Applied Psycholinguistics **35** (2014), 933–942
doi:10.1017/S0142716414000289

AUTHORS' RESPONSE

The future of bilingualism research: Insufferably optimistic and replete with new questions

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The scientific process applied to any domain is a thing of power and beauty. It enables its practitioners to systematically and rigorously pursue questions of importance, in a manner that is, by necessity, adaptive and tenacious. As was his way with most matters of relevance to psychology, the words of William James (*The Principles of Psychology*, 1890) are illustrative here:

> Romeo wants Juliet as the filings want the magnet; and if no obstacles intervene he moves towards her by as straight a line as they. But Romeo and Juliet, if a wall be built between them, do not remain idiotically pressing their faces against its opposite sides like the magnet and the filings with the card. Romeo soon finds a circuitous way, by scaling the wall or otherwise, of touching Juliet's lips directly. With the filings the path is fixed; whether it reaches the end depends on accidents. With the lover it is the end which is fixed, the path may be modified indefinitely. (p. 7)

Anyone who studies bilingualism will likely sympathize with this Shakespearian parable. As Byers-Heinlein boldly states in the first sentence of her Commentary, "Studying bilingualism is complicated" (see also Kroll & Fricke). This sentiment is echoed in our original paper, and in virtually all other commentaries. For example, Bialystok discusses the challenges associated with precisely defining bilingualism; Genesee highlights the relatively untapped role of sociocultural factors on bilingual neurocognition and questions what it means to be "native-like" in a first language (L1); Kapa and Kemper correctly note how little we really know about executive control itself much less its relation to bilingualism; Klein and Watkins question what differences in brain structure and function actually

mean, for example, should a thinner versus thicker cortex be interpreted as good or bad over developmental time. Morton reiterates many of the themes discussed in our original paper regarding the potential problems associated with suitably matching bilingual and monolingual groups; and Phillips wonders if it will ever be possible to empirically capture the myriad ways that individual bilingual and multilingual people function in the real world. Yes, the challenges are many, and bilingualism researchers have many walls to scale.

However, in stark contrast with the literary fate of Shakespeare's adolescent lovers, there is much to be gained from our research enterprise, much has already been gained, and in actuality all scientific domains worth studying pose comparable if not greater challenges. It is clear that work on bilingualism has demonstrated great flexibility and tenacity already, and it is likely that this empirical and methodological agility will increase as methods and theoretical approaches in cognitive neuroscience generally become more refined. Similar optimistic sentiments are present in our original paper and in the commentaries. For example, Abutalebi and Rietbergen emphasize how much we already do know about the neurofunctional and neuroanatomical connection between bilingualism and executive control; Bialystok compellingly argues here (and elsewhere) the many benefits associated with a "neuroplasticity" framing of the question rather than the "advantages" framing, which notably include broadening our focus to encompass the environmental factors that drive neuroplasticity (see also Genesee); Green supports (and has long argued for) more nuance in characterizing intersubject variability and raises new challenges that have yet to be addressed (e.g., conversational-level bilingual language processing, multimodal aspects of bilingual language processing; see also Kroll & Fricke); Klein and Watkins argue that the field of neuroimaging is only now becoming methodologically poised to take on the challenges of intersubject variability; and Kroll and Fricke raise new challenges involving the identification of limits and constraints on language-induced neuroplasticity, but they generally highlight the value of the enterprise in providing new knowledge and new lines of inquiry, even if the current findings are variable.

Thus, we reiterate a key point raised in our original Keynote: if the mark of a productive line of scientific inquiry is that it dispels as myth that which was previously considered "fact" (e.g., bilingualism is harmful, bilinguals are impaired), shines a light on new questions that were never before considered, generates a rich database from which scientists can draw upon both now and in the future, and elevates the value of the entire domain in the eyes of the scientific community and general public, then it is without doubt that the field is wildly successful. Moreover, it is increasingly clear that the era of simple yes–no questions regarding bilingual advantages versus disadvantages has already begun to draw to a close, if the great consensus found in the commentaries and recent papers on the utility of framing bilingualism as a paradigmatic case of neuroplasticity are any indication. Thus, as Morton indicated, we are quite optimistic about the great advances of knowledge that are on the horizon for our field.

