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Commentary

Neuroplasticity as a model for bilingualism: Commentary on Baum and Titone

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Un grand merci à Shari Baum and Debra Titone for their review paper, "Moving Toward a Neuroplasticity View of Bilingualism, Executive Control and Aging." (Please note the code switch in that previous sentence is part of my effort to build my cognitive reserve.) The authors are to be congratulated for providing a timely and constructive review of the current state of our understanding of the potential impact of bilingualism on executive control processes and cognitive aging. In this short commentary, I will comment on one of their key contributions to the discussion, namely, that researchers should embrace and address the individual variability among bilinguals.

The research discussion has gotten bogged down in a distracting debate as to whether a "bilingual advantage" for cognitive control exists. Rather than choosing sides, I would argue that studies that demonstrate credible nonreplications or partial replications can be just as informative (if not more so) than studies that provide full replications of studies that demonstrate a bilingual advantage. For instance, the findings from my research group in Montréal have not provided consistent support for a bilingual advantage when comparing generally well-matched groups of monolingual and bilingual subjects. For instance, we did not observe evidence of a behavioral bilingual advantage in young adults on a Stroop task (Kousaie & Phillips, 2012b) or on three measures of cognitive control (Kousaie & Phillips, 2012c) that are commonly used to test this hypothesis in bilinguals. However, the latter study did demonstrate event-related brain potential differences between the groups, raising the challenge of how we might interpret group differences in brain activation (and anatomy) in the absence of behavioral differences on specific measures of cognitive control (for group differences on magnetoencephalography measures but not behavioral measures, see Bialystok et al., 2005). It is interesting that our current work in older adults (Kousaie & Phillips, 2012a; and work in progress) indicates a behavioral advantage for bilinguals on one cognitive control task and a behavioral disadvantage on another task, suggesting that the relationship between bilingualism and cognitive control is not a straightforward one. What does all of this mean? Is it possible that all of the good Montréal bilinguals, the ones capable of managing competition between their two languages, fled the city in the 1990s to inhabit cities where advantages are more frequently documented? Apart from this tongue-in-cheek explanation, these failures to replicate should drive all of us toward more thoughtful attempts to understand the nuances of the bilingual experience that contribute to a greater or lesser extent to neuroplasticity. In other words, what is it about language use in Montréal (a city where the majority of bilinguals are bilingual not because they are immigrants and learned their

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languages sequentially but because they are exposed to both official languages of Canada through media, education, and community and are likely to use both languages to varying degrees throughout a typical day) that makes it different from language use in other cities where bilinguals are studied?

In a similar vein, Chertkow et al. (2010), using a patient sample recruited in Montréal, a predominantly bilingual city in Canada, did not find evidence supporting the proposal that bilingualism per se is associated with a delay in onset of the symptoms of Alzheimer disease, especially when immigration status was considered. A subsequent study from the Toronto group (Craik, Bialystok, & Freedman, 2010) also attempted to address the issue of immigration status by comparing age of onset of attention-deficit symptoms in small subgroups of monolinguals and bilinguals, both of which comprised immigrants and nonimmigrants. Although this is a perfectly reasonable thing to do, it highlights a very complex issue, namely, how do we recruit groups that are truly well matched on meaningful and measureable dimensions? Suppose for a moment that the effect of sociocultural variables like immigrant status really do have an impact. Now, consider how truly difficult it is to compose participant samples of immigrant monolinguals and immigrant bilinguals that are meaningfully matched on many of the variables that could possibly have an impact on neuroplasticity. For instance, monolingual immigrants to most cities in Canada are emigrating from English-speaking countries (e.g., the United Kingdom, the United States, or Australia) where the access to education, good nutrition, and health care is relatively high and life experiences (e.g., exposure to war or famine) are relatively homogeneous at a population level. These participants are then compared with bilinguals emigrating from scores of different countries (e.g., Poland, Germany, Rumania, or China) where there is much greater heterogeneity in these variables that potentially have an impact on neuroplasticity. Again, my point is not to suggest that one group of research results might be right or wrong. My point is to use this unresolved example of whether immigration status may moderate the effect of multilingualism on neuroplasticity to highlight the challenges we face when trying to quantify highly complex interrelated variables.

