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SHORT REPORT

Are bilingual children better at ignoring perceptually misleading information? A novel test

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Abstract

Does speaking more than one language help a child perform better on certain types of cognitive tasks? One possibility is that bilingualism confers either specific or general cognitive advantages on tasks that require selective attention to one dimension over another (e.g. Bialystok, 2001; Hilchey & Klein, 2011). Other studies have looked for such an advantage but found none (e.g. Morton & Harper, 2007; Paap & Greenberg, 2013). The present study compared monolingual and bilingual children's performance on a numerical discrimination task, which required children to ignore area and attend to number. Ninety-two children, ages 3 to 6 years, were asked which of two arrays of dots had 'more dots'. Half of the trials were congruent, where the numerically greater array was also larger in total area, and half were incongruent, where the numerically greater array was smaller in total area. All children performed better on congruent than on incongruent trials. Older children were more successful than younger children at ignoring area in favor of number. Bilingual children did not perform differently from monolingual children either in number discrimination itself (i.e. identifying which array had more dots) or at selectively attending to number. The present study thus finds no evidence of a bilingual advantage on this task for children of this age.

Introduction

The impact of bilingualism on children's cognitive development has been an active yet controversial field of study for the better part of a century. The earliest scientific studies on this question tended to frame it in terms of general intelligence. In fact, many early studies in the first half of the twentieth century reported that bilingualism posed a threat to intellectual development (see Darcy, 1953; Diaz, 1983 for reviews). However, some later studies (e.g. Peal & Lambert, 1962) reported the opposite – better performance by bilingual children on measures of both verbal and nonverbal intelligence.

Today the question is framed not in terms of general intelligence, but in terms of more specific cognitive skills. For example, a recent meta-analysis investigating the cognitive correlates of bilingualism found that bilingualism is associated with increased attentional control, metalinguistic awareness, problem solving, symbolic and abstract representational abilities, and working memory (Adesope, Lavin, Thompson & Ungerleider, 2010). There

has been particular interest in the possibility that bilingualism may confer an advantage in suppressing misleading or irrelevant information, a key component of executive function (see Bialystok, 2001, 2009; Carlson & Meltzoff, 2008; Hilchey & Klein, 2011, for reviews). *Executive function* (EF), also called *cognitive control* (Miller & Cohen, 2001), is a broad term for several interrelated higher-order cognitive processes, including inhibition, working memory, and cognitive flexibility. These processes assist in monitoring conflict and controlling attention (Diamond, 2006; Garon, Bryson & Smith, 2008; Miyake, Friedman, Emerson, Witzki, Howerter & Wager, 2000).

Nonlinguistic interference tasks have often been used to investigate the effects of bilingualism on EF. For example, on the Simon task (Simon, 1969), participants must ignore the position of a square presented on either side of a display, and attend only to its color, which corresponds to a left or right key press. Green squares, for instance, may correspond to a left key press and red squares to a right key press. On congruent trials, the square is

presented on the same side as its associated key press (e.g. green squares on the left). However, on incongruent trials, the square is presented on the conflicting side (e.g. green squares on the right).

Performance on interference tasks such as this one has often been measured in two ways, by calculating: (1) an *interference effect*, or the difference in performance between congruent and incongruent trials, and (2) *overall performance*, or superior performance on both congruent and incongruent trials (Costa, Hernández, Costa-Faidella & Sebastián-Gallés, 2009; Hilchey & Klein, 2011). Evidence of a bilingual advantage has been asserted when either one or both of these effects have been found, meaning when bilinguals showed a smaller interference effect and/or better overall performance (e.g. Bialystok, Craik, Klein & Viswanathan, 2004; Bialystok & Martin, 2004; Bialystok, Martin & Viswanathan, 2005; Costa, Hernández & Sebastián-Gallés, 2008; Martin-Rhee & Bialystok, 2008).

There are at least two competing proposals for how bilingualism might enhance executive function. One prominent view originating from Green (1998) and promoted by Bialystok (2001) is that an advanced inhibitory control mechanism enables bilinguals to coordinate the joint activation of their two languages through the suppression of irrelevant information (i.e. the language not being used). Thus, bilinguals gain extensive practice selectively attending to relevant stimuli while ignoring irrelevant stimuli, resulting in a *specific* advantage in inhibitory control (Bialystok, 2001; Green, 1998). If bilinguals have a specific advantage in inhibitory control, then they should perform better than monolinguals only on incongruent trials because this is when misleading information is present. Better performance on incongruent trials would result in a smaller interference effect on nonlinguistic interference tasks (Costa *et al.*, 2009; Hilchey & Klein, 2011).

