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Inhibition Accumulates Over Time at Multiple Processing Levels in Bilingual Language Control

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Abstract

It is commonly assumed that bilinguals enable production in their nondominant language by inhibiting their dominant language temporarily, fully lifting inhibition to switch back. In a re-analysis of data from 416 Spanish-English bilinguals who repeatedly named a small set of pictures while switching languages in response to cues, we separated trials into different types that revealed three cumulative effects. Bilinguals named each picture (a) faster for every time they had previously named that same picture in the same language, an asymmetric repetition priming effect that was greater in their nondominant language, and (b) more slowly for every time they had previously named that same picture in the other language, an effect that was equivalent across languages and implies symmetric lateral inhibition between translation equivalents. Additionally, (c) bilinguals named pictures in the dominant language more slowly for every time they had previously named unrelated pictures in the nondominant language, exhibiting asymmetric language-wide global inhibition. These mechanisms dynamically alter the balances of activation between languages and between lemmas, providing evidence for an oft-assumed but seldom demonstrated key mechanism of bilingual control (competition between translations), resolving the mystery of why reversed language dominance sometimes emerges (the combined forces of asymmetrical effects emerge over time in mixed-language blocks), and also explaining other longer-lasting effects (block order). Key signatures of bilingual control can depend on seemingly trivial methodological details (e.g., number of trials in a block) because inhibition is applied cumulatively at both local and global levels, persisting long after each individual instance of selection.

Keywords

bilingualism; switching; inhibition; implicit learning

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Supplementary material

Trial-level data and analysis scripts are publicly available at <https://osf.io/zcb52/>.

Declaration of interest

Conflicts of interest: none.

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1. Introduction

Although bilinguals can easily express most concepts in two languages, they rarely use the wrong language by mistake (Poulisse & Bongaerts, 1994). This is particularly impressive given that bilinguals often appear unable to entirely shut off activation of the language they don't want to speak; that is, they activate words in both languages even when planning to speak in just one language (e.g., Colomé, 2001; Costa, Caramazza, & Sebastián-Gallés, 2000; Hoshino & Kroll, 2008). How do bilinguals accomplish this feat of executive control? Though they do not seem to be equipped with an on/off switch (a dimmer might provide a better analogy), an emerging consensus identifies *inhibition* – operating at multiple processing levels – as a central mechanism enabling such feats, as well as related feats in psycholinguistics and cognitive psychology.

1.1. Global language control

In an influential paper, Green (1998) proposed that bilinguals use inhibition to facilitate selection of the target language while speaking. Green assumed that bilinguals have two language nodes and that each word representation (lemma) is connected to one of these nodes, tagging it for language membership. According to his model, when a non-target language becomes active, inhibition is applied in proportion to its activation level. This inhibition is global in scope; i.e., it is applied directly to a language node, and subsequently spreads to lemmas in that language. The inhibition persists until the inhibited language needs to be selected for production, at which point it is lifted – an action that is costly (in terms of time) in proportion to the quantity of inhibition that was previously applied.

This proposal neatly accounts for several phenomena that are often observed in studies of bilingual language switching. In many such studies, bilinguals name a series of digits or pictures, each of which is accompanied by a cue indicating the language to be used. A nearly universal finding is that bilinguals take longer to begin speaking when they need to use a different language relative to what they used on the preceding trial, and these *switch costs* are often asymmetrical: Counterintuitively, they are greater in the dominant language (Meuter & Allport, 1999; for a review, see Bobb & Wodniecka, 2013). Under Green's (1998) account, switch costs index the time needed to release inhibition from the target language to prepare it for production. Because the dominant language is more accessible than the nondominant language, it receives more inhibition when it is the non-target language, and this extra inhibition in turn leads to asymmetric switch costs. Sometimes, bilinguals may apply so much inhibition that they end up responding faster in their nondominant language than in their dominant language. This pattern of *reversed dominance* has been observed sporadically in experimental studies of language switching by several different investigators (Costa & Santesteban, 2004; Costa, Santesteban, & Ivanova, 2006; Christoffels, Firk, & Schiller, 2007; Declerck, Thoma, Koch, & Philipp, 2015; Gollan & Ferreira, 2009; Gollan, Kleinman, & Wierenga, 2014; Gollan & Goldrick, 2016, 2017; Gollan, Schotter, Gomez, Murillo, & Rayner, 2014), though it remains largely a mystery as to what conditions lead reversed dominance to emerge.

An alternative view is that bilinguals instead rely on global activation of the nondominant language rather than globally inhibiting the dominant language, or even that they may employ both global inhibition and global activation at the same time (Branzi, Martin, Abutalebi, & Costa, 2014; Costa & Santesteban, 2004; for discussion of how such an account is difficult to rule out on the basis of data from most language switching studies, see Declerck & Philipp, 2015; Philipp, Gade, & Koch, 2007). According to this proposal, bilinguals who have just spoken in their nondominant language are especially slow to switch into their dominant language because the extra nondominant activation increases competition between languages (see Verhoeft, Roelofs, & Chwilla, 2009 for a variant of this account; but see Fink & Goldrick, 2015). Similarly, reversed dominance effects can be observed if enough activation is applied to the nondominant language, though this possibility has been considered less often, perhaps because of inherent limitations on the extent to which a less dominant language can be activated (at least at some processing levels; see Gollan & Goldrick, 2017). As activation-based accounts are less mainstream than inhibition-based accounts, however, we will frame the experimental setup in terms of global inhibition and revisit the inhibition-versus-activation debate in Section 4.5.1.

1.2. Local language control

In addition to altering the balance of global activation between language nodes, Green (1998) suggested that bilinguals use inhibition to exert local language control at the lemma level. Many models of word production assume that lemmas compete with each other for selection (cf. Levelt, Roelofs, & Meyer, 1999). As a mechanism for resolving this competition in the bilingual lexicon, Green claimed that translation-equivalent lemmas (e.g., *dog* and *perro*) directly inhibit each other when both lemmas are active. This resembles similar claims in research on monolingual language production that words which regularly compete for selection might be linked to each other via lateral inhibitory connections. On this view, when speakers attempt to select a single word for production, promising lexical candidates (e.g., other semantically related words) also become activated and inhibit each other in proportion to their own activation levels until a winner emerges. Lateral inhibition is relatively common in models of language comprehension (Coltheart, Rastle, Perry, Langdon, & Ziegler, 2001; Dijkstra & van Heuven, 2002; Grainger & Jacobs, 1996; McClelland & Elman, 1986) but has less commonly been proposed as a feature of the language production system (Berg & Schade, 1992; Cutting & Ferreira, 1999; Howard, Nickels, Coltheart, & Cole-Virtue, 2006), and researchers have turned to lateral inhibition to explain their results relatively infrequently in the domain of bilingual production (Declerck & Philipp, 2017; Khateb, Shamshoum, & Prior, 2017; Runnqvist, Strijkers, Alario, & Costa, 2012).

A major challenge for the possibility of mutually inhibitory connections between translation equivalents is the absence of strong evidence for competition between them in studies of bilingual language production. In fact, there is striking evidence instead for *facilitation* between translation equivalents (i.e., that translations mutually activate each other). For example, when bilinguals must name a picture while attempting to ignore a simultaneously presented distractor word, naming times are reliably faster when the distractor is a translation equivalent of the picture name than when it is an unrelated word (Costa, Miozzo, & Caramazza, 1999; for a recent replication in three languages, see Dylman & Barry, 2018;

for a review, see Hall, 2011). Similarly, bilinguals are less likely to fall into a tip-of-the-tongue state if they know the word in both languages than if they know it in just one language (in which case the translation equivalent couldn't possibly compete for selection; Gollan & Acenas, 2004; see also Gollan, Montoya, Fennema-Notestine, & Morris, 2005). Such results, and also cross-language priming effects in studies of bilingual word recognition, have even led some researchers to suggest that translation equivalents are directly linked via facilitative – not inhibitory – connections (Dylman & Barry, 2018; Gollan, Forster, & Frost, 1997; Keatley, Spinks, & de Gelder, 1994; Kroll & Stewart, 1994). By contrast, evidence for the opposite (i.e., lateral inhibition) has been decidedly lacking in the literature to date.

1.3. Measuring how language control unfolds over time

In the present study, we take a different approach to the study of lateral inhibition and bilingual language control more generally by examining how the activation of both language-wide and word-specific representations change over time within mixed-language naming blocks. This approach hinges on a key observation about the timescale of inhibition: “previous episodes of suppression may [continue to] exert their effects, since it takes time for the effects of prior inhibition to be overcome” (Green, 1998, p. 72). In keeping with this claim, bilingual performance in a dominant-language block is often worse following a nondominant-language block, suggesting that the effects of inhibition are not immediately overcome (Guo, Liu, Misra, & Kroll, 2011; Misra, Guo, Bobb, & Kroll, 2012; Van Assche, Duyck, & Gollan, 2013; see also Declerck & Grainger, 2017).

This pattern shows that inhibition can persist across blocks; however, it does not address how that inhibition is established. One possibility is that some quantity of inhibition is applied to the dominant language at the start of the nondominant-only block, and each instance of nondominant retrieval simply refreshes that inhibition, keeping it at the same level throughout (and, for a limited time, after) the block. Another possibility is that residual inhibition of the dominant language accumulates over time even within a block, gaining force with each instance of nondominant retrieval – though inhibition may eventually plateau when more distantly applied inhibition expires or when the dominant language is maximally inhibited.

However, a recent study offers evidence against the idea that competition between languages diminishes over the course of a single-language block. Mercier, Pivneva, and Titone (2015) used a visual world task to measure the activation of both the target and non-target languages during comprehension. Bilinguals fluent in English and French were instructed on each trial to click on one of four objects presented on screen. On some trials, a distractor picture had a French name (*fille*) belonging to the same cohort as the target picture's English name (*field*; this task was always conducted in English). Bilinguals looked less often at the target when this French competitor was present, and this effect grew as the experiment progressed. However, this pattern was observed only among bilinguals who had not performed a picture naming task in French immediately beforehand, suggesting that the bilinguals who did speak French subsequently applied global inhibition to the French language in order to perform the English comprehension task. Thus, although these

bilinguals were listening only to English speech, French words became more (not less) activated over time – in contrast to the accumulating inhibition hypothesis presented here.

Further evidence against this hypothesis comes from the paper that originally reported asymmetric language switching costs, in which Meuter and Allport (1999) showed that in a bilingual digit naming task, switch trial RTs were unaffected by the number of immediately preceding digits that were named in the other language. A more recent study (Zheng, Roelofs, & Lemhöfer, 2018) even showed fewer errors – i.e., *improved* control – when switching to the dominant language after long naming runs in the nondominant language. However, a proper test of the inhibition accumulation hypothesis would examine how often each language was used on *all* preceding trials, as opposed to restricting such an analysis to the immediately preceding run. For comparison, there should be no sign of inhibition accumulation in a block consisting only of dominant-language trials, as bilinguals do not need to inhibit a language that is always the target.

This approach also has the potential to detect the after-effects of lateral inhibition between translation equivalents if it exists. Interestingly, a recent proposal in support of such inhibition was advanced by Runnqvist et al. (2012), who (like us) used a bilingual picture naming task to show how a particular kind of interference accumulates over time. Their task relied on the observation that naming a picture (say, *dog* or *perro*) slows the subsequent naming of other semantically related pictures (*cat* or *gato*), even when the two pictures are presented non-consecutively (Howard et al., 2006). By having bilinguals name multiple pictures in each category spread over dozens of trials, Runnqvist and colleagues were able to study how interference accumulates within and between languages without the facilitative influence of semantic priming – a factor that may have contributed to their support for lateral inhibition between all words in the lexicon, including all semantically related words within both languages and translation equivalents. In contrast, translation distractors might facilitate picture naming in the picture-word interference task because the paradigm itself generates semantic priming on every trial, overpowering any lateral inhibition that might also be present.

