What absent switch costs and mixing costs during bilingual language comprehension can tell us about language control.

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Abstract

In the current study, we set out to investigate language control, which is the process that minimizes cross-language interference, during bilingual language comprehension. According to current theories of bilingual language comprehension, language-switch costs, which are a marker for reactive language control, should be observed. However, a closer look at the literature shows that this is not always the case. Furthermore, little to no evidence for language-mixing costs, which are a marker for proactive language control, has been observed in the bilingual language comprehension literature. This is in line with current theories of bilingual language comprehension, as they do not explicitly account for proactive language control. In the current study, we further investigated these two markers of language control and found no evidence for comprehension-based language-switch costs in six experiments, even though other types of switch costs were observed with the exact same setup (i.e., task-switch costs, stimulus modality-switch costs, and production-based language-switch costs). Furthermore, only one out of three experiments showed comprehension-based language-mixing costs, providing the first tentative evidence for proactive language control during bilingual language comprehension. The implications of the absence and occurrence of these costs are discussed in terms of processing speed and parallel language activation.

Keywords: Bilingualism; Language comprehension; Language control; Switch costs; Mixing costs

Public significance statement: This study indicates that switching languages, during bilingual language comprehension, is not always more difficult than staying in the same language. This indicates that bilinguals do not necessarily need to control for cross-language interference during bilingual language comprehension.

Introduction

When bilinguals speak or comprehend a language, the other language is seemingly also activated (e.g., Costa, Caramazza, & Sebastian-Galles, 2000; Lauro & Schwartz, 2017; Spivey & Marian, 1999; Thierry & Wu, 2007; Van Heuven, Dijkstra, & Grainger, 1998). This parallel activation causes cross-language interference, which is resolved by language control. Studies investigating production-based language control have provided convincing evidence for the implementation of language control (for reviews, see Abutalebi & Green, 2007; Declerck & Philipp, 2015; Kroll, Bobb, Misra, & Guo, 2008). It is assumed that language control also inevitably occurs during bilingual language comprehension and might even rely on the same underlying mechanism as during bilingual language production (Grainger, Midgley, & Holcomb, 2010), with the only difference being how they are triggered (exogenous vs. endogenous, respectively). An example of when language control could be necessary during bilingual language comprehension would be when reading an English book as a French-English bilingual. It is assumed that any unintentional activation of French words (e.g., due to interlingual neighborhood words; Meade, Midgley, Dijkstra, & Holcomb, 2018; van Heuven et al., 1998) that occur during reading will be resolved by language control, so that these non-target language words would not interfere with the comprehension of English. This assumption of language control occurring during bilingual language comprehension is mainly based on the observation of language-switch costs with language comprehension tasks. However, a closer look at the literature indicates that the evidence for this claim might be less conclusive than previously thought. In the current study, we set out to further investigate comprehension-based language control by examining language-switch costs and language-mixing costs with comprehension-based language switching tasks.

Language-switch costs during bilingual language comprehension

The comprehension variant of the language switching task (for a review, see Declerck & Philipp, 2015) typically consists of visually presented words of two languages (see Figure 1 for an example of a trial sequence with a comprehension-based language-switching task). The task of the bilingual participants is then to categorize each word (e.g., does the number word represent an odd or even number) by pressing one of two buttons. Since words from more than one language are presented across trials, a word might be in the same language (repetition trial) or the other language (switch trial) as the previous word. It has been reported that performance is worse when the current word is in a different language than the prior word, relative to when it is in the same language. This performance decrease is called "language-switch cost" and is considered a marker of reactive language control (e.g., Declerck & Philipp, 2015; Green, 1998), which is the control process that is implemented when the non-target language disrupts the selection of target language words. Several language switching studies have found language-switch costs with a lexical decision task (Orfanidou & Sumner, 2005; Thomas & Allport, 2000; Von Studnitz & Green, 1997), a semantic categorization task (Declerck & Grainger, 2017; Macizo, Bajo, & Paolieri, 2012; Von Studnitz & Green, 2002), and a number categorization task (Hirsch, Declerck, & Koch, 2015; Jackson, Swainson, Mullin, Cunnington, & Jackson, 2004). Furthermore, language-switch costs have also been observed in the context of sentence comprehension in a picture-sentence matching task (Philipp & Huestegge, 2015), a silent reading task (Dussias, 2003), and with a visual world task (Olson, 2016).

--Figure 1--

These language-switch costs can be explained by different models of bilingual language comprehension. According to the Bilingual Interactive Activation model (BIA; Grainger & Dijkstra, 1992; van Heuven et al., 1998) and its developmental variant (BIA-d; Grainger, Midgley

& Holcomb, 2010), comprehension-based language control starts with the activation of a word representation (e.g., “dog”) that automatically activates its corresponding language node (English in the example used before), which is a mental representation of language membership. This language node, in turn, will inhibit all word representations of the other language (e.g., “porte”, which means door in French).

In terms of language-switch costs during bilingual language comprehension, this would mean that when a word is being processed its corresponding language node is activated. In turn, this language node inhibits all words that do not belong to this language. When the next word is part of the language that was inhibited on the previous trial (i.e., switch trial), performance will be worse. When, on the other hand, the same language is used in two succeeding trials (i.e., repetition trial), no such inhibition effect should occur. Thus, since there is inhibition during switch trials, but not during repetition trials, language-switch costs should be observed.

The BIA+ (Dijkstra & van Heuven, 2002; van Heuven & Dijkstra, 2010), on the other hand, proposes that language control does not occur during language processing (called the “word identification system” in the BIA+), but after language processing by the task/decision system, which is an executive control system. More specifically, in the word identification system, orthographic, phonological/articulatory, and semantic processing occurs. Output from the word identification system is put forward to the task/decision system, where executive control processes and processes related to the task are executed. In the instance of language control this could, for example, occur through altering the language-specific recognition thresholds.

The BIA+ accounts for language-switch costs differently depending on the task. When the task requires no access to language-specific knowledge (e.g., semantic classification task), it is assumed that there is an adaptation of the recognition thresholds for one or both languages when switching a language. Hence, when a different language is used in trial n, the recognition thresholds will be adjusted in favor of this language. In turn, when a different language has to be used in the following trial (i.e., switch trial), performance will be worse than when the same language has to be used as in the previous trial (i.e., repetition trial). Additionally, in the context of tasks that require no access to language-specific knowledge, there should be attention switching of the stimulus-response mappings. More specifically, participants need to verify the connection between the stimulus and response when changing the language, but not the response (e.g., responding in two consecutive trials with a right button press), which should lead to language-switch costs.

When the task requires knowledge about the specific languages at hand (i.e., language-specific lexical decision task, where the bilinguals indicate whether a word belongs to language X or Y), there is also the possibility of adapting the language-specific recognition thresholds. Additionally, a language switch with such a task also requires a switch in response, which should also affect the language-switch costs.

From this short overview of the literature, it would seem that comprehension-based language-switch costs are a robust finding. This is further enforced by the models described above, whose architecture accounts for language-switch costs. However, when we take a closer look at the results obtained in language switching studies that investigated bilingual language comprehension, it appears that this might be an oversimplification of the actual state of affairs. Several comprehension studies that relied on manual responses only found language-switch costs in some conditions (Bultena, Dijkstra, & van Hell, 2015; Declerck & Grainger, 2017; Hirsch et al., 2015; Hut, Helenius, Leminen, Mäkelä, & Lehtonen, 2017; Jackson et al., 2004; see Struys, Woumans, Nour, Kepinska, & Van den Noort, 2018 for no overall language-switch costs when taking participants and items into consideration as random factors in the analysis). Furthermore, there are also studies that observed a reversal of language-switch costs (i.e., language-switch benefit) during bilingual language comprehension when a different manual response had to be given across two trials (Orfanidou & Sumner, 2005; Thomas & Allport, 2000; Von Studnitz & Green, 2002). When the same manual response had to be given on trial n-1 and trial n (e.g., press of a right button when the presented number word refers to an odd number), these studies indicate that the expected language-switch costs are observed on trial n.

The absence of language-switch costs in bilingual language comprehension studies has not been restricted to studies measuring manual responses, but also consists of studies measuring eye movements (Dussias, 2003; Olson, 2016; Philipp & Huestegge, 2015). As a matter of fact, most studies that investigated language switching with eye movements did not provide ample evidence for language-switch costs. Philipp and Huestegge (2014), for example, investigated comprehension-based language switching with eye movements by employing a picture-sentence matching task. The results showed no significant language-switch costs in sentence comprehension, sentence reading times, and fixation times with eye movements. However, they did observe language-switch costs for the percentage of regressions with eye movements.