However, in a recent meta-analysis of studies that have looked for a bilingual advantage on interference tasks, Hilchey and Klein (2011) concluded that the smaller interference effect among bilinguals was scattered or nonexistent. Yet, there was a consistent and robust effect of better overall performance, casting doubt on the theory that a specific advantage in inhibitory control is responsible for bilinguals' better performance (Hilchey & Klein, 2011). An alternative view, most formally put forth by Hilchey and Klein (2011) and supported by recent research (e.g. Costa *et al.*, 2009; Kovács & Mehler, 2009), is that an advanced conflict-monitoring system allows bilinguals to adjust the level of executive functioning necessary to resolve a conflict between two competing, jointly activated representations (i.e. their two languages) to ensure an appropriate response.

Therefore, bilinguals must selectively construct and access representations for each language, and continuously monitor and control the appropriate language during communicative interactions. This need to constantly manage two languages produces a domain-general executive functioning advantage that should be evident on a variety of cognitive tasks. If bilinguals have a general advantage in executive functioning, then they should perform better on both congruent and incongruent trials, resulting in better overall performance on these tasks (Hilchey & Klein, 2011).

Nonetheless, some studies have failed to find evidence for any bilingual advantage at all (either general or specific). For instance, Morton and Harper (2007) found that bilingual and monolingual children (matched on ethnicity and socioeconomic status) performed similarly on the Simon task. Likewise, Namazi and Thordardottir (2010) compared bilinguals and monolinguals on the Simon task, and found that children's working memory abilities, rather than their language status, determined their superior performance on the task. Furthermore, Paap and Greenberg (2013) conducted three studies comparing bilingual and monolingual adults on several nonlinguistic interference tasks, including the Simon and color-shape switching tasks (Studies 1–3), as well as the antisaccade (Study 1) and flanker (Study 3) tasks. Consistent with Hilchey and Klein's (2011) meta-analysis, Paap and Greenberg found no evidence of a smaller interference effect across any of their tasks. However, contrary to Hilchey and Klein, Paap and Greenberg also found no evidence of better overall performance in bilinguals than monolinguals (see Kousaie & Phillips, 2012; Humphrey & Valian, 2012, for related findings), and concluded that there is no clear evidence of bilingualism conferring other cognitive advantages.

One type of interference task that has not yet been used to explore bilingualism's effects is a numerical discrimination task. Bialystok and Codd (1997) did compare monolingual and bilingual preschoolers' understanding of cardinality (i.e. the idea that the last number used in a counting sequence tells the number of items in the whole set). In that study, children were shown pairs of block towers: one constructed from Lego blocks and the other constructed from Duplo blocks. These plastic blocks were identical except that the Lego blocks were half the size of the Duplo blocks. Children were asked to count the number of blocks in each tower to determine which tower had more. Bilinguals performed better on the task, which according to Bialystok and Codd was because they were better at ignoring the size of the blocks and attending to their number.

The task used in the present study differs from the one used by Bialystok and Codd (1997) in that it is

nonsymbolic (no number words or number symbols are used) and does not involve counting. In this task, children are shown a card with two arrays of dots. The child must decide which array has 'more dots'. This task is a standard one for assessing nonverbal numerical ability (e.g. Halberda & Feigenson, 2008; Halberda, Mazocco & Feigenson, 2008; Wagner & Johnson, 2011; Xu & Spelke, 2000). In recent years, performance on this task has also been linked to performance on standardized math tests and school math achievement (Halberda *et al.*, 2008; Halberda, Ly, Wilmer, Naiman & Germine, 2012; Libertus, Feigenson & Halberda, 2011).

