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Cognitive Advantage in Bilingualism: An Example of Publication Bias?

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Abstract

It is a widely held belief that bilinguals have an advantage over monolinguals in executive-control tasks, but is this what all studies actually demonstrate? The idea of a bilingual advantage may result from a publication bias favoring studies with positive results over studies with null or negative effects. To test this hypothesis, we looked at conference abstracts from 1999 to 2012 on the topic of bilingualism and executive control. We then determined which of the studies they reported were subsequently published. Studies with results fully supporting the bilingual-advantage theory were most likely to be published, followed by studies with mixed results. Studies challenging the bilingual advantage were published the least. This discrepancy was not due to differences in sample size, tests used, or statistical power. A test for funnel-plot asymmetry provided further evidence for the existence of a publication bias.

Keywords

bilingualism, executive functions, inhibition, publication bias

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Many studies have suggested that bilinguals have an advantage over monolinguals in executive control. Evidence for this claim has been obtained in studies with children (Bialystok & Martin, 2004), young adults (Costa, Hernández, & Sebastián-Gallés, 2008), and older adults (Gold, Kim, Johnson, Kryscio, & Smith, 2013), using tasks showing smaller interference effects in bilinguals than in monolinguals, including Simon (Bialystok, Craik, Klein, & Viswanathan, 2004), flanker (Costa, Hernández, Costa-Faidella, & Sebastián-Gallés, 2009), and task-switching (Prior & MacWhinney, 2010) paradigms.

The argument of bilingualism enhancing cognitive control is also extensively discussed in books (e.g., Bialystok & Barac, 2013), special issues of journals (e.g., Bobb, Wodniecka, & Kroll, 2013; Kroll, Christoffels, & Bajo, 2013), and conferences (e.g., the annual International Workshop on Bilingualism and Cognitive Control). On the basis of these studies, the media have often presented a picture of a strong bilingual advantage, as expressed in titles such as "Bilingual Brains Are More Healthy" (Fox, 2011) or "Why Bilinguals Are Smarter" (Bhattacharjee, 2012), which suggests that the idea has been consolidated and accepted as common wisdom. Despite this ongoing belief, not all studies have found that bilinguals have an advantage over monolinguals. Some of these studies have been published (e.g., Gathercole et al., 2014; Paap & Greenberg, 2013), but we suspected that many other studies of this nature have not.

We ourselves are guilty. We contributed to the creation of the accepted wisdom of a cognitive advantage in bilinguals by publishing a study reporting an effect of bilingualism in a spatial negative-priming task (Treccani, Argyri, Sorace, & Della Sala, 2009). This effect, supporting the theories of enhanced inhibitory control in bilinguals, was obtained in one experiment. Three other tasks (Simon, color negative priming, and spatial cuing), however, were administered at the same time and to the same participants and did not show any differences between

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bilinguals and monolinguals. The only experiment that we submitted for publication was the one showing an effect of bilingualism. Similarly, another study from our research group (using the same spatial negative-priming paradigm that was successful in Treccani et al., 2009) failed to replicate the observed effect of bilingualism. Because of the same file-drawer bias (cf. Spellman, 2012), this study was not submitted either.

We then wondered if the claim that bilinguals have a cognitive advantage is a correct reflection of all research in this field. Recently, Paap (2014) raised the concern that the literature on bilingualism and executive control might be affected by a confirmation bias to report positive results only. In fact, the file-drawer problem is not the only bias that marks scientific literature: The well-known *publication bias* is another obvious one (see Chambers, 2013; Cumming, 2014; Easterbrook, Gopalan, Berlin, & Matthews, 1991; Fanelli, 2010; Francis, 2012). To investigate whether and to what extent studies showing a bilingual advantage are more likely to be published than data challenging the bilingual-advantage hypothesis, we looked at a sample of conference abstracts on the topic of bilingualism and executive control. We classified the abstracts on the basis of their outcomes and assessed which abstracts were subsequently published in a journal.

Method

We searched for conference abstracts on bilingualism and executive control in 169 conferences (31 different national and international meetings) organized between 1999 and 2012. The topics of these conferences included bilingualism, psycholinguistics, cognitive neuroscience, psychology, and psychiatry (see Table 1 for an overview of all conferences).

