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Claiming Evidence from Non-evidence: A Reply to Morton and Harper

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Abstract

Morton and Harper (2007) argue that research presented in support of a bilingual advantage in the development of executive control has been confounded with social class, the actual mechanism for group differences. As evidence, they report a study in which a small group of monolingual and bilingual 6- and 7-year olds performed similarly on a Simon task. The present paper points to weaknesses in their experimental design, analysis, and logic that together undermine their criticism of the conclusion that bilingualism is responsible for the reported group differences.

Sometimes research results fail to support our hypotheses and reveal instead a significant contrary effect. In these cases, we must reconsider the conceptualization that led to the incorrect prediction. But sometimes research results fail to indicate anything at all, and our statistical tools indicate no systematic pattern and no basis for accepting the experimental hypothesis. This null result is a common outcome, but how is it to be interpreted? For Morton and Harper (2007), their decision was to dismiss research that had reported the effect that they had failed to obtain and conclude that the previous body of research was confounded by poor experimental design. By any standard of evidence, this is an extraordinary claim to base on a single null result.

The phenomenon in this case is that bilingualism improves executive control. This finding was first reported by Bialystok and Codd (1997) for children and by Bialystok et al. (2004) for adults and older adults and has since been replicated in both children (e.g., Carlson & Meltzoff, 2008; Mezzacappa, 2004; Yang, Shih, & Lust, 2005) and adults (e.g., Blumenfeld, & Marian, 2007; Costa, Hernández, & Sebastián-Gallés, 2008; Colzato et al., 2008; Kavé et al., 2008; Treccani et al., in press; Tzelgov et al., 1990; Zied et al., 2004). Morton and Harper (2007) report a study in which 17 monolingual and 17 bilingual children who were 6- to 7-years old performed a Simon task. Contrary to the results of a similar study by Martin and Bialystok (2003), they found no difference between the children in the two groups on either accuracy or reaction time. Although they claim that theirs was the “exact task” (p. 722) used by Martin and Bialystok (2003) and by Bialystok et al (2004), they never report the details. However, the procedures in the two cited studies were not the same, so the details of their task are not known. This is important because task parameters are crucial in determining outcomes. In research with both children (Martin-Rhee & Bialystok, 2008) and adults (Bialystok, 2006), small changes in the presentation of the Simon task altered the outcome, with a bilingual advantage occurring only the most demanding conditions. More importantly, the children in the Morton and Harper study were approximately 1½ years *older* than the 5-year-olds in the study they were replicating. This is a considerable difference in developmental time and a particularly active period for the development of

executive processes (Diamond, 2002). It is possible that by 7-years old, initial differences in this particular task have been overcome.

Aside from issues of replication, there are concerns with the data reported by Morton and Harper. The range of mean RT is 901 to 1099 ms with an SD range from 243 to 496. With a sample size of only 17 children per group, it is not surprising that there are no reliable between-group effects. For analyses of accuracy, the mean number of errors ranges from 0.3 to 1.7 out of 28, suggesting ceiling effects and making ANOVA inappropriate. Thus, their failure to find group differences might be explained on statistical grounds.

Morton and Harper have a different interpretation for their failure to replicate, namely, that previous studies confounded bilingualism with ethnicity and social class (SES). They never say what they mean by ethnicity, so I cannot comment further. Their argument for the role of SES is described in more detail.

It is uncontroversial that SES is formative in children's development of executive control, with higher SES children demonstrating more precocious development. The study by Noble et al. (2005) is a particularly elegant example of this research. Morton and Harper's (2007) contribution is to report a moderate ($r = -.35$, $p < .05$) correlation between SES and errors in the Simon task (but note the possible problem of distribution in the error data). However, whether or not bilingualism affects performance, the effects of bilingualism and SES are independent. In Carlson and Meltzoff's (2008) study, lower SES bilingual children outperformed higher SES monolingual children on critical tests of executive control. One factor does not preclude the influence of the other.

Morton and Harper (2007) repeat several times that none of my research has measured SES, the factor they claim is actually responsible for group differences. They are right: I never measure SES. The reason is that SES is always carefully controlled, with both monolingual and bilingual children attending the same schools and living in the same homogeneous middle-class neighbourhoods. However, to support their claim for an SES confound in my research, Morton and Harper explain that Canadian immigration policy selects for well-educated candidates and quote statistics indicating that immigrants to Canada are, on average, more educated than native-born Canadians. However, such population statistics are irrelevant: we do not sample randomly from across Canada's urban and rural, northern and southern, industrial and agricultural regions, the population on whom the demographic statistics are calculated; instead, we carefully target schools in middle-class urban neighbourhoods where residents are educated and professional and children share the same experiences. This is explained in every paper. Morton and Harper's solution to the putative confound between bilingualism and SES is to base their study on a comparison between monolingual and bilingual children for whom ethnicity and SES are identical, namely, monolingual Anglophones and French-English bilinguals who are also native-born Canadians. It is a good choice and one that we have used many times (Bialystok, 1997; Bialystok & Martin, 2004, Study 2; Martin & Bialystok, 2003; Martin-Rhee & Bialystok, 2008), but our results from those studies are consistent with our comparisons based on immigrant bilinguals.

The study reported by Morton and Harper (2007) that fails to detect differences between monolingual and bilingual children may or may not be a methodological replication of my previous research but their sample was 1 to 2 years older than the children in the study they criticize and the data analyses may be invalid. Their arguments justifying the charge that SES is confounded with bilingualism in my research are unsupported and inconsistent with our sampling methods. So what can be concluded from their experiment that shows no

difference between groups? In Morton and Harper (2007), the lack of evidence provides no evidence at all.

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