of switch, orthography, and lexicality: RT, $F(1,23) = 0.1$; errors, $F(1,23) = 0.03$].

The second interaction of major interest was that of switch cost with response repetition. As shown in Figure 3, repeating a response had the opposite effect for switch trials and nonswitch trials. Thus, the predicted interaction between response repetition and switch was highly robust in RT [$F(1,23) = 23$, $MS_e = 6{,}093$, $p < .001$] and error

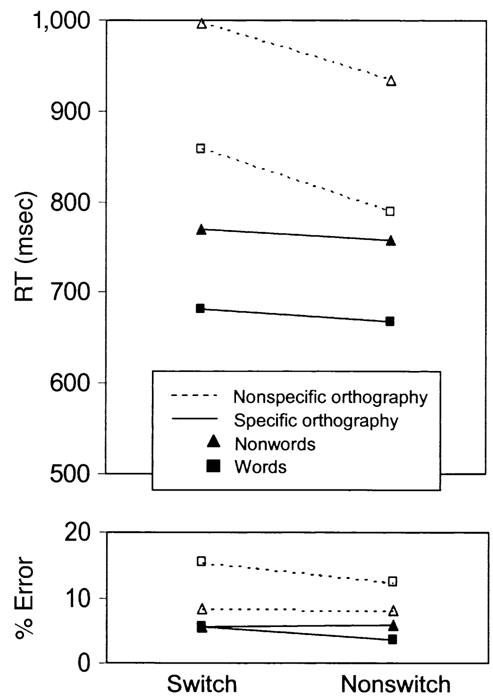

Figure 2. Effect of orthographic specificity in Experiment 1. The switch cost (the difference between performance on switch trials and on nonswitch trials) was reduced for stimuli with language-specific orthography, in comparison with nonspecific orthography.

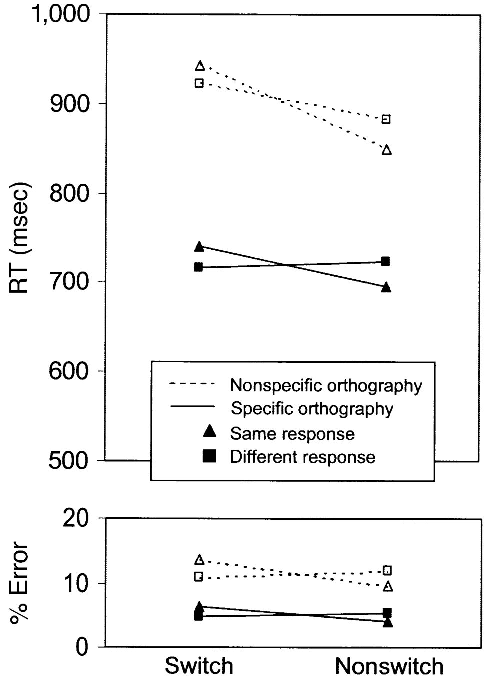

Figure 3. Effect of response repetition in Experiment 1. The switch cost was larger when the present and previous responses were the same. Word and nonword data have been combined.

rate [$F(1,23) = 19$, $MS_e = 42$, $p < .001$]. There was no difference in this effect between the specific and nonspecific stimuli [three-way interaction of switch, response repetition, and orthography: RT, $F(1,23) = 0.005$, $MS_e = 4,310$, $p > .9$; errors, $F(1,23) = 1.4$, $MS_e = 45$, $p > .25$].

Other effects. The following effects were also found, but since they were not of a priori interest and an ANOVA does not correct for multiple comparisons (Thompson, 1994), we do not place any theoretical weight on them. There were two further interactions with switch: The switch cost in error rate was larger for words than for nonwords [$F(1,23) = 6.7$, $MS_e = 39$, $p < .05$] and in RT the effect of switch on response repetition, reported above, was more pronounced for words than for nonwords [$F(1,23) = 11$, $MS_e = 4,292$, $p < .01$]. Finally, orthography interacted with lexicality in opposite directions for RT and for errors (without affecting switch cost), such that in RT the word/nonword difference was simply more pronounced for nonspecific stimuli than for specific stimuli [$F(1,23) = 11$, $MS_e = 16,705$, $p < .01$], whereas in error rate, the word/nonword difference was reversed for nonspecific stimuli [$F(1,23) = 50$, $MS_e = 76$, $p < .001$; see Figure 2].

Confirmation of language-exclusive LDT. As mentioned in the Method section, we included eight words of one language as nonwords in the other language (necessarily in the nonspecific category) in order to ensure that participants were performing a language-exclusive LDT as directed (i.e., that they had not set themselves an inclusive task set to look for words in either language). The majority of such words in the wrong language were correctly classified as nonwords (>80%), and we were therefore satisfied that subjects were performing a language-exclusive LDT as directed. Unsurprisingly, classifying such words as nonwords produced 9% longer RT and 15% higher error rate than did standard nonspecific nonwords.

Discussion

Our three important findings were (1) that the switch cost was much smaller with language-specific orthography, (2) that response repetition helped performance on nonswitch trials but harmed performance when the language switched, and (3) that effects (1) and (2) did not interact. The fact that the two effects did not influence each other suggests that they may come from different sources, one associated with stimulus features and one at the level of response coding or initiation. We follow the suggestion of von Studnitz and Green (2002a) that the response repetition effect implies that there is a difference between switch trials and repeat trials in the process of mapping decisions onto responses. However, this seems to be just one factor affecting switch costs, but not the only one.

