Bilingualism and Executive Function: An Interdisciplinary Approach

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Discussion of Panel 1: Kroll & Friedman
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I'd like to thank our two speakers for setting the stage for this workshop so well. Judith Kroll demonstrated the richness of the field of bilingualism – independent of its implications for executive function as well as considering those implications. Naomi Friedman demonstrated the richness of the field of executive function – independent of its implications for research in bilingualism, as well as considering those implications. Irina Sekerina's and my goal for this workshop – and NSF's goal – is for these two fields, which have so much to offer each other, to influence each other more. This is a wonderful beginning.

In my discussion today I'll raise questions for Judith and Naomi about some of the themes they presented. The remainder of my discussion will focus a) on interpretations of neuroscience data, and b) on comparisons of bilingualism with other kinds of cognitive challenging experiences.

Three questions for Naomi Friedman

1. Friedman notes that executive functions do not map neatly onto tasks measuring executive function. Every task is "impure", meaning that it tests different aspects of executive function to different degrees, and also tests cognitive processes that are not aspects of executive function, such as visual perception. Tasks that superficially look as if they should correlate do not necessarily correlate. In general, we do not understand enough about what the tasks tap. One question I have for Naomi is whether the latent variable analysis that she provides oversimplifies that relations among the variables and the tasks. For example, is it really possible to label most tasks as composed of a general factor plus only one of the two specific factors? And what about the cognitive processes that aren't part of executive function?

   A couple of specific comparisons will indicate what I'm after. Consider the Simon and flanker tasks from the point of view of their similarities and differences. Both tasks require the participant sometimes to use one finger and sometimes a different finger in responding. Both tasks have congruent and incongruent trials. The similarity stops there.

   The stimuli are different – rectangles vs arrows; one is non-directional and the other is inherently directional. More important, the incongruency in the flanker has a different source from the incongruency in the Simon. In the flanker, incongruency is due to a conflict between the direction of the target arrow in the *focus* of attention and the direction of the arrows in the *periphery* of attention. The congruent item is focal and the incongruent surround is peripheral (Guiney & Machado, 2013; Valian 2015a). The flanker requires one to ignore the arrows surrounding the target.

In the Simon task, incongruency is due to a lack of alignment between the spatial position of the stimulus and the spatial position of the key to be pressed. There is a single stimulus and it is always in the participant's focal attention whether it is congruent or incongruent. Another difference is that the Simon requires inhibition of a prepotent response whenever the stimulus is on the other side of the screen from the keyboard response, while the flanker does not (Poarch & Van Hell, 2012).

Although the task differences might seem minor, they have consequences. Average reaction time, independent of congruency condition, correlates well between the Simon and flanker tasks: people who are fast overall on the Simon task are fast overall on the flanker task. But the cost of incongruency does not correlate well between the two tasks. Individuals who show a low cost of incongruency on the Simon do not show a similarly low cost on the flanker. Several people have found that (Paap & Greenberg, 2013; Humphrey & Valian, 2012; Poarch & Van Hell, personal communication, 30 Dec 2012).

More surprisingly, even the verbal and numerical versions of the Stroop do not correlate significantly with each other, as Duñabeitia and colleagues found (Duñabeitia, Hernández, Antón, Macizo, Estévez, Fuentes & Carreiras, 2014). Since the tasks are conceptually extremely similar, it is likely that cognitive processes outside of executive function are responsible for the differences in responding. As a result, it is difficult to know whether differences, when they are found, are due to the aspects of a task that measure executive function or to aspects that measure other cognitive processes.

2. The idea of unity and diversity in executive function raises the question of whether the common factor should be labeled inhibition – since the common factor maps completely onto what used to be called inhibition. Friedman argues that it should be seen as a common factor, since it is related to all the tasks that have been analyzed. The gloss for that common factor is "active maintenance of goals (in working memory) and the use of these goals to bias ongoing processing". That is a much more general conceptualization than 'inhibition', but I wonder whether any experimental data specifically bear on that.