These divergent conclusions (i.e., facilitation implied by picture-word interference vs. inhibition implied by consecutive instead of simultaneous presentation of translations) could suggest that priming and interference from translation equivalents are potentially separable on a longer timescale, making it possible to isolate lateral inhibition effects using a paradigm without distractors and considering contributions of different types of repetition over time. If lateral inhibition accumulates over time, as with language-wide inhibition according to Green (1998), participants should name a picture more slowly for each time they previously named that same picture in the other language (in addition to the global effects described above, which should accumulate with each and every other picture named in the other language).

1.4. The present study: Hundreds of bilinguals

To determine whether global and/or local inhibition accumulate over time, we present a re-analysis of previously collected language switching data from Spanish-English bilinguals ($n=416$). Bilinguals in our experiments repeatedly named a set of nine pictures in several

blocks, including a cued switching block in which the same pictures were named 6 times in each language, as well as English-only and Spanish-only blocks in which the same pictures were named 12 times in a single language (leading to 108 trials in every block). To tease apart the potential influences of language-wide and lateral inhibition, we decomposed within-block trial number into three variables (hereafter referred to as *trial number components*), each of which represents the influence of a different psychological phenomenon:

Repetition priming: It is well established that people map a stimulus to a response more quickly when they have previously engaged in that same mapping (for a review, see Henson, Eckstein, Waszak, Frings, & Horner, 2014). In the context of picture naming, people name pictures more quickly that they have named previously, though the benefit for each extra repetition decreases rapidly with the number of repetitions (e.g., Gollan et al., 2005; Griffin & Bock, 1998; Oldfield & Wingfield, 1965), a pattern in line with sublinear effects of repetition priming from other domains (e.g., Logan, 1990). Bilinguals show repetition priming in picture naming for both languages, but this priming is stronger in the nondominant language (Francis, Augustini, & Sáenz, 2003), likely because the baseline level of activation is greater for the dominant language (giving nondominant activation more room to grow; cf. Dijkstra & van Heuven, 2002; Gollan, Montoya, Cera, & Sandoval, 2008; Gollan, Slattery, Goldenberg, Van Assche, Duyck, & Rayner, 2011). We represented the influence of repetition priming with a variable we will refer to as *same-language target repetition*, which for each trial was set to the number of times the same picture had been previously presented with the same language cue in the same block (range: 0–5 for the cued block and 0–11 for single-language blocks). Furthermore, to account for the sublinear effect of repetition priming, we log-transformed this variable (as described in more detail below). We expected that in line with prior results, same-language target repetition would be negatively correlated with RTs for both languages, but that this effect would be larger for the nondominant language.

Lateral inhibition between translation equivalents: As noted, participants in the cued switching block named the same pictures in both languages. If translation equivalents laterally inhibit each other, naming a picture in one language should make it more difficult – not less difficult – to name the same picture in the other language (assuming facilitation from repeated recognition of the same picture can somehow be factored out). Furthermore, if this inhibition is applied in proportion to the activation level of the non-target lemma (Green, 1998), it should affect the dominant language more than the nondominant language. We represented the potential influence of lateral inhibition with a variable we will refer to as *different-language target repetition*, which for each trial was set to the number of times the same picture had been previously presented with the non-target language cue in the same block (range: 0–6 for the cued block; not applicable in the single-language blocks). We expected that different-language target repetition would be positively correlated with RTs for both languages but especially so for the dominant language.

Language-wide inhibition: If bilinguals use global inhibition to facilitate language mixing, naming *any* picture in the nondominant language should slow the naming of *any* picture in

the dominant language. To index the item-independent portion of this inhibition, we represented the potential influence of language-wide inhibition with a variable we will refer to as *non-target repetition*, which for each trial was set to the number of times that different pictures (i.e., pictures other than the target on the current trial) had been previously presented with either language cue in the same block (range: 0–96 for all blocks). We were unable to directly separate out the influence of naming same-language and different-language non-target pictures due to collinearity (as described below), but accomplished this indirectly by comparing single-language and mixed-language blocks (see Analysis 3). We expected that non-target repetition would be positively correlated with RTs, but that asymmetric inhibition would lead to a greater inhibitory effect for the dominant language in the cued block.

On every trial, the sum of these three trial number components – same-language target repetition, different-language target repetition, and non-target repetition – was equal to the number of pictures that were previously presented in the same block (range: 0–107). For example, if the first five trials of a cued switching block were *ballena*, *star*, *hand*, *bone*, *mano*, that fifth trial (*mano* – *hand* in Spanish) would have the following values:

same-language target repetitions: 0 [no prior *mano* trials]

different-language target repetitions: 1 [*hand* on trial 3]

either-language non-target repetitions: 3 [*ballena*, *star*, *bone*]

These values would be unchanged for trial 5 even if trials 1–4 were shuffled: They are sensitive to the aggregated identities of preceding trials, but are independent of their sequence. (Note that as our definitions count the number of times picture-cue pairs were previously presented, they are independent of whether bilinguals accurately named the pictures presented on prior trials. Given that error rates were low, however (see Section 2.3), we will assume that prior responses were correct by referring to (e.g.) “the number of times bilinguals previously named the same picture in the same language”.)

To understand how inhibition evolves over time, we tracked how naming latencies changed in each block and for each language as a function of these continuous variables. In addition, to shed light on how these forms of inhibition relate to the kind of global inhibition that gives rise to asymmetric switch costs, we checked for interactions between these variables and trial type (stay vs. switch) in the cued block.

2. Method

2.1. Participants

Four hundred sixteen Spanish-English bilinguals from the University of California, San Diego participated for course credit. All participants gave informed consent. Not counting two participants who were excluded due to technical errors, this sample size represents the total number of participants who were run in the qualifying experiments: 288 bilinguals across the three experiments presented in Kleinman and Gollan (2016), and 128 bilinguals from a previously unpublished language switching experiment. As is typical for this population, most bilinguals were English-dominant ($n=381$, 91.6%) as determined by their

performance on the Multilingual Naming Test (MINT; Gollan, Weissberger, Runnqvist, Montoya, & Cera, 2012; see Kleinman & Gollan, 2016 for more details of most participants' language backgrounds). Although the analyses presented here include data from all participants, another set of analyses was run only on data from the English-dominant bilinguals as a way to reduce heterogeneity in the sample; only one minor difference was found (see Supplementary Analysis 1 in the Appendix).

2.2. Materials and procedure

Critical target picture naming stimuli were black-and-white line drawings of pictures. Across experiments, 20 unique pictures were used (11 from the International Picture Naming Project picture database, Székely et al., 2004; the other 9 were found online and drawn in a similar style): *bell-campana*, *bone-hueso*, *book-libro*, *door-puerta*, *dress-vestido*, *glass-vaso*, *grapes-uvas*, *hammer-martillo*, *hand-mano*, *horse-caballo*, *key-llave*, *king-rey*, *leaf-hoja*, *money-dinero*, *octopus-pulpo*, *pencil-lápiz*, *star-estrella*, *tree-árbol*, *whale-ballena*, and *whistle-silbato*. In every block, participants named 9 of these pictures 12 times each for a total of 108 trials per block. Most participants named the same set of pictures in four blocks ($n=288$, 69.2%); the rest performed two blocks of picture naming with either the same or different sets of pictures in each block (both $n=64$, 15.4% each).

All bilinguals completed a cued switching block in which each language was cued on 50% of trials. The cued switch rate for each language was either 33% ($n=288$, 69.2%) or 50% ($n=128$, 30.8%). Most bilinguals ($n=288$, 69.2%) also completed two single-language blocks using the same set of pictures and language cues as in the cued switching block.

Furthermore, all participants completed another mixed-language block in which they were given some degree of voluntary control over which language to use on each trial, though this block will not be discussed further as it is beyond the scope of the present paper (see Kleinman & Gollan, 2016 for details). Within each experiment, block order was fully counterbalanced. Every block began with a practice block of 12 trials, a brief instruction stating that the practice was complete, then another practice trial that was followed immediately (without further notice) by 108 critical trials. (Pictures presented on practice trials were not presented on critical trials.)

For each experiment's cued switching block, an initial list was constructed as described above with three additional constraints: No picture was allowed to repeat on consecutive trials (a constraint shared by single-language blocks), there were never more than five consecutive stay trials, and there were never more than two consecutive switch trials. There was substantial variability in the lag at which pictures were repeated: Across all experimental lists in the cued blocks (24–32 unique lists per experiment; some lists were used in multiple experiments), pictures were repeated after an average of 7.5 intervening pictures, but this distribution had a long right tail ($SD = 6.5$, median = 5, maximum = 42). The single-language blocks, which shared identical sequences of pictures across lists, therefore had similar statistics (mean = 7.6, $SD = 6.5$, median = 6, maximum = 36); for example, if one bilingual produced *horse-hand-door* in an English-only block, another bilingual produced *caballo-mano-puerta* in an analogous Spanish-only block. Lists for other participants were created by replacing all presentations of one picture with presentations of

another picture, and/or (for the cued block) by switching the language cues for all trials. As a result, each picture had identical lag distributions across lists in English-only and Spanish-only blocks, and for English and Spanish trials in the cued block, with minimal differences in lag distributions between pictures. This variability made it possible to dissociate effects of previously naming the same picture vs. previously naming other pictures without introducing confounds of item or language. Although collinearity between factors was often high – i.e., simple correlations between different trial number components (scaled as below) ranged in r^2 values from .56 to .77 – the large amount of data (more than 100,000 usable trials) ensured that the analyses were sufficiently well-powered to detect the unique variance that remained for each factor.¹ (Potential influences of collinearity on the results will be considered further in Section 4.5.4.)

2.2.1. Trial structure—As described in Kleinman and Gollan (2016), stimuli were presented using PsyScope X software (Build 57; Bonatti, n.d.; Cohen, MacWhinney, Flatt, & Provost, 1993) on an iMac 7 computer with a 20-in. color monitor. In every block, each trial started with a fixation cross presented for 350 ms, followed by a 150-ms blank screen. A language cue then appeared on the screen, 7.7 cm above the center of the fixation cross. Depending on the condition, the cue was a United States flag, signifying that the picture was to be named in English, or a Mexican flag, signifying that the picture was to be named in Spanish. In single-language blocks, the same language cue appeared on every trial; in the cued switching block, the sequence of language cues was determined based on counterbalancing constraints described above. After 250 ms, the target picture appeared in the center of the screen while the cue stayed on-screen. The cue and target remained until the participant responded, or for a maximum of 3,000 ms. An 850-ms intertrial interval preceded the next trial.

2.3. Analysis

The 416 bilinguals provided data for 62,208 critical trials in single-language blocks and 44,928 critical trials in cued switching blocks, of which 101,462 total (94.7%) were analyzed. Trials were excluded when bilinguals produced a response that did not match the target or an acceptable alternative (4.6%), when the voice key was not triggered at speech onset (1.7%), or when bilinguals responded faster than 250 ms (0.2%) or did not respond within 3000 ms (0.3%). (Some trials violated multiple criteria.) Regarding the use of alternative names, participants used such names (e.g., “stallion” for *horse* and “cup” for *glass*) on 0.3% of usable trials.

