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Rolling off the Tongue into the Top-of-the-Head: Explaining Language Effects on Public Opinion

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Abstract Growing evidence shows that mass opinion varies by interview language, yet modest theory exists to explain this result. I propose a framework where language impacts survey response by making some political concepts more mentally accessible. I claim that concepts vary by how associated they are with certain languages, which means people are more likely to acquire a construct when it is tied to the tongue one speaks. Hence, recalling concepts from memory should be easier when the language a construct is linked to matches the tongue one interviews in, thereby intensifying people's opinions. I test my theory by manipulating the interview language in two U.S. surveys of English/Spanish bilingual Latino adults. I generally find that language influences the accessibility of concepts. For example, subjects report higher opinion levels for concepts that are tied more to their interview language, such as American identity among English interviewees. Subjects who interview in English are also less likely to refuse completing items measuring knowledge about U.S. politics, and more likely to answer them quickly. Items reflecting constructs that are highly labile (e.g. anti-Obama affect) or very crystallized (e.g., partisanship) do not display these patterns. I then rule out that language effects are mostly mediated by a heightened sense of anxiety, anger, pride or efficacy that emerges when bilingual subjects interview in one of their languages.

Keywords Language · Public opinion · Accessibility · Survey experiments · Latino politics

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Can language affect people's views about politics? Growing evidence suggests public opinion varies by interview language (cf. Pérez 2011; Wong et al. 2011; Garcia 2009; Lien et al. 2004). Most recently, Lee and Pérez (2014) reveal that Latinos' political attitudes differ significantly by whether they interview in English or Spanish. For example, Latinos who interview in English report nearly 10 % more knowledge about U.S. politics than those interviewing in Spanish. Patterns like these have been reproduced across surveys, topics, and populations (e.g., Wong et al. 2011; Garcia 2009; Welch et al. 1973). Yet political scientists still lack a coherent theory to explain them, as evidenced by calls for "greater research on the political psychology of … language effects (Pérez 2011, p. 436)."

Prior research designs are partly responsible for this situation. Correlational studies find robust associations between interview language and political opinions (e.g., Lee and Pérez 2014; Lien et al. 2004). But these analyses raise concerns about omitted variable bias since interview language is self-selected by respondents. Scholars often confront this threat by adjusting estimates of language effects for observed covariates (e.g., education, English fluency) (Garcia 2009; Welch et al. 1973). Yet this ignores unobserved individual differences and makes any results model dependent (Ho et al. 2007). One could randomly assign interview language to isolate its causal effect (Ross et al. 2002). Yet an experiment like this still demands a theory to explain how language impacts survey response.

Current theorizing offers limited guidance here. Most language-opinion studies are notable for their focus on what these patterns are not (Wong et al. 2011; Garcia 2009; Lien et al. 2004; Welch et al. 1973). For example, Lee and Pérez (2014, p. 20) argue that language-opinion gaps "cannot be *reduced to* a technical matter about omitted variable bias, measurement error, or status deference (Lee and Pérez 2014, p. 20)." But in ruling out explanations for these results, studies like these neglect to clarify how language actually shapes opinions. This omission is curious since the direction of language-opinion gaps varies across political topics (Lee and Pérez 2014; Wong et al. 2011; Garcia 2009), suggesting a possible link between the subject being broached and the tongue one speaks.

I propose here one mechanism for language-opinion effects. Psychologists find that people mentally recall material more easily when there is a match between how they learned it (*encoding*) and how they draw it from memory (*retrieval*) (Tulving and Thomson 1973). Indeed, a few scholars have shown that language can guide the recall of memories and information (Marian and Neisser 2000; Marian and Fausey 2006). For example, if asked to "name a statue of someone standing with a raised arm while looking into the distance," Mandarin-English bilinguals are more likely to say the *Statue of Liberty* if cued in English and the *Statue of Mao Zedong* if cued in Mandarin (Marian and Kaushanskaya 2007). These results, however, are from small experiments with student subjects ($N < 25$), raising doubts about the external validity of these findings for mass opinion (Sears 1986; McDermott 2011).