Although this task is typically used to measure numerical estimation acuity, it has also been used in number/size congruency and Stroop-like interference paradigms (Cordes & Gelman, 2005; Gebuis, Kadosh, de Haan & Henik, 2009; Hurewitz, Gelman & Schnitzer, 2006). For example, Hurewitz *et al.* (2006) asked adults to judge which of two side-by-side dot arrays had more dots. All of the dots in any single array were the same size, but one array had larger dots than the other. On congruent trials, size was congruent with number (i.e. the array with larger dots contained more dots). The inverse occurred on incongruent trials (i.e. the array with smaller dots contained more). Participants were significantly better at determining which array had more dots on congruent than incongruent trials, suggesting that dot size interfered with numerical judgments. A similar finding was reported by Halberda and Feigenson (2008) with 3- to 5-year-old children.

These results suggest that people automatically extract a continuous quantity dimension (i.e. they can't help noticing the size of the individual dots), and that this dimension competes with number for attention. This makes the task a useful one for investigating possible effects of bilingualism on executive function in children.

In the present study, monolingual and bilingual children were presented with a standard, nonsymbolic, numerical discrimination task. We reasoned that any of three possible outcomes would be interesting. First, bilinguals might show a smaller interference effect than monolinguals, supporting the idea that bilingualism confers a specific advantage in inhibitory control (e.g. Bialystok, 2001; Green, 1998). Alternatively, bilinguals might perform better than monolinguals overall, supporting the idea that bilingualism confers a general executive functioning advantage on this type of task (e.g. Costa *et al.*, 2009; Hilchey & Klein, 2011). A third possibility was that we might find no differences in performance between bilinguals and monolinguals, either in terms of interference or overall (e.g. Morton & Harper, 2007; Paap & Greenberg, 2013). This result (which was in fact the result we found) provides no

support for the idea of a bilingual advantage in executive function, either general or specific.

Method

Participants

Participants included 92 children with a mean age of 4 years, 9 months ($SD = 10.2$ months, range = 3;0 to 6;5). All participants were recruited from private or university-affiliated preschools in southern California where English was the language of instruction. At the time of recruitment, parents filled out a demographic form including a question about household income (see Duncan & Petersen, 2001). Income in all households exceeded \$75,000 per year – the highest income category listed. Thus, no participants came from low-income households. Families received a prize (e.g. a small stuffed animal) when they signed up to participate in the study; no prizes were given at the time of testing.

Parental report was also used to estimate the percent of time the child was exposed to English and/or another language at home. How this information was collected depended on which of two larger studies the participant had originally been recruited for. Most participants (83%) were recruited as part of a larger study where parents were asked to list the family members and caregivers with whom the child interacted within a typical week (outside of preschool and not counting the time when the child was asleep), to indicate the number of hours the child spent with each person, and the language(s) spoken with the child. If the same person used English and another language with the child, one-half of the time was allotted to English and one-half to the other language. Based on parents' responses, the number of hours of exposure to a language other than English was divided by the total number of hours of exposure to English and/or another language, and then converted to a percentage to estimate the percent of time the child was exposed to a language other than English at home. The remaining participants were recruited as part of a different study where parents were asked questions regarding what language they spoke most often with their child and what language their child spoke most often with them. Responses were based on a 5-point Likert scale where the options were 'English only'; 'Mostly English'; 'Both languages about equally'; 'Mostly another language'; and 'Only another language'. These responses were converted to a percentage that reflected the percent of time a language other than English was used at home ('English only' was converted to 0%; 'Mostly English' to 25%; 'Both languages about

equally' to 50%; 'Mostly another language' to 75%; and 'Only another language' to 100%.

Of all participants, 32 children were only exposed to English at home ($M = 4;7$, $SD = 9.7$ months), 40 children were exposed to English and another language at home ($M = 4;9$, $SD = 10.4$ months), and 20 children were only exposed to a language other than English at home ($M = 4;9$, $SD = 10.8$ months). A total of 60 children were exposed to a language other than English: Chinese ($n = 24$), Mandarin ($n = 8$), Spanish ($n = 7$), Hindi ($n = 3$), Tamil ($n = 3$), Cantonese ($n = 2$), Farsi ($n = 2$), Japanese ($n = 2$), Korean ($n = 2$), Czech ($n = 1$), French ($n = 1$), Gujarati ($n = 1$), Hebrew ($n = 1$), Italian ($n = 1$), Tagalog ($n = 1$), and Vietnamese ($n = 1$).

Each child was tested once in English; testing occurred individually at the child's preschool. An additional 22 children were tested but not included in the data analysis: 12 (nine English only, three English & another language) were excluded because they did not complete the training trials, and 10 (six English only, four English & another language) were excluded for not performing significantly above chance (56%) on any of the test trials. Children excluded from the analysis did not differ in any other ways from those who were included.