Analyses using mixed-effects models (Baayen, Davidson, & Bates, 2008) with maximal random effects (Barr, Levy, Scheepers, & Tily, 2013) were conducted on picture naming latencies using the lme4 package (version 1.1–13; Bates, Maechler, Bolker, & Walker, 2015) of R (version 3.3.3; R Core Team, 2016). All models contained random intercepts for participants and pictures, random slopes allowing every fixed effect to vary by participants

¹In the cued block, simple correlations between predictors were weakest between trial number components indexing the repetition of same-language targets and different-language targets ($r^2 = .56$), stronger for same-language targets and non-targets (i.e., unrelated pictures; $r^2 = .67$), and strongest for different-language targets and non-targets ($r^2 = .77$). In the single-language blocks, same-language target repetition and non-target repetition were highly correlated ($r^2 = .75$).

and pictures, and (initially) a full correlational structure. For models that failed to converge, correlations between random effects were removed (as recommended by Barr et al., 2013), which always resulted in convergence; such models are noted in the text. To determine statistical significance and 95% confidence intervals, denominator degrees of freedom were computed using the Satterthwaite approximation with the lmerTest package (version 2.0–33; Kuznetsova, Brockhoff, & Christensen, 2016). To reduce collinearity, continuous predictors were centered and nominal predictors were contrast coded such that levels were separated by one, and the average weighted value was zero. Additionally, some predictors were linearly scaled to facilitate model convergence; all reported values are de-scaled.

For the reasons described above, we expected the difference in RT between presentations 1 and 2 to be larger than the difference in RT between presentations 11 and 12 (cf. Griffin & Bock, 1998; Logan, 1990). Accordingly, same-language target repetition is represented in all models on a logarithmic scale (base 2),² which makes interpreting the coefficient of this predictor straightforward. For example, an effect with $B = -100$ would mean that RTs decreased by 100 ms every time the number of same-language repetitions was doubled. As we did not have similar a priori hypotheses about different-language target repetition or non-target repetition, those predictors were linearly scaled.

Trial-level data and analysis scripts are publicly available at <https://osf.io/zcb52/>.

3. Results

To address our theoretical questions of interest, we present three sets of analyses that collectively examine how inhibition during picture naming evolves over time: Analysis 1 focuses on the cued switching block, Analysis 2 focuses on the single-language blocks, and Analysis 3 compares across the two kinds of blocks. For each one, we first list the relevant predictors and the structures of statistical models in the analysis, then describe statistically significant results in plain English, and finally report the results of the models in full.

3.1. Analysis 1: Cued mixed-language blocks

Analysis 1 used the three trial number components to explore local and global mechanisms of inhibitory control and whether these reflect the same underlying cognitive control mechanisms as switch costs. Thus, the analysis of the cued switching block included a main effect of language (dominant or nondominant), a main effect of trial type (stay trial vs. switch trial), and main effects corresponding to all three trial number components described above: same-language target repetition (range: 0–5, transformed to $\log_2(1-6)$), different-language target repetition (range: 0–6), and non-target repetition in either language (range: 0–96). As noted above, in the latter factor, we could not separate same-language from different-language non-target repetition as these variables were almost perfectly correlated in the cued switching block (and thus only counting different-language non-target repetitions led to identical results, as reported in Supplementary Analysis 4 in the Appendix); however,

²This is not intended as a theoretical commitment to a logarithmic scale of repetition priming, only to a sublinear scale. For comparison, when the more extreme inverse transformation was used, the results were nearly identical: 41/43 p -values reported in Analyses 1–3 had the same statistical significance (greater than or less than .05) across transformations, and the other 2/43 were consistent with even greater differences in non-target repetition effects across languages than when the log transformation was used.

this point is addressed in Analysis 3 by comparing the effects of naming non-targets in both languages (in the cued block) to naming non-targets in only one language (in single-language blocks). In addition, all trial number components were allowed to interact with language, with trial type, and with both language and trial type simultaneously, though trial number components were not allowed to interact with each other in this or any other analysis. To explore significant interactions, separate analyses were conducted for each language. Both the omnibus model and the subset models converged only after correlations between random effects were removed.

Mean by-participant latencies are shown in Fig. 1, and more detailed visualizations of trial number component effects are shown in Fig. 2.³ (All figures were created using the ggplot2 software package; Wickham, 2009.) Predictably, bilinguals named pictures 57 ms slower on switch trials than on stay trials, an effect that was larger in the dominant language (67 ms) than in the nondominant language (45 ms) – a switch-cost asymmetry by dominance. More interestingly, the model detected dissociable effects in the predicted directions for each trial number component, including (a) same-language target repetition facilitation effects: naming latencies were 64 ms faster for each doubling in how often bilinguals named the target picture in the same language, an effect that was larger in the nondominant language (81 ms) than in the dominant language (51 ms); (b) different-language target repetition interference effects: 10 ms slower for each time bilinguals previously named the target picture in the other language, an effect that did not differ between languages; and (c) non-target repetition interference effects: 0.79 ms slower for each time bilinguals previously named any non-target picture in either language, an effect that was greater for the dominant language (1.07 ms) than for the nondominant language (0.52 ms). (Given that 96 non-target pictures were named in the cued block, this naming accounted for a cumulative block-wide increase of 103 ms for the dominant language and 50 ms for the nondominant language.)

These observations were statistically supported by a significant main effect of trial type, $B = 57$, 95% CI = [50, 65], $t(22) = 15.75$, $p < .001$, which interacted with language, $B = -20$, 95% CI = [-33, -7], $t(18) = -3.30$, $p = .004$ (switch costs in the dominant language: $B = 67$, 95% CI = [58, 76], $t(24) = 15.91$, $p < .001$; in the nondominant language: $B = 45$, 95% CI = [33, 57], $t(17) = 8.01$, $p < .001$). There was a facilitative main effect of same-language target repetition, $B = -64$, 95% CI = [-76, -53], $t(19) = -11.45$, $p < .001$, which interacted with

³As mixed-effects models evaluate the statistical significance of each factor while holding other factors constant at their means, Fig. 2 depicts the models' effects in a similar way. To make (say) the same-language target repetition figure for the single-language blocks, the estimated contribution of same-language *non*-target repetition was subtracted from RT on every single-language block trial. For example, consider a dominant-only block trial in which a participant took 709 ms to name a picture of a pencil on the 69th trial in the block, after previously naming the same picture 9 times and naming other pictures 59 times. The single-language analysis reported in Analysis 2 estimated the effect of naming each same-language non-target picture as 0.79 ms, and that this effect was 0.15 ms larger for the nondominant language than for the dominant language. Due to the centering of continuous variables and weighted recoding of nominal variables as reported in the Methods section, a same-language non-target repetition value of 59 was assigned a numeric value of 10.91 (after subtracting 48.09, the mean value of the same-language non-target repetition variable across both single-language blocks) and the dominant language was assigned a numeric value of -0.49 in the analysis (vs. 0.51 for the nondominant language, because dominant trials constituted 51% of usable trials in the block and these numbers are separated by 1 and yield a weighted average of 0). As removing each effect requires subtracting the product of its effect size and the relevant numeric value(s), removing the effect of same-language non-target repetition required subtracting $0.79 * 10.91 = 8.62$, and removing the effect of its interaction with language required subtracting $0.15 * 10.91 * -0.49 = -0.80$. Thus, overall RT for this trial was adjusted by $8.62 + -0.80 = 7.82$ ms, leading to an estimated RT of $709 - 7.82 = 701.18$ ms for a dominant-only trial with a same-language target repetition value of (log) 9 – a pair of values that was entered into the LOESS fit. Analogous subtractions were performed for all figures. Code to produce this figure is provided in Supplementary Material.

language, $B = -23$, 95% CI = $[-38, -9]$, $t(41) = -3.31$, $p = .002$ (effect of same-language target repetition for the dominant language: $B = -51$, 95% CI = $[-60, -43]$, $t(34) = -12.79$, $p < .001$; for the nondominant language: $B = -81$, 95% CI = $[-101, -62]$, $t(19) = -8.89$, $p < .001$). There was also an inhibitory main effect of different-language target repetition, $B = 10$, 95% CI = $[6, 13]$, $t(25) = 5.08$, $p < .001$, which did not interact with language, $B = 1$, 95% CI = $[-4, 6]$, $t < 1$. Finally, there was an inhibitory main effect of non-target repetition, $B = 0.79$, 95% CI = $[0.57, 1.01]$, $t(1614) = 7.05$, $p < .001$, which interacted with language, $B = -0.59$, 95% CI = $[-0.98, -0.20]$, $t(38128) = -2.95$, $p = .003$ (effect of non-target repetition for the dominant language: $B = 1.07$, 95% CI = $[0.78, 1.36]$, $t(2340) = 7.31$, $p < .001$; for the nondominant language: $B = 0.52$, 95% CI = $[0.23, 0.82]$, $t(1738) = 3.47$, $p < .001$). No other effects were significant, including the main effect of language and all six two- and three-way interactions involving trial type and a trial number component, all $|t|s < 1$.

3.2. Analysis 2: Single-language blocks

Analysis 1 demonstrated that naming non-target pictures in a mixed-language block slows naming in the dominant language more than in the nondominant language (though this inhibitory effect did not interact with switch costs, an important point that will be discussed in Section 4.2.3). At the same time, some of the observed slowdown may be due to factors such as a decrease in attention or motivation over the course of the block, although it seems less likely that such factors could account for the interaction with language. To determine how much slowing is uniquely attributable to language mixing, it is necessary to compare interference in mixed-language and single-language contexts.

Accordingly, Analysis 2 explored the effects of target and non-target repetition in single-language blocks to provide a baseline against which to measure the results of Analysis 1. This analysis of the single-language blocks included a main effect of language (dominant or nondominant) as well as main effects corresponding to two of the three trial number components described above: (same-language) target repetition (range: 0–11, transformed to $\log_2(1–12)$) and (same-language) non-target repetition (range: 0–96). (By definition, there was no different-language target repetition in single-language blocks.) In addition, both trial number components were allowed to interact with language. To explore significant interactions, separate analyses were conducted for each language. In the omnibus model only, correlations between random effects were removed to facilitate convergence.

In contrast to the cued switching block, naming latencies were 21 ms slower in the nondominant language than in the dominant language, a standard language dominance effect. As before, however, the model detected dissociable effects for both trial number components. Naming latencies were 43 ms faster for each doubling in how often bilinguals named the target picture in the same language, an effect that was greater in the nondominant language (56 ms) than in the dominant language (32 ms). In addition, naming latencies were 0.79 ms slower for each time bilinguals previously named any non-target picture, an effect that – in contrast to the cued switching block – did not differ between languages (and in fact showed a numeric trend toward greater interference in the nondominant language, a pattern opposite to that observed in the cued switching block).

These observations were statistically supported by a significant main effect of language, $B = 21$, 95% CI = [7, 34], $t(32) = 3.12$, $p = .004$. There was a facilitative main effect of same-language target repetition, $B = -43$, 95% CI = [-54, -33], $t(14) = -9.14$, $p < .001$, which interacted with language, $B = -22$, 95% CI = [-35, -10], $t(14) = -3.94$, $p = .001$ (effect of same-language target repetition in the dominant language: $B = -32$, 95% CI = [-41, -23], $t(14) = -7.63$, $p < .001$; in the nondominant language: $B = -56$, 95% CI = [-75, -38], $t(16) = -6.45$, $p < .001$). There was also an inhibitory main effect of non-target repetition, $B = 0.79$, 95% CI = [0.60, 0.98], $t(12) = 9.09$, $p < .001$, which did not interact with language, $B = 0.15$, 95% CI = [-0.18, 0.49], $t(12) = 1.00$, $p = .335$.