It should be noted at this point that not observing switch costs in domains other than bilingual language comprehension is very uncommon. Switch costs are considered to be very robust in general, with switch costs observed when switching between different tasks (e.g., Rogers & Monsell, 1995; Rubinstein, Meyer, & Evans, 2001; for reviews, see Koch, Poljac, Müller, & Kiesel, 2018; Kiesel, Steinhauser, Wendt, Falkenstein, Jost, Philipp, & Koch, 2010), when switching between different stimulus modalities (e.g., Lukas, Philipp, & Koch, 2010; Kreutzfeldt, Stephan, Sturm, Willmes, & Koch, 2015) or between different response modalities (e.g., Philipp & Koch, 2005, 2011) or between different stimulus and response modalities (e.g., Stephan & Koch, 2010, 2015), and when switching between different languages in a production task (e.g., Declerck, Koch, & Philipp, 2012; Meuter & Allport, 1999; for reviews, see Bobb & Wodniecka, 2013; Declerck & Philipp, 2015). Even when switching between cues, which indicate the task/modality/language that needs to be performed on any given trial, there is a cost relative to keeping the same cue across trials (e.g., Heikoop, Declerck, Los, & Koch, 2016; Schneider & Logan, 2005; for a review, see Jost, De Baene, Koch, & Brass, 2013). Interestingly, these latter studies have provided evidence that task- and language-switch costs can be observed over and above cue-switch costs.

In the task switching literature, several manipulations have been tested to abolish switch costs. One of the most prevalent manipulations revolves around task preparation, which is achieved by either letting participants know which task is coming up or not (i.e., predictability manipulation; e.g., Gotler, Meiran, & Tzelgov, 2003; Heuer, Schmidtke, & Kleinsorge, 2001; Koch, 2001, 2005; Ruthruff, Remington, & Johnston 2001; Sohn & Carlson, 2000) or by letting participants know which task is coming up for a short or longer time (i.e., preparation time manipulation; e.g., Altmann, 2004; Koch, 2001; Logan & Bundesen, 2003; Mayr & Kliegl, 2003; Meiran, 1996; Monsell & Mizon, 2006). While task-switch costs can be diminished by task preparation, switch costs typically remain, even with very long task preparation (however, see Verbruggen, Liefooghe, Vandierendonck, & Demanet, 2007). Several authors have also tried to abolish task-switch costs by prior training over a long period of time (e.g., Berryhill & Hughes, 2009; Stoet & Snyder, 2007; Strobach, Liepelt, Schubert, & Kiesel, 2012). Similar to the task preparation manipulation, the attempts to entirely abolish task-switch costs with this manipulation were met with little success.

In the language switching literature on bilingual language production, there have also been several attempts to abolish switch costs. Similar to the task switching literature, language preparation has been investigated (Costa & Santesteban, 2004; Declerck, Philipp, & Koch, 2013; Declerck, Koch, & Philipp, 2015; Festman & Mosca, 2016; Fink & Goldrick, 2015; Ma, Li, & Guo, 2016; Mosca & Clahsen, 2016; Philipp, Gade, & Koch, 2007; Reynolds, Schlöffel, & Peressotti, 2016; Stasenko, Matt, & Gollan, 2017). With the exception of Mosca and Clahsen (2016; see also Kleinman & Gollan, 2016, for a study that showed absent language-switch costs), most studies found that language preparation generally does not abolish language-switch costs. This holds true even when the actual response word itself, in the correct language, can be prepared (Declerck et al., 2013, 2015; Philipp & Koch, 2016).

Taken together, the previous results indicate that switch costs can be absent in domains other than bilingual language comprehension (e.g., Mosca & Clahsen, 2016; Verbruggen et al., 2007), but it is a seldom observed pattern. During bilingual language comprehension, on the other hand, switch costs seem to be less robust and possibly tied to highly specific experimental conditions. In light of bilingual language comprehension models this is surprising, as these models predict that language-switch costs should typically be observed.

Language-mixing costs during bilingual language comprehension

Next to reactive language control, there has been another type of language control identified in the literature, namely proactive language control, which entails a control process that is implemented as an anticipation, hence the term proactive, of non-target language interference disrupting the selection of words in the target language. For example, in a pure English block, French could be inhibited by a French-English bilingual throughout the block. A marker of proactive language control with the language switching task is “language-mixing cost” (e.g., Christoffels, Firk, & Schiller, 2007; Declerck et al., 2013; Gollan & Ferreira, 2009). These language-mixing costs consist of the performance decrease in repetition trials in mixed language blocks relative to trials in pure language blocks.

Models of bilingual language comprehension do not explicitly account for proactive language control. However, the models above could account for proactive language control by assuming that it relies on the same underlying mechanism as reactive language control. Within the BIA and BIA-d, this would mean that proactive language control would consist of a constant amount of inhibitory control from the language nodes. Within the BIA+, on the other hand, proactive language control could be accomplished by modifying the language-specific recognition thresholds over a longer time.

In terms of language-mixing costs, this would mean that according to the BIA and BIA-d, in pure language blocks the non-target language is proactively deactivated in order to circumvent interference from the non-target language. In the mixed language blocks, less, if any, proactive inhibitory control would be implemented. Hence, this would entail that more cross-language interference should occur in the mixed language blocks than the pure language blocks, and thus worse performance should occur in repetition trials of mixed language blocks. In the BIA+, on the other hand, the language-specific recognition threshold would be higher for the non-target language, and lower for the target language in the pure language blocks, whereas there would be little change of the language-specific recognition thresholds in the mixed language blocks. Similarly, this would lead to worse performance in repetition trials of mixed language blocks.

So far little research has gone into comprehension-based language-mixing costs. Grainger and Beauvillain (1987) investigated language-mixing costs during bilingual language comprehension, examining French-English bilinguals with a generalized lexical decision task (i.e., is the string of letters a word in either language or not). However, no evidence was observed for this marker of proactive control processes in the study of Grainger and Beauvillain (1987).

While little to no evidence has been provided for comprehension-based language-mixing costs, mixing costs are considered a very robust effect in task switching (e.g., Hübner, Futterer, Steinhauser, 2001; Koch, Prinz, & Allport, 2005; Philipp, Kalinich, Koch, & Schubotz, 2008; Rubin & Meiran, 2005) and production-based language switching (e.g., Christoffels et al., 2007; Declerck et al., 2013; Gollan & Ferreira, 2009; Ma et al., 2016; Peeters & Dijkstra, 2018; Wang, Kuhl, Chen, & Dong, 2009). Though, unlike with switch costs, little research has gone into abolishing these costs in the task switching and production-based language switching literature. One recent study that has looked into the effect of language preparation time on production-based language-mixing costs indicated that language preparation decreases language-mixing costs, but does not abolish it entirely (Mosca & Clahsen, 2016). More specifically, significant language-mixing costs were observed with a cue-to-stimulus interval of 0 ms (language-mixing costs: 160 ms) and 800 ms (language-mixing costs: 20 ms), and a trend towards significant language-mixing costs with a cue-to-stimulus interval of 500 ms (language-mixing costs: 13 ms). While Mosca and Clahsen (2016) observed language-mixing costs, a production-based language-mixing benefit has sometimes been observed when bilinguals could freely choose which language to produce in the mixed language blocks (e.g., De Bruin, Samuel, & Duñabeitia, 2018; Gollan & Ferreira, 2009). This is in line with theories that suggest that little to no control processes are necessary in dense language switching contexts that allow for free language switching (e.g., Green & Abutalebi, 2013).

Taken together, whereas mixing costs are a typical finding in the task-switching literature and production-based language-switching literature, so far little to no evidence has been observed for this effect in the bilingual language comprehension literature. This is in line with models of bilingual language comprehension, since they do not explicitly account for proactive language control, and thus do not provide an explicit explanation of language-mixing costs. However, the lack of evidence for comprehension-based language-mixing costs could be due to a lack of research. Hence, it is important to further investigate this possible effect.

Current study

Current models of bilingual language comprehension assume that language-switch costs should typically occur, even though such costs are not prevalent in the literature. Hence, in the current study we aimed to establish the conditions under which comprehension-based language-switch costs occur and under which conditions they are absent (for an overview of the experiments, see Table 1). By gaining further insight into this matter, we could further develop our theoretical and conceptual understanding of reactive language control during bilingual language comprehension.

Unlike language-switch costs, language-mixing costs are not expected according to the models discussed above, since they do not explicitly account for proactive language control. Moreover, so far there is no conclusive evidence for proactive language control in the literature. Though, it should be noted that very few studies have investigated this effect. Consequently, the lack of evidence for comprehension-based language-mixing costs might just be due to the scarcity of studies investigating it. To this end, we further investigated this cost, and thus proactive language control during bilingual language comprehension.

--Table 1--

Experiment 1

To establish the effect of language-switch costs during bilingual language comprehension, we started out by conducting a standard comprehension-based language-switching task. More specifically, half of the French-English bilinguals performed a magnitude task (i.e., larger or smaller than 5 and 6; see also Hirsch et al., 2015) with French and English number words. The other half of the French-English bilinguals performed a parity task (i.e., odd or even; see also Jackson et al., 2004) with French and English number words. We chose to implement two comprehension tasks, since this would allow us to generalize our results to a larger extent.