Materials¹

For each trial of the numerical discrimination task, participants were shown two side-by-side arrays, containing between 20 and 100 black dots each, on a 21.5×12.5 cm laminated card. The arrays were generated in Matlab, printed on white paper, given a colored border (a different color was used for each ratio), and laminated. For the training trials, there were three blocks at an easy ratio of 1:3 ($= .33$). The 1:3 ratio was used as training because previous research has shown that even preverbal infants can discriminate numbers at that ratio (Feigenson, Dehaene & Spelke, 2004). For the test trials, there were nine blocks with ratios at 1:2 ($= .50$), 7:12 ($= .58$), 2:3 ($= .66$), 17:24 ($= .71$), 3:4 ($= .75$), 4:5 ($= .80$), 5:6 ($= .83$), 7:8 ($= .87$), and 9:10 ($= .90$). There were eight trials per block, and all trials within each block contained the same ratio.

To generate the exact number of dots used for a given trial, first a lower number was chosen uniformly from 20 to 100 by Matlab. Then, it checked to see if there was an exact match that was also between 20 and 100, for a given ratio. For example, to generate a trial with a ratio of 2:3, first the number 50 was randomly chosen from 20

to 100 for one array, and then 75 was chosen as an exact match to make the other array ($50:75 = 2:3$). However, if the number 53 was randomly chosen first, then this would require the other array to have either 35.3 dots ($35.3:53 = 2:3$) or 79.5 dots ($53:79.5 = 2:3$), which is impossible. Therefore, the program would randomly draw another number until an exact match was found. The sizes of the dots were generated in a similar way (generate a random size and see if there is an exact match, repeat as necessary). The dots within each array were all the same size. In each array, dots were clustered within a circle that was twice the summed area of all the dots. Therefore, the dots in the less numerous array did not seem more spread out, and the ratio of black to white space within the circle was always the same. The smaller array had to take up at least 20% of one side of the laminated card so that the dots could still be discriminated from each other.

There were two trial types (see Figure 1). On congruent trials, the numerically greater array was greater in total area (measured as the combined surface area of all of the dots in that array).² On incongruent trials, the numerically greater array was smaller in total area. The area ratio was the numeric ratio raised to an exponential power (1.5 for incongruent, 2.5 for congruent), in order to make it stand out clearly during the training trials. Half of the trials were congruent and half were incongruent. The side with the correct answer was counterbalanced, and the trial order was randomized within each block.

Procedure

The study began with training trials to ensure that participants understood the task (e.g. to make sure that children did not think they were supposed to pick the side with bigger dots or greater total area). The experimenter said to participants, 'Look at this card. This card has two sides. There are some dots on this side, and some dots on this side. (Experimenter points to each side.) You need to point to the side that has *more* dots. Which side has *more* dots?' If participants picked the wrong side on the training trials, the experimenter explained why it was wrong to help cue them into the task-relevant dimension of number rather than area. For example, if participants chose the side with bigger dots when there were more dots on the side with smaller dots, the experimenter said: 'Well these dots are bigger, but

¹ Our thanks to Jessica Sullivan and David Barner for sharing a version of the task.

² Due to simple constraints of geometry, congruent trials were also congruent with respect to individual dot size, summed perimeter (the combined perimeter of all of the dots in an array), and incongruent with respect to density (the number of dots per unit area in the smallest possible circle around all of the dots in an array).

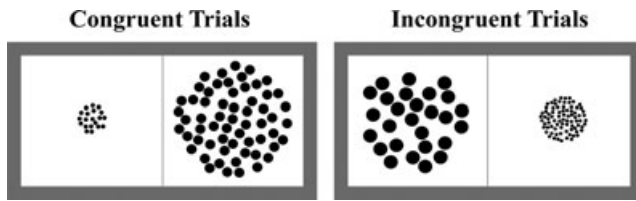


Figure 1 An example of the difference between congruent and incongruent trial types using training cards at the 1:3 ratio.

this side has *more* dots. They're smaller, but there's *more* of them.' To ensure that participants were not guessing, they had to answer eight training trials correct in a row before moving on to the test trials. (If necessary, trials were repeated in a cycle.) On the test trials, the experimenter asked, 'Which side has more dots?' Feedback was given after every trial (e.g. 'That's right – this side has more' or 'Uh oh, this side has more dots, you see'); however, participants were no longer told why their response was incorrect. Trials were presented too rapidly for children to count the dots, and no children were observed attempting to count.