What is the source of the influence of orthographic specificity? The effect is consistent with a source of switch costs within the word-recognition system (Grainger & Beauvillain, 1987). There are at least two possible such sources. First, lexical representations of one language may be partially deactivated by word-recognition processes in the other. Since lexical access seems to be nonselective and involves competition between any lexical representations activated by a stimulus (e.g., Dijkstra et al., 1999; Dijkstra & van Heuven, 2002; von Studnitz & Green, 2002b), interference from inappropriate lexical representations could be enhanced when switching languages, because the appropriate lexical representations start off relatively deactivated (e.g., Dijkstra & van Heuven, 1998; Grainger & Beauvillain, 1987; Green, 1998a; Thomas, 1997; van Heuven et al., 1998). Language-specific orthography would reduce the possibility of interference from inappropriate lexical representations and would therefore reduce switch costs. Second, spelling–sound correspondences, which are known to affect word recognition (e.g., Brysbaert, Van Dyck, & Van de Poel, 1999), differ between languages. For example, not only are many letters pronounced differently in Greek and English, but also there is a regular correspondence of letters (or letter clusters) with pronunciation in Greek, whereas perfect regularity is not found in English. Use of one language presumably activates the appropriate spelling–sound mappings and strategies, and if these are inappropriately activated by a stimulus in the new language, switch costs would arise (e.g., Jared & Kroll, 2001; von Studnitz & Green, 2002a). Language-specific orthography would reduce the possibility of such inappropriate activation of spelling–sound mappings and would therefore reduce switch costs.

However, studies of switching between nonlanguage tasks, such as responding to different spatial locations, digits, or colors, have found that switch costs are smaller for univalent stimuli, which uniquely cue one task, compared with bivalent stimuli, which are associated with both tasks (Allport et al., 1994; Dreisbach et al., 2002; Jersild, 1927; Meiran, 2000; Rogers & Monsell, 1995; Spector & Biederman, 1976; Sumner & Ahmed, in press). Thus, the reduction of switch cost with language-specific stimuli could be interpreted as an example of this general difference between univalent and bivalent stimuli, and the source of such a difference may be similar across the various tasks, rather than being associated exclusively with processes of word recognition. This would be more consistent with the task schema account of language switching costs. On the other hand, a similar pattern of results for language switching and task switching does not necessarily mean that word recognition makes no contribution to switch costs. It may be that in language switching, the effect of language-specific orthography arises purely from word-recognition processes, and that in task switching studies the higher switch costs with bivalent stimuli arise from a source or sources that do not occur in language switching (such as shifting attention between stimulus attributes; Meiran, 2000).

In sum, the reduction of switch cost with language-specific stimuli may indicate that word-recognition processes contribute to language switching costs. Alternatively, the effect of orthographic specificity might be interpreted in the wider literature of task switching as another example of smaller switch costs with univalent stimuli, resulting from reduced interference between task schemas. How might we distinguish between these two possibilities? The first proposal fits well with our finding that the effect of orthographic specificity did not interact with the effect of response repetition. But this finding is not inconsistent with the second proposal: The two effects could come from different sources, neither of which is within the word-recognition system.

There is a way we can decide between the two proposals if we look again to the differences between our experiment and those of Thomas and Allport (2000), which did not find an effect of orthographic specificity. Our experiment differed from those of Thomas and Allport in two main ways: Our Greek and English language-specific stimuli were more language exclusive than their French and English stimuli could be, and they intermingled language-specific and nonspecific stimuli in the same blocks, whereas we separated them into separate blocks. If the difference between our results and theirs was caused by the differences in our stimuli, this would be consistent with the main source of the effect being word-recognition processes. If, on the other hand, the difference was caused by the type of block in which the stimuli occurred, this would suggest that the effect reflects strategic differences in task schemas rather than any trial-by-trial word-recognition process. For example, in language-specific blocks, subjects might rely on the stimuli themselves to tell them when to change language, whereas this is not possible in nonspecific blocks or in intermingled blocks. Therefore, in Experiment 2 we used the same language-specific and nonspecific stimuli as in Experiment 1, but we intermingled them in the same blocks.

Having intermingled the two types of stimuli, we might gain another clue from examining the effects of the preceding trial's orthographic specificity on switch and nonswitch trials. According to the word-recognition account of language switching costs, RT is raised on a switch trial because nonspecific stimuli activate both languages, and representations or processes in the now-irrelevant language have just been primed. Now, since in a nonspecific trial both languages are activated, if a nonswitch trial follows a nonspecific trial, representations in the wrong language have just been activated, which could slow RT for that nonswitch trial. When a switch trial follows a nonspecific trial, the "wrong" language has become the now-needed language, so the correct language is slightly primed already, which might reduce RTs on switch trials. So when the preceding trial has nonspecific orthography, according to the word-recognition account, nonswitch trials should suffer but switch trials should gain (see Lukatela & Turvey, 1998, p. 1064, for the related concept of "alphabet biasing"). The task schema account, on the other hand, predicts no such effect. If on all trials in the intermingled blocks subjects are forced to adopt a strategy similar to that in the nonspecific blocks (because, for example, they cannot rely on the stimuli to tell them when to change language), there may be no effect of the previous trial's orthographic specificity. Furthermore, ideas of inhibition in task-set control (e.g., Allport et al., 1994; Allport & Wylie, 1999; Goschke, 2000; Mayr, 2002; Mayr & Keele, 2000) make a prediction opposite to that from the word-recognition account above. In trials with nonspecific stimuli, more inhibition might need to be applied to the competing language schema or task set. If this inhibition carries over to the next trial, we might see this reflected in higher RTs for switch trials when there was a nonspecific stimulus in the preswitch trial. So in sum, if a nonspecific trial harms performance on the following switch trial, or if nonspecific and specific trials have equal effect on the following trial, this favors the task-schema account. But if a nonspecific trial harms performance on a following nonswitch trial but improves performance on a following switch trial, this would favor a word-recognition source for language switching costs.