3.3. Analysis 3: Comparing global language control across block types

Taken together, the previous analyses indicate that the inhibitory effect of naming non-target pictures is greater for the dominant language than the nondominant language, but only in a mixed-language block – not in single-language blocks. To study how these inhibitory effects vary with block type for each language, we compare the cued and single-language blocks. To this end, it was necessary to collapse same- and different-language target repetition into a single variable because the single-language blocks did not include different-language target repetitions or indeed any different-language trials at all. Although this makes potential differences between blocks in target repetition difficult to interpret, it maintains comparability across blocks on non-target repetition, a variable of greater theoretical interest. Accordingly, Analysis 3 included variables representing target repetition in either language (range: 0–11, transformed to $\log_2(1+12)$) and non-target repetition in either language (range: 0–96). Both were allowed to interact with block type (cued vs. single-language), with language, and with both block type and language simultaneously. Similarly, because single-language blocks do not have switch trials, we collapsed across trial type (stay vs. switch) for cued-block responses in this analysis. (Alternate versions of this analysis are presented in the Appendix. These include an analysis restricted to participants who named pictures in both single-language and cued blocks (Supplementary Analysis 2), making block type a fully within-participant variable, as well as an analysis restricted to non-switch trials (Supplementary Analysis 3), increasing comparability between blocks.) To explore significant interactions, separate analyses were conducted for each language; correlations between random effects were removed from the omnibus and dominant language-only models to facilitate convergence.

Bilinguals named pictures 163 ms slower in the cued block than in the single-language blocks, reflecting the cost of mixing and switching languages. Across blocks, they named pictures 19 ms slower in the nondominant language than in the dominant language, but this dominance effect was greater in the single-language blocks (consistent with the significant effect of language that was present in the single-language blocks but not in the cued block, where it was numerically reversed). Turning to effects involving trial number components: Across blocks, naming latencies were 46 ms faster for each doubling in how often bilinguals previously named the target picture (collapsing across language). This picture repetition effect – which, again, did not take into account the language(s) in which the target picture was previously named – was larger for the nondominant language (67 ms) than for the dominant language (30 ms; as seen separately in both cued and single-language blocks), and

larger in the single-language blocks than in the cued block – likely reflecting the division of picture repetitions in the cued block between same- and different-language repetitions (especially given that same-language repetition priming effects were 64 ms in the cued block and 43 ms in the single-language blocks). Across blocks, naming latencies were 0.82 ms slower for each time bilinguals previously named any non-target picture (in either language). Crucially, however, this effect was modulated by a three-way interaction with block type and language. This reflects the fact that naming non-target pictures inhibited naming in the dominant language more than in the nondominant language, but only in the cued block. Breaking down the interaction by language reveals that each non-target picture inhibited naming in the dominant language 0.28 ms more in the cued block than in the single-language block (a marginally significant effect), whereas each non-target picture inhibited naming in the nondominant language 0.28 ms less in the cued block than in the single-language block (though this effect was not significant).

These observations were statistically supported by main effects of block type, $B = 163$, 95% CI = [149, 176], $t(53) = 24.13$, $p < .001$, and language, $B = 19$, 95% CI = [2, 36], $t(18) = 2.29$, $p = .035$, and an interaction between block type and language, $B = -29$, 95% CI = [-44, -15], $t(24) = -4.18$, $p < .001$. For effects of trial number components, there was a main effect of target picture repetition, $B = -46$, 95% CI = [-56, -36], $t(19) = -9.36$, $p < .001$, which interacted with language, $B = -24$, 95% CI = [-34, -15], $t(23) = -5.45$, $p < .001$ (effect of target picture repetition in the dominant language: $B = -30$, 95% CI = [-37, -23], $t(16) = -9.21$, $p < .001$; in the nondominant language: $B = -67$, 95% CI = [-86, -48], $t(18) = -7.44$, $p < .001$), and (separately) interacted with block type, $B = 15$, 95% CI = [9, 21], $t(717) = 4.79$, $p < .001$. Finally, there was a main effect of non-target repetition, $B = 0.82$, 95% CI = [0.64, 0.99], $t(15) = 10.16$, $p < .001$, and a three-way interaction between non-target repetition, block type, and language, $B = -0.41$, 95% CI = [-0.80, -0.01], $t(99) = -2.05$, $p = .043$ (interaction between non-target repetition and block type in the dominant language: $B = 0.28$, 95% CI = [0.00, 0.57], $t(154) = 1.96$, $p = .051$; in the nondominant language: $B = -0.28$, 95% CI = [-0.70, 0.14], $t(23) = -1.37$, $p = .184$). The remaining two- and three-way interactions were not significant, all $|t|s < 1$.

4. Discussion

Over the course of a cued language switching block, bilinguals demonstrated asymmetric repetition priming (greater facilitation for the nondominant language), symmetric lateral inhibition (equal inhibition for the dominant and nondominant languages), and asymmetric global inhibition (greater inhibition for the dominant language). Together, these effects shed light on several mysteries of bilingual language control: The combined forces of the asymmetrical effects can deliver reversed language dominance effects in mixed-language blocks, and the symmetrical effect provides a signature of local inhibition that is often assumed but seldom demonstrated empirically. We begin by summarizing each of these results in greater detail, then discuss their collective theoretical implications for models of bilingual language control.

4.1. Summary of results

Repetition priming: Robust repetition priming was observed for both languages in both the cued block and single-language blocks (64 ms and 43 ms per doubling in repetitions, respectively). Consistent with prior research (Francis et al., 2003), this effect was larger in the nondominant language than in the dominant language in all blocks. Although we did not directly compare overall repetition priming effects across blocks for reasons described in Analysis 3, the pattern of means is consistent with an account in which less active targets benefit more from repetition priming (e.g., Griffin & Bock, 1998). This would lead to more repetition priming for the nondominant language because it likely has a lower baseline level of activation than the dominant language (Gollan et al., 2008; 2011), and more repetition priming for the cued block because competition between languages during selection was greater than in the single-language blocks, which decreased the activation of the target language and target lemmas.⁴

Lateral inhibition between translation equivalents: In the cued switching block, bilinguals named pictures in each language approximately 10 ms slower for each time they had previously named those same pictures in the non-target language. In other words, naming *dog* hindered the naming of *perro* on all subsequent trials, and vice-versa to the same extent – different-language target repetition was the only main effect that did not significantly interact with language in the cued block. The size of this effect was *a full order of magnitude greater* than the inhibitory effect exerted by naming non-target pictures. This indicates that local control is powerful; i.e., there really is something special about naming translation equivalents, and competition for selection between translations is particularly fierce. Furthermore, this competition can be symmetrical even in the presence of clear asymmetries in other aspects of data from the same bilinguals in the same task.

Khateb et al. (2017), who also used a cued picture naming task, reached a similar conclusion: When bilinguals were presented with a language cue *after* a picture was presented, they demonstrated symmetrical switch costs, whereas cues shown before the pictures led to marginally larger switch costs in the dominant language than for the nondominant language (i.e., asymmetric switch costs). To account for these results, they suggested that when pictures preceded the language cues, bilinguals initially prepared both names for the picture and then used lateral inhibition to ‘turn off’ the non-target name after the cue appeared. Their finding of symmetric switch costs is consistent with our conclusion that these inhibitory connections are equally strong in both directions. By contrast, when bilinguals do not prepare both names, language control operates at a more global level, and is stronger for the dominant than for the nondominant language. A question for further study concerns the extent to which the two types of control are automatic: While global control seems readily associated with habitual and purposeful selection of a default language in fully

⁴Alternatively or in addition, the slope of repetition priming effects would be greater for the cued block (which had six same-language repetitions) than for the single-language blocks (with twelve same-language repetitions) if repetition priming effects diminished over the course of the block even on a logarithmic scale. The fact that the effect of each trial number component might not be constant across its entire range – i.e., the necessity of appropriate scaling – is why we chose to keep the range of the non-target repetition component (0–96) identical for cued and single-language blocks in Analysis 3, as opposed to dividing the 108 trials in each block into ‘same-language target repetitions’ and ‘everything else’ (which would have had different ranges).

connected speech (Gollan & Goldrick, 2017; Myers-Scotton & Jake, 2009), it is less clear how lateral inhibition might be controlled with preparation as suggested by Khateb et al. (though the similarity in conclusions is striking given the very different types of bilinguals tested in the two studies).

Language-wide inhibition: For both languages in all blocks, repeatedly naming non-target pictures led to a slowdown in RTs that accumulated over time. In the single-language blocks, the dominant and nondominant languages were slowed by non-target repetition to the same degree (approximately 0.79 ms per picture for each language). Given that bilinguals would have no reason to apply language-wide inhibition to the target language at any time in a single-language block, this effect may reflect changes in attention and motivation over the course of the block (or within-language implicit learning; see Section 4.5.2 for further consideration of this point). At any rate, the lack of a difference between languages reinforces the likelihood that it indexes processes orthogonal to the theoretical questions of interest, especially given the striking contrast with analogous effects in the cued block.

In the cued block, an asymmetry emerged such that the dominant language was slowed more than the nondominant language. This asymmetry was numerically driven by an increase in dominant-language inhibition and a decrease in nondominant-language inhibition (relative to the single-language blocks), though only the change in dominant-language inhibition approached significance ($p = .051$). The changes between blocks are attributable to the fact that half of the pictures in the cued block were named in the non-target language. In other words, as the significant three-way interaction showed, naming unrelated pictures in a non-target language inhibited the dominant language more than the nondominant language.

4.2. Implications for models of bilingual language control

4.2.1. When is inhibition applied, and how long does it persist?—A key finding of the present study is that naming pictures in the nondominant language slows the subsequent naming of unrelated pictures in the dominant language more and more over time. To explain this finding under a global inhibition account of language control (Green, 1998) requires the assumption that every time bilinguals name a picture in the nondominant language, the dominant language receives some inhibition. Not only does this inhibition persist over trials, it also accumulates without plateauing (for at least as long as measured herein; i.e., 96 trials) such that the combined inhibitory effect is greatest at the end of the block (see Fig. 2). In contrast, the nondominant language receives no extra inhibition in the cued block relative to the slowdown experienced in the single-language block, consistent with the lack of an interaction between block type (single-language vs. cued) and non-target picture repetition in that language.

Is it necessary to assume that *every instance of nondominant naming generates increasingly accumulating inhibition*, or can the data be equally well accommodated by the weaker assumption that the dominant language only receives long-term inhibition on, say, nondominant switch trials (when global inhibition is freshly applied according to Green, 1998)? Although this alternative account could easily explain the larger inhibitory effect of non-target picture repetition on the dominant language in the cued block vs. in the single-

language blocks (which do not contain switch trials), it could not explain how performing a task in a nondominant-only block inhibits performance in a subsequent dominant-only block (e.g., Guo et al., 2011; Van Assche et al., 2013). In contrast, assuming that this long-term inhibition accumulates on every nondominant naming trial neatly explains block order effects, as inhibition should build even within a nondominant-only block. It further makes the prediction (which to our knowledge has not yet been tested) that performance in a dominant-only block should be worse when the preceding nondominant-only block was longer, as more nondominant trials means more opportunities to generate inhibition for the dominant language – a question we address in greater detail in Supplementary Analysis 5 in the Appendix.

A related but more symmetric mechanism appears to operate at the lemma level in the form of lateral inhibition, such that naming *dog* inhibits *perro* and vice-versa to the same extent. As discussed previously, this long-term inhibitory effect contrasts with facilitative effects of translation equivalents that are apparent in tasks with short-term semantic priming (e.g., naming a picture while ignoring a distractor word; Costa et al., 1999), and is potentially more in line with findings from tasks that measure cross-language effects at longer lags (Runnqvist et al., 2012). Similarly to the global inhibition effect described above, this inhibition accumulated over time without reaching a plateau, as different-language target repetition continued to exert a positive influence on RT throughout the block (see Fig. 2). In a sense, then, this effect is in direct conflict with repetition priming: Each time a bilingual names a picture in one language, naming the same picture on subsequent trials gets faster in the same language and slower in the other language – the result of increased activation of the target lemma and increased inhibition of its translation equivalent. This back-and-forth causes activation to trade off between translation equivalents. In other words, repetition made lateral inhibition an especially useful mechanism (one of several) to keep total activation in check.