Data analysis

Before conducting the main analysis, each child's overall performance and interference effect were calculated. Overall performance was computed by averaging proportion correct on both congruent and incongruent trials. The interference effect was calculated by taking the difference in proportion correct between congruent and incongruent trials. However, adjustments needed to be made to the calculation of the interference effect because participants were tested on nine blocks of trials that each contained a progressively harder numeric ratio. Therefore, at some point, the blocks became so hard that children were no longer performing above chance (56%). This increase in difficulty would mask any interference effect, as chance performance might reflect difficulties discriminating at that ratio. Hence, an individual cutoff point was determined for each child by calculating the proportion correct on every block to establish at what ratio the trials became too difficult for that particular child. Then, only the blocks in which the child performed above chance were used to analyze the interference effect.

For the main analysis, age and percent of time a child was exposed to a language other than English at home were used as continuous variables. This allowed us to place children on a continuum of 0–100% exposure to a language other than English at home. On the extreme left of the continuum were children with sole exposure to English at home, in the middle were children with relatively equal exposure to English and another language

at home, and on the extreme right were children with sole exposure to a language other than English at home.

However, since all of the children attended English-speaking preschools, it is not clear which children should be considered the 'most bilingual'. Thus, we approached the analysis using two different assumptions: (1) children with sole exposure to a language other than English at home are the most bilingual, and (2) children with relatively equal exposure to English and another language at home are the most bilingual. The logic behind these two assumptions is that children are more likely to spend time at their English-speaking preschools when they are older (e.g. 5 years old) than when they are younger (e.g. 3 years old). Therefore, according to the first assumption, at older ages, children with sole exposure to a language other than English at home and exposure to English at school might arguably be 'more bilingual' than children with relatively equal exposure to English and another language at home and exposure to English at school. To examine this assumption, a linear fit was used to determine whether children on the extreme right of the continuum would have better overall performance and/or a smaller interference effect than children on the rest of the continuum. In contrast, according to the second assumption, at earlier ages, children with relatively equal exposure to English and another language at home might be more bilingual than children with sole exposure to a language other than English at home. To examine this assumption, a quadratic fit was used to determine whether children in the middle of the continuum would have better overall performance and/or a smaller interference effect than children on the two extremes.

Finally, in order to make our results more comparable with extant literature (most of which treats bilingualism as a categorical variable), in a secondary analysis participants were divided into the following three language groups: (1) children who were only exposed to English at home, (2) children who were exposed to English and another language at home between 40% and 60% of the time (i.e. relatively equal exposure to two languages), and (3) children who were only exposed to a non-English language at home. This excluded 25 children who did not fit into these categories. Children were then split into two groups based on median age: younger children (less than 4 years, 7 months) and older children. Using these categories, we analyzed the effects of age and language group, as well as an interaction between them.

Results

The average overall performance for all children was .695 ($SD = .078$), meaning that children correctly chose the side

Table 1 Linear and quadratic regression analyses

Predictor	Overall performance			Interference effect		
	Model 1 (Linear)	Model 2 (Linear)	Model 3 (Quadratic)	Model 4 (Linear)	Model 5 (Linear)	Model 6 (Quadratic)
Constant	.4539*** (.0384)	.6828*** (.0116)	.6753*** (.0130)	.4979*** (.1359)	.1020** (.0361)	.1144** (.0408)
Age	.0510*** (.0080)			-.0863** (.0283)		
Percent exposure		.0003 (.0002)	.0012 (.0008)		-.0003 (.0006)	-.0018 (.0024)
Percent exposure ²			.0000 (.0000)			.0000 (.0000)
R^2	.311	.022	.039	.094	.002	.007
F	40.613***	2.059	1.814	9.288**	.210	.324

Note: Unstandardized coefficients. Standard errors in parentheses. ** $p < .01$; *** $p < .001$.