Method

Participants. To determine the number of participants to typically observe language-switch costs with a comprehension task, we decided to look into prior research that found significant language-switch costs (for a similar technique, see e.g., Chen & Saunders, 2018; Questienne & van Dijck, 2018; Won & Geng, 2018).[1] When we started this study, published comprehension-based language switching studies had tested between 16 (Thomas & Allport, 2000) and 26 participants (Hirisch et al., 2015). Hence, we decided to collect data from 20 participants, as this is in the range of prior research.

Thus, 20 French-speaking participants took part that spoke English as their second language (6 male, mean age = 22.2). Prior to the experiment, the participants filled in a questionnaire about their French and English proficiency and completed a French (Brysbaert, 2013) and English (Lemhöfer & Broersma, 2012) vocabulary test. The questionnaire consisted of questions about their age-of-acquisition, the average percentage of current language use, and the participants had to rate their level of speaking and reading skills in French and English on a 7-point scale, with one being *very bad* and seven being *very good* (see Table 2).

--Table 2--

Material and task. All participants were presented numbers 1-10, except 5 and 6, as written words in French and English. Half of the participants had to perform a magnitude task (i.e., indicate whether the number was larger or smaller than 5 and 6), whereas the other half performed a parity task (i.e., indicate whether the number was odd or even). The participants indicated their magnitude or parity classification by pressing the key “j” or “f” on a keyboard (the mapping of the response keys to either magnitude/parity was counterbalanced across participants).

Procedure.[2] Prior to the experiment, the instructions were presented both orally and visually, with an emphasis on speed and accuracy. Following the instructions, the participants performed a practice block of 20 trials. The following six experimental blocks consisted of 40 trials each. There was an equal amount of French and English trials in each of these blocks, both of which consisted of 50% switch trials and 50% repetition trials.[3]

Each trial started with a written word presented in the center of the screen, which stayed visible until a response was registered. After the participant´s response there was a 600 ms interval until the next written word would be presented.

Analysis. The first trial of each block, error trials, and trials following errors were excluded from reaction time (RT) analyses. Furthermore, RTs that were larger or smaller than three standard deviations from the mean (per participant) were discarded as outliers. Taking these criteria into account resulted in the exclusion of 5.5% of the data.

All data analyses were run with the lme4 package (Bates, Maechler, Bolker, & Walker, 2014) in the statistical software R (RdevelopmentCoreTeam, 2008). The RT data were analyzed using mixed-effects models (Baayen, Davidson, & Bates, 2008). The error data were analyzed using a logistic mixed model (Jaeger, 2008). Both participants and items were considered random factors, with all fixed effects varying by all random factors (Barr, Levy, Scheepers, & Tily, 2013). Finally, *t*- and *z*-values larger or equal to 1.96 were deemed significant (Baayen, 2008).

The effect sizes were calculated for a design with random participants and random items (Westfall, Judd, & Kenny, 2014; see also Brysbaert & Stevens, 2018). However, not all our models included random factors for both participants and items for each experiment, due to convergence issues (see below). Hence, we could only include the effect sizes where both participants and items were fully random. Moreover, since no residual variance is given for binomial data, we could not include effect sizes for the error rates.

The factor we were interested in was language transition (switch vs. repetition trials). We chose not to include the variable language in any of the experiments, since we did not make any hypotheses about this variable. Moreover, most comprehension-based language switching studies show that switch costs are not affected differently by the languages (Hirsch et al., 2015; Macizo et al., 2012; Orfanidou & Sumner, 2005; Philipp & Huestegge, 2015; Thomas & Allport, 2000; von Studnitz & Green, 2002). This was also the case for each of the separate experiments of the current study ($0.1 < t$s < 1.5). There was also no interaction between language and language-mixing costs in any of the experiments of the current study ($0.2 < t$s < 1.5).

Results and Discussion

The RT data revealed no significant main effect of language transition, $b = 3.10$, SD = 7.93, $t = 0.38$, $d = 0.013$ (for the means, see Table 3). As can be seen in Table 5, the error data also showed no significant effect of language transition, $b = 0.25$, SD = 0.30, $z = 0.85$.

In a separate analysis, we also found that language-switch cost, or more specifically the absence thereof, were similar for those bilinguals that used the magnitude task (language-switch costs: -1 ms) and parity task (language-switch costs: -5 ms), $b = 0.33$, SD = 13.32, $t = 0.03$.

--Table 3--

Taken together, surprisingly no significant comprehension-based language-switch costs were observed. As a matter of fact, the responses in switch trials (621 ms) were numerically slightly faster than in repetition trials (624 ms). This absence of language-switch costs could be due to a number of factors. In the following experiments, we further examine the absence of language-switch costs during bilingual language comprehension and set out to investigate the conditions under which language-switch costs during bilingual language comprehension are absent.

Experiment 2

Experiment 1 provided evidence that comprehension-based language-switch costs do not always occur. In Experiment 2, we wanted to see whether switching other characteristics than the stimulus language would also result in absent switch costs with our setup. We chose to examine task-switch costs, next to comprehension-based language-switch costs, since prior research has indicated that language switching and task switching rely, at least partially, on similar mechanisms (e.g., De Baene, Duyck, Brass, & Carreiras, 2015; Declerck, Grainger, Koch, & Philipp, 2017; Prior & Gollan, 2011; Stasenko et al., 2017). Some models have even claimed that the underlying mechanisms measured with task switching and language switching are identical (Dijkstra & Van Heuven, 2002; Van Heuven & Dijkstra, 2010; Meuter & Allport, 1999). Hence, if we found substantial task-switch costs, but still no language-switch costs in Experiment 2, it would be unlikely that the absence of language-switch costs was due to our setup.

Furthermore, we let the bilinguals switch between languages and tasks within the same blocks. This was done because prior research has indicated that task switching and additionally switching between two other factors (e.g., stimulus modality switching) can increase switch costs overall, relative to switching just between tasks (Dreisbach & Wenke, 2011; Hunt & Kingstone, 2004; Philipp & Koch, 2010; however, see Murray, De Santis, Thut, & Wylie, 2009). Hence, this might increase the chances of observing comprehension-based language-switch costs.

Method

Participants. 20 new French-speaking participants who spoke English as their second language took part in this experiment (8 male, mean age = 21.0). Prior to the experiment, they filled in the same questionnaire and vocabulary tests as in Experiment 1 (see Table 2).

Material and task. The material and task was identical to that of Experiment 1, except that all participants had to perform both the magnitude and parity task. Furthermore, to indicate which task had to be performed during a given trial, which was necessary since each stimulus could be performed with either task (i.e., bivalent stimuli), a task cue was used. More specifically, this cue was a square frame in blue or green presented around each word. The color of the frame signaled the participants to perform either the magnitude or parity task. The cue-to-task mapping was counterbalanced across participants.

Procedure. The procedure was identical to that of Experiment 1, with the main difference that the participants had to perform both the magnitude and parity task in each of the blocks. Hence, next to the words being in French for half of the trials and in English for the other half, half the trials required the participants to perform a magnitude task and the other half of the trials they had to perform a parity task. Identical to Experiment 1, half of the French and English trials consisted of switch trials and the other half consisted of repetition trials. Of these language-switch trials, half consisted of task-switch trials, whereas the other half of task-repetition trials. This was also the case for the language-repetition trials.

Each trial started with a written word presented in the center of the screen, framed by the task cue, both of which stayed visible until a response was registered. After the participant´s response there was a 600 ms interval until the next written word and cue would be presented.

Analysis. The data analysis method was identical to that of Experiment 1.[4] The exclusion criteria were also identical, which resulted in the exclusion of 8.7% of the data. However, the factors we were interested in were language transition (switch vs. repetition trials) and additionally task transition (switch vs. repetition trials).

Results and Discussion

The RT data revealed no significant main effect of language transition, b = 39.49, SD = 31.04, t = 1.27, d = 0.015 (for the means, see Table 4). We did find a significant effect of task transition, with slower responses during task-switch trials (1408 ms) than during task-repetition trials (970 ms), b = 449.00, SD = 72.44, t = 6.20, d = 0.602. The interaction was not significant, b = 55.95, SD = 42.60, t = 1.31, d = 0.085. The error data showed no significant effect of language transition, b = 0.05, SD = 0.18, z = 0.28, task transition, b = 0.18, SD = 0.18, z = 0.99, nor their interaction, b = 0.22, SD = 0.24, z = 0.90.

--Table 4--

Taken together, similar to Experiment 1, no language-switch costs were observed in Experiment 2. On the other hand, substantial task-switch costs were observed. These results indicate that even when task-switch costs can be observed, language-switch costs do not necessarily occur, even though they rely, at least partially, on the same processes (e.g., De Baene et al., 2015; Declerck et al., 2017; Dijkstra & Van Heuven, 2002; Prior & Gollan, 2011; Stasenko et al., 2017; Van Heuven & Dijkstra, 2010). Thus, it is unlikely that the absence of language-switch costs was due to our setup.