Another solution to the problem of boundless activation accumulation is activation decay, which has been proposed in the context of non-linguistic task switching paradigms (e.g., Altmann & Gray, 2008). Although the time course of this decay may not be fast enough to account for within-trial effects (cf. Horoufchin, Philipp, & Koch, 2011), it could potentially operate on longer timescales. In the context of the present study, however, it would be difficult for decay-based mechanisms to fully supplant our proposed inhibitory mechanisms, as (minimally) it would be necessary to suppose faster decay rates for the dominant language than for the nondominant language – and at any rate it is not clear how such an account could simultaneously explain both symmetric lateral inhibition and asymmetric global inhibition.

4.2.2. How does stimulus repetition affect the application of inhibition?—Given that bilinguals in our experiments repeatedly named small sets of pictures in both languages, it is worth considering to what extent the accumulation of asymmetric global inhibition and symmetric lateral inhibition depend on this repetition, which does not characterize typical bilingual language production. If – as we propose – these mechanisms give rise to block order effects as described above, the answer is that this accumulation may not depend on repetition very much at all. For example, Van Assche et al. (2013) showed that English-

dominant Chinese-English bilinguals produced fewer responses in an English letter fluency task if they had previously performed a Chinese phonemic fluency task, even for fluency categories that did not overlap between languages. This demonstrates that nondominant language production can inhibit the dominant language at an item-independent level, and – crucially – that this global inhibition can be sustained over time, far after the next switch into the dominant language.

It is less straightforward to consider whether item repetition is necessary to the accumulation of lateral inhibition given that we think producing a word in one language is what causes its translation equivalent to be inhibited, and many published studies were not designed to address this question. For example, Francis et al. (2003, Experiment 1) showed that Spanish-English bilinguals named pictures more quickly in either language when they had previously named those same pictures once in the other language than when they had never previously named those pictures. Although this may indicate that lateral inhibition effects only emerge with repetition, we think it is more likely that the facilitation from having previously seen the stimulus before (vs. not at all) dwarfed the inhibitory effect of having named it in the other language. To address this concern more conclusively, future research could employ task designs that keep item repetition to a minimum while contrasting the effects of previously naming a picture in the other language vs. performing some other task with it (such as semantic categorization) that involves pre-lexical processes. We predict that holding item familiarity constant in this way should allow effects of lateral inhibition to emerge.

4.2.3. Remaining puzzles—A remaining question concerns why none of our trial number factors interacted with trial type (stay versus switch) even though we observed a highly robust switch cost asymmetry. Particularly surprising along this front are the absence of interactions between our two signatures of inhibitory control and trial type, as the switch cost asymmetry is typically assumed to reflect greater application of inhibition to the dominant language than to the nondominant language on switch trials (cf. Bobb & Wodniecka, 2013). The asymmetry in the present experiment indicated that bilinguals paid larger switch costs in their dominant than in their nondominant language – or, sliced by trial type, that bilinguals showed larger reversed dominance effects on switch trials than on stay trials, especially in the second half of the block. It is possible that the switch cost asymmetry reflects a third, more short-lived form of inhibition (or some other short-lived mechanism) that dissipates quickly, with the amount of that inhibition determined solely by the languages of the immediately preceding and current trials. In this circumstance, switch costs would not be captured by our analyses of trial number, which in turn exclusively reflect accumulation of inhibition over time in mixed-language blocks. (For another example of a dissociation between effects of preceding trials on shorter and longer timescales in the cued switching block, see Section 4.4.) Note too that it is not likely that our failure to observe significant interactions with trial type reflects a lack of power for observing such interactions: Based on the 95% confidence intervals computed for each effect, the statistical power for detecting interactions involving each trial number component was very similar, and sometimes even greater, for trial type (with which no trial number component interactions were observed) compared with language (with which two such interactions were observed).

However, we note that additional analyses of block order (see Supplementary Analysis 6 in the Appendix) revealed three-way interactions between block number, trial type, and two trial number components. Specifically, when bilinguals named more single-language blocks prior to the cued switching block, repetition priming effects in the cued block decreased on switch trials, and the inhibitory effect of naming non-target pictures in the cued block increased on stay trials. These results suggest that stay trials and switch trials may be differentially sensitive to the carryover of cumulative effects between blocks, even though accumulation within the cued block itself was the same across trial types.

Although the lack of within-block interactions between trial number components and switch costs may be surprising, this result obviates a potential confound stemming from our use of only a single cue (a flag) for each language. As a result of this design decision, switch trials required participants both to switch languages and to process a different cue relative to the previous trial. Using multiple cues for each task, researchers have shown that in cued switching paradigms, participants experience a cue encoding benefit on stay trials and a task switch cost on switch trials, both of which increase switch costs (in language switching: Heikoop, Declerck, Los, & Koch, 2016; in non-linguistic task switching: Logan & Bundesen, 2003). Thus, the switch costs in our study do not represent a “pure” measure of language switch costs. However, unless cue switch costs and language switch costs interacted with the same trial number component(s) in opposite directions – a possibility we find unlikely – we can conclude that neither cue switch costs nor language switch costs interacted with any component. Thus, the interpretation of our key results is not dependent on the source of our observed switch costs.

Another remaining question concerns why global inhibition was applied asymmetrically but local inhibition was applied symmetrically in the present analyses. The joint observation of symmetry and asymmetry in signatures of inhibition is not entirely unusual (e.g., see Christoffels et al., 2007; Prior & Gollan, 2011; both observed symmetrical switch costs but asymmetrical mixing costs, signatures of local and global control, respectively). The distinction between local and global control in the present study might reflect differences in relatively automatic bottom-up mechanisms (for which our bilinguals might have been relatively balanced, particularly given substantial repetition of a small set of items; Kleinman & Gollan, 2016) versus more controlled top-down mechanisms for managing dual-language activation (English is more clearly dominant in the environment at UCSD). Additionally, if this form of local control is relatively automatic (or “bottom-up” as described in Kleinman & Gollan, 2016), it means that the language system may naturally bias the expression of certain concepts in certain languages over time, leading to larger asymmetries between translation equivalents and thus making language control relatively easy – especially given the size of this lateral inhibition effect. Conversely, when such a bias does not emerge, substantial global control (“top down” control) will be needed to resolve competition between translations instead. Further study of how bilingual control evolves over time across different types of bilingual populations will be needed to answer these questions.

4.3. Implications for observing and interpreting reversed dominance effects

Our results also shed light on when and where reversed dominance effects are likely to be found. In the present study, bilinguals exhibited standard language dominance effects in the single-language blocks, though this dominance effect decreased over time in line with asymmetric repetition priming effects. In the cued switching block, bilinguals initially showed a standard dominance effect plus asymmetric switch costs (see Fig. 1). As the block progressed, however, the combination of greater repetition priming for the nondominant language plus greater inhibition for the dominant language from naming non-target pictures caused language dominance to fully reverse on both stay trials and switch trials. Without taking this evolution into account, the typical strategy of computing block-wide means for each condition could yield different results depending on block length: standard dominance effects in a short block, no dominance effects in a medium block, and reversed dominance effects in a long block.

To demonstrate that this dominance reversal over time can be captured using simpler statistical models, we conducted an F1 ANOVA on the cued block data using the three factors shown in Fig. 1: Language, Trial Type and Block Quarter (coded here as a nominal variable with four levels). There was a significant main effect of reversed dominance, $F(1, 414) = 16.2, p < .001$, and a linear trend showing that this reversal increased over the block, $t(1242) = -9.77, p < .001$. Specifically, standard dominance effects were significant in the first quarter but reversed dominance effects were significant in every other quarter, all $|t|s > 2.97$, all $ps < .01$. This dominance reversal was driven by a quadratic trend in the nondominant-language RTs, indicating that an initial decrease leveled off later in the block, and a linear trend in the dominant-language RTs, indicating that they increased at a relatively fixed rate throughout the block, both $|t|s > 6.10$, both $ps < .001$.

Furthermore, to check that the null interactions between switch costs and trial number components observed in the mixed-effects models were not driven by collinearity, we performed similar analyses separately for each trial type. On stay trials, a standard dominance effect was significant in the first quarter and reversed dominance effects were significant in every other quarter (dominance effects for quarters 1–4: 42 ms, –16 ms, –20 ms, –28 ms), all $|t|s > 2.02$, all $ps < .05$. On switch trials, the same pattern was obtained (17 ms, –16 ms, –43 ms, –55 ms), all $|t|s > 2.01$, all $ps < .05$. Trial type did not interact with block quarter (either with or without language), both $Fs < 1.36$, both $ps > .25$. Finally, consistent with the null three-way interaction, reversed dominance effects were significantly larger on switch trials than on stay trials by a relatively constant amount (reflecting the switch cost asymmetry) in block quarters 1, 3 and 4, all $|t|s > 2.16$, all $ps < .05$, though there was no difference in quarter 2, $|t| < 1$. Thus, dominance reversed over time on both stay and switch trials, and did so by approximately the same amount.

These results may explain in part why the appearance of reversed dominance effects in language switching studies has been so fickle (Declerck & Philipp, 2015). Experiments across labs and even in the same labs often differ widely on parameters such as item repetition and block length, both of which appear to be linked to the emergence of reversed dominance. Furthermore, different populations of bilinguals often show different effects; for example, highly proficient bilinguals may be more likely to show symmetric switch costs

and reversed dominance than second language learners (e.g., Costa & Santesteban, 2004; Costa et al., 2006). Note, however, that we observed stable asymmetric switch costs and a dominance effect that significantly reversed later in the block. This suggests that these signatures of bilingual language control may not be in opposition, but may simply exist on different timescales.

Our results indicate that differences in reversed dominance effects between populations could potentially be driven by how quickly they can adjust the balance of activation between their languages. Specifically, if applying global inhibition to the dominant language when selecting words in the nondominant language constitutes an adaptive strategy to facilitate language mixing, highly proficient bilinguals may apply this inhibition at a faster rate, leading to larger reversed dominance effects (though this explanation does not account for differences between populations in the switch cost asymmetry).

4.4. Learning at different timescales

Our account has focused primarily on learning effects that accumulate and persist over the course of a block. Here, we look at effects of trial sequence that emerged on a shorter timescale. (We also performed several analyses of block sequence that emerged on a longer timescale; those are presented in Supplementary Analyses 5 and 6 in the Appendix.)

As a rule, the effect of trial sequence that has garnered the most attention in the bilingual language switching literature is the switch cost – that is, the effect of trial $n-1$ on trial n – with some additional focus on trial $n-2$ repetition costs as a less ambiguous marker of language-wide inhibition (with trilinguals: Declerck et al., 2015; Philipp et al., 2007; for a review of analogous phenomena in non-linguistic task switching, see Koch, Gade, Schuch, & Philipp, 2010). Is the cost of switching languages on trial n greater when a bilingual used the non-target language not only on trial $n-1$ but on trial $n-2$ (and $n-3$, and $n-4$, etc.) as well? After all, if every act of retrieval in the nondominant language hinders subsequent retrieval in the dominant language, we might expect that switches into the dominant language should be more costly after longer stretches of nondominant language use.

As we noted above, Meuter and Allport (1999) investigated this question using a digit naming task and found that switch trial RTs were unaffected by the number of consecutive same-language cues immediately preceding the switch. (In keeping with their terminology, we use the term *run length* to describe the number of consecutive same-language trials that were named immediately before the target. For example: Given the sample sequence of trials mentioned earlier (*ballena*, *star*, *hand*, *bone*, *mano*), *mano* (a switch trial) would have a preceding run length of 3 and *bone* (a stay trial) would have a preceding run length of 2 – because in each case, that is the number of consecutive same-language trials that immediately preceded it.) Instead, Meuter and Allport found that stay trial RTs increased for both languages as run length increased. This suggests that repeating the same language may have led bilinguals to expect a language switch, causing them to be slower when it repeated (though see the discussion of “within-run slowing” by Altmann & Gray, 2008, for an alternative task-set decay account of a similar pattern in non-linguistic task switching). Using a speeded picture naming task, Zheng et al. (2018) found that bilinguals made fewer errors when switching into L1 after a longer (L2) run than after a shorter run – a pattern

consistent with the idea that bilinguals increasingly expected a switch into L1 – but the difficulty of switching into L2 was unaffected by (L1) run length, leading to a language asymmetry that makes it difficult to describe their results in terms of expectancy (as it is not clear why bilinguals would increasingly expect a language switch more into L1 than into L2).