We identified 128 abstracts (presented at 52 different conferences) that focused on bilingualism and executive control. We included abstracts for all research in which the relationship between bilingualism and executive control was investigated in any age group, either with nonlinguistic control tasks (116 abstracts; both standard executive-control tasks, e.g., the Simon task, or tasks with a clear executive-control component, e.g., working memory updating tasks) or with linguistic control tasks (12 abstracts, e.g., homograph-interference task). We included executive-control tasks with linguistic stimuli to get a complete overview of the publication bias in the general field of bilingualism and executive control. We did not include conference abstracts that report studies looking at effects of bilingualism in lexical tasks without a clear executive-control component (e.g., word-learning or picture-naming tasks). Twenty-four conference abstracts could not be classified because the abstract did not contain enough information about the results (15 abstracts), the study was lacking a (monolingual) control group (8 abstracts), or the abstract was a review of previous studies (1 abstract). Two authors independently classified the remaining 104 abstracts according to their reported results. Any disagreement, which occurred in 11 cases, was resolved by discussion.

Classification

We classified the abstracts into four categories (see the Supplemental Material available online for an overview of all abstracts and their classifications). The first category contained studies that yielded data that only support the bilingual advantage.

The second category consisted of studies that yielded mixed data that, on the whole, support the bilingual-advantage hypothesis. These studies did not show a bilingual advantage in all tasks or analyses, but their results are still compatible with the prevalent idea of bilinguals showing enhanced abilities in executive control (the authors report no bilingual advantage in experimental conditions in which an effect of bilingualism was not expected). This category included studies that provide neuroimaging or electrophysiological evidence consistent with the idea of more efficient executive-control functions in bilinguals than in monolinguals. It also included studies that show bilingual advantages (a) for high-executive-control conditions (e.g., in flanker tasks involving strong interference effects) but not for low-executive-control conditions (e.g., in flanker tasks involving weaker interference effects; five studies); (b) for executive-control tasks in which a bilingual advantage was expected (e.g., domain-general control tasks, such as Simon tasks) but not for other tasks in which no bilingual advantage was expected (tasks in which performance depends on expertise in a particular field, such as music, or tasks tapping executive functions that are not directly related to the ability of controlling two or more antagonist cognitive networks, such as the two languages in a bilingual, e.g., the impulse-delay task; six studies); (c) for high-proficiency bilinguals but not low-proficiency bilinguals (1 study), or for switching balanced bilinguals but not for nonswitching balanced bilinguals (1 study); and (d) for unimodal but not bimodal bilinguals (i.e., people proficient in one spoken language and one sign language; 1 study).

The third category consisted of studies in which mixed data partly challenge the bilingual advantage. The authors of these studies report some results that support the bilingual-advantage idea, but they also report experiments in which a bilingual advantage was expected but not found or data indicating that the bilingual advantage in some tasks could have other explanations than the

Table 1. Conferences From Which the Analyzed Abstracts Were Taken

Conference	Year
American Aging Society Conference	2005–2011
Architectures and Mechanisms for Language Processing Conference	2002–2004, 2007–2011
Association for Psychological Science Annual Convention	2003–2012
Boston University Conference on Language Development	2008–2012
Canadian Society for Brain, Behaviour, and Cognitive Science Annual Meeting	2004–2012
Cognitive Science Society Annual Conference	2003–2012
Cognitive Neuroscience Society Annual Meeting	2003–2012
CUNY Conference on Human Sentence Processing	2006–2012
European Brain and Behaviour Society Meeting	2003–2009
European Congress of Psychology	2009
European Federation of Neurological Societies Meeting	2005–2011
European Society for Cognitive and Affective Neuroscience Conference	2012
European Society for Cognitive Psychology Conference	2007, 2009, 2011
FENS Forum of Neuroscience	2002, 2004, 2006, 2008, 2010, 2012
International Association for the Study of Child Language Conference	2005, 2008, 2011
International Conference on Cognitive Neuroscience	2011
International Conference on Models of Interaction in Bilinguals	2009
International Neuropsychological Society Annual Meeting	2003–2010
International Symposium on Bilingualism	2003, 2007, 2009, 2011
International Symposium of Psycholinguistics	2011
Midwestern Psychological Association Annual Meeting	2004, 2005, 2006, 2010, 2011, 2012
Neurobilingualism Conference	2009
Society for the Neurobiology of Language Conference	2009–2012
Nordic Conference on Bilingualism	2009, 2012
Psychonomic Society Annual Meeting	1999–2012
Society for Neuroscience Annual Meeting	2000–2012
Society for Psychophysiological Research Annual Meeting	2004–2012
Society for Research in Child Development Biennial Meeting	2005, 2007, 2009, 2011
Workshop on Bilingualism	2005–2008, 2011
Workshop on Neurobilingualism	2010
Bilingual & Multilingual Interaction Conference	2012