Seizing these insights, I reason that some political concepts are more strongly linked to certain tongues. This does not preclude speakers of varied languages from understanding the same political idea. Political constructs (e.g., democracy) are often imported from other languages (e.g., Greek) for use in one's own (e.g., English). But the degree of correspondence between a tongue and political concept

has two implications for the latter's accessibility. First, a stronger link between a language and a construct means a person is more likely to encounter and learn said concept in that tongue. Second, someone who encodes a concept in its associated language will more easily summon that construct from memory given the closer fit between the construct and the person's tongue. This yields the hypothesis that retrieving a political concept from memory is easier when the tongue used to summon it *matches* the tongue it is linked to (Tulving and Thomson 1973; Marian and Neisser 2000), thus intensifying one's opinions conditional on interview language.

I test my claims with two national survey experiments that randomly assigned the interview language to English/Spanish bilingual Latino adults. Post-treatment, subjects answered items tapping many constructs that are more connected to U.S. English, and some more tied to Latin American Spanish. These are concepts whose referent objects are peculiarly American, such as U.S. political knowledge (Delli Carpini and Keeter 1996), or distinctly Latin American, such as knowledge about the region's history. Subjects also completed items gauging other concepts theorized to be strongly tied to English or Spanish (e.g., American identity), and concepts where language effects should be harder to consistently unearth because a construct is highly labile (e.g., affect toward a political object) or very crystallized (e.g., partisanship). As such, this two-study design lets me test for language-opinion effects, set some boundaries for them, and assess their replicability.

I generally find that language impacts the accessibility of political concepts. For example, subjects assigned to interview in English report more knowledge about U.S. politics than those assigned to interview in Spanish. Those interviewing in English are also less likely to refuse answering knowledge items and quicker at completing them. I further observe subjects expressing higher opinion levels on constructs that match their interview language (e.g., American identity—English). These effects are mostly replicated across my studies, save for highly unstable (i.e., affect) or very stable (i.e., partisanship) constructs. I then show that language effects are not due to an elevated sense of anger, anxiety, pride, or efficacy that arises when bilingual subjects are assigned to interview in one of their tongues (Brader and Marcus 2013; Niemi et al. 1991). I discuss my results' implications for mass opinion in polyglot societies, generally, and U.S. Latino opinion, specifically.

Toward a Theory of Language Effects on Public Opinion

In a world where language *does not* affect opinions, what one thinks about politics depends on the question being asked and the considerations it evokes (Zaller and Feldman 1992; Tourangeau et al. 2000). Survey questions activate political concepts that are stored in long-term memory, which is associatively organized (Lodge and Taber 2013). This means concepts are linked to each other in a lattice-like network where stimulation of one energizes others via spreading activation (Collins and Loftus 1975). Once relevant concepts are aroused, they are drawn from long-term memory into working memory—the "top of the head"—where one shapes them into an opinion (Zaller 1992).

But in a world where language *does* impact opinions, I claim the accessibility of political concepts depends on the tongue one interviews in. Here, people's memory is still associatively organized, but I contend that language affects how concepts are *encoded to* and *retrieved from* memory. I relax the assumption that political concepts are linguistically neutral, arguing instead that they are more strongly associated with some tongues than others. Thus, people are more likely to encode a concept to memory if it matches the language they speak. This "fit" between the tongue a concept is tied to and the tongue one speaks makes it less cognitively demanding to integrate a new idea into one's long-term memory, which is already populated by other concepts sharing the same language.

Now, just because a concept is tied more to a specific language does not mean speakers of another tongue cannot learn it. Consider members of New Guinea's Dani tribe, who were able to learn the English set of color categories even though their own language had but two words for colors (Heider 1972; Rosch 1975). Still, a stronger link between a construct and a language will affect a concept's resonance in one's tongue. If the language a construct is tied to matches the language of other considerations in memory, then via spreading activation, it will be rehearsed more in that tongue. And concepts that are rehearsed regularly are more accessible (Fazio et al. 1986).

I also claim that language affects one's recall of political concepts. Specifically, I reason that concepts that are more accessible in a given language are more likely to be reported when the tongue that is used to retrieve them is the same. Psychologists find that people more easily summon information from memory when details that were present during learning are also available during retrieval (*encoding specificity principle*, Tulving and Thomson 1973). For example, divers who encoded word lists under water or in dry land more effectively recalled word lists learned on dry land when these were learned in that setting (Godden and Baddeley 1975; Grant et al. 1998).