However, it is also clear that our original paper, by necessity, failed to address many relevant points regarding the relation between bilingualism and executive control, both within and outside the scope of aging populations. In addition, reading the commentaries inspired other more global thoughts regarding how we

should proceed as a community of scientists engaged in a specific scholarly pursuit. Thus, in what follows, we briefly discuss three of these points: (a) the status of existing neuroscience evidence, (b) early developmental approaches that bear on the question, and (c) general strategies for optimizing how we might continue to move forward as a field.

THE STATUS OF EXISTING NEUROSCIENCE EVIDENCE

Certainly much is already known about the neural underpinnings of bilingual language representation and process and bilingual language control. However, there is clearly much we do not know. Of note, the two commentaries that discussed brain–behavior relationships in detail seem to embody these different "what we already know" and "what we have yet to learn" perspectives.

Abutalebi and Rietbergen suggest that we may have understated how far neuroimaging studies focused on bilingualism have already come. For example, they state that we already know the answer to the question "Which aspects of using more than one language induce neuroplastic changes in the human brain?," which is that bilingual language processing is orchestrated by an inhibitory control network that involves the anterior cingulate cortex for conflict monitoring, the presupplementary motor cortex for initiation of language switching, the left prefrontal cortex for interference control, and the inferior parietal lobe for maintenance of task representations. Subcortical structures, such as the left caudate, also play a role in switching between languages and the control of verbal interference. This framework for understanding the neurofunctional underpinnings of bilingualism is embodied in a very well established model that has had a great impact on the field (Abutalebi & Green, 2007), as we discussed in our original Keynote. This model is perhaps the currently most important neurally based mechanistic account of bilingualism to date. Moreover, the authors report that in the latest version of the model, two new neural circuits are included: a prefrontal cortex and right cerebellum connection and a right inferior frontal cortex and subcortical connection to the thalamus and putamen that are involved in the detection of salient cues.

Thus, how does this model answer the question "Which aspects of using more than one language induce neuroplastic changes in the human brain?" Abutalebi and Rietbergen argue that it generally does so by accounting for structural brain changes between bilinguals and monolinguals. They state that because bilingual, but not monolingual, language processing engages different inhibitory control circuits, one would expect adaptive changes over time in a neuroanatomical sense because of Hebbian dynamics, an entirely logical assumption. Further, Abutalebi and Rietbergen state that current neuroimaging evidence can already distinguish between quantitative versus qualitative differences, between bilinguals and monolinguals, and among bilinguals, which is a key point that we raised in our original review. Accordingly, a *qualitative difference* is evidenced when a brain area is active for bilinguals but totally unresponsive for monolinguals. For example, they describe a study where differences were found between bilinguals and monolinguals in a language switching task in which bilinguals were instructed to switch between languages and monolinguals to switch noun and verb production. The

results showed a qualitatively different pattern of results: a blood oxygen level dependent (BOLD) increase in the left putamen of the caudate for bilinguals but a BOLD decrease for monolinguals. However, activity in the supplementary motor cortex was the same for switching in both groups. Thus, these two groups are showing a qualitative difference but not a *quantitative difference*, because if the latter were the case, bilinguals would have differed from monolinguals in degree of activation of the same brain structures.

Although many aspects of these arguments undoubtedly have merit, we nevertheless believe that important questions remain for at least two reasons. First, the existing data still largely hinge on the interpretation of group differences and are thus open to additional questions concerning the many other ways bilinguals could vary, as described in our original Keynote, and by others in their commentaries, most notably Green himself. Second, the distinction between quantitative and qualitative aspects of bilingualism as described by Abutalebi and Rietbergen may not be so simple. We believe that the authors may have misinterpreted what we originally intended in terms of the qualitative versus quantitative distinction (which, of course, is undoubtedly our failing, so we thank these authors for giving us the opportunity to clarify here). Regarding the first point, it is true that existing work is rich in showing that bilinguals and monolinguals recruit different brain circuits during language processing. However, there are many unresolved questions with the existing work which include the following: does a similar network extend to bilinguals who currently or formatively use their languages in a densely code-mixed environment or language compartmentalized environment, or to bilinguals who routinely experience other forms of enrichment in their current lives or formatively (e.g., musical training, high socioeconomic status generally), or to bilinguals who differ in linguistic environment socioculturally or in terms of linguistic typology? In the end, any study that compares bilinguals and monolinguals as a group without respect to different kinds of individual differences within groups and constraints in the population of bilinguals sampled (e.g., all sequential, simultaneous, mixed) will be open to such questions. However, group comparisons of this sort are pioneering as a start insofar as prior to these studies we knew very little.

