

Sunny review casts a foreboding shadow over status quo bilingual advantage research

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For several years now, headlines around the world have been trumpeting the cognitive control advantages enjoyed by bilinguals. As the story goes, a lifetime of experience selecting between two competing languages leads to improvements in control that generalize beyond the domain of language. It even protects the brain from the untoward effects of aging.

Despite the wide adulation of this view, critical readers like me find the whole story to be an insufferable mixture of excessive claims and weak evidence. Many papers claim to be studying bilinguals but do not measure the first language and second language proficiency of bilingual participants (Bialystok, 1999, 2010; Bialystok, Craik, Klein, & Viswanathan, 2004), claim that confounds, such as socioeconomic status, are controlled when these variables are not measured (Bialystok, 1999; Bialystok & Martin, 2004; Martin-Rhee & Bialystok, 2008), claim that bilinguals show an advantage in cognitive control when bilinguals respond faster and more accurately on incompatible *and* compatible trials (Bialystok, 2010; Bialystok et al., 2004), and claim that the bilingual advantage emerges from a lifetime of experience managing two languages even though the advantage is evident in prelexical infants (Kovács & Mehler, 2009) and disappears in adults after four blocks of trials (see experiment 3 of Bialystok et al., 2004).

Needless to say, Baum and Titone's theoretical review of the bilingual advantage is a welcome departure from this impossibly stale status quo. Brimming with enthusiasm and warm optimism, the authors implore those invested in the study of bilingualism to acknowledge the complexity of the basic phenomena and raise more nuanced empirical questions. Baum and Titone argue that bilingual language processing, and any advantage it may have for domain-general cognitive proficiencies, should be viewed as specific instances of neuroplasticity, which are changes in the functional and structural organization of the brain induced by experience. The idea that experience shapes the brain is certainly not new in the field of neuroscience (e.g., Hubel, Wiesel, & Levay, 1977), but it has become a focus of intense interest within the neuroimaging community given advances in technologies such as magnetic resonance imaging. Magnetic resonance imaging allows scientists to rapidly interrogate both the structure and the function of the human brain in vivo and potentially examine the fingerprint of experience therein. However, as Baum and Titone are quick to caution, new empirical horizons do not obviate the need for sound theory, careful experimental design, and judicious interpretation. In bringing together the many threads of their sunny review, they highlight three themes for guiding future research:

Theme 1: Bilinguals not only vary from monolinguals but also from one another.

Theme 2: It remains unclear whether bilingual language status is the cause or the consequence of cognitive advantage.

Theme 3: Whether bilinguals are advantaged in cognitive control may be a question that is too simple to answer.

These are wonderful themes, and they deserve careful consideration. Let us start with Theme 2, arguably the most important. Is bilingualism the cause or the consequence of cognitive advantage? For completeness, bilingualism researchers should also consider the third possibility, namely, that bilingualism is neither a cause nor a consequence of cognitive advantage, but a mere correlate. In any case, we actually already know something about this. Seven-month-old “bilingual” infants outperform 7-month-old “monolingual” infants on cognitive control tasks (Kovács & Mehler, 2009). This finding has several interpretations, and none is particularly savory for those invested in the standard lexical-selection practice account. One possibility is that bilingualism leads to cognitive advantage, but the causal effect of bilingualism (whatever it is) has nothing to do with a lifetime of practice with lexical selection. Another possibility is that bilingual and monolingual infants were reared differently by their respective parents (owing perhaps to differences in parental socioeconomic status, education background, etc.), and these rearing differences had implications for infants’ cognitive control. Embrace this interpretation and language status becomes a mere correlate of cognitive advantage. A final possibility is that the infants born into bilingual families were genetically advantaged at birth relative to those born into monolingual families. Cognitive control is highly heritable, with estimates of $h > 0.9$ in some studies (Friedman et al., 2008), and not highly amenable to change through practice (Owen et al., 2010; Redick et al., 2013). Embrace this interpretation and language status becomes either a correlate or a consequence of cognitive advantage but certainly not a cause. Obviously, the jury remains out. We do not know the answers. However, Baum and Titone should be applauded for prioritizing this issue; it is of fundamental importance.

Another critical issue Baum and Titone highlight is that bilinguals vary considerably from one another. This is a wonderful point. If, as the lexical selection account maintains, the bilingual advantage finds its origin in practice, why not test hypotheses just with bilinguals based on what we know about practice effects? Consider the importance of when practice occurs (i.e., when second language acquisition begins). Should early in life not differ from later in life? To the extent that training leads to any generalized gains in cognitive control (which is debatable), the effects are more consistently observed in very young children (Wass, Porayska-Pomsta, & Johnson, 2011). One simple prediction then would be that the bilingual advantage should be larger among individuals who acquired the second language earlier rather than later in life (e.g., among 10-year-olds who have been bilingual over the course of their entire life compared with 40-year-olds who have been bilingual since the age of 30)? Another simple prediction: From a straight-up practice of lexical selection account, should the bilingual advantage not be larger among older than younger bilinguals given that older individuals have more years of practice with lexical selection than younger bilinguals? There are lots of possibilities to explore here; who knows which way to go is best? However, it would be refreshing if bilingual researchers moved beyond documenting the bilingual

advantage one more time with some new task (e.g., Bialystok, 2010) and actually began testing hypotheses, or at least measured the second language proficiency of their bilingual participants.

Baum and Titone's final directive is that perhaps bilingual researchers should move beyond the question of whether the bilingual advantage does or does not exist. Of course, they should! As I have stated elsewhere, to the extent that claims about the bilingual advantage are based on group comparisons rife with uncontrolled confounds, these claims will be open to alternative explanation. If these confounds cannot be adequately controlled despite the best efforts of the experimenter, perhaps it is time to change the question.

Thus, read carefully. Beneath Baum and Titone's sunny tone is a foreboding message for status quo bilingual advantage research.

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