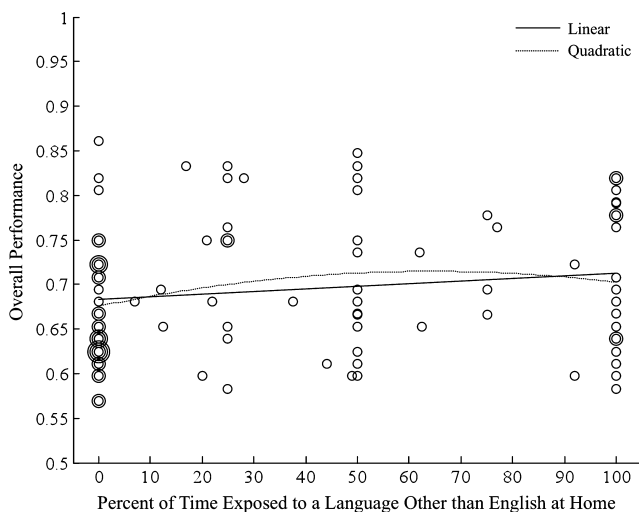


Figure 2 Percent of time exposed to a language other than English at home was not a significant predictor of overall performance (i.e. averaging proportion correct on both congruent and incongruent trials) using either a linear or quadratic fit. Note that additional concentric markers were used to denote when data overlapped.

with more dots 69.5% of the time. Proportion correct was significantly higher on congruent ($M = .794$, $SD = .131$) than incongruent trials ($M = .596$, $SD = .171$), $t(182) = 8.827$, $p < .001$. Calculating an individual cutoff point removed a mean of 4.3 blocks ($SD = 2.3$ blocks). The average interference effect for all children was .090 ($SD = .240$).

Four simple linear regressions and two quadratic regressions were run for the main analysis (see Table 1). There was a significant linear effect of age on overall performance, $R^2(90) = .311$, $p < .001$, with older children performing better than younger children. In contrast, there was no significant linear effect of percent of time

exposed to a language other than English at home on overall performance, $R^2(90) = .022$, $p = .155$, nor a significant quadratic effect, $R^2(90) = .039$, $p = .169$ (see Figure 2). Age was a significant linear predictor of the interference effect, $R^2(90) = .094$, $p = .003$. However, percent of time exposed to a language other than English at home was not a significant linear predictor of the interference effect, $R^2(90) = .002$, $p = .648$, nor a significant quadratic predictor, $R^2(90) = .007$, $p = .724$ (see Figure 3).

A 2 (Median Age; Younger than 4 years, 7 months vs. Older) \times 3 (Language Group; English only exposure vs. Relatively equal exposure vs. Non-English only exposure) ANOVA was run for the secondary analysis. Table 2 shows the means and standard deviations for congruent trials, incongruent trials, overall performance, and the interference effect separated out by median age and language group. There was a main effect of age, with older children performing better, both in terms of overall performance, $F(1, 61) = 15.277$, $p < .001$, and the interference effect, $F(1, 61) = 5.687$, $p = .020$. However, there was no main effect of language group for either overall performance, $F(2, 61) = 1.814$, $p = .172$, or the interference effect, $F(2, 61) = .505$, $p = .606$. Furthermore, there was no interaction between age and language group for overall performance, $F(2, 61) = 1.809$, $p = .172$, or the interference effect, $F(2, 61) = .995$, $p = .376$. Thus, the secondary analysis produced the same findings as the main analysis: older children performed better than younger children, but there was no effect of bilingualism on performance.

Discussion

The goal of the present study was to use a novel task (the nonsymbolic numerical discrimination task) to look for

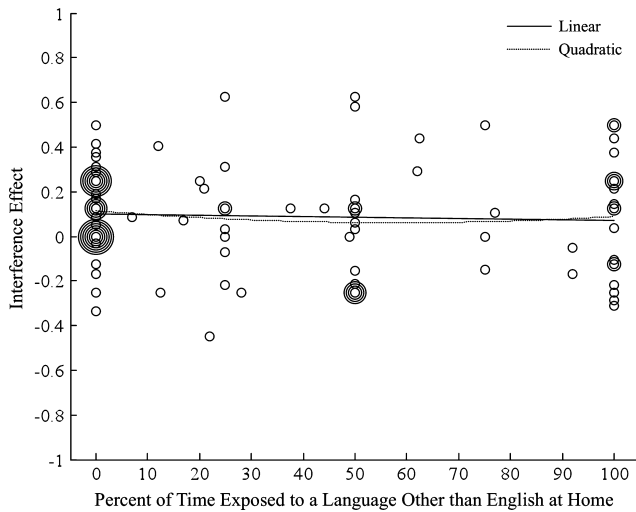


Figure 3 Percent of time exposed to a language other than English at home was not a significant linear or quadratic predictor of the interference effect (i.e. the difference in proportion correct between congruent and incongruent trials).

differences between bilinguals and monolinguals, either in terms of an interference effect or overall performance. We found no such differences.