Moreover, there are methodological questions that can arise for any particular study that complicate interpretation. For example, much of the existing literature hinges on experimental tasks that involve language switching; however, language switching may be more or less normative for different kinds of bilinguals and may trigger a different pattern of brain activity compared to more language-compartmentalized bilingual situations. In addition, given that any observation of BOLD differences hinges, by definition, on a comparison of experimental and baseline or control tasks, can we be absolutely sure that any given task is constructed properly for drawing the kinds of conclusions it wishes to draw? For example, consider the study described in detail by Abutalebi and Rietbergen. Is the demand to switch languages during production comparable to the demand to switch between nouns and verbs? If not, then when we find a different pattern of brain activation for bilinguals and monolinguals, is it because of fundamental differences between these groups or that the two groups did not do comparable tasks? Could there ever be a switching task that is equally as effortful for

monolinguals as language switching is for bilinguals? The answer could be yes, but somehow it does not seem that the field has completely gotten there yet. Finally, it is relatively unclear how bilingual language control dynamics play out across different domains of language. For example, are they the same for production as they are for comprehension? Are they the same for semantics or word-level processing as they are for syntax? How do they play out in the context of real-world or multimodal interactions (see also Green's Commentary).

This discussion brings us to several important points raised by Klein and Watkins, who also focus on brain–behavior linkages. Like those who wrote other commentaries (see below), they focus on the question of how any neurofunctional effects of bilingualism on general cognitive capacities may change over the life span, including early development. It is often assumed in the literature that the effect of bilingualism is constant over the life span but may be more or less easy to experimentally detect at different stages. For example, the argument has been made that one is more likely to find benefits of being bilingual in terms of executive control in childhood or older adulthood than in young adulthood precisely because the former groups have a preexisting hit on executive control functions, whereas the latter group are functioning at peak executive function capacity. However, the assumption of constant pressure exerted by one's bilingual experience over the life span may not be correct. It may also be possible that the effect of bilingual language experience fundamentally differs among that of childhood (where language learning and executive control capacities are currently unfolding) and of young adulthood (where there is a relative plateau to both) and in older adults (where there is the retention of linguistic knowledge but a decline in executive control function). These are all fascinating empirical questions whose study would be advanced by the use of more complex (and expensive) methods such as longitudinal designs, an approach we highlighted in our original paper and that has almost universal acceptance within the commentaries.

Moreover, Klein and Watkins describe a very interesting study focusing on structural brain changes in bilinguals versus monolinguals, which actually challenges the simple idea that "more bilingualism" equals "more plasticity." In their study, they showed that patterns of cortical thickness were equivalent between monolinguals and early, simultaneous bilinguals, who presumably have a maximal dosage, whereas late, sequential bilinguals differed from both groups. Thus, it appears that one's formative learning experience of being bilingual interacts with one's current language context to result in a particular pattern of brain structure (although as they also point out, it is unclear which pattern of cortical thickness is advantageous). That is, perhaps the sequential bilinguals showed the greatest change in cortical thickness because they alone had the experience of learning a single language formatively (like monolinguals) but then had to add a second language (L2) late in life that potentially competes in terms of real-time processing with their L1. In contrast, simultaneous bilinguals who learned both of their languages in an interleaved fashion may not have had the same cognitive experience of cross-language conflict. Of course, such distinctions go back to older notions of compound versus coordinate bilingualism, among others, and more recent accounts involving the role of bilingual behavioral ecology (see Green). It is also noteworthy that the

bilinguals tested in Klein and Watkins were from Montreal, where on average, a high degree of English–French mixing is normative (although bilingual individuals within this context may have a different distribution of mixed vs. single-language exposure). Thus, it remains to be seen if this result would extent to other bilingual populations, particularly those for whom rampant language mixing is not the norm.