Marian et al. have used these insights to show that recall of academic knowledge, memories, and tourist sites is facilitated when the tongue the material was acquired in matches the tongue in which it is retrieved (Marian and Neisser 2000; Marian and Kaushanskaya 2004; Marian and Fausey 2006). For instance, Mandarin-English bilinguals who were asked to name tourist spots were more likely to proffer Chinese sites (e.g., *Great Wall of China*) than U.S. sites (e.g., *Grand Canyon*) when cued in Mandarin (Marian and Kaushanskaya 2007). Yet these results have been mostly yielded via small lab experiments on college students, casting doubt on whether this process operates in surveys with less control over larger and more heterogeneous samples (Sears 1986; McDermott 2011).

Braiding these insights, I hypothesize that people will report higher opinion levels when their interview language matches the language that relevant political concepts are associated with (H1). Many opinions here hinge on a closer fit between the language of concepts evoked by survey questions and the language of interview, which increases the salience of relevant constructs at the "top of the head." I expect this accessibility to manifest itself in more intense opinions (Hong et al. 2000; Ross et al. 2002; Trafimow et al. 1997). I do not expect language to impact all opinions, however. In particular, I hypothesize that language effects will be harder to observe consistently on very crystallized constructs (H2a). For instance, partisanship is so stable that it should be impervious to language effects (Green et al. 2002; see also Krosnick and

Smith 1994). I also hypothesize that it is harder to yield consistent language effects on very labile concepts (H2b). For example, one may hold negative affect toward President Obama, yet these feelings will shift easily in light of his performance. These conditions set one lower and upper bound to my predicted language effects.

Is It Really Language?

I consider two alternative explanations for language effects on opinion. First, speaking a minority tongue can trigger strong emotions in people. Since minority languages often have lower status within societies (Garcia Bedolla 2005; Milroy 1982), people interviewing in one of them might become anxious or angry. It is not that majority tongues fail to induce emotions, as all political thinking is "hot" (Lodge and Taber 2013). Rather, people become *exceedingly* emotional when interviewing in a minority tongue, with downstream effects on opinions (Brader and Marcus 2013).

The mechanism here is stereotype threat, where making salient a stigmatized status stirs negative emotions (Steele et al. 2002). For example, anxiety directs attention to threats, yielding more careful information processing (Marcus et al. 2000; Brader 2005). In turn, anger yields less careful information processing as one modulates it by, e.g., acting swiftly against an agent held responsible for one's ire (Huddy et al. 2007). Of course, people can instead feel proud as they "show off" their language skill in an interview. Pride is multi-faceted, with "authentic" pride arising when the self makes attributions to internal, unstable, and *controllable* causes (e.g., I won because *I* practiced). Such pride affirms self-worth and heightens expressions of pro-social orientations, such as group identity (Tracy and Robins 2007).[1] Insofar as subjects feel one of these emotions, it will mediate language's impact on opinion in a way that reflects an emotion's action tendencies (H3).[2]

Second, language might shape opinion because people feel more efficacious if they speak a majority tongue (Niemi et al. 1991), spurring better concept recall (Prior and Lupia 2008). In the U.S., English is a pillar of national identity (King 2000), which privileges this tongue vis-à-vis non-English ones (Milroy 1982). Garcia Bedolla (2005) teaches us that such asymmetry leads non-English speakers to sense their language is stigmatized (cf. Crocker and Major 1989). As one of her respondents lamented: "I feel as if I am not in my place—as if I were in a strange place,…because I cannot express myself as I would like (Garcia Bedolla 2005, p. 69)." Thus, language-opinion gaps may arise because majority tongues induce greater political efficacy (H4a), which mediates language's impact on opinions. Alternatively, greater efficacy might be felt by those interviewing in a minority tongue. Here, efficacy is heightened as minority language speakers bolster their tongue's worth against its implied devaluation (Ellemers et al. 2002), with efficacy mediating language effects (H4b).

[1] In turn, "hubristic" pride results from attributions to internal, stable, *uncontrollable* causes (e.g., I won because I'm always great). I focus on "authentic" pride because respondents do control the concepts they retrieve and use to form opinions.

[2] Stereotype threat might also mediate the impact of language via depletion of people' executive functioning (Steele et al. 2002), a possibility that future research should further explore.