EXPERIMENT 2

Participants alternated between performing a language-exclusive LDT in Greek and in English. Language-specific and language-nonspecific stimuli were randomly intermingled in the same blocks. All aspects of the procedure and stimuli were the same as in Experiment 1. The only differences in design and analysis are explained below (aspects not mentioned were the same as in Experiment 1).

Method

Participants. The same participants were used as in Experiment 1. The two experiments were designed together, and the participants were recruited to do both of them, in a counterbalanced order: Half of the highly proficient group and half of the less proficient group did Experiment 2 first.

Design. The language-specific and language-nonspecific stimuli were presented in the same blocks (and the participants were informed that both types would occur). Each participant performed two blocks of each run length (2 and 4), in a counterbalanced order within and between subjects. The starting language was also counterbalanced within and between subjects. The four blocks were divided into two sessions (of about 30 min each, with a break of several minutes between them), with one block of each type per session. In each block, there were 96 switches of language, and thus blocks with run length 2 had 192 trials, and blocks with run length 4 had 384 trials, in the middle of which a break was introduced. Within each language in each block, there were 4 categories of stimulus (word/nonword × specific/nonspecific), with 12 of each category assigned to each run position and presented in a randomly shuffled order. As in Experiment 1, subsets of stimuli were allocated to each trial type of each block in a counterbalanced way across subjects, and in such a way that no stimulus was repeated within the same session. Stimuli could occur in both sessions, but always in a different type of block. The participants were given a 40-trial practice block of each type, administered in a counterbalanced order across subjects. The practice stimuli were all different from the test stimuli.

Results

As for Experiment 1, our primary interests in this experiment were the interactions of the switch cost with language specificity and response repetition, which are shown in Figures 4 and 5, respectively, and described below. Preliminary analysis showed no overall difference in performance in Greek and English but an interaction between language and proficiency, such that on English trials, the less proficient group's RT was 78 msec slower than the highly proficient group's [$F(1,22) = 8.2$, $p < .01$]. There was no interaction of language and switch cost, and neither did language or proficiency interact with any other factor. As in Experiment 1, there was no effect of run length on the nonswitch trials, and when the switch and nonswitch trials for each run length were compared, there was no main effect of run length in RT [$F(1,23) = 1.5$; $F(1,23) = 1.8$], but there were slightly more errors in the blocks with run length 4 than in blocks with run length 2 [9.3% vs. 8.1%; $F(1,23) = 11$, $MS_e = 51$, $p < .01$], possibly because they were longer. The switch cost was also 20 msec larger for run length 4 [55 vs. 35 msec; $F(1,22) = 5.4$, $p = .03$]. Run length did not interact with any other factor.

Main analysis. The main ANOVAs, performed on RT and error rates, had factors of switch (switch or nonswitch trial), orthography (specific or nonspecific), orthography of previous trial (specific or nonspecific), previous response (same or different), and lexicality (word or nonword). As expected, the main effect of switch (the overall switch cost) was robust in RT [845 msec for switch trials vs. 800 msec for nonswitch trials; $F(1,23) = 15$, $MS_e = 52{,}950$, $p = .001$] and error rate [9.0% vs. 8.3%; $F(1,23) = 5.1$, $MS_e = 33$, $p < .05$]. Also as expected, overall performance was better on the specific stimuli than on nonspecific stimuli, both in RT [780 vs. 864 msec; $F(1,23) = 52$, $MS_e = 51{,}973$, $p < .001$] and error rate [5.5% vs. 11.8%; $F(1,23) = 163$, $MS_e = 95$, $p < .001$]. Like Experiment 1, performance for words was reliably faster than for nonwords [754 vs. 891 msec; $F(1,23) = 32$, $MS_e = 227{,}429$, $p < .001$], but it was less accurate [10.2% error vs. 7.1% error; $F(1,23) = 21$, $MS_e = 182$, $p < .001$]. The main effect of previous trial orthography was not significant [RT, $F(1,23) = 3.2$; errors, $F(1,23) = 0.4$], and neither was the main effect of response repetition in RT. In the error rate, however, responses overall were slightly more accurate if the previous response had been the same [8.0% error vs. 9.3% error; $F(1,23) = 4.5$, $MS_e = 135$, $p < .05$].