Relevant here are the points raised by Kapa and Kemper, which reveal that there are some crucial lapses of knowledge with respect to the executive function side of the bilingualism–executive control equation. For example, cognitive neuroscience abounds with executive control tasks that measure slightly different yet overlapping things and do so using tasks that depend on similar or completely different response effectors (fingers, eyes, voice). From a behavioral standpoint, this has resulted in a situation where there are many available executive control tasks out there, which superficially should reflect the common element of *executive control* in some way (e.g., the unity of executive control according to Miyake and colleagues) but should also reflect what is different across tasks either because different subcomponents of executive control are assessed (i.e., the diversity of executive control) or less interesting superficial differences across tasks are assessed (e.g., that one task requires a participant to speak, make a button press, or an eye movement response). The reality of this in terms of the behavioral assessment of executive function (and this will undoubtedly extend to the neural assessment of executive control in some fashion) is that a given laboratory (e.g., ours) can administer a battery of executive control tasks for which participant performance does not always correlate across tasks in terms of a simple "cost" measure.

Thus, what is the best course of action in terms of assessing executive control if one is interested in linking executive control to bilingualism? Do we create composite measures across tasks to try to maximize the construct we care about that is presumably common across these tasks (e.g., the cost or "incongruency" difference score), ignoring the differences across tasks and the presumed underlying neural activity leading to such differences? Do we create our bilingual–executive control relationship stories on the basis of individual executive control tasks, living serenely with the possibility that the story may not generalize to other executive control tasks? Do we consider the very real possibility that the cost score might be in actuality a red herring, and that in fact, what is happening on congruent trials may also be affected by the overall context of any given executive control task. Do we undertake the really hard work of trying to piece together systematically both sides of the bilingualism–executive control equation, such that we can forge explicit links between subcomponents of each? (As an aside, we place our vote here.) Clearly, as authors, we have the luxury of being able to raise such hard questions in the context of this editorial style paper, which does not demand that we provide convincing answers at this time. However, if we are to be honest with ourselves as a field, this is also part of the complex reality with which we are dealing. Though again, to be optimistic, coming to grips with these complex realities will undoubtedly push forward not just the field of neuroplasticity in bilingualism but also the more general field of neuroplasticity in executive control.

ISSUES PERTAINING TO EARLY LANGUAGE DEVELOPMENT

Another theme across commentaries has to do with the role of early language development (see Bialystok; Byers-Heinlein; Genesee; Kroll & Fricke; and van Hell & Poarch). We did not address this theme in our original review, because it was nominally about the other end of the life span, but clearly what happens during early development cannot be ignored, as it can potentially color all that comes after. Thus, we focus here on a handful of important points pertaining to early development raised by the Commentary authors.

The Commentary by Genesee picks up on the point raised in our original paper that bilinguals differ in ways that matter and discusses how earlier scientists have addressed this question. For example, Lambert's distinction between additive and subtractive bilingualism implied that bilingual advantages should accrue in social situations where being bilingual was additive (i.e., advantageous economically or otherwise) compared to when it was subtractive. Thus, his view was that the advantages accruing were brought about by sociocultural factors. Similarly, Bialystok in her Commentary stated that one of the benefits of the neuroplasticity framework is that it focuses attention on the environmental factors that shape human neuroplasticity. This leads to the fascinating questions of how sociocultural pressures drive cognitive processes and also potentially how inherent cognitive capacities change how individuals respond to sociocultural pressures. Genesee fleshes this argument out in the context of some very exciting applied work on bilingual versus majority-language forms of education for students in the United States and Canada. The bottom line of this work is that Lambert was correct insofar as generalizing student success from one sociocultural context (i.e., Canada) to another (i.e., the United States) may not be possible. For example, research by Genesee and others on bilingual education in Canada suggests that unilingual school instruction in an immersive L2 context (e.g., French) that differs from the home L1 (e.g., English) leads to good outcomes in terms of both L1 and L2 attainment (see papers cited in Genesee). However, this is not the case in the United States, where bilingual school instruction for largely Spanish L1 children yields better outcomes than for those who are immersed in English-only schools.