Experiment 3

In Experiment 2, we observed no language-switch costs, but we did observe substantial task-switch costs. However, it could be argued that these task-switch costs mainly occurred due to endogenous control processes, whereas language switching with a comprehension task mainly requires exogenous control processes (cf. Grainger et al. 2010). Thus, in Experiment 3 we used another type of switching instead of task switching, that relies on exogenous control processes, namely stimulus modality switching (e.g., Lukas et al., 2010; Kreutzfeldt et al., 2015), which entails switching between stimulus modalities (e.g., visual vs. auditory input) across trials.

Method

Participants. 20 new French-speaking participants who spoke English as their second language took part in this experiment (6 male, mean age = 21.9). Prior to the experiment, they filled in the same questionnaire and vocabulary tests as in Experiment 1 (see Table 2).

Material and task. The material and task was identical to that of Experiment 1, except that all participants had to perform a parity task. Furthermore, the numbers were either presented visually or auditorily.

Procedure. The procedure was identical to that of Experiment 1, with the main difference that the numbers were presented visually or auditorily in each of the blocks. Hence, next to the words being in French for half of the trials and in English for the other half, half of these numbers were presented visually and the other half auditorily in each block. Identical to Experiment 1, half of the French and English trials consisted of switch trials and the other half consisted of repetition trials. Of these language-switch trials, half consisted of modality-switch trials, whereas the other half of modality-repetition trials. This was also the case for the language-repetition trials.

Analysis. The data analysis method was identical to that of Experiment 1.[5] The exclusion criteria were also identical, which resulted in the exclusion of 9.0% of the data. The factors we were interested in were language transition (switch vs. repetition trials) and modality transition (switch vs. repetition trials).

Results and Discussion

The RT data revealed no significant main effect of language transition, b = 1.65, SD = 20.29, t = 0.08, d = 0.020 (for the means, see Table 5). We did find a significant effect of modality transition, b = 71.64, SD = 16.96, t = 4.22, d = 0.210, with slower responses during modality-switch trials (841 ms) than during modality-repetition trials (780 ms). The interaction was not significant, b = 18.62, SD = 21.05, t = 0.89, d = 0.060. The error data showed no significant effect of language transition, b = 0.18, SD = 0.18, z = 1.03, modality transition, b = 0.21, SD = 0.19, z = 1.15, nor their interaction, b = 0.25, SD = 0.27, z = 0.91.

--Table 5--

Taken together, in line with Experiments 1 and 2, no language-switch costs were observed in Experiment 3. As a matter of fact, responses in switch trials (804 ms) were even numerically slightly faster than in repetition trials (810 ms). However, substantial modality-switch costs were observed. These results indicate that even though stimulus modality-switch costs are found, language-switch costs do not necessarily occur, even though they both rely on exogenous control processes.

Experiment 4

So far, we have shown that language-switch costs do not always occur during bilingual language comprehension, while task-switch costs and stimulus modality-switch costs can be observed. Before going further, we wanted to be sure that language-switch costs could be observed with our setup and our type of bilinguals. Thus, we let French-English bilinguals, from the same participant pool as those in the previous experiments, perform a production-based language switching task. To make the method as similar as possible to Experiments 1-3, we opted for a reading aloud task, in which written words of the two languages are read out loud. Prior research has indicated that language-switch costs can be observed with such a task (Filippi, Karaminis, & Thomas, 2012; Macizo et al., 2012; Reynolds et al., 2016; Slevc, Davey, & Linck, 2016).

Method

Participants. 20 new French-speaking participants that spoke English as their second language took part in this experiment (7 male, mean age = 22.0). Prior to the experiment, they filled in the same questionnaire and vocabulary tests as in Experiment 1 (see Table 2).

Material, Task, Procedure, and Analysis. The material, task, procedure, and analyses were identical to that of Experiment 1. The only exceptions were that participants had to perform a reading aloud task, which also means that a vocal response had to be given instead of the manual responses used in the previous experiments, and that the errors were coded online by the experimenter. The exclusion criteria were also identical to those used in Experiment 1, which resulted in the exclusion of 6.3% of the RT data.

Results and Discussion

The RT data revealed a significant main effect of language transition, b = 16.08, SD = 3.86, t = 4.16, d = 0.141 (for the means, see Table 6), with slower responses in language-switch trials (573 ms) than in language-repetition trials (555 ms). The error data showed no significant effect of language transition, b = 14.57, SD = 12.89, z = 1.13.

--Table 6--

To be sure that the language-switch costs observed with the production task were significantly different from those observed with a comprehension task, we contrasted the results of Experiment 4 with those obtained in Experiment 1. The results showed a significant difference between language-switch costs, b = 19.64, SD = 9.41, t = 2.09. Importantly, the bilinguals in

Experiments 1 and 4 all came from the same participant pool, and there was no significant difference between the French-English bilinguals of Experiment 1 and 4 on any of the language questionnaire items or language vocabulary scores, *t*s < 1 (see Table 2). Hence, the switch-cost difference is unlikely to be due to any difference between the bilingual participants in Experiments 1 and 4.

Taken together, these results provide evidence that language-switch costs could be observed with our setup when producing language.

Experiment 5

So far, we did not observe any language-switch costs with a language comprehension task, which indicates that there was no substantial reactive language control implemented in Experiments 1-3. In Experiment 5, we set out to see whether, next to language-switch costs, language-mixing costs could be observed. Language-mixing costs are considered a measure of proactive language control, and there has been no conclusive evidence for this effect with a language comprehension task. However, it should be noted that very few studies have investigated this effect.

Furthermore, we used a smaller response-to-stimulus interval than in the previous experiments (cf. 300 ms instead of 600 ms). This adjustment was implemented because prior research in the production literature has shown that reducing the interval between responses can increase language-switch costs (Ma et al., 2016). So, by using a smaller response-to-stimulus interval, the chances increased to observe such costs.

Method

Participants. 20 new French-speaking participants that spoke English as their second language took part in this experiment (8 male, mean age = 20.8). Prior to the experiment, they filled in the same questionnaire and vocabulary tests as in Experiment 1 (see Table 7).

--Table 7--

Material and task. The material and task were identical to that of Experiment 1.

Procedure. The procedure was identical to that of Experiment 1, with the main difference that there would also be six pure language blocks. The pure language blocks would always be presented consecutively, as would the mixed language blocks. The order of pure and mixed language blocks was counterbalanced across participants, as was the order of pure French and pure English blocks.

Analysis. The data analysis method was identical to that of Experiment 1.[6] The exclusion criteria were also identical, which resulted in the exclusion of 8.5% of the data. However, we defined two nonorthogonal contrasts. First, we investigated language switching in mixed language blocks in the *language-switch cost contrast*. Secondly, we investigated the performance difference in the pure language blocks with repetition trials of mixed language blocks in the *language-mixing cost contrast*.

In the language-switch cost contrast, the factor we were interested in was language transition (switch vs. repetition trials), whereas in the language-mixing cost contrast, this was trial type (pure language vs. language-repetition trials).

Results and Discussion

Language-switch cost contrast. The RT data revealed no significant main effect of language transition, $b = 2.05$, SD = 7.51, $t = 0.27$, $d = 0.011$ (for the means, see Table 8). The error data showed no significant effect of language transition, $b = 0.27$, SD = 0.26, $z = 1.02$.

--Table 8--

Language-mixing cost contrast. The RT data revealed no significant main effect of trial type, *b* = 9.80, SD = 10.26, *t* = 0.96, *d* = 0.046. The error data also showed no significant effect of trial type, *b* = 0.21, SD = 0.21, *z* = 1.00.

Taken together, similar to Experiments 1-3, no language-switch costs were observed. Moreover, in line with Grainger and Beauvillain (1987), no language-mixing costs were found.

Experiment 6

So far, we have only relied on one group of bilinguals, namely French-English bilinguals. To be sure that the absence of language-switch costs and language-mixing costs was not due to this specific pairing of languages, we examined language-switch costs and language-mixing costs with French-Spanish bilinguals.

Interestingly, many French and Spanish number words are cognates (e.g., *quatre* and *cuatro*, which means four in French and Spanish, respectively), which are orthographically similar translation-equivalent words. More specifically, in this experiment seven out of eight stimuli are cognates in French-Spanish, whereas only four out of eight stimuli are cognates in French-English. We also calculated the corrected (for word length) orthographic Levenshtein distance scores, which is a measure of orthographic overlap across words between 0 (non-cognate) and 1 (full cognate) (e.g., Duñabeitia, Ivaz, & Casaponsa, 2016; Schepens, Dijkstra, & Grootjen, 2011). These scores showed that there is more orthographic overlap between French-Spanish number words (0.43) than between French-English number words (0.19). Hence, there are more cognates in the French-Spanish number words. A study from Thomas and Allport (2000) has shown that cognates can increase comprehension-based language-switch costs, which could be due to an increase in parallel language activation that in turn requires more control processes. So, the language combination of French and Spanish makes it more likely that we will find language-switch costs, and maybe even language-mixing costs.

Method

Participants. 20 new French-speaking participants that spoke Spanish as their second language took part in this experiment (8 male, mean age = 21.3). Prior to the experiment, they filled in a similar questionnaire as in Experiment 1 for French and Spanish. Similar to Experiment 1, we conducted a French (Brysbaert, 2013) and Spanish (Izura, Cuetos, & Brysbaert, 2014) vocabulary tests (see Table 7).