Interactions of primary interest. As Figure 4 shows, although the RT switch cost was slightly smaller for specific stimuli than for nonspecific stimuli (40 vs. 50 msec), the difference was much less than in Experiment 1, and it was not reliable [$F(1,23) = 1.1$, $MS_e = 7{,}972$, $p > .3$]. The equivalent interaction in the errors also was not reliable [$F(1,23) = 1.4$, $MS_e = 60$, $p > .2$]. Figure 4 also shows that this pattern of results was the same for words and for nonwords [$F(1,23) < 1$]. Figure 5 shows that as in Experiment 1, response repetition interacted with language switching such that repeating a response on nonswitch trials speeded RTs (810 to 790 msec) and decreased errors (9.7% to 6.9%), whereas on switch trials repeating a response prolonged RT (822 to 868 msec) and slightly increased errors (8.8% to 9.1%). This interaction was highly robust for RT [$F(1,23) = 23$, $MS_e = 17{,}454$, $p < .001$] and error rate [$F(1,23) = 12$, $MS_e = 73$, $p < .01$]. The size of the effect was similar to that in Experiment 1, and also like Experiment 1, there was no difference in this effect between the specific and nonspecific stimuli [three-way interaction of switch, response

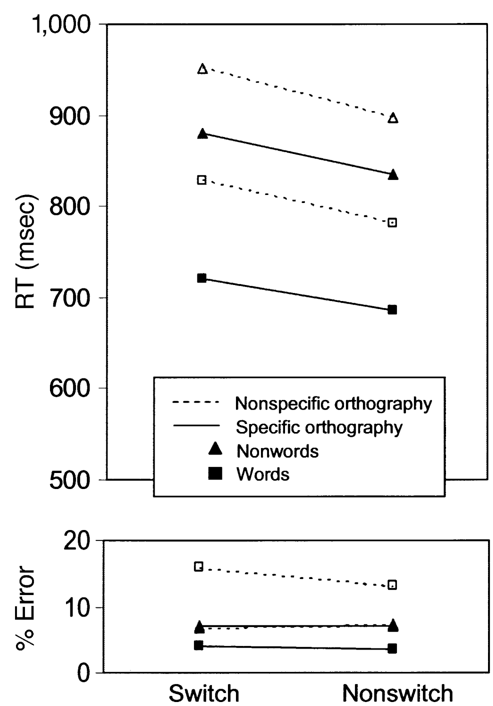

Figure 4. Effect of orthographic specificity in Experiment 2. The switch cost for stimuli with language-specific orthography was similar to that for nonspecific orthography now that the two types of stimuli were intermingled in the same blocks.

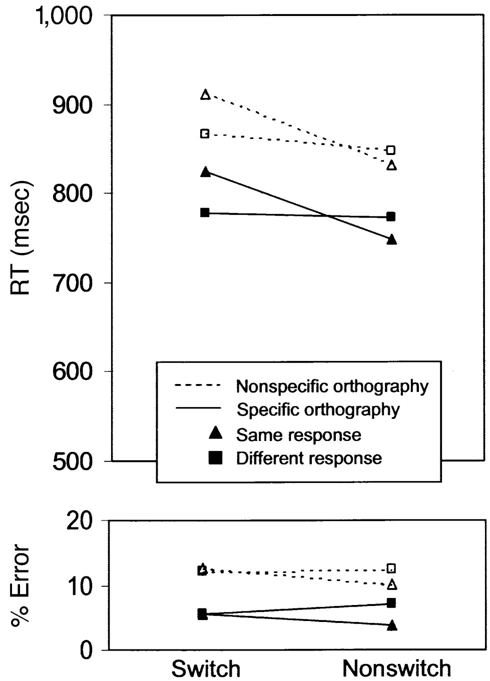

Figure 5. Effect of response repetition in Experiment 2. As in Experiment 1, the switch cost was larger when the present and previous responses were the same. Word and nonword data have been combined.

repetition, and orthography: RT, $F(1,23) = 0.05$, $MS_e = 5,637$, $p > .5$; errors, $F(1,23) = 0.01$, $MS_e = 70$, $p > .9$]. One further interaction of some a priori interest was a potential carryover effect of the orthography of the previous trial. When the previous trial had a nonspecific stimulus, RT was possibly slightly raised on switch trials (852 vs. 837 msec), but there was no effect on nonswitch trials (801 vs. 798 msec). However, the interaction of switch and previous trial orthography was not significant [$F(1,23) = 2.0$, $MS_e = 5,818$, $p = .17$].

Other effects. Among all the unplanned comparisons, no interactions with switch cost were significant to a criterion of $p < .01$. In fact, the only further interaction to meet this criterion in RT (from 26 possible interactions) was lexicality × response repetition [$F(1,23) = 11$, $MS_e = 16,281$, $p < .01$], such that repeating a response slightly lowered RT overall for words but had the opposite effect overall for nonwords, and since lexicality is confounded with response anyway (one button for *word*, one for *nonword*), this effect could result purely from response processes. In error rate, the only further interaction to meet the criterion was orthography × lexicality: There were more errors on nonspecific words than on specific words, but the error rate for nonwords hardly differed [$F(1,23) = 91$, $MS_e = 172$, $p < .001$].

Comparison Between Experiments 1 and 2

In order for Experiments 1 and 2 to be directly compared using a within-subjects ANOVA, the same participants performed both experiments in a counterbalanced order. As stated above, intermingling the language-specific and nonspecific stimuli in Experiment 2 had the effect of raising the switch cost for the specific stimuli, compared with Experiment 1 (cf. Figures 2 and 4). This interaction of experiment, orthography, and switch was significant [$F(1,22) = 5.0$, $MS_e = 876$, $p < .05$]. Baseline RT was not the same for all conditions, so we checked that this interaction was unaltered when, instead of using the absolute RT for switch and nonswitch trials, the switch cost in each condition was calculated as a percentage of nonswitch RT for that condition [$F(1,22) = 4.9$, $MS_e = 22$, $p < .05$]. In addition, the interaction was not affected by the order in which each participant did the experiments [whether the switch cost was calculated as the absolute or proportional difference in RT: $F(1,22) = 1.2$; $F(1,22) = 0.7$]. Analyses of each experiment individually also showed that experiment order had no effect on any interaction of interest. Thus, factors such as stimulus repetition or practice at the task did not affect the pattern of results, and we can confirm that the effect of orthography on switch cost was different in each experiment.