Research Design

I test my claims with two national surveys that randomized the language of interview among English–Spanish bilingual Latino adults. I fielded Study 1 in April 2013 and designed it to yield five hundred and thirty (530) subjects.[3] Using a difference-in-means, the power of my test in this sample equals .70, which falls a notch below the traditional .80 level (Cohen 1992). Consequently, I have a somewhat higher probability here of *failing* to reject the null of no language effect when it is actually false. Since a mean difference with Cohen's $d = .20$, $p < .10$, two-tailed requires $N = 472$, my slightly larger sample guards against any further loss of power due to missing data. In turn, Study 2 doubled my initial sample size in order to gauge the replicability of any results in the first experiment. Again using a difference-in-means, the power of my test in this sample is set at a higher .90 level. I fielded Study 2 in June 2015 and it yielded 1131 subjects, thus equipping me with enough power to unearth meaningful and highly reliable results, if they exist.[4] Since a mean difference with Cohen's $d = .20$, $p < .05$, two-tailed requires $N = 1054$, my larger sample, again, allows for some missing data.

Both studies were administered online by GfK (formerly Knowledge Networks), which maintains a nationally representative panel of Latino respondents. In each study, GfK identified bilinguals with four (4) items tapping self-reported ability to: (a) converse and (b) read in English and Spanish, respectively (see appendix for item wordings). Response options for these items were: (1) very well, (2) pretty well, (3) just a little, or (4) not at all. Bilinguals chose "1" on all items. These subjects were then randomly assigned to interview in English or Spanish.[5]

Random assignment to interview in English or Spanish kicked off each survey. Both treatments noted that during GfK enrollment, subjects had reported fluency in English and Spanish. Thus, they were now being asked to complete a study in one of these languages. Each treatment set a distinct language milieu by informing subjects that they were to answer all questions in a given tongue.[6] The English version of the treatment read as follows[7]:

> "When you enrolled to participate in surveys through our organization, you reported fluency in both English and Spanish. Given your ability in these two languages, we would like for you to complete the following survey in English.

[3] 37 % of subjects report usually completing GfK surveys in Spanish, while 63 % usually complete them in English.

[4] 31 % of subjects report usually completing GfK surveys in Spanish, while 69 % usually complete them in English.

[5] Subjects completed these items when they first enrolled to GfK's respondent panel. As might be expected (Abrajano and Alvarez 2010; Alba and Nee 2003), about half of bilinguals in both samples are foreign-born and Mexican origin, with the median education in both samples being "some college." See table A in the supporting information (SI) for more characteristics and any nuances between them across samples.

[6] Table B (SI) reports a non-linear regression predicting assignment to an experimental condition as a function of several covariates, suggesting my treatment was effectively randomized. Table C (SI) shows these covariates are balanced across my experimental cells. These results imply that my bilingual respondents are alike in all respects, save for the interview language treatment.

[7] A full comparison of both treatments can be found in table D (SI).

> All of the questions in this survey will be in English. Please read carefully and answer each question as an English speaker. Answer each question in English to the best of your ability. This is not a test. We are simply interested in your opinions as an English speaker.
>
> It is important that you complete the entire survey at one time. For this reason, we will record the times at which you start and finish answering the whole questionnaire.
>
> Click "Next" to continue with the survey."

This manipulation aims to test the accessibility of political concepts based on interview language. Accessibility can be operationalized in many ways. One can time people in milliseconds as they answer questions, with quicker response times reflecting more accessibility (Fazio et al. 1986; Fazio et al. 1995). One can also measure the proportion of information that people report, with more information indicating greater accessibility (Marian and Neisser 2000; Marian and Kaushanskaya 2007). Finally, one can operationalize accessibility via opinion reports, with higher opinion levels implying more accessibility (Hong et al. 2000; Ross et al. 2002; Trafimow et al. 1997). My studies mainly operationalize accessibility through opinion reports. Where feasible (e.g., political knowledge), I also analyze subjects' response times and refusal rates.[8]

Measures

Post-treatment, subjects answered items measuring ethnic and national identity. Many scholars deem language a marker of social categories, such as one's ethnicity or nation (Baker 2001; Cho 2000; Garcia Bedolla 2005; see also Schildkraut 2005; Wallace 2013). Shared language is often "the ideal vehicle to express the unique character of a social group, and to encourage common social ties on the basis of a common identity (Jaspal 2009, p. 17)." Insofar as identities are more linked to a specific language (e.g., American identity and English), interviewing in that tongue should make a relevant attachment more accessible, leading subjects to report more of it. In study 1, subjects answered two (2) items measuring Latino identity ("Being Latino is unimportant to me") and three (3) items gauging national origin identity ("I am pleased to be [Mexican]"). Subjects also completed two (2) items on American identity ("I see myself as a typical American") (Leach et al. 2008). I build scales of each identity with these items.[9] Study 2 fielded a similar set of items, the main difference being an increased number and mix of those tapping American

[8] The response times I analyze are indexed in seconds, rather than milliseconds, because GfK can only collect response times in seconds at this time. This will make it harder to uncover any accessibility effects by language.