Baum and Titone encourage us to "embrace the individual variability among bilinguals by adopting experimental and statistical approaches that respect the complexity of the questions addressed." This is a laudable but difficult goal. A recent conversation with my French–English bilingual graduate student highlights the complexity of the problem. He is highly proficient in his second language (English), with a barely perceptible accent. I was quizzing him on the varying pattern of usage of his languages over his lifetime. He was raised in a primarily French-speaking home and socialized primarily with Francophone children; however, he began learning English when he was approximately 12 years old. He attended a Francophone university and estimated that, at that point, he was speaking 90% French and 10% English. Three years ago he started a job at an Anglophone college. One year ago he began graduate studies at Concordia University (where all instruction is in English and the language of the lab is English). His girlfriend of 3 years is English speaking, and he now estimates his language usage to be 5% French and 95% English. With respect to its impact on neuroplasticity, does it make a difference that this shift in his language usage happened when he was 23

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years old? What if it had happened when he was 15? Does it matter if one language is used exclusively in a work/educational context and the other language is used in a social context? All of these factors might potentially impact the need for and the scope of bilingual language control, which is the proposed mechanism underlying enhanced neuroplasticity. What impact might these variations in the behavioral ecology of the bilingual speaker (Green, 2011) have on neuroplasticity? Now, multiply all of these potential variations by $n = 24$ (or 14, or 54) other bilingual participants in any given research study, and we begin to realize how subtle and ephemeral the experimental effects may be. The challenge that Baum and Titone present to us is that these individual differences must be identified, reliably and validly quantifiable, reported in our research publications, and then subjected to experimental manipulation.

As Baum and Titone suggest, population-based studies may provide the sample sizes and variability in individual differences necessary to explore some of these questions. The accrual of data over the past decade has been sufficiently provocative and exciting to create interest among funding agencies. Twenty years ago it would have been unheard of that large-scale population-based studies would have even considered multilingualism as a variable to measure; today, by framing it as one of several possible avenues to neuroplasticity, we have a hope of beginning to understand its potential impact. However, we must realize that population-based studies are rarely going to have sufficient information on the subtle behaviors (e.g., age of second language acquisition, degree of proficiency, and sociocultural factors in language use) that are likely to underlie the relationship between the bilingual's language behavior and cognitive outcomes (for a further discussion of these and other challenges, please see Duncan & Phillips, in press). Two large-scale cohort studies highlight the challenges involved. Kave, Eyal, Shorek, and Cohen-Mansfield (2008) examined over 800 older bilingual adults as a representative sample of the Israeli Jewish population. The number of languages spoken correlated positively with cognitive screening test scores over and above the effect of other demographic variables (e.g., age, sex, gender, education, place of birth, or age at immigration). Of note, the study did not contain any monolinguals; all participants spoke Hebrew and at least one other language. Participants self-reported which languages they spoke, which ones they spoke at home, and which language they were most comfortable speaking; however, amount of time speaking each language, age of acquisition, and proficiency were not assessed.

Another cohort study, by Bak, Nissan, Allerhand, and Deary (2014), assessed later-life cognition in 853 older English native speakers, 262 of whom reported having learned another language to a degree "allowing them to communicate." These older adults were classified as being bilingual and were shown to perform better than age-matched monolinguals on measures of general intelligence and reading, which stands in contrast to experimental studies that, when benefits are found, typically observed them on measures of executive functioning and not general intelligence or language-related tasks. Although results of the study garnered media attention from the BBC, the CBC, and elsewhere, a critical read of the study shows that the participants would not meet criteria for bilingualism by many researchers in the field. All participants were born and raised in Edinburgh, Scotland. On the plus side, this eliminated any potential effects of immigration

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and minimized certain cultural variables. On the negative side, one must question the degree to which the participants were able to engage in any meaningful communication activities in their second language given that they were English native speakers born and living in an officially English-speaking country. Twenty-five percent of them did not learn their second language until after the age of 18, and 65% of them did not use their second language in their everyday lives. This study might be better thought of as one which reveals the later-life cognitive benefits of having had the opportunity to study a second language in one's youth (along with any other unmeasured enriched experiences that may accompany such an opportunity).

Thus, it is clear that both experimental studies and cohort/longitudinal/population-level studies are required. Research progress will be required on both fronts to solve these complicated questions. Regardless of the direction taken, we owe a huge debt of gratitude to the earlier researchers who have put bilingualism on the research map and on the top of the pages of our newspapers, magazines, and web searches.

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Natalie A. Phillips
Concordia University

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