Discussion

The main finding from Experiment 2 was that the reduction in switch cost for language-specific orthography, found in Experiment 1, no longer existed. This must be the result of intermingling the language-specific stimuli with nonspecific stimuli. It is difficult to attribute the rise in switch cost for specific stimuli to any other factor, given that Experiments 1 and 2 were alike in virtually every other way, used the same subjects in a counterbalanced order, and the order in which each subject performed the experiments did not affect the pattern of results.

What does this tell us about the possible sources of language switching costs? If higher RT on switch trials was mainly caused by extra interference from lexical representations of the wrong language, language-specific stimuli ought to always produce considerably less interference, and thus smaller switch costs (even if the presence of language-nonspecific stimuli raises the level of interference globally). In addition, on the basis of the word-recognition account, the presence of nonspecific stimuli should help reduce switch costs for specific stimuli, because we predicted higher RT for nonswitch trials that followed nonspecific stimuli and lower RT for switch trials that followed nonspecific stimuli. The same arguments apply if a major source of language switch costs was extra interference between spelling–sound mappings, for example, instead of lexical representations.

On the other hand, the disappearance of any major difference between the switch costs for specific and nonspecific stimuli in the intermingled blocks can readily be explained if processes at the task schema level caused the difference between the specific and nonspecific blocks in Experiment 1. This might be because subjects could rely on the stimulus itself to cue a change of language in specific blocks but not in nonspecific or intermingled blocks. Alternatively, it might be because in nonspecific and intermingled blocks, but not in specific blocks, subjects actively inhibited the language (or language schema) that was not relevant, in order to reduce interference. Note that although the source of the inhibition is thus outside the word-recognition system, the recipients of the inhibition need not be. For example, the Bilingual Interactive Activation model (Dijkstra & van Heuven, 2002; van Heuven et al., 1998) allows top-down inhibition of all lexical items in a nontarget language from a "language node."

The task schema account also readily accommodates the clue from analyzing the effect of orthographic specificity of the previous trial. On the basis of the word-recognition source for switch costs, we predicted longer RTs for nonswitch trials that followed nonspecific stimuli and shorter RTs for switch trials that followed nonspecific stimuli. On the basis of the task schema account, we predicted either no effect of previous stimulus or longer RTs on switch trials that followed nonspecific stimuli because of inhibition. There was a hint of the latter effect in our data, although it could not be reliably distinguished from the no-difference situation. Either way, this is more consistent with the task schema predictions than with the word-recognition predictions.

GENERAL DISCUSSION

The task schema account of language switch costs has previously been supported by two main findings. Thomas and Allport (2000) found that French and English language-specific stimuli did not reduce the language switch cost. von Studnitz and Green (2002a) found that the language switch cost interacted with response repetition. We have extended these results in three main ways, finding that (1) the switch cost *is* markedly reduced in blocks of language-specific stimuli that are specific at the letter level; (2) the effect of language specificity is modulated by the context (i.e., in blocks that also contain nonspecific stimuli, switch cost for the specific stimuli is increased); and (3) these effects of stimulus specificity do not interact with the effect of response repetition. We therefore conclude that there must be at least two separate sources of language switch costs.

Since Experiments 1 and 2 found a similar-sized interaction between response repetition and switch cost, and this effect was not influenced by the orthographic specificity of the stimuli, we conclude that one source of language switch costs is at the level of initiating responses, or mapping decisions to them (von Studnitz & Green, 2002a), and this is not affected by stimulus specificity or the arrangement of such stimuli into separate or intermingled blocks. Experiment 1 showed that another contribution to language switch costs *is* affected by stimulus specificity, which would be consistent with a source within the word-recognition system. However, we argued that the effect of stimulus specificity is also consistent with a task schema account because it is known from task switching studies that univalent stimuli produce smaller switch costs than do bivalent stimuli (e.g., Allport et al., 1994; Dreisbach et al., 2002; Rogers & Monsell, 1995; Sumner & Ahmed, in press). Experiment 2 showed that the stimulus-sensitive contribution to language switch costs is also sensitive to the arrangement of stimuli within blocks, which we argued favored the task schema account. The tentative clue from the analysis of previous trial orthography also favored the task schema account. Finally, if the switch cost were due mainly to reduced interference in word recognition, we might expect further reduction in interference in the second and third nonswitch trials (when the run length was 4) and therefore further reduction in RT. We found no evidence for this further improvement of performance in the nonswitch trials, which is consistent with the task schema account, because this has been found for predictable switching between other tasks (Monsell et al., 2003; Rogers & Monsell, 1995; Sumner & Ahmed, in press).

We therefore support the positioning of language switching within the framework of task switching in general (Green, 1998a; Thomas & Allport, 2000; von Studnitz & Green, 2002a) and conclude that switching between language schemas causes costs from at least two independent sources, one at the level of response initiation and one at an earlier level that is sensitive to stimulus properties and context via their implications for strategic control. The latter source could result from inhibition applied to the competing language schema, which might be adjusted in a "just enough" manner (Goschke, 2000; Monsell et al., 2003; Yeung & Monsell, 2003).