Before attempting to reconcile these findings with the block-wide effects considered in this paper, we wanted to perform an analysis comparable to those reported by Meuter and Allport (1999) and Zheng et al. (2018). To do this, we examined how RTs changed as a function of the immediately preceding run length (which ranged from 1 to 6) on both stay and switch trials, using the same definition of run length as Meuter and Allport (1999) and including all trials in the cued switching block for all participants. Data were analyzed separately for each trial type, as well as separately for each combination of trial type and language; to facilitate convergence, random correlations were removed from the cross-language model with stay trials.

Surprisingly, and despite the longer-term inhibitory effects reported above, switch trial RTs were 11 ms *faster* for each extra picture named in the other language during the run immediately preceding the switch, $B = -10.7$, 95% CI = $[-14.4, -7.1]$, $t(93) = -5.86$, $p < .001$ (dominant language: $B = -10.7$, 95% CI = $[-15.6, -5.7]$, $t(82) = -4.28$, $p < .001$; nondominant language: $B = -11.8$, 95% CI = $[-16.5, -7.2]$, $t(648) = -5.01$, $p < .001$). Interestingly, and in direct contrast to the results of Meuter & Allport (1999), the same pattern was observed for stay trial RTs, which were 8 ms faster for each preceding picture named in the same language in the current run, $B = -8.0$, 95% CI = $[-12.5, -3.4]$, $t(6) = -4.32$, $p = .005$ (dominant language: $B = -8.5$, 95% CI = $[-13.5, -3.4]$, $t(7) = -3.94$, $p = .005$; nondominant language: $B = -10.0$, 95% CI = $[-15.2, -4.8]$, $t(28) = -3.98$, $p < .001$). Also in contrast to the results of Zheng et al. (2018), language did not interact with run length for either trial type (both $|B|$ s and both $|t|$ s < 1). Given this pattern, the speedup does not reflect processes related to expecting a switch (or else it would not have been evident on stay trials) or to language activation asymmetries. In this, our results differ from those of Meuter and Allport (1999) for both stay and switch trials, and they differ from those of Zheng et al. (2018) for the nondominant language. Instead, our results seem to suggest that repeatedly using a single language – either language – somehow facilitates word retrieval in *both* languages over the short term.

The mechanism for this facilitation is unclear. Perhaps it reflects autocorrelations between RTs on consecutive trials, such that faster RTs during language runs (facilitated due to repeated use of the same language) speed *all* subsequent processing. That does not explain why other researchers have not observed this pattern before, though this difference in results may be attributable to differences between studies in task (Zheng et al. employed substantial time pressure) and materials or participants (Meuter and Allport had only $n=16$ bilinguals with varying language background likely including late and relatively low-proficiency bilinguals, and they named digits rather than pictures). More generally, it reinforces the value of our present approach by indicating another divergence between short- and long-term effects on naming latencies: Naming a picture in the nondominant language impedes dominant language production for the rest of the block (as shown in the analyses presented

in the manuscript) even though – in the short term – switching into the dominant language is apparently easier immediately after naming a longer sequence of pictures in the nondominant language (as shown in this section and by Zheng et al., 2018). Given these opposing effects, it is perhaps less surprising that switch costs – which reflect a temporal influence even more local than the preceding run – do not interact with any trial number components: One general takeaway seems to be that variables representing the influence of prior trials on different timescales do not need to agree in polarity or operate via the same mechanism.

4.5. Alternative explanations

Although we have framed our results in terms of language-wide and lateral inhibition, other mechanisms and explanations can account for at least some facets of the data. Those alternatives are considered here.

4.5.1. Inhibition-free account: Global activation of the nondominant language

—As previously noted, it is possible to account for many empirical results in the bilingual language switching literature by assuming that, to speak in their nondominant language, bilinguals globally increase the activation of their nondominant language instead of (or in addition to) globally decreasing the activation of their dominant language via inhibition (though some results from trilingual language switching experiments seem impossible to explain without inhibition; cf. Declerck et al., 2015; Philipp et al., 2007). Although the present results are not an exception to this rule, the combination of assumptions needed to make an exclusively activation-based account work are rather tortuous (see also Gollan & Goldrick, 2017).

First, as noted above, we replicated prior findings (e.g., Meuter & Allport, 1999) of asymmetric switch costs in the cued block that were larger for the dominant language than for the nondominant language. However, as this asymmetry can be explained both by the global inhibition and global activation accounts (Bobb & Wodniecka, 2013), it is not diagnostic of how bilinguals adjust the balance of language activation to facilitate language mixing.

The asymmetric effects of non-target repetition, however, are more informative. Under an exclusively global activation account, instead of reflecting cumulative inhibition of the dominant language, the inhibitory effect of non-target picture repetition reflects cumulative activation of the nondominant language. Several versions of this account are plausible: This activation might accumulate every time a picture is named in the nondominant language (a) regardless of block or trial type (i.e., always); (b) in a task requiring language mixing (such as a cued switching block) regardless of trial type; or (c) only on switch trials, when competition between languages is greatest. In all cases, the underlying idea is that this extra activation increases between-language competition when bilinguals need to produce the dominant language.

If naming a picture in the nondominant language always increases its activation level regardless of context, language and non-target repetition should have interacted both in the cued block and (to a smaller degree) in the single-language blocks. This is because the

accumulating activation on the nondominant language should have counteracted some of the slowdown from factors like waning attention and motivation. As no such interaction was observed in the single-language blocks (where in fact the slowdown was numerically larger – not smaller – for the nondominant language), this account must be discarded.

The alternative is for nondominant activation to accumulate only in the cued switching block, either on all trials or only on switch trials. However, under either account, a separate mechanism would be needed to explain how a nondominant-only block can hinder subsequent performance in a dominant-only block (e.g., Guo et al., 2011; Van Assche et al., 2013). Thus, although it is possible to account for both block order effects and these within-block effects using two separate inhibition-free mechanisms, we conclude that it is more parsimonious to assume a single global inhibition-based mechanism as described above. (Note that in either case, an additional mechanism – such as symmetric inhibition between translation equivalents – is needed to account for the inhibitory effects of previously naming the target picture in the non-target language, and that our results do not rule out the possibility that both inhibition- and activation-based mechanisms operate simultaneously.)

4.5.2. Implicit learning—Given the surprising longevity and seemingly endlessly-accumulating nature of the inhibitory effects at both global and local levels, it is fair to ask whether these slowdowns represent inhibition at all. Although bilingual language control clearly requires several mechanisms operating on different timescales, activation and inhibition are more commonly construed as impermanent phenomena representing the rise and fall of accessibility within a system. If naming a picture causes long-term changes to the language system, a central mechanism of change may be implicit learning.

This alternative would be in keeping with a recent trend in psycholinguistics toward viewing the language system as a dynamic system that is constantly retuning itself to facilitate future acts of production and comprehension (Chang, Dell, & Bock, 2006; Dell & Chang, 2014; Fine & Jaeger, 2013; Howard et al., 2006; Oppenheim, Dell, & Schwartz, 2010; Pickering & Garrod, 2013; Runnqvist et al., 2012). Under one such proposal, for example, a speaker who successfully selects the name of a picture of a dog increases the strength of the connections between active semantic nodes (<ANIMAL>, <PET>) and the target lemma (*dog*), and simultaneously decreases the strength of the connections between those semantic nodes and active non-targets (e.g., *cat*). This will make it easier to select *dog* in the future because it will receive more activation from those semantic nodes – leading to repetition priming – but it will make it more difficult to select *cat* because it will receive less of this activation (leading to interference in the form of slower RTs; Oppenheim et al., 2010). Crucially, this implicit learning process is error-driven, meaning that the connection between <ANIMAL> and each non-target will be weakened in proportion to the activation level of that non-target. As a result, the connection to *cat* will likely be weakened more than the connection to *octopus*, and connections to a non-target that received no activation would not be reweighted at all.

Applied to a bilingual language switching context, such a mechanism naturally accounts for some of the results observed in the present study, as it permits the selection of a word to affect the balance of activation between languages – or between words across languages – on

all subsequent trials. Thus, selecting the lemma *dog* strengthens the connections from the shared semantic features to *dog* and weakens the connections from those features to *perro*, generating both facilitative effects of same-language target repetition and inhibitory effects of different-language target repetition. In addition, assuming that bilinguals were able to identify a response set of potential picture names (not a difficult task given that most bilinguals only named nine unique critical pictures throughout the experiment), lemmas in the response set would typically be highly active on each trial. This could in turn cause the act of lemma selection to weaken links between a contextual response set node and the non-target lemmas, leading to interference between unrelated pictures – even within the same language, potentially accounting for some of the slowdown observed in single-language blocks. Furthermore, if the dominant language is generally more highly activated than the nondominant language, this weakening would be greater for the dominant language, leading to asymmetric interference (as observed in the cued block; but see Runnqvist et al., 2012).

Many details of this account remain to be worked out. For example, it is not clear why such a mechanism should have led the dominant language to become *less* active than the nondominant language by the end of the cued blocks, given that both languages were used equally often and thus reweightings should have become more symmetric as the asymmetry in language activation decreased. Such a result potentially indicates that, rather than being mutually exclusive alternatives, implicit learning and inhibitory mechanisms may work in tandem. Accordingly, determining whether and how implicit learning facilitates language switching represents a promising avenue for future research.

4.5.3. Competing stimulus-response or stimulus-task bindings—Although naming a picture in one language clearly slowed naming of the same picture in the other language, this interference may be attributable to competing stimulus-response bindings rather than lateral inhibition. To investigate this kind of competition, Waszak, Hommel, and Allport (2003, 2005) used a complex task-switching paradigm in which participants were presented with picture-word stimuli and engaged either in word reading (ignoring the picture) or picture naming (ignoring the word) on each trial. Participants were slower to read a word when it was paired with a picture that the participant had previously named (vs. an unnamed picture), even when that naming instance occurred many trials previously, but this cost was only apparent on switch trials (though see Koch & Allport, 2006, for discussion of how this cost is only apparent when the interval between cue and stimulus is very short). This indicates that the act of naming the picture caused participants to form a binding between the stimulus and its associated response. The binding was reactivated by the subsequent presentation of the picture on a word naming trial, leading to a response (the picture name) that had to be suppressed – but this reactivation only occurred when the task schema was active; i.e., when the participant had engaged in picture naming on the preceding trial (see also Allport & Wylie, 2000, Experiment 5). In contrast, although participants were slower to name a picture when it was paired with a word that the participant had previously read aloud (vs. an unread word), this effect manifested on both stay and switch trials. This reflects the fact that word reading – the more automatic of the two tasks – retained an active task schema throughout the experiment, interfering even with runs of consecutive picture naming trials.

This finding is potentially relevant to the present study, as it points to another potential locus of the interference from naming the same pictures in two languages. Specifically, naming a picture of a dog in English may strengthen its stimulus-response binding to the word *dog*, making it more difficult to ignore *dog* when subsequently trying to select *perro* and leading to competition. Furthermore, stimulus-response bindings can accumulate strength over multiple repetitions (Waszak et al., 2003), matching the cumulative nature of the different-language target repetition effect. However, given that this effect was statistically equivalent on stay and switch trials, and even numerically trended (weakly) toward *less* interference on switch trials, it is difficult to see how the stimulus-response binding explanation fully accounts for it. It would be necessary to assume that picture naming in *both* languages behaved throughout the experiment as a “less automatic” task; i.e., that each language interfered with the other on every trial. This assumption is difficult to reconcile with the asymmetric activation observed here and in other studies, and would necessitate a model in which each language simultaneously generates substantial interference and is substantially affected by interference from the other language (contra the idea that language activation can be represented by a single measure). As a result, we conclude that while stimulus-response bindings may well affect the ease of language switching in paradigms that repeat items across languages (Kleinman & Gollan, 2016), they cannot fully explain the inhibitory effect that this repetition generated in the present study.