Again turning to Lambert's original work, one possible source of the difference could go back to the notion of additive versus subtractive bilingualism, insofar as being bilingual in Canada (and possibly even in Quebec) is additive socioculturally rather than subtractive. However, in the United States, bilingualism, which often covaries with differences among people in other factors like socioeconomic status, is relatively more subtractive than additive. Thus, the real-world implications of this work are that taking policy-level steps to counter the perceived subtractive nature of bilingualism by welcoming use of multiple languages in the schools leads to better educational outcomes. Moreover, such sociocultural variables with respect to bilingualism may have implications for how we interpret the data from various bilingual advantages studies that currently exist. That is, it might be the case that under certain sociocultural circumstances bilingualism can create neurocognitive change, but under others, this neurocognitive change may be counteracted or masked by equally powerful sociocultural influences that provide pressures in an opposing direction.

Genesee also reiterates the important point that considering monolinguals as the norm may be highly problematic as a general research strategy for at least two reasons. One is in terms of how you count up differences. For example, it is now common knowledge that the historical disadvantage of vocabulary knowledge in bilinguals disappears when one counts all the unique words known across languages. The second is in terms of how you look for differences; that is, it is often simply assumed that monolinguals are the gold standard for L1 language performance. However, the field of psycholinguistics, which largely stands upon the shoulders of innumerable monolingual language performance studies (involving rarified groups of young, healthy university students to boot), clearly shows that individual differences in language ability are more the norm than the exception. Thus, we strongly believe that one must be highly cautious when one hears the term *nativelike* used in the context of comparing bilinguals to monolinguals.

Van Hell and Poarch also highlight the importance of earlier stages of L2 learning and the emergence of executive control advantages by asking the specific question: "How much bilingual experience is needed to affect executive control?" As described in prior sections, this question is complicated because the sheer *quantity* of bilingual experience may be somewhat orthogonal to the *quality*, that is, the distributional differences in bilingual experience (e.g., the cortical thickness data described by Klein & Watkins).

Along these lines, van Hell and Poarch describe their work showing that bilingual and trilingual 5- to 8-year-old children (both groups raised with multiple languages in the home) had better executive control performance (Simon, ANT tasks) than monolingual or second-language learners, and the L2 learners had better performance than the monolinguals. Thus, they assert that preliminary exposure to a L2 exerted some effect, and these advantages unfolded over developmental time, consistent with other functional magnetic resonance imaging work showing that limited L2 training in increasing L2 proficiency can induce changes in the neural structures associated with language (e.g, left inferior frontal gyrus and left anterior temporal pole). Thus, van Hell and Poarch ask, does this mean that simply increasing L2 proficiency leads to these changes, or is there a critical threshold? Similarly, it is an open question as to how long-lasting these effects are, particularly given some evidence that early musical training increases capacities such as brain stem responses to sounds long after training has stopped and other evidence that early language experiences can facilitate language relearning later in life within some limits (i.e., not in relearners over 40; all cited in their Commentary). Kroll and Fricke raise similar points about language relearning, as well as the acquisition of new languages.

However, we would argue again that the situation may be even more complex than just this. Implicit in van Hell and Poarch's Commentary is the idea that turning up the volume on L2 proficiency can exert some effect either continuously or past some key threshold and that these pressures would presumably be constant over the life span (to the extent that language coactivation is the source of the pressure and this does not change over the life span). However, there are other possibilities. For example, bilingual and trilingual children might reap executive control advantages over L2 and monolingual children not because of the amount of L2 knowledge on-board but rather because of correlated patterns of use with

respect to possessing that knowledge. Accordingly, the bilingual and trilingual children described differ from the L2 and monolingual children not just in terms of amount of L2 knowledge but also in the likelihood that they live a good part of their lives in a mixed-language situation given that they come from homes where both parents speak different languages or more than one language. In contrast, both the L2 learners and the monolinguals, by definition, were raised in unilingual homes. Because the amount of L2 knowledge and distribution of L1/L2 use might be correlated in this particular study, it is difficult to tease these two factors apart, though in principle doing so is theoretically possible in certain bilingual communities. It is also possible that the mental exercise provided by being bilingual during language learning does not outlast the point in time after which a given bilingual knows their two (or more) languages well.

HOW TO OPTIMALLY MOVE FORWARD AS A FIELD?