Our results therefore seem to provide more evidence that task schemas play an important role in the use of language (e.g., Dijkstra & van Heuven, 2002). But it is worth asking to what extent are language switch costs relevant during natural word recognition or speech. Thomas and Allport (2000, p. 62) suggested that there may be no costs in natural language use, but other authors do not agree (e.g., von Studnitz & Green, 2002a, p. 249). We have shown that switch costs can be affected by the experimental context, but this does not mean that they are produced only in experimental contexts. Language switch costs have been measured not just in decision experiments like ours, but also in naming experiments (Costa et al., 1999; Hernandez et al., 2001; Jackson et al., 2001; Meuter & Allport, 1999) and reading (Macnamara & Kushnir, 1971), and even when there is just a change in script but not language (Shafiullah & Monsell, 1999). *Code switching* (the use of different languages in the same utterance) also carries some cost (Grosjean, 1995; Li, 1996), and interestingly the cost was found to be reduced when the code-switched word carried language-specific phonetics compared with when it was phonetically neutral. Since language switch costs occur in a variety of paradigms, they are likely to have relevance for natural language processing. This, in turn, is likely to be because all uses of language are associated with language schemas of one sort or another, and if the language changes, the schema must change. Thus, we speculate that switch costs may be relevant not only for bilinguals, but also for monolinguals switching between, for example, a polite schema and a slang schema or between technical and colloquial schemas.

For the study of task switching and cognitive control processes, we can go on to ask what implications our results, and other potential studies of language switching, could have for theories of task switching. First, and most straightforward, if the difference found in Experiment 1 between the switch cost for specific and nonspecific stimuli is equivalent to the difference between univalent and bivalent stimuli in other tasks, we can predict that the latter difference should be markedly reduced if the stimuli are intermingled within the same block. Second, we might be able to take advantage of some of the differences between language switching paradigms and other task switching paradigms in order to tease apart possible contributions to switch costs. There are two main differences between the paradigms, one concerning response mappings and one concerning stimuli: (1) In language switching, the responses mean the same thing for the two languages (e.g., *word* always maps to one button, and *nonword* always maps to the other), whereas in task switching a remapping or reactivation of response codes is normally necessary at a task switch (e.g., if the tasks are classifying digits as *odd/even* and *high/low*, in one task *odd* may map to button A and *even* to button B, but in the other task *high* may map to A, B, or some other button C). A component of switch costs may be due to the retrieval from memory of such response codes (see, e.g., Mayr & Kliegl, 2000), but this component cannot contribute to the switch cost in language switching. (2) In task switching paradigms, a small set of stimuli is usually repeated many times, whereas in language studies there is usually a very large set of stimuli, which are rarely or never repeated. This means that language switch costs cannot arise from interference from the kind of direct stimulus–response associations that arise when a small set of stimuli are used repeatedly. Nor can language switch costs arise from inhibition invoked to suppress such interference. A major puzzle in task switching is why patterns of task set interference suggested by patterns of RT do not always conform to interference patterns indicated by *congruency effects* (the difference between performance when the stimulus would map to the same response in either task and when the stimulus maps to different responses; see Monsell et al., 2003; Sumner & Ahmed, in press). This may be explained if two sources of interference contribute to congruency effects: interference between task sets and interference between direct stimulus–response associations. The latter should be reduced for larger stimulus sets. Arrington & Logan (2004) have recently found that switch costs were not affected by the number of times a stimulus was previously presented. It would be interesting to see whether congruency effects, however, are affected.

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(*Continued on next page*)