3. The third question is what we should expect from bilingualism on executive function tasks. The tantalizing suggestion that Friedman left us with was: bilinguals – at least, life-long balanced bilinguals – are very highly practiced. The exquisite control that Kroll mentions that bilinguals have over which of their languages to use at any given time seems a highly practiced, almost automatic, skill. Should we expect that the skill involved in suppressing the irrelevant language, and switching to it when it's appropriate to do so, will be related to performance on executive function tasks, given that those EF tasks are novel? If we should not expect transfer to executive function tasks, are we wasting our time by examining bilinguals' performance on them?

   Alternatively, maybe language processing is always more of a controlled process for bilinguals compared to monolinguals. For example, bilinguals take longer to

understand and produce words than monolinguals do. Further, bilinguals are less fluent than monolinguals when they are asked to list, say, as many animals as they can, though that appears to be due to bilinguals' smaller vocabulary (Bialystok, 2009). My third question, then, is whether we are looking in the right place for cognitive consequences of bilingualism.

Three questions for Judith Kroll

1. Kroll notes that both of a bilingual's languages are always active. One language has an effect on the other in vocabulary tasks and in grammar tasks. I have several questions about these important findings. First, what are the limits? I know French – kinda sorta – but I only use it on the all too rare occasions when I am in a French-speaking country, or reading a menu in a French restaurant, or showing off my knowledge of French phrases. Is French active when I'm speaking English? Second, since bilinguals who actively use their two languages seem highly practiced at switching, is this an example of a highly automatized procedure that ends up having few implications for cognition? Bilingual babies – who cannot be said to be inhibiting or updating or switching in the usual sense of those terms – nevertheless appear to show enhanced executive functions. What could the underlying mechanism be?

2. The second theme is that the juggling of two or more languages has neural consequences and, often, but not always, behavioral consequences. This dissociation between the brain and behavior crops up in various areas. My question is about the significance of this fact. One possibility is that neuroscience work tells us less that we might have thought about behavior, and I'll argue for that later in this discussion. Another possibility is that neuroscience work gives us a more sensitive indicator than behavior does of underlying cognitive processes; I think that is an underlying assumption of much work in cognitive neuroscience. My question is how we can decide between those two possibilities on any given occasion.

3. The third theme is that bilingualism may have its parallels in non-linguistic domains, suggesting that the underlying mechanisms are domain-general and reflect coordination of control mechanisms rather than simple main effects. The fact that both bilingualism and non-linguistic experiences affect executive functions should have implications for teasing out which aspects of experience matter. What might those aspects be?

Neuroscience data

I mentioned earlier the fact that neural differences are not always accompanied by behavioral differences. That is true in the domain of cognitive sex differences as well. The neuroendocrinologist Geert De Vries noted in 2004 that the functional significance of most sex differences in the brain was not known. He writes, "We are heavily invested in the idea that sex differences in brain structure cause sex differences in behavior. We rarely consider the possibility that sex differences in brain structure may also *prevent* [my emphasis] sex differences in overt functions and behavior, by compensating for sex differences in physiological conditions, such as gonadal hormone levels that may generate undesirable sex differences if left unchecked."

De Vries (2004) gives the following extended example concerning prairie voles. Both parents take care of the pups and there are no qualitative differences in parental behavior between males and females, except that females can nurse their young. But the underlying neural-hormonal mechanisms are different. Virgin female prairie voles are unresponsive to pups or even infanticidal, but exposure to gonadal steroids during pregnancy makes them behave like good parents, not just nursing, but being parental overall.

In males, gonadal steroids don't play a role. Instead, parental behavior depends on activation of arginine vasopressin (AVP) receptors in the lateral septum. Female voles have very few AVP fibers in their septum, whereas males have a dense AVP fiber network. Therefore, de Vries suggests, male prairie voles, who do not become pregnant and therefore are never exposed to the hormonal changes associated with pregnancy, may have compensated for that absence by using the male-biased AVP innervation to stimulate parental responsiveness. The underlying neural-hormonal mechanism is different in female and male voles, but the behaviors are the same.