[9] For Study 1: Latino identity ($\alpha = .54$); National origin identity ($\alpha = .86$); American identity ($\alpha = .81$). For Study 2: Latino identity ($\alpha = .54$); National origin identity ($\alpha = .87$); American identity ($\alpha = .74$).

identity and new items measuring knowledge about Latin American history (see the Appendix).[10]

Subjects also completed items gauging political knowledge. Elementary and secondary schoolchildren are often exposed to facts about politics and their political system (Delli Carpini and Keeter 1996). This makes it more likely that people will learn civic facts in the language of instruction. Thus, interviewing in English should make U.S. political facts more accessible. In Study 1, subjects answered eight (8) knowledge items (see appendix for wording), including "Whose responsibility is it to determine if a law is constitutional or not?" and "What job or political office is now held by Sonia Sotomayor?" I coded all eight items as 1-correct/0-inccorect and created a summated scale. I also built a subscale with three (3) Latino-focused items (e.g., "What job is now held by Sonia Sotomayor?"; "What office does Marco Rubio now hold?"). This subscale centers on people or events associated with English *and* Spanish. For example, Sotomayor is a Spanish name, yet Sonia's political role is linked to English. Such mixed stimuli should make it harder to uncover a reliable language effect.[11]

In turn, Study 2 fielded three (3) traditional U.S. knowledge items (e.g., "How long is the term of office for a U.S. Senator"). This reduction in knowledge items let me field two (2) new items I designed to gauge subject's knowledge about Latin American history, which I deem more closely tied to Spanish (e.g., "Who is known as an effective military and political leader that led various South American nations to independence from Spain?", answer: Simón Bolivar).[12]

Sandwiched between the identity and knowledge batteries in both studies, subjects completed single items on ideology and partisanship. After the knowledge batteries in each study, subjects then completed a single item on political efficacy, a feeling thermometer rating of Barack Obama, and self-reports of sensed anxiety, anger, and pride. The ideology, partisanship, and Obama rating allow me to test whether language effects are less likely on constructs that are highly labile (anti-Obama affect, ideology) or highly stable (partisanship). In turn, the self-efficacy item and self-reported emotions let me test whether language effects are mediated, rather than direct.[13]

[10] In both studies, most of the concepts that I measure are theorized to be linked to English. I did this by design, since U.S. polls typically ask about American constructs, which are tied to English. Still, as I describe in the text, I measure one Spanish-linked construct often asked about in U.S. academic surveys (i.e., national origin identity), and designed two items tapping knowledge about Latin American history in order to better test my hypotheses.

[11] Since my knowledge items are dichotomous, I report average tetrachoric correlations (rho) rather than alphas (α). For Study 1: Traditional U.S. knowledge (average rho = .58); Latino knowledge (average rho = .54).

[12] For Study 2: Traditional U.S. knowledge (average rho = .37); Latin American knowledge (average rho = .53).

[13] One might expect these items to precede my opinion measures. But asking people about their efficacy and emotions right after my language manipulation risks an artificial increase in the correlation between interview language and emotions, which works against finding mediation effects (e.g., the more variance in anxiety that interview language explains, the less variance in anxiety to explain opinions). Also, gauging opinions should have a trivial effect on measuring efficacy and emotion, since any sense of these has already occurred by the time opinions are reported.