Material, Task, Procedure, and Analysis. The material, task, procedure, and analyses [7] were identical to those in Experiment 5. The only difference was that Spanish number words were used instead of English number words. Moreover, instead of “one” and its translation equivalent, we used “five” in this experiment, since the French word for “one” (i.e., “un”) is also a word in Spanish (i.e., “a”). This also entails that the magnitude task for this experiment was slightly different: the participants had to indicate whether the number was larger or smaller than 6 (instead of larger and smaller than 5 and 6). Using the same exclusion criteria as the prior experiments resulted in the exclusion of 9.9% of the RT data.

Results and Discussion

Language-switch cost contrast. the RT data revealed no significant main effect of language transition, b = 11.59, SD = 16.71, t = 0.69, d = 0.030 (for the means, see Table 9). The error data also showed no significant effect of language transition, b = 0.45, SD = 0.29, z = 1.53.

--Table 9--

Language-mixing cost contrast. As can be seen in Table 11, the RT data revealed a significant main effect of trial type, b = 65.31, SD = 31.08, t = 2.10, d = 0.143, with slower responses in repetition trials (715 ms) than in pure language trials (665 ms). The error data showed no significant effect of trial type, b = 0.29, SD = 0.32, z = 0.90.

Taken together, we did not observe any language-switch costs in Experiment 6. As a matter of fact, responses in switch trials (705 ms) were even numerically faster than in repetition trials (715ms). Since a different group of bilinguals was used (French-Spanish) than in the previous experiments (French-English), but with the same result, we can deduce that the lack of language-switch costs is not due to the specific combination of French and English.

However, unlike Experiment 5, we did observe language-mixing costs in Experiment 6. Since prior research has shown that larger switch costs can occur due to cognates (Thomas & Allport, 2000), it could be that larger mixing costs occurred in Experiment 6 due to more French-Spanish number words being cognates than French-English number words. However, because the current study was not specifically set up to investigate this, more research is needed to validate this claim.

Experiment 7

All of the experiments above have investigated number categorization (i.e., parity and magnitude tasks). According to the study of Von Studnitz and Green (1997), comprehension-based language-switch costs can be greatly affected by the type of task that is used. Moreover, Declerck et al. (2012) showed that number processing can instigate smaller language-switch costs than non-numbers. To generalize our findings we investigated comprehension-based language switching with another task, namely an animacy task (i.e., does the word represent a living object or not). This also entails that no number words were used, but words relating to objects, and thus we used a larger set of words than in the prior experiments (80 instead of 16 words).

Furthermore, all experiments above had 20 participants, which is similar to the amount of participants used in prior studies that investigated comprehension-based language-switch costs. To assure that the absent language-switch costs were not due to a lack of statistical power, we examined 80 participants in Experiment 7. This is the most participants tested in a comprehension-based language-switching experiment so far.

Method

Participants. 80 new French-speaking participants that spoke English as their second language took part in this experiment (19 male, mean age = 21.6). Prior to the experiment, they filled in the same questionnaire and vocabulary tests as in Experiment 1 (see Table 7).

Material and Task. Participants had to classify 40 written French words and their translation equivalent English words, none of which were cognates or contained diacritics, as a living or a nonliving object. The participants indicated their animacy classification by pressing the key "q" or "l" on a keyboard (the mapping of the response keys to the two categories [i.e., living or nonliving object] was counterbalanced across participants).

Procedure. The procedure was identical to Experiment 5, except for the following differences: Each word appeared once in each of the pure language blocks. The same words were used in the mixed language blocks, where each of the concepts was presented once in a block (i.e., the translation-equivalents never appeared in the same block).

Analysis. The data analysis method was identical to that of Experiment 1.[8] Taking these criteria into account resulted in the exclusion of 16.9% of the RT data.

Results and Discussion

Language-switch cost contrast. the RT data revealed no significant effect of language transition, b = 8.50, SD = 8.75, t = 0.97 (for the means, see Table 10). The error data also revealed no significant effect of language transition, b = 0.07, SD = 0.10, z = 0.72.

--Table 10--

Language-mixing cost contrast. The RT data revealed no significant effect of trial type, b = 6.82, SD = 10.84, t = 0.63, d = 0.022. The error data also revealed no significant effect of trial type, b = 0.03, SD = 0.06, z = 0.47.

Taken together, no language-switch costs were found with a different task and stimuli than in the previous experiments, and with a larger amount of stimuli and participants. This indicates that the absence of language-switch costs was not due to the task, the number words of the prior experiments, or the statistical power. Moreover, no language-mixing costs were observed, which is in line with Experiment 5, but not with Experiment 6.

General Discussion

In the current study, we set out to examine language-switch costs and language-mixing costs during bilingual language comprehension. Across six experiments (Experiments 1-3 and 5-7), no language-switch costs were observed with different comprehension tasks, stimuli, and different bilinguals, while we did observe task-switch costs (Experiment 2), stimulus modality-switch costs (Experiment 3), and production-based language-switch costs (Experiment 4) with the same setup.

Comprehension-based language-mixing costs were examined in three experiments (Experiments 5-7). Language-mixing costs were not observed with French-English bilinguals using a parity, magnitude, and animacy task. However, language-mixing costs were found with French-Spanish bilinguals.

Language-switch costs during bilingual language comprehension

Absent language-switch costs?

From these results we can deduce that comprehension-based language-switch costs are less robust than previously assumed. This absence of language-switch costs is along the lines of what has been observed in the bilingual comprehension literature, as most studies did not consistently observe language-switch costs (e.g., Declerck & Grainger, 2017; Hut et al., 2017; Jackson et al., 2004; Olson, 2016; Orfanidou & Sumner, 2005; Philipp & Huestegge, 2015; Struys et al., 2018; Thomas & Allport, 2000; Von Studnitz & Green, 2002). However, our results do not indicate that comprehension-based language-switch costs cannot be observed, since most published studies have observed such costs in at least some conditions (e.g., Hirsch et al., 2015; Von Studnitz & Green, 1997). What our results thus show is that comprehension-based language-switch costs are not as common as previously assumed.

This also leads to the question of whether comprehension-based language-switch costs were actually absent in our study. When taking a closer look at the average data of each of the experiments, all comprehension experiments show minimal differences between language repetitions and language switches (the largest difference was 10 ms). Furthermore, all experiments show a non-significant tendency in the RT and/or error rates towards a language-switch benefit instead of a cost. Hence, the data seem to really indicate that there are little to no language-switch costs.

We also found switch costs with three different types of switching other than comprehension-based language switching (i.e., task switching, stimulus-modality switching, and production-based language switching). So, it would seem as if there was enough power to find switch costs in general.

Further along the lines of power, in Experiment 7 we tested the highest number of participants of any comprehension-based language switching experiment (80 participants and 9600 observations per cell overall), and still found no significant language-switch costs. Moreover, in a new omnibus analysis of all participants (180 participants and 21600 observations per cell overall), we also found no significant language-switch costs (average switch costs: 3 ms), b = 5.16, SD = 4.87, t = 1.06, d = 0.008.[9] These findings provide another indication that the absence of language-switch costs was not due to a lack of power.

To statistically examine whether the comprehension-based language-switch costs were absent, we additionally ran Bayesian null hypothesis analyses (e.g., Rouder, Speckman, Sun, Morey, & Iverson, 2009) for this effect, which allows for a statistical test in favor of the null hypothesis. The results showed positive evidence in all experiments favoring the null hypothesis over the alternative hypothesis (BF_{01} = 2.99 - 3.95; Kass & Raftery, 1995). This entails that the null hypothesis was three to four times more likely to explain the data than the alternative hypothesis. We also observed positive evidence favoring the null hypothesis over the alternative hypothesis with the combined data of all 180 participants that were tested in this study (BF_{01} = 6.66). Together this indicates that comprehension-based language-switch costs were absent in the current study.

Most models have difficulty explaining the absence of language-switch costs. Within the framework of the BIA and BIA-d models, language-switch costs are assumed to occur due to activation spreading from the target word representation to its language node, which inhibits words in the non-target language. If on the next trial another language is used (i.e., switch trial) this inhibition will persist and has to be overcome, whereas this is not the case when the same language has to be processed in the following trial (repetition trial). Hence, the language inhibition during switch trials should result in comprehension-based language-switch costs according to the BIA and BIA-d. However, this is not what our data showed.

The BIA+ assumes that language-switch costs occur outside of the word identification system, namely in the task/decision system. More specifically, with number and semantic classification tasks, which were the type of tasks used in our experiments, language-switch costs occur due to an adaptation of the language-specific recognition thresholds, which should be in favor of the non-target language when switching trials, and thus make performance worse during switch than repetition trials. Moreover, language-switch costs should also occur due to a verification process of the mapping of the stimulus and response when changing languages. More specifically, participants would verify that a stimulus with a different language than the prior trial still needs the same response according to this model. This additional process, which should not occur during repetition trials, should also lead to language-switch costs. Hence, this model proposes that generally there should be a cost to language switching during bilingual language comprehension, which is not in line with our findings.

