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Publication Bias and the Validity of Evidence: What's the Connection?

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Ellen Bialystok¹, Judith F. Kroll², David W. Green³, Brian MacWhinney⁴, and Fergus I. M. Craik⁵ **[AQ: 1][AQ: 2][AQ: 3]**

¹Department of Psychology, York University; ²Department of Psychology, Pennsylvania State University; ³Experimental Psychology, Faculty of Brain Sciences, University College, London; ⁴Department of Psychology, Carnegie Mellon University; and ⁵Rotman Research Institute at Baycrest, Toronto, Ontario, Canada

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In a recent article, de Bruin, Treccani, and Della Sala (2015) questioned the reality of the bilingual advantage in executive control found in many studies, arguing that the reported effect reflects a publication bias favoring positive results over null and negative results. The implication of their argument is that this publication bias invalidates the credibility of the positive published evidence. These are serious charges and need to be carefully scrutinized.

Their primary evidence comes from an analysis of 104 conference abstracts presented between 1999 and 2012, for which they found that 63% of the presentations reporting a bilingual advantage were subsequently published, whereas only 36% of the abstracts challenging the concept were published. Their conclusion, however, is undermined by three errors in reasoning that concern (a) the relation between conference abstracts and published articles, (b) the difference between null and negative findings, and (c) the differential effects of bilingualism on verbal and nonverbal task performance.

The argument that publication decisions have been skewed to prevent the appearance of challenges to the positive effects of bilingualism can be demonstrated in only one way: by comparing the acceptance rates of journal submissions that report a positive effect with those that do not. The authors did not have access to rejected journal submissions, so they used conference abstracts as a proxy, but there is no way of knowing how many abstracts that reported positive results and how many that reported negative results were submitted for publication. Given a case in which 100 well-designed studies with positive results and 100 well-designed studies with null or negative results were submitted to journals, and then 70 of the positive studies but only 30 of the null or negative studies were accepted for publication, one could conclude that there was bias, but this kind of evidence was not provided. Moreover, conference presentations

are not representative of studies submitted to journals: They are often preliminary reports, based on small samples, testing new methods, and crucially are rarely peer-reviewed. Techniques that fail to work in preliminary studies are often subsequently refined. Without knowing how many of the conference abstracts in this analysis were actually submitted for publication, or without the inclusion of a control condition in which similar numbers of abstracts are evaluated for any other topic in psychology, no conclusions can be made.

The second error is that de Bruin and her colleagues combined abstracts showing null or negative effects into a single category. These two types of reports must surely receive very different treatments both as conference abstracts and journal submissions. If the main point of the article is to demonstrate that journals prefer to publish articles that show significant effects over those that show no effects, then their point is correct, and no further analyses are needed. There is an understandable preference for journals to publish studies that show some effect over those that show no effect—imagine the state of journals if these studies were published with the same frequency—and this preference may be particularly strong in the case of striking new findings. It seems, therefore, that the real purpose of the de Bruin et al. article is to use publication bias as a means of discrediting evidence for bilingual effects on cognition. The crucial evidence, therefore, is the publication fate of studies showing negative results; only these can provide evidence of bias for a particular outcome. If conference presentations are a better reflection of the true state of affairs, and the bilingual advantage is false, one should expect to

Corresponding Author:

Fergus I. M. Craik, Rotman Research Institute at Baycrest, 3560 Bathurst St., Toronto, Ontario M6A 2E1, Canada
E-mail: fcraik@research.baycrest.org

see as many cases of bilingual disadvantage as bilingual advantage. However, de Bruin and her colleagues found that only 4 of the 104 abstracts reported such a bilingual disadvantage.

The third error is the conflation of studies using verbal tasks with those using nonverbal tasks. Many of the studies showing a bilingual disadvantage in the forest plot shown in de Bruin and her colleagues' Figure S1 (see their Supplemental Material available online) used verbal tasks for which there is a well-documented advantage for monolinguals (Bialystok, 2009). Bilinguals are generally slower than monolinguals to retrieve words, even in their native language (Gollan, Montoya, Cera, & Sandoval, 2008). The source of this disadvantage has been examined in the literature on bilingual language processing, with some researchers arguing that the same mechanisms that produce costs in lexical retrieval may contribute to the observed benefits in executive function (Kroll & Gollan, 2014). Thus, the finding of fewer cases of published negative results is not bias but rather a reflection of the use of verbal tasks. What is lacking is evidence of bias against studies showing a bilingual disadvantage on the cognitive tasks that are typically associated with the reported advantages of bilingualism. Without a more nuanced understanding of the underlying mechanisms, the overall count of aggregated advantages and disadvantages is meaningless.

The authors also provided a funnel plot from a meta-analysis of studies in 41 of the published articles based on these abstracts. This plot shows an asymmetry in which studies with larger standard errors are more likely to find evidence in favor of a bilingual advantage. In the absence of a true bilingual advantage, the average bilingual-monolingual difference should be close to 0, yet the average standardized mean difference between the language groups was 0.30 in favor of bilingual advantages, $z = 8.21$, $p < .0001$. If all other things were equal, then a difference in variability across studies might be a factor of interest. Given the errors we have noted, the value of the meta-analysis is questionable, because the data analyzed did not provide adequate sensitivity to allow one to draw even tentative conclusions.

The cognitive consequences of bilingualism have been reported in a recent upsurge of research investigating infants, children, young adults, older adults, and patients, using diverse methods including behavioral performance, electroencephalography, neuroimaging, and eye tracking. Positive effects have been found in labs in North America, Europe, and Asia (for reviews, see Baum & Titone, 2014; Bialystok, Craik, Green, & Gollan, 2009; Hervais-Adelman, Moser-Mercer, & Golestani, 2011). Failures to replicate the effects have also been published (e.g., Paap & Greenberg, 2013), but such findings need to be used in conjunction with the positive findings to

understand the reasons for these differences rather than to deny the existence of the effect. The crucial question is not the presence or absence of publication bias but whether or not convincing evidence exists to support claims that bilingualism has systematic and enduring consequences for cognitive and brain function.

The research on bilingualism is complex: Who is bilingual? What tasks are decisive? How can individual differences be incorporated into the models? These are difficult questions, but they are essential to resolve in order to find some consistency in the empirical record. Part of the problem is in the scientific method, which requires researchers to infer population characteristics from samples. Not every study of climate change that samples weather conditions from a specific time and place finds that temperatures are rising, but researchers (mostly) accept that climate change is occurring because of global warming. Not every study of 60 individuals in which half are bilingual in some way results in a statistically significant advantage for bilinguals on a specific set of tasks, but a large and growing body of literature not only indicates that these changes are real but also increasingly points to the brain mechanisms by which these changes occur (Green & Abutalebi, 2013). Journals prefer to publish significant results over null results, but there is no evidence that there is a preference for these results to be either positive or negative. For these reasons, we reject the authors' conclusion that journals' preference for positive results over null or negative results has produced an incorrect belief regarding the cognitive consequences of bilingualism.

Author Contributions

All authors contributed equally to the preparation of this article.

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.

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