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Discussion

Science does not disengage

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ABSTRACT

In a recent commentary, Goldsmith and Morton (in press) argue that the results of a study demonstrating smaller sequential congruency effects (SCEs) for bilinguals than for monolinguals (Grundy, Chung-Fat-Yim, Friesen, Mak, & Bialystok, 2017) is incorrect in its interpretation of SCEs. Moreover, their overall framework is that there is no evidence for any cognitive differences between monolingual and bilingual young adults. Here, we provide evidence in support of our original interpretation and challenge their basis for arguing that there are no language group differences on these cognitive measures.

It has been known for a long time that experience has a profound effect on children's cognitive functioning, leading to the establishment of such institutions as public education to level the field and efforts such as Head Start to address potential gaps created by differences in opportunity. It is a more recent insight, however, that experience continues to affect cognitive and brain function throughout life (Pascual-Leone, Amedi, Fregni, & Merabet, 2005). Support for this idea has come from a wide range of sources, including taxi driving in London (Maguire et al., 2000), video-game playing (Bavelier & Davidson, 2013), musical training (Chan, Ho, & Cheung, 1998), and juggling (Draganski et al., 2004), among others. If we are to understand cognition, then it is imperative to understand how it is impacted by experience. Yet, of all activities in which humans engage, nothing is as intense or sustained as using language; language is integrated into all aspects of cognition, it is actively involved in our thoughts for all our waking hours, and the massive interconnectivity of the brain assures that traditional language regions are implicated in major brain networks, such as the fronto-parietal control network (Vincent, Kahn, Snyder, Raichle, & Buckner, 2008). Surely, language experience must be included in efforts to understand the role of experience in cognition.

Systematic research investigating the effect of bilingualism on children's language and cognitive development has been carried out since the 1970s (review in Barac, Bialystok, Castro, & Sanchez, 2014) and detailed studies of language processing in bilingual adults have been conducted since the 1980s (review in Kroll & Tokowicz, 2005). However, the first study that connected these areas and explored the possibility that bilingualism also had cognitive effects in adulthood was published by Bialystok, Craik, Klein, and Viswanathan (2004). That study led to a rapid growth in research on the cognitive (reviews in Bialystok, 2017; Bialystok, Craik, Green, & Gollan, 2009) and brain effects (reviews in Grundy, Anderson, & Bialystok, 2017; Li, Legault, & Litcofsky, 2014) of bilingualism across the lifespan, producing a substantial body of evidence. Questions remain about the details of these effects, the mechanisms responsible for them, and the types of bilingual experience and testing conditions that are most likely to reveal them, but none of that undermines what we *do* know: bilingualism has a role in reshaping the mind.

A recent paper by Goldsmith and Morton (in press) is a response to a study published by our group on this topic (Grundy, Chung-Fat-Yim, Friesen, Mak, & Bialystok, 2017) but is also presented as a fundamental criticism of the enterprise of investigating cognitive effects of bilingualism for the purpose of revealing group differences on these measures. It is a serious charge for a researcher to suggest that an entire area of study is misguided, so the arguments supporting that position must be cogent and persuasive. In the present case, neither the criticisms of the specific study nor the general comments about the broader field that examines the cognitive consequences of bilingualism are accurate.

The paper by Grundy, Anderson, et al. (2017) reports results from three studies in which young adults performed a flanker task; monolinguals and bilinguals achieved comparable behavioral outcomes using traditional RT analyses but a more detailed analysis based on the relation between adjacent trials in terms of sequential congruency effects (SCEs) indicated a smaller influence of previous trial congruency on current trial performance for bilinguals. Study 3 included data from electrophysiology and showed that deflections in the N2 and P3 waveforms indicated similar persistent effects of previous congruency on the current trial for monolinguals but not bilinguals. Our interpretation was that bilinguals were more efficient than monolinguals at disengaging attention from the previous trial in order to devote resources to the current trial. We speculated about the role that this disengagement

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might have in understanding other results in the literature concerned with cognitive effects of bilingualism.

Goldsmith and Morton begin their paper with a skeptical[1] summary of the research showing cognitive benefits of bilingualism in adulthood and describe this recent study as a "marked departure from the Bialystok group's own work (e.g., Bialystok et al., 2004) insofar as they fail to replicate the bilingual advantage in cognitive control using standard methods." To determine whether any departure has occurred, it is important to be clear about the original study and the subsequent trajectory of the research. In the 2004 paper, we compared monolinguals and bilinguals who were middle-aged (40-years old) or older adults (70-years old) performing a Simon task and found better performance by bilinguals in both age groups, with larger differences for the older adults. However, in the first study to extend these findings to young adults published in the following year, Bialystok, Martin, and Viswanathan (2005) replicated these effects for children and older adults but found no difference between language groups for young adults (20-years old). This absence of group difference was confirmed in subsequent studies (e.g., Bialystok, 2006) and explored in more detail by other researchers such as Costa and colleagues who attempted to determine the task features that were associated with the presence or absence of these effects (e.g., Costa, Hernández, Costa-Faidella, & Sebastián-Gallés, 2009). Therefore, by the time that Paap and Greenberg (2013) published their "replication failure", it was well known that these effects were rarely seen in the reaction time record of young adults. Thus, the overall RT results of our recent paper present no departure from our previous work.