Another possibility is that the appearance of lateral inhibition effects was driven by competing stimulus-*task* bindings. In a task switching experiment using digit stimuli, Koch and Allport (2006) had participants use manual button presses to categorize either magnitude (greater or less than 5) or parity (odd or even). When participants repeatedly performed the same task with a given stimulus, they were subsequently much slower to perform the opposite task with the same digit even when the unpracticed task required the same manual response as the practiced task (with, e.g., both ‘less than 5’ and ‘odd’ requiring left button presses for the digit 3). As that stimulus-response binding was consistent across tasks, this cost indicates that the competition in question occurred between stimulus-*task* bindings.

Given that bilinguals in our experiments almost exclusively called every picture (stimulus) by a single name (response) in each language (task) – resulting in three-way stimulus-*task*-response bindings – we are unable to dissociate the potential influences of stimulus-response and stimulus-*task* bindings on language switching. Although it would be possible to tease these apart using, for example, pictures that could be named in two ways in each language (cf. Dylman & Barry, 2018), or pairs of pictures for each concept, these stimulus-*task*-response bindings represent an underlying truth about language switching specifically and the linguistic domain more generally: That the set of possible responses is many orders of magnitude larger, and the stimulus-response and task-response bindings are far more practiced, than in non-linguistic task switching paradigms. As a result, we think it would be unclear how to interpret the separate contributions of each binding to the present results when considered in isolation from the other.

4.5.4. Artifacts of collinearity—As noted, collinearity between trial number components in the present study (and between their interactions with language and/or trial type) was quite high. For this reason, it is important to consider a theoretically uninteresting but

mathematically possible hypothesis: That our statistical models essentially manufactured inhibitory effects of different-language target repetition and non-target repetition to counteract a genuine effect of repetition priming that decreased in strength over the course of the block. In other words, the model could have overestimated the facilitative effect of repetition priming at the end of the block, thereby underestimating those RTs and needing another mechanism – such as cumulative inhibition – to offset that underestimation.

We think this is unlikely to explain away our key results for several reasons. First, as is clear from Fig. 1, dominant-language RTs unquestionably increased over the course of the cued block (see Section 4.3). Although nondominant-language RTs decreased overall in the same block, they did not exhibit the monotonically decreasing trend that was apparent in the single-language blocks, seeming to increase slightly toward the end. Repetition priming alone cannot explain why RTs increased; thus, other mechanisms are needed.

Second, *even if* non-target repetition effects emerged as a statistical fluke to offset the effects of repetition priming, this could not explain the observed pattern of data. Specifically, in the cued switching block, bilinguals showed greater facilitative effects of same-language target repetition in the nondominant language and greater inhibitory effects of non-target repetition in the dominant language. In contrast, a ‘fluke’ account would predict that greater facilitation in one language would be offset by greater inhibition in that same language.

Finally, it is noteworthy that all three trial number components yielded significant main effects in the directions that psycholinguistic theory led us to predict. Although it might have been possible to interpret (say) a small facilitative effect of different-language target repetition as a stimulus priming effect or perhaps as long-term facilitation from translation equivalents, no statistical model could have convinced us that naming a particular picture in a particular language becomes more difficult with practice or that naming unrelated pictures in the non-target language could facilitate naming.

4.6. Conclusions

Most research into bilingual language switching has assumed that bilinguals switch between several different static states – essentially, various language activation profiles – that remain constant. Here, we have shown that assumption to be false: The states themselves change continuously over time during increasingly longer periods of language mixing. Furthermore, the joint appearance of symmetric and asymmetric effects within the same experiment marks the operation of multiple control mechanisms at different processing levels. In a re-analysis of data from 416 Spanish-English bilinguals who performed a picture naming task, we showed that naming a picture in either language hindered its subsequent naming in the other language, that naming a picture in the nondominant language hindered the naming of all pictures in the dominant language, and that both of these effects built over time. These results are most parsimoniously explained by assuming that bilinguals exercise language control by employing persistent, cumulative inhibition at both global and local levels. Additional mechanisms may be needed given that, as we demonstrated, performance in language switching tasks is influenced by short- and long-term effects unfolding over (at least) four different timescales: the previous trial (switch costs), the preceding run of trials (Section 4.4), all earlier trials in the same block (Analyses 1–3), and earlier blocks

(Supplementary Analyses 5 and 6 in the Appendix for the single-language blocks and cued switching block, respectively).

Importantly, as inhibition is a construct that applies much more broadly than bilingual language control – perhaps especially the global form of inhibition observed here – it would seem important to consider accumulation over time in related domains of cognitive psychology. What we have shown here is that failure to do so could easily obscure important findings and lead to seemingly inconsistent results across studies that vary in small but often critically important factors such as the order and number of trials presented to each participant, item set size, and whether and how often items are repeated.

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Appendix A

The statistical models presented in this paper necessarily reflect our subjective decisions about which data to include and which factors to analyze. Although our guiding principles were to include as much data as possible in each analysis and to exclude covariates, we present additional analyses here to supplement those in the main body of the text by testing different hypotheses and addressing possible alternative explanations. The six supplementary analyses consider the effects of using different groups of participants (1: only English-dominant bilinguals; 2: only bilinguals who named single-language blocks), conditions (3: only stay trials in the cued block), or factors (4: different-language non-target picture repetition), as well as effects of block order (5: on single-language blocks; 6: on the cued block).

Analyses restricted to subsets of participants

Supplementary Analysis 1: Restricting all analyses to English-dominant bilinguals

Our sample of 416 participants consisted of 381 (91.6%) English-dominant bilinguals and 35 (8.4%) Spanish-dominant bilinguals. To reduce heterogeneity in our sample, we

conducted a set of supplementary analyses of all blocks (Analyses 1, 2, and 3) using only the data from the English-dominant bilinguals.

The only effect that reached a different level of statistical significance in any analysis (crossing the $p=.05$ threshold) was the three-way interaction reported in Analysis 3 between non-target repetition, block type, and language, which was significant with all participants, $B = -0.41$, $p = .043$, but reached only marginal significance here, $B = -0.42$, 95% CI = $[-0.86, 0.02]$, $t(46) = -1.92$, $p = .061$. However, as the size of the effect was the same across analyses, this likely reflects a slight decrease in power from using a smaller (if more linguistically homogeneous) sample.

Supplementary Analysis 2: Restricting cross-block analyses to participants who named single-language blocks

All bilinguals named pictures in a cued language switching block, but only some (288/416) also named pictures in two single-language blocks. This means that participants in our cross-block analysis (Analysis 3) contributed data to variable numbers of conditions. To determine whether our key results changed if this analysis was performed wholly within-participant, we conducted a supplementary cross-block analysis using only the data from participants who named both a cued block and two single-language blocks.

Two effects reached a different level of significance (crossing the $p=.05$ threshold). First, the main effect of language – which was significant with all participants, $B = 19$, $p = .035$ – was not significant here, $B = 11$, 95% CI = $[-3, 25]$, $t(22) = 1.68$, $p = .107$. This likely reflects the fact that some participants in the restricted sample named a block of nondominant-only pictures before the cued block, thereby reducing standard dominance effects. Second, when looking only at the dominant language, the two-way interaction between block type and non-target picture repetition – which was marginally significant with all participants, $B = 0.28$, $p = .051$ – was significant here, $B = 0.35$, 95% CI = $[0.06, 0.63]$, $t(183) = 2.39$, $p = .018$. This shows that the inhibitory effect on the dominant language from naming unrelated pictures in the nondominant language was stronger when examined entirely within-participant. Consistent with this, the three-way interaction between block type, language, and non-target picture repetition remained significant, $B = -0.42$, 95% CI = $[-0.79, -0.06]$, $t(903) = -2.27$, $p = .023$.

Analyses restricted to subsets of conditions

Supplementary Analysis 3: Restricting cross-block analyses to non-switch trials

When comparing performance across single-language blocks and cued switching blocks, researchers often restrict their analyses to stay (i.e., non-switch) trials, which holds constant the influence of the previous trial's language cue and thus permits a purer measure of how language production differs between contexts in which bilinguals do vs. do not need to maintain readiness to use the other language (e.g., Christoffels et al. 2007; Gollan & Ferreira, 2009; Wang, Kuhl, Chen, & Dong, 2009). We chose not to do this for our cross-block analysis (Analysis 3) for two reasons, one theoretical and one practical. From a theoretical standpoint, the fact that we found no interactions whatsoever between trial

number components and trial type (all $|t|s < 1$) meant we had no reason to expect that excluding switch trials would change the key pattern of results. From a practical standpoint, since we already had more usable data from single-language blocks (60,180 trials) than from cued blocks (25,812 stay trials and 15,470 switch trials), we did not want to discard 37% of the cued-block data, which would reduce our statistical power and increase the imbalance of usable data between block types from 1.5:1 to 2.3:1. Nevertheless, as it is reasonable to ask whether our key effects change when we remove switch trials from the cross-block comparison, we conducted such an analysis.

The only effect that reached a different level of statistical significance (crossing the $p=.05$ threshold) was the three-way interaction reported in Analysis 3 between non-target repetition, block type, and language, which was significant with all participants, $B = -0.41$, $p = .043$, but was not significant here, $B = -0.33$, 95% CI = $[-0.76, 0.11]$, $t(168) = -1.49$, $p = .138$. This lack of significance does not appear to be driven solely by the loss in power from discarding 37% of cued trials, as the modest increase in SE (relative to the model reported in the paper) is proportional to the amount of data discarded; instead, it more reflects the 20% decrease in the numeric size of the effect (from -0.41 to -0.33). This potentially suggests that the three-way interaction is carried by a greater slowdown over time for dominant switch trials than for nondominant switch trials.

However, note that this does not imply that the three-way interaction is simply redundant with the switch cost asymmetry we observed (greater switch costs in the dominant language than in the nondominant language in the cued block). The observed three-way interaction would be expected only if that asymmetry increased over the course of the cued block.

Additionally, the evidence for the asymmetry increasing over the course of the block is relatively weak. When looking at the data from each fourth of the cued block (as analyzed in Section 4.3), dominant switch costs were greater than nondominant switch costs by 24 ms to 27 ms in block quarters 1, 3 and 4; the only exception to this trend was in quarter 2, where switch costs in each language were equal (difference = 0 ms).

Analyses with different factors

Supplementary Analysis 4: Effects of naming different-language non-target pictures on cued language switching

In our analysis of the cued block, we included a trial number component that indexed the number of non-target pictures previously named in either language. In the main text, we noted that due to collinearity, we were unable to separate out the influences of previously naming non-target pictures in the same language vs. in a different language. To reinforce this point, we conducted an analysis of the cued block data that was identical to Analysis 1 except that the non-target picture repetition variable only counted non-target pictures previously named in the non-target language. As expected, all effects reached the same level of statistical significance.

Analyses of block order

Supplementary Analysis 5: Effects of block order on single-language blocks

In our attempt to reconcile block order effects and within-block effects using a common set of mechanisms – item-specific lateral inhibition for both languages and global inhibition for the dominant language – we implicitly assumed that naming a picture in either language triggers inhibitory processes regardless of whether it is named in a single-language block or a cued switching block. If it is true that naming pictures in either language generates lateral inhibition but that only naming pictures in the nondominant language generates global inhibition, this should be evident in block order effects as well: The dominant-only block should have been slowed when bilinguals previously used the nondominant language (in the nondominant-only or cued switching blocks) to a greater extent than the nondominant-only block was slowed when participants previously used the dominant language (in the dominant-only or cued switching blocks).

