



## AUTHORS' RESPONSE

### Bilinguals show an advantage in cognitive control – the question is why

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This is a response to the commentary on Morton and Harper (2007) by Bialystok (2009).

Why do bilinguals show an advantage in cognitive control relative to monolinguals? According to Bialystok, it is because they are highly practiced at exercising control, owing to the fact that they constantly select and inhibit words from their target and non-target languages during everyday language use. However, bilinguals not only manage two lexicons, but also two phonologies, two systems of grammar, and often two different cultures. And they can differ from monolinguals in other ways as well. For example, Bialystok (1999) reported that 3- to 5-year-old Chinese-Canadian bilinguals outperformed age-matched English-Canadian monolinguals on a rule-use task. Although Bialystok attributed the difference to the effect of language status, these differences could have just as easily been attributed to the effect of culture (see Sabbagh, Xu, Carlson, Moses & Lee, 2006). In other studies, Bialystok has compared bilinguals of many cultural backgrounds and English monolinguals. These designs partially mitigate the confounding influence of culture, but still confound language status with immigration status and potentially SES, and make it virtually impossible to assess whether bilinguals are actually proficient in both of their languages. All of these differences form the basis of alternative explanations. We raise this issue not because we believe that Bialystok's hypothesis is wrong but to point out that until these alternative explanations are ruled out, her hypothesis remains unproven.

The goal of our study (Morton & Harper, 2007) was to see whether we could rule out alternative explanations based on SES and ethnicity (defined as the cultural beliefs and practices of the people with whom an individual most closely identifies). For brevity, we focus here on the issue of SES. SES comprises an individual's financial means, level of education, and occupational status, and is associated with social conditions and childrearing practices that promote the development of executive control (Mezzacappa, 2004; Noble, Norman & Farah, 2005). Bialystok dismisses our study because she claims

she has always rigorously controlled for differences in SES. We disagree. As she admits, she does not measure SES. This would not be a problem if she randomly assigned her participants to groups – random assignment controls for the confounding influence of unmeasured variables – but of course random assignment is not an option in studies that compare bilinguals and monolinguals. Therefore, she cannot claim that she rigorously controls for SES. But, Bialystok contends, we have no reason to assume that monolingual and bilingual children in her studies differ in SES because these children all come from the same neighbourhoods. Again, we disagree. Bialystok's bilingual samples often include a large number of Canadian immigrants, who as a group, earn less money but are more educated than the Canadian non-immigrants that make up her monolingual samples.

Thus in our study, we administered the Simon task to a group of monolingual and a group of bilingual children and ensured through measurement that the groups were indistinguishable in terms of non-verbal IQ, cultural/ethnic background, and SES. We also confirmed, through measurement, that bilingual children were proficient in and reported daily use of both languages. We found no evidence of a bilingual advantage on the Simon task, but a small association between SES and accuracy.

Bialystok challenges our findings on several fronts. First, she points out that we did not report all of the details of our task and its administration. This was an unfortunate oversight on our part. For the record, we modelled our task on Bialystok's procedures (Bialystok, Craik, Klein & Viswanathan, 2004, Experiment 1). Thus, on each of 28 trials (14 incongruent), a 1000 ms blank screen was followed by an imperative stimulus that was cancelled by a response. Trials in her task were the same, except that an 800 ms fixation cross and then a 250 ms blank screen were presented prior to the imperative stimulus. Second, she argues that we failed to find a bilingual advantage because any group differences that

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our task may have detected in younger children would likely disappear by the age of 7 years. This seems pretty unlikely given that Bialystok's task, which is virtually identical to ours, produces robust group effects in healthy adults (Bialystok *et al.*, 2004, Experiment 1). Third, she dismisses our data on statistical grounds, arguing that we failed to detect group differences because we had only 34 participants and RTs and *SDs* ranged from 901 ms to 1099 ms and 243 to 496, respectively. We are not exactly sure what her point is. The 200 ms Simon effect was highly significant and the data were not inordinately variable. Indeed, Bialystok administered virtually the same task to 40 adults and found robust group differences even though *SDs* were higher than 1000 (Bialystok *et al.*, 2004, Experiment 1). Fourth, she points out that the correlation between SES and Simon performance was quite moderate. This is true of course, but our study was designed to control for, not investigate, the effects of SES on executive control. The fact of the matter is that we used methods that were very comparable with Bialystok's, we controlled for differences in ethnicity and SES, and we failed to replicate her findings.

Obviously, our study is small and preliminary, and our conclusions are suggestive at best. At the same time, the findings should not be dismissed simply because they do not fit with conventional wisdom. We believe they should give us pause.

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