Letter to the Editors of *Psychological Science*: Response to Luk et al. (What Have We Learned About Bilingualism?). Regarding Nichols et al. (2020)

Luk et al. (2021) state that "while the quest for a so-called *bilingual advantage* seems to dominate recent research on bilingualism, this question fails to account for the complexity of bilingual experience in the 21st century" (para. 5). There always will be studies in which a particular subgroup of the bilingual population (perhaps those who speak a certain second language) might be better at one specific cognitive test, but we must be cautious about such observations. For example, in many cases indicating a bilingual advantage, there are similar studies that have shown exactly the opposite (that is, the results do not replicate) (e.g., Antón et al., 2014; D'Souza, Moradzadeh, & Wiseheart, 2018). Second, when these studies are assessed in aggregate through meta-analyses, the results generally are inconsistent (Gunnerud et al., 2020; Lehtonen et al., 2018). In short, selecting studies that support a particular hypothesis deflects attention from the central question addressed in our paper, which is whether bilingualism affords general cognitive advantages in the broader population (Nichols et al., 2020). Here, we see strong parallels with the brain training literature, which is replete with claims that a specific type of training in a particular subgroup of individuals at very specific intervals leads to gains in one or more specific cognitive tasks. Again, such findings usually fail to replicate and meta-analyses of the data show little consistency across studies (Owen et al., 2010; Sala & Gobet, 2017).

Along similar lines, Luk and colleagues suggest that "by restricting the matched sample to individuals from English-speaking countries, it is possible that the bilingual group is heterogeneous" (para. 4). Yet, if we were to focus on a truly homogeneous group of bilinguals, our results would only be applicable to those specific circumstances and subpopulations. This practice is undesirable as it lends itself to selective interpretation.

We also draw attention to an issue heretofore not adequately addressed in the bilingualism literature. If there is a "bilingual advantage" (either in the general population or in some subgroup with particular characteristics), what is the neural mechanism by which such an advantage could occur (Blanco-Elorrieta & Caramazza, 2021)? The main claim is that language joint activation, monitoring, and selecting rely on domain-general processes that in turn are strengthened through their use in bilingual language control (Bialystok, 2017). Yet, this is not a mechanistic explanation. Rather, it is an inference based on a selected group of studies. This issue has recently been dealt with in the brain training literature, where diffusion tensor imaging has been used to show *why* training on one cognitive task would not be expected to lead to enhancements on other (even highly) related tasks (Nichols et al., 2021). What is lacking from the bilingualism literature is a similar neuroscientific explanation for how bilingualism would be expected to lead to improvements in any aspect of cognition. Given that no such neuroscientific mechanism has been established, the results of our study showing that bilingualism does not lead to cognitive advantages in the general population remain unsurprising.

Emily S. Nichols

Faculty of Education & Brain and Mind Institute, The University of Western Ontario

enicho4@uwo.ca

Adrian M. Owen

Brain and Mind Institute; Department of Physiology and Pharmacology, Schulich School of Medicine and Dentistry; & Department of Psychology, The University of Western Ontario

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