mere knowledge of two languages and the ability to master them. This category included studies that show (a) a bilingual advantage in some executive-control tasks but not in other parts of the tasks in which an effect of language group was expected (20 studies), (b) a bilingual advantage for certain language groups but not others (5 studies), (c) some inconsistent effects of language group in neuroimaging or electrophysiological data but no bilingual advantage in behavioral data (reaction times, or RTs; 6 studies), (d) a bilingual advantage for some age groups but not others (1 study), and (e) a bilingual advantage that could be explained by other factors, such as the socioeconomic status of participants (1 study).

The fourth category was for studies that yielded results that fully challenge the bilingual advantage. These studies demonstrated a bilingual disadvantage or did not show any difference between monolinguals and bilinguals.

We based our classification on the results and conclusions reported in the conference abstracts. In some cases, the study described in the abstract ended up being published in a scientific journal, and the conclusions drawn by the authors in the abstract did not match those in the published article. For example, the abstract by Luk, Anderson, Bialystok, Craik, and Grady (2009) does not discuss the absence of a bilingualism effect on RTs but focuses only on the bilingual "advantage" observed in functional MRI data. On the basis of this abstract, we classified their study as supporting the bilingual advantage. These authors also describe this study in a published article (Luk, Anderson, Craik, Grady, & Bialystok, 2010), in which they mention the absence of an RT effect. Based on this article, a classification in our third category (mixed results partly challenging the bilingual advantage) would have been more appropriate. To avoid differences between published and unpublished studies, however, we based our categorization on conference abstracts only.

After classifying the abstract, we identified whether the results presented in it had been published in a

Table 2. Overview of the Abstracts Analyzed

Result type	Number of abstracts	Number published	Percentage published
All abstracts supporting the bilingual advantage	54	34	63
Bilingual advantage	40	27	68
Mixed data supporting the bilingual advantage	14	7	50
All abstracts challenging the bilingual advantage	50	18	36
Mixed data challenging the bilingual advantage	33	13	39
No bilingual advantage	17	5	29
Bilingual disadvantage	4	2	50
No differences between monolinguals and bilinguals	13	3	23

journal. We classified the results as being published if they had been accepted for publication by an international scientific journal on or before February 20, 2014 (regardless of whether they had already appeared in a journal issue). We did not include book chapters or published conference proceedings. We also classified an abstract as published if the results were part of an article with additional experiments or participants. If two conference abstracts from the same research group reported different studies (e.g., Paap, Greenberg, Guerrero, & Mejia, 2010; Paap, Greenberg, & Liu, 2012) but were later combined to form one journal publication, we classified both abstracts as being published. However, when two abstracts presented at different conferences reported exactly the same study findings, only the first conference presentation was included.

We also identified three factors that could potentially confound the results: year of conference presentation, number of participants per language group, and number of executive-control tasks administered in the study. We included the number of participants per group rather than the total number of participants, as some studies included many different groups (e.g., different language combinations) or multiple tasks, which thus led to very high numbers of participants.

Meta-analysis

We performed a meta-analysis of the published studies that provided suitable data and assessed the presence of a publication bias by means of a funnel plot. Of the 50 published articles from our conference abstracts assessment, 41 were included in our meta-analysis. We contacted the authors if the article did not contain the required descriptive statistics. Nine studies could not be included in the analysis because we could not obtain the descriptive statistics, the article focused on neuroimaging data only, or the authors did not allow inclusion of their study's results in the analysis. We included all behavioral executive-control tasks described in the articles, but we did not include neuroimaging data, and we analyzed bilingual-monolingual differences only on the critical dependent variables (e.g., if the article focused on RTs, we only included RTs and not accuracy results). For tasks that reported overall RTs as well as conflict effects (e.g., Simon or flanker tasks), we included only the conflict effects. If the study compared multiple bilingual or monolingual groups, we analyzed those groups separately. In total, our analysis contained 176 comparisons. We used MetaXL software (Version 2.0; Barendregt, 2014) and the metafor software package in R (Viechtbauer, 2010) for our statistical analysis.