As we noted in opening, studying complex phenomena is difficult, and bilingualism is certainly complex. Thus, it stands to reason that the outcome of different studies across different labs, cities, and researchers from different domains (e.g., developmental researchers, adult psycholinguists, cognitive neuroscientists) are going to be variable. This is true of any complex interdisciplinary domain under study. Of course, variability across studies is often frustrating on a personal level, particularly when we as individual researchers are unable to reproduce the same results as other researchers despite our best efforts, either because the results are not replicable or because of other factors relating to one's ability to perfectly reproduce the methods of a past study. However, another possibility is that what may seem variable at first may in fact be quite systematic when reframed theoretically or in the light of new evidence.

Byers-Heinlein's Commentary may be the most relevant to the current metasituation faced by researchers who are now intellectually challenged by the interplay between bilingualism and executive control, even though her Commentary has nothing to do specifically with the topic of executive control. Rather, Byers-Heinlein reports on "two examples of reasonable, justifiable, and prudent experimental designs that initially led to misleading conclusions about the effects of bilingualism on development," both within the domain of early speech processing. We will not recount her Commentary here, but suffice it to say that Byers-Heinlein makes two very important interrelated points that we should keep in mind when we try to sift through different studies that have variable outcomes, some of which may be studies of our own, or studies that we are serving as reviewers or editors for in high profile journals. The first is that the same task may not be measuring the same thing in bilinguals versus monolinguals, and the second is that identical stimulus materials may not be functionally equivalent for bilinguals versus monolinguals. Again, we refer you to her Commentary for the specific details within the domain of bilingual speech development that led to her these conclusions; however, like William James' Shakespeare-inspired observations about creativity and adaptability within science, Byers-Heinlein provides us with a compelling parable that is relevant to discussions about bilingualism and executive control, the bottom line of which is that there is great value in working in good faith to discover why

different results are obtained across different experimental circumstances, as this kind of systematic understanding can only lead to both expected and unexpected advances in knowledge attainment.

Of course, scientists are human, and thus occasionally personal feelings take center stage. For example, in his Commentary, Morton expresses concerns about the current popularity of bilingualism research. He gives an impression of frustration with prior work suggesting that bilingualism confers executive control advantages, which have been "trumpeted" in "headlines around the world" and which he finds to be an "insufferable mixture of excessive claims and weak evidence." Although any individual researcher is entitled to his or her own opinions, potentially polarizing reactions are unfortunate, because ultimately, regardless of the outcome or interpretation of prior studies, the recent surge of bilingualism in the news and elsewhere has only elevated the study of language into the sphere of public awareness. This can only be good news for those of us who spend our professional careers researching language and offering health and educational services to people who experience language disorders and for those of us who experience the impact of having a language disorder and need evidence-based advice. It is also worth noting that bilinguals have historically been woefully underserved with respect to the provision of health and educational services, particularly given that it has only recently become mainstream to see bilingualism as not harmful (and many people in different corners of the world still have their doubts on this point).

Fortunately for us, Morton gives the impression of being less frustrated by our paper, which he describes as "a welcome departure from this impossibly stale status quo" and a "sunny review." He then adds, "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." We are certainly appreciative of these positive sentiments (although we would not necessarily describe our recommendations for future research as "foreboding" as he later does) and for his and all the other commentators' time and energy in responding to our original paper. However, it is abundantly clear from existing and evolving work in the field, and from comments of the other authors here, that we are not alone in staking out nuanced positions with respect to bilingualism and executive control.

Rather, perhaps our greatest contribution in our Keynote Article is to create a forum where many of the issues of relevance to the study of bilingualism can be aired and different perspectives gathered in one place. It is certainly our hope that this endeavor collectively acts as a forward-looking conversation starter that may assist bilingualism researchers in breaking down a wall or two where relevant and that may ultimately help move the field in a positive direction. With or without this article, move it will, because such is the nature of all scientific endeavors. Thus, to conclude, we again express our great enthusiasm about where the study of bilingualism, executive control, and neuroplasticity over the life span will go in the coming years, because it will certainly tell us about a great deal more than the local limits of each subdomain individually. We also take this opportunity to thank the commentators for their contributions to this endeavor and look forward to continuing the discussion as we move forward as a field potentially in unanticipated and exciting ways.