



## Discussion forum

# Still waiting for real answers<sup>☆</sup>



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On one level, I cannot help but be impressed with the body of evidence suggesting multilingualism bestows an advantage in cognitive control. Owing in large measure to the distinguished scholarship of people such as Elizabeth Peal, Wallace Lambert, David Green, Ellen Bialystok, Jacques Mehler, and others, the so-called bilingual advantage has been detected all the way from the pre-lexical days of infancy to the final sobering moments of neurological decline. The advantage is evident both in language tasks that demand control, such as detecting grammatical errors in semantically correct utterances, and non-linguistic tasks that demand control, such as eye movement control tasks, and stimulus-response compatibility tasks. And now, thanks to sophisticated new neuroimaging techniques, we can see glimpses of the indelible imprint a lifetime of managing two languages leaves on the neurobiological circuitry of the brain. This is an impressive achievement born of the efforts of many fine scholars over the last 50 years.

At the same time, I cannot help being both deeply skeptical of the bilingual advantage hypothesis, and deeply critical of the way this research program has proceeded. The core hypothesis, at least as articulated by Green and Bialystok, is that through a lifetime suppressing translation equivalents in support of target language production and comprehension, bilinguals become highly practiced exercising control over their thoughts and actions. This in turn has implications for cognitive functioning outside of the domain of language. While available evidence, at least viewed broadly, is consistent with this hypothesis, it is also consistent with a whole variety of alternative explanations, owing in large measure to the fact that almost all available evidence comes from between-subjects comparisons. While dissention itself is not a problem – this is, after all, science; we don't all have to agree – it becomes a problem when dissenting opinions are simply

dismissed. When they are, ideas stagnate, and people lose interest and trust in the larger debate. Regrettably, this is exactly what is happening in the bilingualism field. Key stakeholders have flatly refused to acknowledge the shortcomings of their own findings and hypotheses, and have failed to provide challengers with reasoned proof alternative explanations are wrong. The result is a litany of festering doubts, many of which are carefully detailed by Paap, Johnson, and Sawi in their fine paper (Paap, Johnson, & Sawi, 2015). Vicious attacks and strident denials have not won the day for the bilingual advantage hypothesis – they have forced a crisis of confidence within our research community.

I remain highly skeptical of the bilingual advantage hypothesis because so often what is presented as supporting evidence is so obviously open to alternative explanation. Consider two highly influential sets of findings. In one, 50 native bilingual, late bilingual, and monolingual kindergartners were administered a battery of 9 cognitive control tasks (Carlson & Meltzoff, 2008) and an English proficiency test. The three groups had statistically indistinguishable scores all on 9 cognitive control measures, and the bilingual children had lower English proficiency scores. The latter result is hardly surprising: after all, these children spent half their waking hours speaking Spanish. However, the authors then went on to control for these L1 differences. When they did, the bilinguals showed higher scores in 3 of the 9 cognitive control tasks as well as a composite measure of cognitive control. These results were interpreted as consistent with the bilingual advantage hypothesis – evidence that bilingual children can do more intellectual work with fewer available resources. That might well be true, but the more obvious interpretation is that there was no evidence of a bilingual advantage at all: it was simply introduced by statistical means. The second influential set of findings comes from a study by Bialystok (Bialystok, 1999) in which 60 Chinese-English bilinguals and

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English monolinguals (all from Toronto, Canada) were administered a pair of cognitive control tasks and an English proficiency test. Bilinguals showed higher scores than monolinguals in both cognitive control tasks. As was the case with the Carlson and Meltzoff study, these results were interpreted as consistent with the bilingual advantage hypothesis. While this may well be true, it is equally plausible these findings were driven by uncontrolled confounding factors endemic to between-subjects designs, and there are a number of clues pointing to this possibility. First, despite the fact the monolinguals in this study had about twice as much experience speaking English as the bilinguals, they were no more proficient in English than the bilinguals. Second, monolinguals not only struggled in the challenging component of the cognitive control tasks – most notably the post-switch trials of the Dimensional Change Card Sort (DCCS) task – they also struggled to remember what they were asked to do in the DCCS task (see “Knowledge Measure” in Bialystok, 1999, Table 2). Beyond this, Asian cultures place a premium on self-regulation, and in many Canadian cities, enjoy socio-economic advantage. Thus, there are reasonable grounds to doubt that the superior performance of the bilinguals was attributable to the effects of language status alone. It is also worth pointing out that both studies claimed to be studying the effects of bilingual language status, but neither measured proficiency of bilinguals in their two languages. Thus, we have two studies of the bilingual advantage that are massively influential, as reflected by citation counts (Carlson & Meltzoff, 2008, over 425 citations; Bialystok, 1999, over 550), but that did not objectively measure bilingual language status, and were rife with alternative explanations. How can this be?

Part of the reason is that proponents of the bilingual advantage hypothesis aggressively attack and then ignore dissenting views. As a provocative corrective on excessive claims, I published a small study comparing bilingual and monolingual children's performance on the Simon task (Morton & Harper, 2007). This was hardly groundbreaking research. We simply measured potential confounds (socio-economic status, cultural background), and cognitive control using received methods [see (Bialystok, Craik, Klein, & Viswanathan, 2004), Experiment 1]. Curiously, we found no effect of language status on measures of cognitive control. On the basis of these findings, we suggested that future studies should carefully control potential cultural and socio-economic confounds, and measure language status so that any observed effects can be more confidently attributed to the effects of language status. Close to 10-years on, and most bilingual advantage research still lacks basic measures of language proficiency and SES, and compares groups of

monolinguals and bilinguals that differ in ways beyond language status. In a healthy scientific milieu, dissenting viewpoints help to refine prevailing ideas, and strengthen the empirical base. In the case of the bilingual advantage hypothesis, dissenting opinions do nothing – they are just ignored.

And so we find ourselves at a crisis point, as Paap, Johnson, and Sawi's paper brings so sharply into focus. Critics call attention to weaknesses in the design and interpretation of empirical studies, and proponents march on ignoring all appeals for higher standards. The sad reality is that it would be relatively easy to resolve some of the uncertainty. Look at CERN for goodness sake. If a team of 3000 theoretical and empirical physicists from over 600 research facilities and 100 nationalities can work together to figure out the structure of the subatomic world, surely a dozen cognitive scientists could put their heads together to figure out the bilingual advantage in cognitive control. Form an international consortium of researchers with widely divergent perspectives, agree to a common battery of measures, implement them in the form of an internet-based measurement system, collect data widely, and make the data available to everyone in the group.

I remain open to being convinced. But give me something I can believe in.

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