Results

Conference abstracts

Of the 104 abstracts included in our analysis, 40 abstracts (38%) reported studies that found a bilingual advantage or results supporting the bilingual-advantage theories. Fourteen studies (13%) found mixed results supporting the bilingual-advantage theories. Thirty-three studies (32%) showed mixed results partly challenging the bilingual-advantage theories. Seventeen studies (16%) found no differences between monolinguals and bilinguals (13 studies) or a monolingual advantage (4 studies). In total, 52 studies were published in 50 articles (50% of all conference abstracts). Sixty-eight percent of the studies that clearly found a bilingual advantage were published, compared with 50% of the studies that found mixed results supporting the bilingual-advantage theories, 39% of the studies that found mixed results partly challenging those theories, and 29% of the studies that found no differences between monolinguals and bilinguals or found a bilingual disadvantage. On the whole, 63% of the studies supporting the bilingual advantage were published, compared with only 36% of the studies that challenged it (see Table 2 and Fig. 1).

Using a binary logistic regression analysis, we found a significant difference between the publication outcomes (published or unpublished) of abstracts that challenge and support bilingual-advantage theories, Wald $\chi^2(1, N = 104) =$

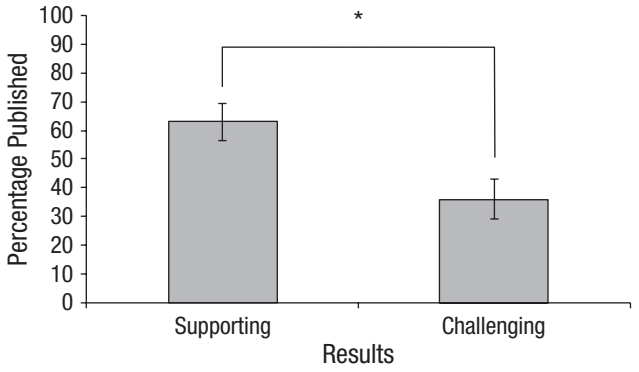

Fig. 1. Percentage of results from conference abstracts supporting or challenging the bilingual advantage that were subsequently published in an international scientific journal. Error bars show ±1 *SEM*. The asterisk indicates a significant difference between groups ($p < .01$).

7.36, $p = .007$, $\eta_p^2 = .073$. When we included all four result types, the analysis still showed a significant effect of result type on publication, Wald $\chi^2(3, N = 104) = 8.86$, $p = .031$, $\eta_p^2 = .089$.

Using independent-samples *t* tests, we found no significant differences between abstracts that supported the bilingual advantage and those that challenged it in terms of year of conference presentation and number of participants per group (see Table 3). Abstracts that challenged bilingual-advantage theories, however, reported more executive-control tasks than did abstracts that supported those theories. Not all abstracts included information on the number of executive-control tasks and the number of participants per group. Among the abstracts supporting the bilingual advantage, 9 did not report information on the number of participants, and 3 lacked detail on the number of tasks. Among the abstracts challenging the bilingual advantage, 15 did not include information on the number of participants, and 3 did not include the number of tasks. Our analyses, therefore, included the majority of studies, but not all studies, so the results should be interpreted with caution.