APPENDIX

Words			
Greek		English	
Nonspecific	Specific	Nonspecific	Specific
AKAIPH	ΑΔΕΙΑΖΕ	ABATE	ABLE
AKOMA	ΑΓΑΘΟ	ABBEY	ADVICE
AKOYNE	ΑΓΩΝΙΑ	ANATOMY	AGE
AKYPE	ΑΘΛΙΕ	ANIMATE	AGENCY
ANETA	ΑΘΩΑ	ANNOY	ALREADY
ANEY	ΑΙΩΝΑΣ	ANXIETY	ALSO
ANIAPE	ΑΛΕΠΟΥ	ANYONE	APPLE
ANIATH	ΑΛΛΟΥ	ANYTIME	ARMY
ANTE	ΑΛΟΙΦΗ	APPETITE	AWARE
APTIA	ΑΝΟΙΞΗ	BABY	AWAY
AYTON	ΑΠΟΨΗ	BAKE	BASE
BAPETA	ΑΦΙΞΗ	BANANA	BEACH
BAPH	ΒΛΕΠΟΥΝ	BANK	BEFORE
BAPY	ΒΛΕΦΑΡΟ	BAPTIZE	BIRTH
BOH	ΒΟΛΤΑ	BENEATH	BIRTHDAY
BOMBA	ΓΛΩΣΣΑ	BIKE	BOUNDARY
BOTANOY	ΓΝΩΜΗ	BIKINI	BUSY
EBAZE	ΓΡΑΦΕΙΟ	BITE	COPY
EMENE	ΔΙΨΑ	BONE	CRAZY
ENOPIA	ΔΙΩΞΗ	BOTH	CURE
ENTIME	ΕΒΓΑΖΕ	BOY	DEBATE
ENTONE	ΕΦΟΔΙΑ	EMANATE	DISH
ENTYNAN	ΕΚΘΕΣΗ	ENEMY	EARTH
ETAZE	ΕΞΗΓΩ	ENTITY	EDGE
ZAPI	ΕΣΠΑΓΑΝ	ENZYME	ELEVATE
KAMIA	ΖΕΣΤΗ	EPITAPH	ENJOY
KANENA	ΖΗΛΙΑ	EXAMINE	EXAMPLE
KANOYN	ΖΥΓΙΖΑ	EYE	FAITH
KAPY	ΗΓΕΣΙΑ	IMITATE	FISH
KATA	ΗΠΙΟΤΑΤΕ	INITIATE	FIVE
KAYTH	ΗΣΥΧΙΑ	INTAKE	GLOVE
KEPIA	ΘΛΙΨΗ	KEYNOTE	GROWTH
KOITH	ΘΥΕΛΛΑ	KITTY	HARDWARE
KOMMA	ΙΣΑΞΙΑ	MAKE	HEALTH
KOPH	ΚΑΘΕΤΗ	MANY	IGNORE
KOYTH	ΚΑΘΗΚΟΝ	MATE	IMAGE
KYPIE	ΚΑΜΨΗ	MAXIMIZE	IMPLY
MATI	ΚΑΡΦΩΝΑΝ	MAYBE	INSIDE
MATIA	ΚΑΤΩΤΑΤΕ	MEAN	INVOLVE
MAYPO	ΚΕΦΙ	MEMO	ISSUE
MAXONTAN	ΛΑΜΨΗ	MINE	JUNE
MEPIA	ΛΕΞΗ	MINIMIZE	JUSTIFY
META	ΛΕΦΤΑ	MONEY	KNIFE
MONO	ΜΑΘΗΤΗ	MONKEY	LICENSE
MYTH	ΜΕΛΕΤΗ	MONOTONY	LOUDLY
NATH	ΜΟΡΦΩΣΗ	MONTH	MACHINE
NAZI	ΝΤΡΟΠΗ	NINETY	MAJOR
NEOTATE	ΝΩΡΙΣ	NINTH	MOUTH
NEPO	ΞΥΡΑΦΙ	NOMINATE	NERVE
NIOTH	ΟΧΘΗ	NONE	NURSE
NOMIZE	ΟΜΙΧΛΗ	NOTE	OBLIGE
NOTA	ΟΠΛΑ	OATH	OFFICE
NOTIA	ΠΑΡΕΛΘΟΝ	OBEY	ORANGE
ONOMA	ΠΕΡΙΠΟΥ	ONE	ORGANIZE
OPATE	ΠΡΩΤΑ	ONTO	PAGE
OPIA	ΡΑΓΔΑΙΕ	OPTIMIZE	PHASE
OTAN	ΣΙΩΠΗ	PATH	POVERTY
PABOYN	ΣΚΕΨΗ	PENNY	QUITE
PIZA	ΣΤΑΘΜΗ	PHONE	REALIZE

APPENDIX (Continued)

Words			
Greek		English	
Nonspecific	Specific	Nonspecific	Specific
TAXY	ΤΑΞΙΔΙ	PINE	REFUSE
TAXYTATE	ΤΙΜΩΡΙΑ	PIPE	SCIENCE
TETOION	ΤΡΟΦΗ	PITY	SWEET
TIMANE	ΤΣΙΓΑΡΟ	POTATO	TAXI
TIMIE	ΥΠΟΘΕΣΗ	TAKE	TRANSFER
TIMIOTATE	ΥΠΟΨΙΑ	TAPE	TRUE
TINAZAN	ΥΦΑΣΜΑ	TEA	UNCLE
TOKIZAN	ΦΑΓΗΤΟ	TIME	UTILIZE
TOMEA	ΦΑΡΔΥ	TIPTOE	VALUE
TONIZE	ΧΡΩΜΑ	TOMATO	VILLA
TOYTH	ΨΑΛΙΔΙ	TYPE	WAKE
XAMENE	ΨΥΧΗ	ZENITH	WASH
XAPH	ΩΣΤΕ	ZONE	ZERO