Similarly, both male and female voles exhibit pair bonding, but the neural substrate is different. Intra-cerebro-ventricular injections of an AVP antagonist blocks pair bonding in males but does not affect pair bonding in females. The opposite is true for an oxytocin antagonist; that affects females but not males. Again, the neural-hormonal substrate is different, but the behavior is the same.

In the rat, both males and females are equally good at social recognition memory but, again, the underlying mediating mechanism differs. When rats are treated with an AVP antagonist, males, but not females, show impairment in social recognition memory. The AVP antagonist induces induces a sex difference that didn't exist before.

In humans, strokes in the same brain regions can have different outcomes in men and women, and functional imaging studies suggest that men and women use cortical regions differently even for functions that do not differ themselves.

De Vries (2004) suggests that differences in brain-behavior correlations exist because some behaviors need to be carried out equally well by both sexes. Neural-hormonal differences that arise as part of sexual dimorphism need to be counterbalanced by mechanisms that will allow equally good performance by both sexes.

Although de Vries's (2004) hypothesis about compensation is directed to sex differences, it applies equally to any two groups. Consider how one would apply the hypothesis to mono- and bilinguals. Both groups need to be able to carry out executive functions. One group may do it with one set of neural pathways and another group may do it with a different set. How each group's behavior is mediated by different neural circuitry is of great interest, but once we entertain the possibility that two groups can accomplish the same task by different means, we no longer have a basis for making clear predictions about their behavior.

Two related issues concerning neuroscience data are what Poeppel (2012) calls the *maps problem* and the *mapping problem*. The *maps problem* between brain and behavior is that spatial and temporal localizations in the brain provide correlations with behavior but they do not provide explanations of behavior. In the case of bilingualism, those correlations are inconsistent (Li, Legault, & Litcofsky, 2014). Even if it will someday be possible to perfectly localize function and identify processing streams, we still will not have an explanation of the mechanism. We will still have a correlation.

Poeppel says, "… systematic relations consistently occur between brain areas and some functions that reappear across studies, but we have no *explanation*, no sense of which properties of neuronal circuits that we understand account for the execution of function". He goes on to say, "We [need to] decompose the cognitive tasks under investigation into computational primitives that can be related to local brain structure and function, in a sense instrumentalizing the computational theory of mind." The point I want to emphasize is that a *cognitive* explanation is not the same as a *neural* explanation, especially if the behaviors at issue are identical. A cognitive explanation will account for the fact that different neural circuits subserve the same cognitive performance. A cognitive explanation will be independent of the neural differences.

That gets us to the *mapping problem* (Poeppel, 2012). We lack linking hypotheses to connect, in this case, bilingual language processing with neural processing. The vocabulary of the two domains is different. The vocabulary of bilingual language processing includes terms like "word retrieval" and "code-switching"; the vocabulary of the brain includes terms like "increased firing" and "network patterns". Those are incommensurate and require a theory that will link them (Poeppel).

Thus, although studies of the brain contribute to our understanding of bilingualism, they can lead to an illusion of greater understanding than we in fact have. We know that brains can operate differently to produce the same result, just as calculators can use different internal logics to yield the same answers to arithmetic problems. We are interested in something fundamental about mental arithmetic that is independent of the particular logical system governing the operation of the calculator. In bilingualism, we are interested in something fundamental about cognition that is independent of the brain.

Bilingualism, variability, and comparisons with other cognitive challenging experiences

Kroll alluded to variability in results with bilinguals. I'd like to expand on that. I think the variability is considerable in all populations that have been studied so far – children, young adults, older adults. There have been a number of different hypotheses about the sources of that variability. Here are the two logical possibilities that I see (and discuss at length in Valian, 2015a, and more briefly in Valian 2015b).