To test this prediction, we conducted an analysis examining how performance in single-language blocks was affected by block order. Specifically, for each language, we compared performance in the appropriate single-language block between three groups of participants who named that single-language block (a) as the first block (i.e., before any other block); (b) as the second block, immediately after the cued switching block; and (c) as the second block, immediately after the other single-language block. This means that (for example) prior to the dominant-only block, participants in (a) named no pictures in either language, participants in (b) named every picture six times in each language, and participants in (c) named every picture 12 times in the nondominant language.

For the long-term inhibitory mechanisms we describe in this paper, bilinguals in group (c) should have shown the most lateral inhibition (for both languages) and global inhibition (of the dominant language). Bilinguals in group (a) should have shown the least (because there were no prior naming trials on which that inhibition could have been generated). Predictions for group (b) are less obvious because participants should also have experienced repetition priming, but its range should be bounded by (a) and (c).

For the analysis of the dominant-only block, groups (a), (b) and (c) had *n*s of 68, 25 and 27, respectively; for the analysis of the nondominant-only block, these groups had *n*s of 77, 23 and 23, respectively. Participants who named both single-language blocks as their first two blocks (*n*=50) were included in group (c) for one single-language block and group (a) for the other single-language block; other participants (*n*=143) were included in only one condition, making most comparisons between-subjects. To contrast the effects of block order on the two single-language blocks, the data were submitted to a mixed-effects model with three fixed effects: language, block order (a three-level nominal variable), and their interaction. As significance testing for nominal variables with three or more levels cannot be performed the same way as above, we recoded block order into two sum-coded variables and performed significance testing of all fixed effects via nested model comparison, holding out the relevant variable(s) while leaving the maximal random effects structure unchanged. The cross-language block only converged after correlations between random effects were removed.

Mean by-participant latencies are shown in Fig. A.1 and reported below for each condition. In the cross-language analysis, participants named pictures more slowly in the nondominant-only block than in the dominant-only block – a standard dominance effect, $\chi^2(1) = 6.74$, $p = .009$. They also showed a significant effect of block order, $\chi^2(2) = 14.23$, $p < .001$, which was marginally different for the two languages, $\chi^2(2) = 5.21$, $p = .073$. To explore this marginal interaction, we considered the effect of block order separately for each single-language block, performing pairwise contrasts between levels of block order using the lsmeans package (version 2.27-61; Lenth, 2016) with family-wise error controlled within each language via the Tukey method, and considered the effect of language separately for each block order.

The effect of block order on the dominant-only block was significant, $\chi^2(2) = 7.86$, $p = .020$. Pairwise comparisons indicated that when the dominant-only block was named first, participants named it faster (666 ms) than when it was named after the cued switching block (710 ms; $t(118) = -2.38$, $p = .0497$) and marginally faster than when it was named after the nondominant-only block (703 ms; $t(123) = -2.11$, $p = .091$), with no difference between the two named-second conditions, $t < 1$.

The effect of block order on the nondominant-only block was marginally significant, $\chi^2(2) = 5.03$, $p = .081$. Pairwise comparisons indicated that the nondominant-only block was named marginally faster when it was named after the cued switching block (695 ms) than after the dominant-only block (750 ms; $t(119) = 2.11$, $p = .092$), but that participants who named the nondominant-only block first (708 ms) did not reliably differ from either group (after cued: $t < 1$; after dominant-only: $t(117) = -1.95$, $p = .130$).

Fig. A.1.

Mean by-participant picture naming latencies for each single-language block (dominant-only and nondominant-only) as a function of block order. Bilinguals were included only if they named a single-language block (a) as the first block, (b) as the second block after a cued switching block, or (c) as the second block after the other single-language block. Of the bilinguals in this analysis, most ($n=143$) contributed data to only one condition; the rest ($n=50$) named the two single-language blocks before any other blocks and thus contributed data to condition (c) for one block and condition (a) for the other. Error bars = 95%

confidence intervals. Statistically significant ($p < .05$) and marginally significant ($.05 < p < .10$) comparisons between block order conditions for each single-language block are denoted with * and ~ symbols, respectively; non-significant comparisons are not shown.

Slicing the two-way interaction by block order instead of by language, bilinguals showed a significant 46 ms standard dominance effect (dominant language faster than nondominant) when those blocks were named first in the block order, $\chi^2(1) = 9.76$, $p = .002$, and a marginally significant 53 ms standard dominance effect when those blocks were named second immediately after the other single-language block, $\chi^2(1) = 3.11$, $p = .078$. However, bilinguals who named single-language blocks second immediately after the cued switching block showed a non-significant 13 ms reversed dominance effect, $\chi^2(1) = 0.48$, $p = .488$.

We are reluctant to draw strong conclusions from these data given the marginally significant results; the large error bars (attributable to largely between-subjects comparisons and small sample sizes for named-second conditions); and the corrections for multiple comparisons when examining effects of block order (which control Type I error rate while increasing Type II error rate). However, the data from the block order analyses suggest that for the dominant-only block, any use of the nondominant language may have slowed RTs. In contrast, for the nondominant-only block, participants were marginally slower only when they had just named the dominant-only block – i.e., when inhibition had built up from repeatedly naming pictures in the dominant language (without being counteracted by repetition priming in the nondominant language, as it was in the cued block).

In some ways, these results are consistent with our account in that only the dominant language showed signs of global inhibition – a pattern also reflected in the elimination of single-language block dominance effects among bilinguals who named the cued switching block first. At the same time, our account also predicted that naming in the dominant-only block should have been slowed more after the other single-language block (relative to the named-first condition) than the nondominant-only block was – and this was not the case. More work remains to be done to determine how the act of language switching affects subsequent language activation and control in single-language blocks, ideally with greater statistical power as block order effects by definition require between-subject comparisons.

Supplementary Analysis 6: Effects of block order on cued language switching

As noted, only some participants (288/416) named pictures in two single-language blocks. Given that block order was fully counterbalanced, participants named the cued block either after zero single-language blocks ($n=224$; 53.8%), after the dominant-only block ($n=47$; 11.3%), after the nondominant-only block ($n=48$; 11.5%), or after both single-language blocks ($n=97$; 23.3%). Given reports of block order effects on language switching performance (Guo et al., 2011; Misra et al., 2012; Van Assche et al., 2013; for possibly related phenomena in voluntary language switching, see Gollan & Ferreira, 2009), we explored the effects of single-language block order on the cued switching block.

To do this, we conducted an analysis of the cued block data that included all factors reported in Analysis 1 as well as a continuous variable indexing the number of single-language blocks named before the cued block (0, 1, or 2). This block order variable, which we will

refer to as Block Number (an imprecise label because it excludes voluntary or bottom-up blocks not considered here; see Kleinman & Gollan, 2016 for details), was permitted to interact with all other main effects and interactions, and appropriate random slopes were added for all new fixed effects. (For participants who named one single-language block before the cued block, we did not distinguish between the dominant-only and nondominant-only blocks, as this would have increased the number of fixed effects and random slopes by item to 64 each.) When the model did not converge even after correlations between random effects were removed, we simultaneously removed all random effects accounting for no variance as well (there were seven; REML deviance was identical regardless of whether these effects were included, and no t value changed by more than .001).

As all fixed effects reported in Analysis 1 had the same statistical significance here, we focus only on effects of block number. For each extra single-language block preceding the cued block, participants named pictures 22 ms faster overall, and the switch cost asymmetry marginally decreased by 14 ms. There were also several interactions between block number and two of the three trial number components. With each extra block, repetition priming effects (the facilitative effect of previously naming the same picture in the same language) marginally decreased by 6 ms overall, the repetition priming asymmetry decreased by 16 ms, and repetition priming effects grew weaker specifically on switch trials (relative to stay trials) by 14 ms. Also, with each extra block, the inhibitory effect of naming non-target pictures increased by 0.36 ms, an increased slowdown that was 0.60 ms stronger on stay trials than on switch trials.

These observations were statistically supported by a significant main effect of block number, $B = -22$, 95% CI = $[-38, -6]$, $t(354) = -2.74$, $p = .007$, a marginally significant two-way interaction between block number and same-language target repetition, $B = 6$, 95% CI = $[0, 13]$, $t(34) = 1.89$, $p = .067$, and a significant two-way interaction between block number and non-target repetition, $B = 0.36$, 95% CI = $[0.09, 0.64]$, $t(115) = 2.66$, $p = .009$, as well as four three-way interactions. Block number marginally interacted with language and trial type, $B = 14$, 95% CI = $[0, 29]$, $t(16) = 2.10$, $p = .052$, significantly interacted with language and same-language target repetition, $B = 16$, 95% CI = $[2, 30]$, $t(74) = 2.35$, $p = .021$, significantly interacted with trial type and same-language target repetition, $B = 14$, 95% CI = $[1, 27]$, $t(29) = 2.22$, $p = .035$, and significantly interacted with trial type and non-target repetition, $B = -0.60$, 95% CI = $[-1.11, -0.08]$, $t(72) = -2.29$, $p = .025$. None of the other nine interactions involving block number were significant, all $|t|s < 1.46$, all $ps > .14$.

To sum up: When participants had previously named more single-language blocks, language asymmetries were smaller (in switch costs and repetition priming), repetition priming effects decreased (for the nondominant language and on switch trials), and the inhibitory effect of naming non-target pictures increased on stay trials. To interpret the interactions with language, it is worth remembering that of the 192 participants who named one or more single-language blocks before the cued block, 145 (75.5%) previously named (at least) the nondominant-only block. It is thus unsurprising that participants who named the cued block later in the block order showed reduced language asymmetries and decreased repetition priming specifically for the nondominant language. Of the two other interactions, both

indicate greater long-term effects on stay trials (vs. switch trials) for participants who named the cued block later.

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Highlights

- 416 Spanish-English bilinguals repeatedly switched languages while naming pictures.
- Naming a picture in one language slowed its subsequent naming in the other language.
- Naming any picture in the nondominant language slowed dominant language production.
- Both inhibitory effects accumulated over time, persisting throughout the block.
- Inhibitory control is long-lasting, and implemented both locally and globally.

Fig. 1.

Mean by-participant picture naming latencies for each block (single-language vs. cued), trial type (stay vs. switch), language (dominant vs. nondominant), and block quarter (the 1st, 2nd, 3rd or 4th quarter of each block). Error bars = 95% confidence intervals.

Fig. 2.

Picture naming latencies for each block (single-language vs. cued), trial type (stay vs. switch), and language (dominant vs. nondominant) as a function of all previous pictures named in the same block. For consistency with analyses, figures depicting the effects of same-language target repetition (left column) represent that variable in log space (hence the non-linear X-axis). For visualization, trial-level RTs are LOESS-smoothed and the figure for each trial number component holds the other component(s) constant at their weighted means. This reflects the results of the mixed-effects models, which evaluate the statistical significance of each factor while holding other factors constant at their means, and is analogous to conducting a multiple regression (e.g., $Y = B_0 + B_1X_1 + B_2X_2 + B_3X_3 + \epsilon$) and subtracting the effects of some independent variables to examine the relationship between the dependent variable and other independent variables ($Y - (B_2X_2 + B_3X_3) = B_0 + B_1X_1 + \epsilon$). Specifically, for cued-block figures (top row), estimates of fixed effects from the cued-block model (Analysis 1) were subtracted from RTs; for single-language-block figures (bottom row), estimates of fixed effects from the single-language block model (Analysis 2) were subtracted from RTs. Ribbons = 95% confidence intervals as determined by LOESS fits.