Meta-analysis

Our meta-analysis of the published studies showed an effect of bilingualism, with an average standardized mean difference of 0.30 (95% confidence interval = [0.23, 0.37], $z = 8.21$, $p < .0001$; see Figure S1 in the Supplemental Material for the forest plot). To examine the potential presence of a publication bias, we created a funnel plot (i.e., a scatter plot in which a measure of study precision—standard errors—is plotted against the bilingualism effects estimated from individual studies—standardized bilingual-monolingual mean differences). The funnel plot shows a clear asymmetry (see Fig. 2). Studies with larger standard errors show a larger standardized mean difference than studies with smaller standard errors. In the absence of a publication bias, the funnel plot should have been symmetrical, with studies with larger standard errors resulting in a similar amount of relatively high and low standardized mean differences. Studies with larger standard errors should then scatter widely at the bottom of the graph (cf. Sterne, Becker, & Egger, 2005). Instead, we observe that less-precise studies (with larger standard errors) more often show large effects than small effects, which suggests that studies with small effect sizes might not have been published. The observed asymmetry in the funnel plot was further supported by Egger's linear regression test, which showed a significant asymmetry ($z = 4.80$, $p < .0001$).

We also calculated the power of the null-effect studies to detect the various effect sizes reported in published articles reporting positive results. We calculated the power for studies concerning the Simon effect, flanker effect, and task-switching costs. On the whole, null-effect studies had a medium-to-high probability of detecting the positive effects reported in published studies using the same tasks. For example, in the Simon task used by Bialystok et al. (2004), the bilingualism effect size (Cohen's *d*) ranged from 1.08 to 2.99. Using G*Power 3.1.8 (Faul, Erdfelder, Lang, & Buchner, 2007) with an alpha level of .05 (two tailed), we calculated the probability to detect this effect, that is, the statistical power ($1 - \beta$) of the null-effect studies that used a Simon task and that provided sufficient information (i.e., the number of participants per group; 12 studies; see the Supplemental Material for full references).

All the studies using Simon tasks had a very high probability of detecting such a large effect (average of .87 to detect *d* of 1.08, and .99 to detect *d* of 2.99). The effect

Table 3. Comparison of Abstracts Supporting and Challenging the Bilingual Advantage

Variable	Abstracts supporting the bilingual advantage	Abstracts challenging the bilingual advantage	Comparison
Year of conference presentation	2008.9 (1.97)	2009.2 (2.76)	$t(102) = 0.50$, $p = .620$
Number of participants per group	31.1 (23.76)	28.3 (16.21)	$t(78) = 0.60$, $p = .554$
Number of tasks	1.6 (1.29)	2.2 (1.25)	$t(96) = 2.35$, $p = .021$

Note: For each of the two categories of abstracts, the table presents means, with standard deviations in parentheses.

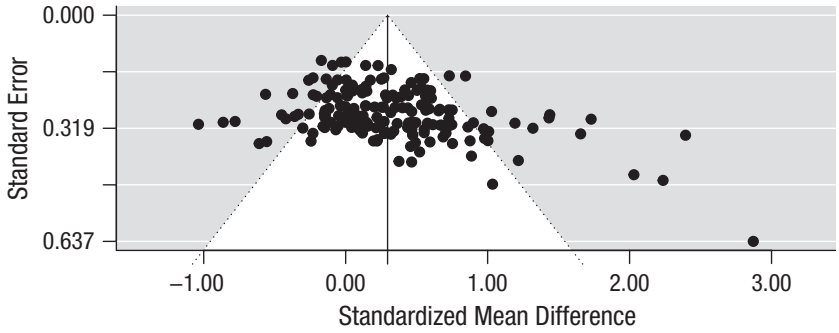

Fig. 2. Funnel plot of the meta-analysis of published studies. Each plotted point represents the standard error and standardized mean difference between bilinguals and monolinguals for a single study. The white triangle represents the region where 95% of the data points would lie in the absence of a publication bias. The vertical line represents the average standardized mean difference of 0.30 found in the meta-analysis.

sizes for Simon effects reported in two other published studies with positive results (i.e., Bialystok, Craik, & Luk, 2008; Salvatierra & Rosselli, 2011) were smaller (.59 and .69, respectively), and null-effect studies had a medium probability of detecting them (average of .52 and .66, respectively). The same procedure was used for flanker and task-switching studies. In the flanker task used by Costa et al. (2009), the effect size of bilingualism was .61. Null-effect flanker-task studies (8 studies) on average had a medium probability (.62) of detecting this effect. In a task-switching paradigm, Gold et al. (2013) found a bilingualism effect size of .68, and null-effect task-switching studies (3 studies) on average had a high probability of detecting it (.94).