Non-crucial factors for absent language-switch costs. By generalizing the pattern over several experiments, we excluded several possibilities as to why no language-switch costs were observed with comprehension tasks in the current study. First, we used non-numeric words next to number words, since prior research has indicated that numbers can result in smaller language-switch costs (Declerck et al., 2012). Hence, the absent comprehension-based language switch costs were not due to the use of number words. Second, by using different response-to-stimulus intervals (300 ms and 600 ms), which has been linked to the size of production-based language-switch costs

(Ma et al., 2016), we made it unlikely that the absent language-switch costs are due to this specific feature. Third, since task-switch costs, stimulus modality-switch costs, and production-based language-switch costs were observed with the same setup, it is unlikely that our specific setup could account for absent language-switch costs during bilingual language comprehension. Fourth, since Von Studnitz and Green (1997) showed that language-switch costs during bilingual language comprehension were affected by the type of task, we implemented three different tasks. Because no switch costs were observed with any of the tasks, we can exclude the possibility that the absent comprehension-based language switch costs are due to the use of a specific task. Finally, we excluded the possibility that absent language-switch costs are due to a specific combination of languages by examining different bilinguals.

Response repetition effects on language-switch costs. The question now is why comprehension-based language-switch costs are so often absent. One possibility is that no language-switch costs were found due to response repetition effects on language-switch costs. When the same manual response had to be given across trials, language-switch costs typically occurred in prior studies, whereas a switch in response generally instigated a language-switch benefit (Orfanidou & Sumner, 2005; Thomas & Allport, 2000; Von Studnitz & Green, 2002). So, it might be that the language-switch benefit nullified language-switch costs, resulting in no overall language-switch costs. However, all studies that observed an interaction between response repetition and language transition also found significant overall language-switch costs (Orfanidou & Sumner, 2005; Thomas & Allport, 2000; Von Studnitz & Green, 2002), making it unlikely that this effect is the main cause for the absence of comprehension-based language-switch costs.

To make sure that the response repetition effect on switch costs did not abolish language-switch costs in our study, we reanalyzed the data (Experiments 1-3 and 5-7) with response repetition (switch vs. repetition trials) and language transition (switch vs. repetition trials) as the two main variables. The results were inconsistent. There was a significant interaction in Experiment 7, *b* = 54.9, SD = 19.2, *t* = 2.9, *d* = 0.178, with larger language-switch costs during response repetitions (language-switch costs: 34 ms, *b* = 30.6, SD = 13.3, *t* = 2.3, *d* = 0.102) than during response switches (language-switch costs: -17 ms, *b* = 16.1, SD = 13.3, *t* = 1.2, *d* = 0.056). In Experiment 3, on the other hand, we found a significant interaction in the opposite direction as that observed in Experiment 7, *b* = 49.15, SD = 18.27, *t* = 2.69, with smaller language-switch costs during response repetitions (language-switch costs: -40 ms, *b* = 13.9, SD = 15.2, *t* = 0.9, *d* = 0.127) than during response switches (language-switch costs: 19 ms, *b* = 33.2, SD = 18.2, *t* = 1.8, *d* = 0.066). However, Experiments 1 (*b* = 1.00, SD = 14.36, *t* = 0.07, *d* = 0.004), 2 (*b* = 56.11, SD = 57.51, *t* = 0.98, *d* = 0.074), 5 (*b* = 5.52, SD = 15.91, *t* = 0.35, *d* = 0.029), and 6 (*b* = 14.42, SD = 29.30, *t* = 0.49, *d* = 0.042) showed no significant interaction. An omnibus analysis of all data showed a trend for the interaction between response repetition and language transition, *b* = 18.3, SD = 9.89, *t* = 1.85, *d* = 0.046, with larger language-switch costs during response switches (language-switch costs: 4 ms, *b* = 10.1, SD = 6.5, *t* = 1.6, *d* = 0.010) than during response repetitions (language-switch costs: 1 ms, *b* = 4.4, SD = 7.7, *t* = 0.6, *d* = 0.002). This ambiguous result across experiments where no overall language-switch cost pattern was observed, together with the significant overall language-switch costs observed in studies that found an interaction between response repetition and language transition in language comprehension studies (Orfanidou & Sumner, 2005; Thomas & Allport, 2000; Von Studnitz & Green, 2002), indicates that it is unlikely that the response repetition effect on language-switch costs is the main cause for absent language-switch costs during bilingual language comprehension. However, the response repetition effect on language-switch costs could be a contributing factor towards absent language-switch costs during bilingual language comprehension

Processing speed. A plausible, at least partial, explanation for the absence of language-switch costs is that the processing speed during language comprehension tasks is quite fast. This is especially the case when compared to picture naming tasks (i.e., language production), which typically result in longer reaction times (e.g., Mosca & de Bot, 2017). Hence, it might be that the language control processes might adjust to the context (cf. Green & Abutalebi, 2013) and speed up accordingly. In turn, it would take less time to return to pre-switch activation levels in comprehension compared to production. This would consequently lead to smaller, and possibly non-existing language-switch costs in bilingual language comprehension tasks. This explanation, that language control processes are faster in comprehension, could also account for the difficulty to observe other cross-trial effects of language control in bilingual language comprehension, such as n-2 language repetition costs (Declerck & Philipp, 2018) and sequential congruency effects with a bilingual flanker task (Eben & Declerck, 2018).

Parallel language activation. Another plausible explanation for the absence of language-switch costs is that the degree of parallel language activation during bilingual language comprehension might not always instigate language control processes to the extent that language-switch costs are observed. Put differently, when little parallel language activation occurs during bilingual language comprehension, there is little to no non-target language interference on the target language and thus there is no need for control processes. In turn, no language-switch costs should be observed. When there is substantial parallel language activation, on the other hand, the non-target language should interfere to a large degree with the target language and thus control processes will be necessary to deal with this cross-language interference. In the latter case, language-switch costs should be observed. In sum, it could be that whether or not language-switch costs are observed depends on the amount of parallel language activation, which in turn determines the degree of cross-language interference that needs to be resolved.

For this account to be able to explain the results of the current study, parallel language activation should not always be substantial during bilingual language comprehension. This is in line with many comprehension studies in which little to no evidence for parallel language activation was observed in at least some conditions (e.g., Baten, Hofman, & Loeys, 2011; Blumenfeld & Marian, 2007; Dijkstra, Timmermans, & Schriefers, 2000; Grossi, Savill, Thomas, & Thierry, 2012; Lemhöfer & Dijkstra, 2004; Marian & Spivey, 2003; Mishra & Singh, 2016; Schröter & Schroeder, 2016; Schwartz & Kroll, 2006; Spivey & Marian, 1999; van Heuven et al., 1998). Furthermore, it also makes sense on a theoretical level: during bilingual language comprehension, lexical access is based on orthographic information (e.g., Dijkstra & van Heuven, 2002; Grainger & Dijkstra, 1992), which should lead to strong activation of the target language. Activation of the non-target language could then be instigated by word types such as cognates, homographs, and words that have a considerable amount of cross-language neighbors, since all of these factors would activate lexical representations of the non-target language, which consequently activate the non-target language. So, parallel language activation is not instigated by all words during bilingual language comprehension.

During bilingual picture naming, on the other hand, it is assumed that lexical access is based on conceptual information, which is assumed to activate both the target word and its translation-equivalent (e.g., Declerck et al., 2015; Green, 1998). In turn, this should lead to strong parallel language activation. The difference in parallel language activation between bilingual language comprehension and production could thus explain why a robust language-switch cost pattern is observed during bilingual language production, but not during bilingual language comprehension.

However, this does not explain why language-switch costs were observed when bilinguals had to read out loud in Experiment 4 (see also Filippi et al., 2012; Macizo et al., 2012; Reynolds et al., 2016; Slevc et al., 2016). Unlike picture naming, reading out loud does not necessarily activate the translation-equivalent, since lexical access is based on orthographic information, similar to what happens during bilingual language comprehension. This raises the question why robust language-switch costs are observed during reading out loud, whereas this is less so during bilingual language comprehension. One important reason might be that production-based language control is dependent on articulation (Philipp & Koch, 2016; Reverberi et al., 2015, 2018). Hence, we might have observed language-switch costs with reading out loud (Experiment 4) due to interference between language-specific phonemes or between the motor registers of each language, which needs to be resolved by control processes, whereas this is not the case during comprehension-based language switching.

So far, there is limited evidence in the literature that increasing parallel language activation can affect comprehension-based language-switch costs (Macizo et al., 2012; Thomas & Allport, 2000). For example, some studies have observed that low proficiency bilinguals activate the non-target language to a lesser degree (e.g., Blumenfeld & Marian, 2007; Mishra & Singh, 2016; see also Van Hell & Dijkstra, 2002). In turn, Macizo et al. (2012) found smaller language-switch costs with low proficiency Spanish-English bilinguals (mean language-switch costs: 19 ms) than with high proficiency Spanish-English bilinguals (mean language-switch costs: 129 ms).