There are several reasons to be confident that the task was developmentally appropriate and measured what it was supposed to measure. First, the numerical discrimination performance of children in this study was consistent with findings from other studies using the same task with children of this same age (e.g. Halberda & Feigenson, 2008). Second, the analysis of interference effects excluded blocks where the child’s performance dropped to chance levels. In other words, we only looked for interference at the ratios we knew the child could discriminate. Third, all children performed better on congruent than incongruent

trials, confirming that area functioned as a distractor dimension. And fourth, older children performed better than younger children (i.e. had both a smaller interference effect and better overall performance), confirming that the task was developmentally sensitive.

Thus, our results differ from studies that have reported a bilingual advantage specifically in inhibitory control (e.g. Bialystok, 2001; Green, 1998), or better performance by bilinguals on another measure of numerical cognitive development (Bialystok & Codd, 1997). Our results also fail to support the idea that bilinguals outperform monolinguals generally on such tasks (e.g. Costa *et al.*, 2009; Hilchey & Klein, 2011), as we found no effects of bilingualism on overall performance.

In short, our findings are most consistent with those previous studies that have argued against a bilingual advantage (either general or specific) in executive function (e.g. Humphrey & Valian, 2012; Kousaie & Phillips, 2012; Morton & Harper, 2007; Namazi & Thordardottir, 2010; Paap & Greenberg, 2013). The present study used exactly the sort of task on which bilinguals should outperform monolinguals, and had sufficient power to detect age-related differences in both the interference effect and overall performance. This suggests that any advantage conferred by bilingualism must be much smaller than the improvement that happens normally with development across this age range.

Of course it is possible that a bilingual advantage exists, but did not show up in this study. For example, our measure of performance (accuracy) might not have been sensitive enough to detect a bilingual advantage on this task. Previous research reporting a bilingual advantage typically used response times in addition to accuracy, because of ceiling effects on accuracy (Hilchey & Klein, 2011). Although response time data would be nice to have, it is notoriously difficult to collect meaningful

Table 2 Means (SDs) for secondary analysis

	English only exposure	Relatively equal exposure	Non-English only exposure		English only exposure	Relatively equal exposure	Non-English only exposure
	Congruent trials				Overall performance		
Younger	.766 (.099)	.790 (.197)	.806 (.162)	Younger	.658 (.056)	.645 (.038)	.665 (.077)
Older	.804 (.136)	.793 (.095)	.796 (.102)	Older	.685 (.082)	.742 (.093)	.755 (.061)
	Incongruent trials				Interference effect		
Younger	.550 (.109)	.500 (.252)	.525 (.154)	Younger	.137 (.218)	.152 (.352)	.179 (.256)
Older	.566 (.128)	.691 (.143)	.713 (.185)	Older	.102 (.168)	-.054 (.163)	-.001 (.259)

reaction times from children this young. However, since our participants responded correctly only 69.5% of the time, we are confident that ceiling effects were not a problem in the present study.

A related possibility is that the processing demands of our task were not high enough to elicit a bilingual advantage. Martin-Rhee and Bialystok (2008) found a bilingual advantage on the Simon task only when children were required to respond immediately after the display was presented. However, monolinguals and bilinguals performed equivalently when a delay occurred before responding, making the task easier. Although our task was presented rapidly and children did respond quickly, it is possible that a bilingual advantage would have appeared if task demands had been even higher. (Note, however, that task demands were high enough to produce interference effects – better performance on congruent than incongruent trials – for all children.)

In sum, we find no evidence for a bilingual advantage, either general or specific, on this task, in children of this age. This finding alone does not negate the many reports of a bilingual advantage on other tasks, but it does contribute to a growing body of evidence that calls the bilingual advantage into question. We hope that these findings will contribute to a more detailed understanding of how speaking one versus more than one language may affect – and also how it may not affect – other aspects of a child's cognitive development.

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