Finally, we calculated the power of studies that both supported and challenged bilingualism-advantage theories to detect the effect size found in our meta-analysis (0.30). Eighty studies (45 supporting and 35 challenging; see the Supplemental Material for references) provided sufficient information to be included in the analysis. For studies classified as supporting the bilingual advantage, the power to detect an effect size of .3 was .19. For studies classified as challenging the bilingual advantage, this power was .17. Both types of studies thus had a comparable but low probability to detect the effect size observed in the meta-analysis.

Discussion

We analyzed conference abstracts presented between 1999 and 2012 on the topic of bilingualism and executive control. Conference abstracts were classified on the basis of their outcome. We observed an effect of result type on publication: Studies were published relatively often (68%) if the data demonstrated a bilingual advantage. In contrast, only 29% of the studies that showed no effect of bilingualism or even a bilingual disadvantage were published. Publication chances of studies with mixed results were in between these two groups, with studies partly supporting the bilingual advantage being more likely to be published than studies partly challenging the bilingual advantage. The asymmetrical funnel plot of published studies also hinted at a publication bias.

This difference in publication percentage based on the outcomes of the study could be the result of a bias during several steps of the publication process: Authors, reviewers, and editors can decide to submit or accept only studies that showed positive results. In the first step of the publication process, the file-drawer problem could play an important role in the observed publication bias. Authors could decide not to publish studies with null or mixed results, or they could choose to submit their results only partially, for example, by leaving out tasks that did not show an effect of bilingualism. The article by Treccani et al. (2009) is an example of the file-drawer problem, as it excluded the experiments that did not show an effect of bilingualism.

On the next level, reviewers and editors might reject manuscripts reporting null, negative, or mixed results more often than manuscripts reporting positive effects. This rejection is often based on the argument that null effects are difficult to interpret, the result of poor stimulus design, or the result of a Type II error (Ferguson & Heene, 2012). Mahoney (1977) asked journal reviewers to referee manuscripts reporting positive, negative, mixed, or null results with identical methodological procedures. Although the methodology was the same, reviewers scored the manuscripts reporting positive results as methodologically better than the manuscripts reporting negative or mixed results. For manuscripts with positive results, reviewers usually recommended acceptance with moderate revisions. For manuscripts with negative results, however, their usual recommendation was major revision or rejection. Manuscripts with mixed results were mostly rejected.

Unfortunately, we cannot determine whether studies were not submitted to a journal or rather rejected after submission. We did ask all authors to take part in a short survey concerning journal submission, but only a small proportion responded. This lack of responsiveness, particularly from very productive research groups, complicates the interpretation of our findings.

In our overview of conference abstracts, almost half of the abstracts (48%) partially challenged the existence of a bilingual advantage by reporting tasks with no effect of bilingualism. We should be careful interpreting null effects. It is worth noting, however, that most studies analyzed here (showing positive, mixed, or null effects) used the same tasks (e.g., Simon, task-switching, or flanker tasks). Our analyses furthermore showed that studies supporting the bilingual-advantage hypothesis and studies challenging this hypothesis did not differ significantly in terms of number of participants per group. The two types of studies also had a similar power to detect the bilingualism effect size found in the meta-analysis. Notably, we did observe a difference between the different abstract types in the number of reported tasks. Abstracts supporting the bilingual advantage reported fewer tasks than abstracts with mixed results or abstracts challenging the bilingual advantage. It is difficult to interpret this difference, as it might reflect a difference in the number of tasks that were reported rather than a difference in the number of tasks that were actually used. Although this is speculative, the difference in the number of tasks reported in studies that support and studies that challenge the bilingual advantage could be the result of reports from some of the studies that "supported" the hypothesis leaving out data that actually did not support it (cf. John, Loewenstein, & Prelec, 2012). Alternatively, a significant effect could be most likely to occur if only one test is used, whereas more tests might also yield nonsignificant or negative results. Researchers could submit a manuscript after one successful task without trying to replicate this effect, even if the positive outcome could be the result of a Type I error (cf. Pashler & Harris, 2012).