Nonwords			
Greek		English	
Nonspecific	Specific	Nonspecific	Specific
ANEMA	ΑΒΛΟ	AIME	ABELITY
ANEMONE	ΑΙΞΑ	AMENABE	ACHAVE
ANOXE	ΑΜΠΑΛΙ	ANAZE	ADJEST
ANTETH	ΑΝΟΨΙΕ	ANNOI	ALWY
ANTIPH	ΑΞΙΘΩ	ANTIBOTE	ANGE
APATH	ΑΠΟΓΙΑ	ATHATE	ARGOY
APIME	ΑΠΕΘΙ	ATIKE	ASTE
ATOME	ΑΡΝΑΔΙΑ	AZONE	ASTIDE
AXONIA	ΑΡΩΤΗ	BAITH	AUTHITY
BAITH	ΑΣΠΗΣΗ	BATTEPH	AVAGE
BAME	ΑΧΩΜΑ	BETH	BALACE
BETINE	ΑΨΕΡΗ	BINA	BEFARE
BIMATE	ΒΑΔΕΖΑ	BIPTH	BIDGE
BINATH	ΒΕΛΙΤΑ	BOITOYN	BOADY
BOBE	ΒΙΖΩ	BOMITE	BRAGE
BOMY	ΒΡΕΙΦΑ	BOXA	BRAIVE
BOYTH	ΓΑΝΔΙ	EATH	CACE
EBIZE	ΓΕΔΟΙΕ	EMANTE	COTAGY
EMATE	ΓΛΑΦΟΥΝ	EMOTIO	DEGRE
EMBYO	ΔΕΨΙΑ	ENZOY	DIVIXE
ENOTH	ΔΙΑΠΑΖΕ	ENZY	DOWE
ENTAPH	ΔΩΡΙΑ	ETANITY	ENARGY
ENTOTE	ΕΚΡΟΞΗ	ETATE	EQUA
EPAZE	ΕΛΑΓΕ	ETIMATE	EXTATE
EPITOME	ΕΝΩΣΤΗ	EXPAZE	FACITY
EPTAZE	ΕΥΩΧΝΑ	EXTEME	FANTIZE
ETOMA	ΖΕΥΤΕΣ	IMETIATE	FINASH
ZAPH	ΖΩΑΣ	INAME	GOILE
ZEBA	ΖΩΜΟΤΑΤΕ	INTA	HAGE
ZIMA	ΗΛΑΓΙΑ	IPITATE	HAWE
ZOMATA	ΗΘΟΚΗ	IPONY	IMPLIZE
INAKE	ΘΡΑΛΙΟ	ITEMO	INJICTE
INOXIA	ΘΥΚΩΝΟΥΝ	KNITE	INVOY
KAIBE	ΙΓΔΙΑ	MANAZE	ISALATE
KATH	ΚΑΠΥΤΗ	MANIKE	IVIDE
KEMA	ΚΛΕΙΞΙΑ	MANTH	JUSA
KETH	ΚΛΥΡΗ	MEMOTY	KLEE
KINENE	ΚΟΡΦΙΑ	MENIA	KNOFE
KOKAPO	ΛΑΘΟ	MENTIOY	LASE
MAZE	ΛΕΣΠΗ	MINOPTY	LIVA
MAKY	ΛΙΓΓΟΥ	MITAKE	MADDLE
MANE	ΜΕΤΟΨΥ	MIXA	MATLE

APPENDIX (Continued)

Nonwords			
Greek		English	
Nonspecific	Specific	Nonspecific	Specific
ΜΑΡΗ	ΜΗΓΕΛΕ	MOMENA	MIFRATE
ΜΕΤΟΥ	ΜΠΑΔΑ	NATIZE	MIVORITY
ΜΟΝΑΤΗ	ΜΨΤΩΓΙΑ	NAYPO	NIRTH
ΜΟΡΗ	ΝΟΜΑΖΕ	NITE	NIXE
ΜΥΟΡΙΑ	ΝΟΨΦΑΠΟ	NONTE	OBSARVE
ΜΥΡΗ	ΝΩΛΗ	NOTH	OMLIA
ΝΑΙΜΕ	ΞΕΔΥΤΟ	OPINATE	OSTIZE
ΝΑΡΗ	ΟΔΑΓΙΑ	OPTATE	OWDER
ΝΕΒΙΖΕ	ΟΦΕΛΟΥ	PAITH	PHROSE
ΝΕΜΕΤΗ	ΟΞΥΤΑΡΗ	PAPE	POITRY
ΝΕΟΝΑ	ΠΑΘΑΣΗ	PATE	POLACY
ΝΙΧΟΝΕ	ΠΑΝΕΔΙΑ	PATIENE	PRAVIDE
ΝΟΤΥ	ΠΡΑΘΥΡΟ	PAZE	QUESTIA
ΟΜΑΝ	ΡΑΓΕΖΑ	PIATY	QUIK
ΟΜΙΝΙΑ	ΡΙΨΩ	POINTE	REGIO
ΟΝΟΜΑΤΕ	ΡΟΤΥΔΑ	POMOTE	REMAZE
ΡΑΤΕΒΟΥ	ΣΚΟΤΕΞΙ	PONITE	STAZE
ΡΑΤΙΖΑΝ	ΣΤΑΘΡΗ	TAIZE	SUGIVE
ΡΙΑΝΟ	ΤΑΙΝΙ	TANE	SUTAPH
ΡΙΤΗ	ΤΕΛΟΞΑ	TAPH	TEWN
ΤΑΒΑΤΕ	ΤΙΠΑ	TENTIO	THARE
ΤΑΒΙΖΕ	ΤΣΑΚΑΤΕ	TEXTINE	TIMPH
ΤΑΒΟΥΝ	ΤΣΕΠΟΥ	TIKY	ULTITATE
ΤΑΡΑΧΕ	ΥΠΑΓΕΙΟ	TITE	UNDE
ΤΕΧΑΝΕ	ΥΠΟΦΤΕ	TOATH	VEACLE
ΤΟΜΕ	ΦΙΛΥ	TOIPY	VOYOJE
ΤΥΡΟ	ΦΛΑΙΓΑ	TOTAY	WHETH
ΧΑΚΕ	ΧΕΦΤΑ	ZEPH	WINDE
ΧΑΝΥ	ΨΩΤΙΑ	ZOME	ZEALIZE
ΧΕΝΤΗ	ΩΣΠΟ	ZYME	ZEAR

(Manuscript received November 10, 2003;
revision accepted for publication June 18, 2004.)