Further evidence comes from Thomas and Allport (2000), who conducted a lexical decision task that contained cognates and non-cognates with English-French and French-English bilinguals.

Since cognates are known to result in substantial parallel language activation (e.g., Casaponsa & Duñabeitia, 2016; Dijkstra, Miwa, Brummelhuis, Sappelli, & Baayen, 2010; Hoshino & Kroll, 2008), larger language-switch costs are expected to occur with cognates than non-cognates if a high level of parallel language activation results in the implementation of language control. In line with this idea, they observed larger language-switch costs with cognates relative to non-cognates.

However, our study provides some evidence against the claim that high parallel language activation results in the implementation of language control. Most of the experiments laid out in this study used a substantial number of cognates (Experiments 1, 2, 3, 5 and especially Experiment 6). So, if parallel language activation is the decisive factor in whether language control is implemented, then we should have seen some language-switch costs in these experiments, which was not the case.

The bilingual models described above would also not predict larger language-switch costs due to increased parallel language activation. In the BIA+, parallel language activation might result in an output of some target language and non-target language words during the word identification system. These words would be fed into the task/decision system, where the language-specific recognition thresholds would presumably be adjusted in favor of both languages. As the recognition threshold for the non-target language would not be adjusted against the non-target language, which is the case when there is no parallel language activation, switching to this language in the following trial would not be difficult. Consequently, smaller language-switch costs should be observed with an increase in parallel language activation.

The BIA and BIA-d would also predict that language control would be decreased, and thus smaller language-switch cost should be obtained, with increasing parallel language activation: the higher the non-target language is activated by non-target language words, the more the target language will be inhibited. In turn, less activation will go from words in the target language to the target language node, and thus the non-target language should be inhibited less by the target language node than if there were less or no parallel language activation. Consequently, smaller language-switch costs should be obtained with increased parallel language activation.

In sum, there is some logic and evidence for the claim that parallel language activation is the driving factor behind whether language control is implemented. On the other hand, the current study provides some evidence against this claim, and current theories are not in line with the idea. Hence, it seems as if more evidence is needed to prove or disprove the claim that substantial parallel language activation leads to the implementation of language control.

Language-mixing costs during bilingual language comprehension

The language-mixing costs observed in Experiment 6 indicate that proactive language control might occur during bilingual language comprehension. However, in the other two experiments that looked into this effect (Experiments 5 and 7) we did not find significant language-mixing costs, next to Grainger and Beauvillain (1987), who also found no evidence for language-mixing costs. To further investigate the null effects in Experiments 5 and 7, we ran additional Bayesian null hypothesis analyses (Rouder et al., 2009) for this effect in these experiments, which provided positive evidence favoring the null hypothesis in Experiment 7 ($BF_{01} = 5.33$), but not in Experiment 5 ($BF_{01} = 0.62$). We additionally analyzed the combined data of Experiments 5-7, which showed significant language-mixing costs of 17ms, $b = 17.90$, SD = 8.95, $t = 2.00$, $d = 0.056$.[10] So, even though language-mixing costs have been observed in the current study, we also found evidence against language-mixing costs. This entails that we need to be careful in assuming that there is proactive language control at all during bilingual language comprehension.

To further investigate whether proactive language control is implemented during bilingual language comprehension, we examined the blocked language order effect (Branzi, Martin, Abutalebi, & Costa, 2014; Guo, Liu, Misra, & Kroll, 2011; Misra, Guo, Bobb, & Kroll, 2012; Van Assche, Duyck, & Gollan, 2013) using the pure language data. This marker of proactive language control, which has previously only been investigated with bilingual language production, reflects worse performance in a pure language block that was performed in language X when previously a pure language block was performed in another language (language Y). The assumption is that during processing of language Y in the first block, language X is inhibited. This inhibition of language X persists, and thus should affect the following block, leading to an overall worse performance of language X in the second block than when language X was presented in the first block. We investigated the blocked language order effect during bilingual language comprehension by combining the pure language data from Experiments 5-7, which resulted in an analysis with 120 participants. Yet, no significant blocked language order effect was observed.[11]

However, since the mixed and pure language blocks were counterbalanced, half the participants first performed in mixed language blocks before the pure language blocks, which could have impacted the pure language blocks (Christoffels, Ganushchak, & La Heij, 2016). Moreover, the same words were used in both the pure language blocks, which could negate the blocked language order effect (Branzi et al., 2014). To circumvent these possible confounds, we ran a novel experiment in which 58 French-English bilinguals solely performed in a pure French block and a pure English block (language order was counterbalanced across participants), both consisting of 80 trials each. To reduce any practice effects from the first to the second block, which would diminish the blocked language order effect, a different task was performed during both pure language blocks (counterbalanced across participants): an animacy task (i.e., does the presented word represent an object that is alive or not) and a size task (i.e., does the presented word represent an object that is smaller or larger than a meter). Each of these tasks had its own set of 40 non-cognate words that contained no diacritics. This experiment also showed no blocked language order effect.[12]

The absence of a blocked language order effect, together with the absent language-mixing costs in Experiments 5 and 7 of the current study and in Grainger and Beauvillain (1987) puts quite some doubt on the existence of proactive language control during bilingual language comprehension. So, more research is needed to ensure the existence of comprehension-based proactive language control, and thus whether we need to adapt current models of bilingual language comprehension to account for this process.

Further implications of the data

An important implication of our data relates to the discussion of the bilingual advantage, which is a controversial topic concerning the effect of bilingualism on executive functions (Duñabeitia et al., 2014; Stasenko et al., 2017; Von Bastian, Souza, & Gade, 2016; for a review, see Paap, Johnson, & Sawi, 2015). The idea that bilingualism leads to enhanced executive functions is based on the assumption that bilinguals need to resolve cross-language interference during language processing. Since some models assume that language control is part of executive control (e.g., Green, 1998; Meuter & Allport, 1999; Schwieter & Sunderman, 2008), bilinguals are supposed to be experts in not just linguistic, but also non-linguistic interference resolution. Our data, however, indicate that comprehension-based language control is only implemented under specific conditions. Hence, this would entail that if there is a bilingual advantage, it is mainly due to bilingual language production, because the training of executive functions during bilingual language comprehension seems to be limited.

Conclusions

Taken together, the present study revealed that language-switch costs during bilingual language comprehension do not always occur, since comprehension-based language-switch costs were not observed with different stimuli, tasks, and types of bilingual populations, even though task-switch costs, modality-switch costs, and production-based language switch costs were found. We assume that the absence of language-switch costs with comprehension tasks is due to the fast processing speed during bilingual language comprehension and/or relatively little parallel language activation that can occur in bilingual language comprehension.

The current study is also the first to observe language-mixing costs during bilingual language comprehension. However, we did not find such a cost in all experiments. The inconsistency of this effect puts further doubt on whether proactive language control is necessary during bilingual language comprehension.

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Footnotes

[1] We ran a power analysis geared toward data with random participants and items, which is in line with the data in the current study (Judd, Westfall, & Kenny, 2017). Since the power analysis of Judd et al. (2017) requires the variance of the fixed and random participant and item factors, next to the residual variance, we had to base this power analysis on previously acquired comprehension-based language switching data (i.e., Declerck & Grainger, 2017). The results showed that even an infinite number of participants would not lead to a standard power of 0.8. However, it should be mentioned that the effect size of language-switch costs in Declerck and Grainger (2017) was low (d = 0.136). While this is in line with the prior literature (see Introduction), it was not helpful to determine our sample size. Furthermore, it was not surprising to find a low effect size for comprehension-based language-switch costs, as prior research and our study has shown that it is an unstable effect.

[2] The experiments were approved by the ethics committee of the Aix-Marseille University.

[3] While switch probability within a block can have an effect on language-switch costs (for evidence in task switching, see Schneider & Logan, 2006) we do not think that it would be the main cause for not observing comprehension-based language-switch costs. This is based on the many production (e.g., Ma et al., 2016; Verhoef, Roelofs, & Chwilla, 2009) and comprehension (e.g., Macizo et al., 2012; Von Stunditz & Green, 1997) studies that used a 50% switch rate within a block, similar to the current study, that found language-switch costs. As a matter of fact, most comprehension studies used a 50% switch rate. Furthermore, production studies that use an even higher switch rate (75%) have also observed language-switch costs (e.g., Declerck, Lemhöfer, & Grainger, 2017).

[4] With regard to the error analysis, the intercept was random for the items and participants, but in the slope only task switching was random for participants. When comparing the fit of our reduced model (AIC: 2149) with a full random effects model (AIC: 2162), we found that there was no difference between the two (p = .27).

[5] With regard to the error analysis, the intercept was random for the items and participants, but in the slope only modality switching was random for participants. When comparing the fit of our reduced model (AIC: 1836) with a full random effects model (AIC: 1856), we found that there was no difference between the two (p = .72).