Only a few of the analyzed studies (4 of the 104 abstracts) found a bilingual *disadvantage*. A lack of such studies could result from the file-drawer bias having occurred at the level of conference submission. Indeed, the finding of a bilingual disadvantage can hardly be interpreted as indicating better executive-control abilities in monolinguals than in bilinguals. The only reasonable conclusion would be that in the tested domain, there is no bilingual advantage, and a Type I error occurred. Authors in such cases might not submit their negative results to a conference. In this respect, it is worth noticing that the file-drawer problem occurring at the conference-submission level might have obscured the existence of differences in publication rates even larger than those we found: Our results might only be the tip of the iceberg.

The small percentage of studies showing a bilingual disadvantage, however, could also suggest that the cognitive bilingual advantage is genuine, albeit smaller and less stable than often presented in the literature. In fact, the existence of a publication bias does not necessarily imply that bilingualism does not have any effect on executive functions. The presence of a publication bias may explain why the magnitudes of many reported positive effects appear to decrease over time (i.e., their size declines as studies exploring them are repeated), even when the effects have been shown to be reliable and are still widely accepted (cf. Schooler, 2011; see also Lehrer, 2010). The presence of a publication bias can help to interpret the exaggerated outcomes often reported in the initial phase of research.

Our overview shows that there is a distorted image of the actual study outcomes on bilingualism, with researchers (and media) believing that the positive effect of bilingualism on nonlinguistic cognitive processes is strong and unchallenged. Recently, however, several researchers (e.g., Paap, 2014; Paap & Liu, 2014) have criticized the findings in the existing literature. Their criticisms focus especially on the impossibility of randomly assigning the independent variable (i.e., language group) and on the differences between bilingual and monolingual groups on background variables such as socioeconomic or immigration status. In light of these issues, it is especially important to avoid publishing positive studies only.

A potential publication bias also poses a problem for meta-analyses. On the basis of an estimation of the number of possible unpublished null-effect studies, Adesope, Lavin, Thompson, and Ungerleider (2010) concluded that it was unlikely that their meta-analysis on cognitive effects of bilingualism could be threatened by a publication bias. Conversely, our overview shows the number of actually conducted unpublished null-effect studies and suggests that the results of a meta-analysis can in fact be affected by such a bias. Hilchey and Klein (2011) reviewed published studies that specifically address the issue of bilingualism and executive control. Although this review rightfully criticized some of the current theories, it was still necessarily based on published work only. Similarly, our meta-analysis did show an effect of bilingualism, but for the aim of the funnel plot, we included only published studies. The bilingual advantage found in this meta-analysis would be smaller if the unpublished abstracts (with more null and negative effects) were included too.

For the literature to facilitate a better understanding of the actual effect of bilingualism and the boundaries of this effect, publication chances should not depend on the direction of the study results. Studies with mixed results, for example, are especially valuable because they can

identify the circumstances under which a bilingualism effect may and may not occur, but, as shown by our analysis, they are published less often than studies that report data in favor of the bilingual advantage. Furthermore, studies showing no effects of bilingualism are often unfairly criticized. Recently, Kroll and Bialystok (2013) claimed that "the considerable literature that reports group differences between monolingual and bilingual participants is greatly more informative than the attempted replications that fail to find significance" (p. 502) and "unless all conditions have been accounted for and all other explanations have been exhausted, it is misleading to call into question the reliability of the phenomena themselves" (p. 503).

While we agree that bilingualism should be conceived, a priori, as a positive and desirable achievement, we are also convinced that educational and political debates addressing the relevance of bilingualism should not be promoted by ignoring null or negative results. Instead of selecting exclusively those tasks and results that support current theories, investigators should attempt to include all conducted tasks and reported findings. On the other hand, reviewers and editors should be more open to studies that challenge the existing theories, especially when these are not yet fully established. All data, not just selected data that supports a particular theory, should be shared, and this is especially true when it comes to data regarding issues that have enormous societal relevance and implications, such as bilingualism.

Author Contributions

All authors contributed to the study concept and design. Data were collected and analyzed by A. de Bruin. Abstracts were classified by A. de Bruin and B. Treccani. A. de Bruin drafted the manuscript, and B. Treccani and S. Della Sala provided critical revisions. All authors approved the final version of the manuscript for submission.

Declaration of Conflicting Interests

The authors declared that they had no conflicts of interest with respect to their authorship or the publication of this article.

Supplemental Material

Additional supporting information can be found at http://pss.sagepub.com/content/by/supplemental-data

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