1. The first possibility is that there is *no* cognitive benefit of bilingualism. In experiments that have found a benefit, the effect would instead be attributed to the accidentally larger number of other positive factors (such as high socioeconomic status), that bilinguals have in that particular sample, or due to the correlation of bilingualism with some other active property that is difficult to separate from bilingualism (such as biculturalism).

2. The second possibility is that there *is* a benefit of bilingualism for executive function, but that the benefit competes with other benefits. Others have frequently commented that bilingualism is but one of many different cognitively challenging activities that might contribute to superior executive function (e.g., Craik, Bialystok, & Freedman, 2010). Depending on the composition of each group in any given experiment, the other benefits may be more plentiful in the monolingual than bilingual group (or sufficiently plentiful in both groups), so that the benefits of bilingualism are invisible. This is the possibility that I favor. There is a benefit, but it competes with other known benefits.

Three considerations lead me to favor the second possibility. *First*, executive function has different components (Miyake & Friedman, 2012). Depending on the tasks we use to measure executive function, one or another component may be primary. We do not have a clear enough theory yet to isolate what components of executive function should be most affected by bilingualism. As Friedman noted, tasks are impure: tasks that tap executive function also inevitably tap other cognitive components that are not part of executive function, such as visual perception. Without an executive function theory, a task theory, and a bilingualism theory, predictions will be very difficult.

Second, we already know, as Kroll suggested, that a range of experiences is associated with superior executive function, delay of dementia, or both. In addition to language status (mono- or bilingual), factors include socioeconomic status; immigrant status; extent of exercise; presence of musical training; experience with action video games; education level; time spent in leisure activities; and, possibly, personality variables (Valian, 2015a). There are no doubt still other factors yet to be systematically investigated. Since managing two or more languages is a cognitive challenge, it would be very surprising if bilingualism were not among the challenging factors that contribute to superior executive function.

Third, in all cases, whether looking at language status or other variables, effects are inconsistent, but generally – not always, but generally – positive when they do occur. No variable seems to trump any other variable. Once an individual has a number of challenging experiences it will be difficult to find a benefit of any of them individually.

In other words, individuals vary in the types of cognitive challenges they have. To digress for a moment: I have thought that New Yorkers are particularly challenged, with, for example, subways that arbitrarily skip stops and have different schedules on weekends, sidewalks that require adroitness to manoeuvre, and people who exhibit a wide range of behaviors that must be accommodated or ignored. But maybe NY is *too* challenging, given the range of resources available to people. People live longer in small towns in Colorado and in the suburbs of Virginia and California than they do in NY. The percentage of seniors with Alzheimer's is 10% in Colorado, 11% in California and Virginia, and 13% in NY. Being challenged, but lacking the necessary resources to cope with the challenges, may do more harm than good. Having many years of education, sufficient money to hire help when needed, and wide experience with different cultures and subcultures, living in a safe neighborhood, being of a gender and ethnicity that is favorably regarded, having a supportive social network, being healthy – those are all characteristics that can make challenges enriching rather than overwhelming.

It is the combination of the three sets of facts that accounts for the inconsistencies in findings. We know relatively little about executive function, though thanks to work like Friedman's, we are getting a handle on it; we know relatively little about the tasks that are used to measure its components; and we know relatively little about the range of cognitively enriching experiences that exist. In any given study, participants have different sets of experiences, many of which cannot be controlled for, or are unknown, but on a par in their benefits with bilingualism. Further, those experiences interact with the tasks in unknown ways.

The range of executive functions, the range of tasks measuring executive function, and the range of experiences that are associated with superior executive function raise an important question about mechanism. Is there a single mechanism or several different mechanisms

underlying superior executive function? If executive function is manifold, if different tasks measure different aspects of it, and if different experiences give rise to better or worse performance on those tasks, it seems likely that there are several different underlying mechanisms. If that is correct, future research should identify the different mechanisms rather than search for a single mechanism.

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