[6] With regard to the error switch-cost analysis, due to a convergence issue, language transition was only random for participants, but not items. When comparing the fit of our reduced model (AIC: 1390) with a full random effects model (AIC: 1392), we found that there was no difference between the two (p = .47).

[7] With regard to the error switch-cost analysis, due to a convergence issue, language transition was only random for participants, but not items. When comparing the fit of our reduced model (AIC: 1431) with a full random effects model (AIC: 1435), we found that there was no difference between the two (p = .82).

[8] With regard to the RT switch-cost analysis, due to a convergence issue, language transition was only random for items, but not participants (due to a significant loss of fit, we chose not to have language transition only random for participants). When comparing the fit of our reduced model (AIC: 217977) with a full random effects model (AIC: 217979), we found that there was no difference between the two (p = .37).

With regard to the error mixing-cost analysis, due to a convergence issue, trial type was only random for participants, but not items. When comparing the fit of our reduced model (AIC:

12709) with a full random effects model (AIC: 12779), we found that there was no difference between the two (p = .87).

[9] Because Struys et al. (2018) recently found significant comprehension-based language-switch costs with an ANOVA but not with linear mixed effects models, we also ran a non-linear mixed effects t-test with the combined data of all 180 participants. The results still showed no significant comprehension-based language-switch costs, $t(179) = 1.10$.

[10] This effect was also significant with a non-linear mixed effects t-test, $t(119) = 2.22$.

[11] Using a 2 (block order: first block vs. second block) x 2 (language: L1 vs. L2) analysis, we observed no significant difference between first (670 ms) and second block performance (659 ms), b = 16.9, SD = 20.88, t = 0.81. This effect was also not mediated by language, b = 64.09, SD = 46.59, t = 1.38. This also entails that block order of the pure language blocks had no effect on mixing costs ($t < 1$ for Experiments 5-7).

[12] Using a 2 (block order: first block vs. second block) x 2 (language: L1 vs. L2) analysis, we observed no significant difference between first (888 ms) and second block performance (900 ms), b = 75.98, SD = 54.02, t = 1.41. This effect was also not mediated by language, b = 125.88, SD = 91.26, t = 1.38.

Table 1. Overview of Experiments 1-7 with respect to bilinguals, stimuli, task(s), switch rate, Response-to-Stimulus Interval (RSI), types of switching and types of costs that were investigated.

Experiment	Bilinguals	Stimuli	Task(s)	RSI	Types of switching	Types of costs
1	French-English	Number words	Magnitude and parity	600 ms	Language	Switch costs
2	French-English	Number words	Magnitude and parity	600 ms	Language and task	Switch costs
3	French-English	Number words	Magnitude and parity	600 ms	Language and modality	Switch costs
4	French-English	Number words	Reading	600 ms	Language	Switch costs
5	French-English	Number words	Magnitude and parity	300 ms	Language	Switch and mixing costs
6	French-Spanish	Number words	Magnitude and parity	300 ms	Language	Switch and mixing costs
7	French-English	Non-numeric words	Animacy	300 ms	Language	Switch and mixing costs

Table 2. Overview of the demographic information for Experiments 1-4. The information consists of the average age-of-acquisition of both languages and the average percentage of time the participants spoke currently. Furthermore, the average self-rated scores for speaking and reading both languages is given, as is the average LexTALE scores for both languages.

	Experiment 1		Experiment 2		Experiment 3		Experiment 4	
	French	English	French	English	French	English	French	English
Age-of-acquisition	1.3	10.1	1.6	9.3	1.6	9.0	0.1	9.9
Currently used	71.5	28.5	70.5	29.5	74	26.0	75.3	24.7
Speaking	5.9	3.7	6.6	4.3	6.6	4.3	5.9	3.7
Reading	6.5	4.5	6.5	5.0	6.5	4.8	6.5	4.5
LexTALE	87.8	66.8	89.8	70.3	87.3	68.5	87.3	66.3

Table 3. Overall RT in ms and percentage of errors (PE; SD between brackets) of Experiment 1, as a function of Language transition (switch vs. repetition trials).

	Language switching		
Dependent variables	Switch	Repetition	Switch costs
RT	621 (108)	624 (119)	-3
Errors	3.8 (8.6)	3.5 (7.3)	0.3

Table 4. Overall RT in ms and percentage of errors (PE; SD between brackets) of Experiment 2, as a function of Language transition (switch vs. repetition trials) and Task transition (switch vs. repetition trials).

	Language switching			Task switching		
Dependent variables	Switch	Repetition	Switch costs	Switch	Repetition	Switch costs
RT	1176 (344)	1168 (350)	10	1387 (466)	952 (240)	435
Errors	6.4 (2.4)	7.2 (2.7)	-0.8	7.0 (5.4)	6.6 (5.5)	0.4

Table 5. Overall RT in ms and percentage of errors (PE; SD between brackets) of Experiment 3, as a function of Language transition (switch vs. repetition trials) and Modality transition (switch vs. repetition trials).

	Language switching			Modality switching		
Dependent variables	Switch	Repetition	Switch costs	Switch	Repetition	Switch costs
RT	804 (148)	810 (138)	-6	838 (132)	775 (154)	63
Errors	4.4 (2.4)	6.0 (2.7)	-1.6	4.4 (2.3)	6.0 (3.1)	-1.6

Table 6. Overall RT in ms and percentage of errors (PE; SD between brackets) of Experiment 4, as a function of Language transition (switch vs. repetition trials).

	Language switching		
Dependent variables	Switch	Repetition	Switch costs
RT	573 (85)	555 (80)	18
Errors	0.6 (0.9)	0.0 (0.2)	0.6

Table 7. Overview of the demographic information for Experiments 5-7. The information consists of the average age-of-acquisition of both languages and the average percentage of time the participants spoke currently. Furthermore, the average self-rated scores for speaking and reading both languages is given, as is the average LexTALE scores for both languages.

	Experiment 5		Experiment 6		Experiment 7	
	French	English	French	Spanish	French	English
Age-of-acquisition	0.2	8.6	2.7	9.8	0.6	9.6
Currently used	72.0	28.0	72.5	27.5	75.4	24.6
Speaking	6.5	4.2	6.6	4.8	6.3	4.2
Reading	6.6	4.9	6.7	5.3	6.6	4.8
LexTALE	90.0	71.6	83.4	65.0	89.1	72.1

Table 8. Overall RT in ms and percentage of errors (PE; SD between brackets) of Experiment 5, as a function of Language transition and Trial type (switch vs. repetition vs. pure language trials).

	Language switching				
Dependent variables	Switch	Repetition	Pure	Switch costs	Mixing costs
RT	611 (69)	609 (74)	600 (81)	2	9
Errors	3.5 (3.1)	3.6 (3.9)	3.7 (5.3)	-0.1	-0.1

Table 9. Overall RT in ms and percentage of errors (PE; SD between brackets) of Experiment 6, as a function of Language transition and Trial type (switch vs. repetition vs. pure language trials).

	Language switching				
Dependent variables	Switch	Repetition	Pure	Switch costs	Mixing costs
RT	705 (112)	715 (104)	665 (130)	-10	50
Errors	3.4 (4.0)	3.9 (3.2)	5.7 (6.9)	-0.5	-1.8

Table 10. Overall RT in ms and percentage of errors (PE; SD between brackets) of Experiment 7, as a function of Language transition and Trial type (switch vs. repetition vs. pure language trials).

	Language switching				
Dependent variables	Switch	Repetition	Pure	Switch costs	Mixing costs
RT	697 (147)	690 (153)	682 (128)	7	8
Errors	6.7 (3.5)	8.6 (4.7)	8.3 (4.3)	-1.9	0.3

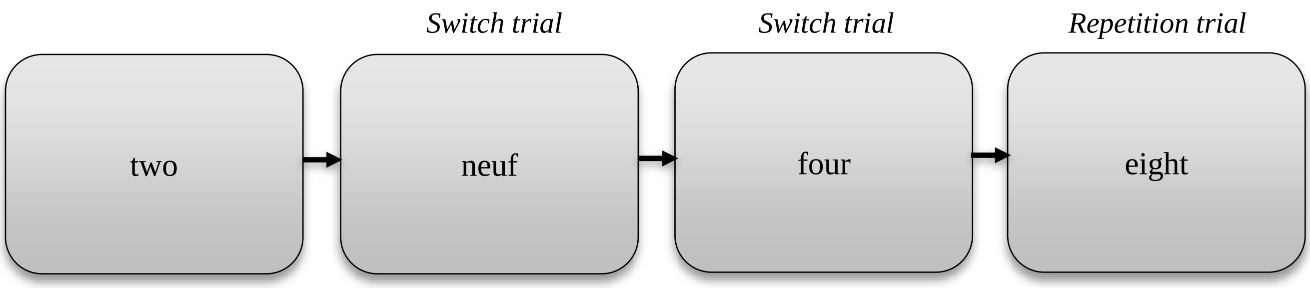

Figure 1. Overview of a trial sequence in a comprehension-based language switching experiment with number words.