The second aspect of the alleged "marked departure" is that we have abandoned "standard methods", but again, no departure has occurred. Following the substantial evidence that behavioral differences between monolingual and bilingual young adults were rarely found, most researchers tried to understand the problem in more detail by including methods such as functional imaging (Abutalebi et al., 2012), structural imaging (Mechelli et al., 2004), and electrophysiology (Kousaie & Phillips, 2012). These studies produced more complex models of bilingual effects than any that had been considered previously by identifying processing strategies or functional networks that distinguished among the language groups even when behavioral outcomes were comparable. Investigators who persisted in using the "standard methods" continued to find no group differences (Paap & Greenberg, 2013; Paap & Sawi, 2014), arguing somewhat bizarrely that there was "publication bias" (De Bruin, Treccani, & Della Sala, 2015) against null results – bizarre in that these papers were all published so their objection must be that papers showing positive effects were *also* published. Our research has incorporated these alternative methods from the beginning, including eye tracking (Bialystok, Craik, & Ryan, 2006), functional brain analyses (Luk, Anderson, Craik, Grady, & Bialystok, 2010), structural brain analyses (Luk, Bialystok, Craik, & Grady, 2011), and electrophysiology (Moreno, Wodniecka, Tays, Alain, & Bialystok, 2014).

The main part of the Goldsmith and Morton paper is a critique claiming that our interpretation of SCEs is mistaken and that the data show the opposite of what we argue. Contrary to their statement that there is "virtually complete consensus" on the meaning of SCEs, the interpretation of these effects is an area of active debate (Braem, Abrahamse, Duthoo, & Notebaert, 2014; Duthoo, Abrahamse, Braem, Boehler, & Notebaert, 2014; Egner, 2014; Schmidt, 2013). However, the debate is centered on the merits of different accounts roughly corresponding to bottom-up learning theories and top-down control theories; Goldsmith and Morton, in contrast, focus on the size differences between groups, arguing that larger SCEs signal better performance. They cite Egner (2014) to support their conclusion that both types of accounts rely on learning and so larger SCEs are an expression of better learning and memory. However, Egner's (2014) purpose was to find common ground between the bottom-up and top-down explanations; what he actually says is "any time that a past experience (e.g., encountering an incongruent trial) affects current performance, we are, by definition, dealing with an expression of memory (or learning)" (p. 4). This is indisputable and wholly irrelevant to our argument. It is certainly irrelevant to a conclusion that more learning is reflected in larger SCEs.

The majority of the SCE literature focuses on the presence or absence of the effect and not its size. For example, proponents of control-based accounts (Botvinick, Braver, Barch, Carter, & Cohen, 2001), feature-integration accounts (Hommel, Proctor, & Vu, 2004), contingency-learning accounts (Schmidt & De Houwer, 2011), and repetition-expectancy accounts (Duthoo, Abrahamse, Braem, & Notebaert, 2014) have examined the conditions under which the SCE effect can be elicited or eliminated by changing task parameters to illustrate aspects of their individual theory. In this way, control accounts suggest that SCEs reflect an adjustment in top-down control in response to conflict signals (e.g., Botvinick et al., 2001) and feature integration theories propose that episodic binding of stimulus and response features are retrieved from one trial to the next (e.g., Hommel et al., 2004). For example, Botvinick et al. (2001) state that "conflict monitoring serves to translate the occurrence of conflict into compensatory adjustments in control" (p.625). In other words, the processes responsible for the SCE are called on when needed by the context.

The hypothesis for our study was that SCE costs would be smaller for bilinguals than monolinguals, a result we obtained in all three studies. According to Goldsmith and Morton, this smaller SCE indicates poorer learning by bilinguals, but the available evidence that does examine relative size of the SCE leads to the opposite interpretation. First, SCEs become smaller with practice (Mayr & Awh, 2009; Van Steenbergen, Haasnoot, Bocanegra, Berretty, & Hommel, 2015), indicating that more fluent performance is associated with smaller SCEs. Second, SCEs become smaller with longer response-to-stimulus intervals (RSIs) where there is more time to adjust performance – a pattern opposite to what would be expected if larger SCEs were more adaptive. Third, conflict trials may be aversive (Botvinick, 2007; Dreisbach & Fischer, 2012) and proponents of the conflict monitoring account suggest that SCEs may reflect a form of avoidance learning (Botvinick, 2007). Thus, the extent to which these conflict trials are perceived as aversive will influence the size of the SCE. Van Steenbergen, Band, and Hommel (2009) showed that SCEs were only present when the task context involved negative or neutral signals and that SCEs were eliminated when participants received reward signals between trials. Relatedly, more anxious individuals show larger SCEs both behaviorally (Schuch & Koch, 2015) and electrophysiologically (Osinsky, Alexander, Gebhardt, & Hennig, 2010) than less anxious individuals. Trikojat, Buske-Kirschbaum, Schmitt, and Plessow (2015) replicated this finding and further showed that patients with seasonal allergic rhinitis, a condition that includes subjective cognitive complaints, showed larger SCEs when their symptoms were worse. Finally, smaller SCEs are associated with higher fluid intelligence (Liu, Xiao, & Shi, 2016). There is simply no evidence to support the interpretation that larger SCEs reflect better cognitive performance than do smaller ones.

Goldsmith and Morton also misinterpret our explanation of disengagement and its relation to EEG studies that examine alpha power between trials (Compton, Huber, Levinson, & Zheutlin, 2012). Contrary to their claim, we say nothing about whether congruent or incongruent trials contribute more to the differences in SCEs between groups but specifically state that bilinguals are faster at disengaging from *all* trial types. If the next trial replicates the previous trial, then this disengagement is in fact detrimental to performance, but if the new trial is different, then it is helpful. Therefore, collapsing across all trials will erase overall group differences and conceal processing differences that distinguish between the groups. Goldsmith and Morton are correct in

[1] The skepticism is conveyed by placing the word *enhancements* in quotation marks.

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