The treatments and all survey questions in both studies were translated via a modified committee approach (Bailey et al. 2012). Three (3) translators independently translated these items from English to Spanish, with each translator speaking one of the main Spanish varieties spoken in the U.S. (e.g., Mexican, Puerto Rican). The translators reconciled any discrepancies and consensually agreed on the best translation of all items. This process yields more accurate translations than the effort of one translator. It also identifies and remedies personal idiosyncrasies and/or uneven skill in translator languages (cf. Bailey et al. 2012; Martínez et al. 2006; Harkness et al. 2004). Pre-tests before each study failed to detect any language issues in the final questionnaire, suggesting my items are equivalent across English and Spanish. Thus, any observed differences in opinion by interview language are unlikely due to uneven item performance across both of these languages.[14]

Results: Study 1

I hypothesize that language influences the accessibility of concepts in memory, thereby intensifying people's opinions. I expect these effects to most clearly emerge when people report their identification with groups and knowledge about U.S. politics. Given Study 1's experimental nature, I regress the following variables on one's randomly assigned interview language: (1) U.S. political knowledge, (2) American identity, (3) Latino identity, and (4) National origin identity.

I consider concepts 1 through 3 to be more associated with English than Spanish. People are more likely to learn American political facts in English (Delli Carpini and Keeter 1996). English is also deemed by many ordinary citizens and academics a key component of American identity (Garcia Bedolla 2005; Citrin et al. 2001). Finally, while some scholars argue that Latino identity is partly rooted in Spanish (Fraga et al. 2011; Garcia 2003), others remind us that Mexicans, Cubans, Puerto Ricans and other Latin American individuals are forged as "Latino" here in the U.S. (Davila 2001; Hattam 2007; Mora 2014)—the result of a 1977 U.S. Census Bureau directive designed to enumerate such individuals.[15] In contrast, national origin identity (e.g., *I identify as Puerto Rican*) is a concept whose referent object is uniquely Latin American, tying it more to Spanish than English.

Per (H1), I should observe those subjects assigned to interview in English reporting reliably higher levels of U.S. political knowledge, American identity, and Latino identity, but reliably lower levels of national origin identity, relative to those interviewing in Spanish. The top panel labeled "Study 1" in Table 1 reports my regression results.[16] All variables here have been recoded to run on a 0-1 interval,

[14] The point of these efforts, then, is not to create translations that are exact in length, word for word. Such a strategy is likely to yield translations of similar length, but with different and often grammatically incorrect meanings (Jacobson et al. 1960). Instead, the aim is to develop translated items that mean the same thing to different people (Pérez 2009).

[15] Indeed, scholars regularly find that Latino identity is more widespread among individuals who have deeper roots in the U.S. and are more conversant in English (Portes and Rumbaut 2006; Abrajano and Alvarez 2010), suggesting a relatively stronger tie between it and the English language.

[16] Although I use a regression approach to estimate my treatment effects, t-tests yield comparable results.

Table 1 Interview Language Intensifies Reported Opinions in Study 1 and Study 2

	Knowledge				Identities			Highly crystallized	Highly labile	Highly labile
	Political knowledge (all items)	Political knowledge (traditional)	Political knowledge (Latino)	Latin American knowledge	American identity	Latino identity	National origin identity	Partisanship (Republican)	Anti-Obama feelings	Ideology (Conservative)
Study 1										
English interview	.053**	.075**	.057	n/a	.010	.041*	−.034**	.022	.033*	.029*
	(.027)	(.030)	(.094)		(.024)	(.028)	(.020)	(.027)	(.026)	(.022)
Constant	.516**	.478**	–	n/a	.749**	.517**	.851**	.330**	.322**	.459**
	(.020)	(.021)			(.017)	(.020)	(.014)	(.020)	(.018)	(.016)
Study 2										
English interview	n/a	.252**	n/a	.059	.116**	.055**	.017	.003	−.031**	−.004
		(.064)		(.074)	(.016)	(.012)	(.014)	(.019)	(.019)	(.015)
Constant	n/a	–	n/a	–	.494**	.591**	.826**	.354**	.454**	.487**
					(.011)	(.009)	(.010)	(.013)	(.013)	(.011)

Models without a constant are ordered probit. All others are OLS. All variables run on a 0 to 1 interval. Unavailable constructs are denoted by n/a. In Study 1, N = 517–530 for all analyses. In Study 2, N = 1,104–1,131 for all analyses.

** $p < .05$, * $p < .10$, one-tailed tests.

with higher values reflecting greater quantities of a given concept. Notice first that all the estimates under the headings "knowledge" and "identities" are in the predicted direction. Consistent with (H1), subjects assigned to interview in English report *higher* levels of opinions based on concepts that are more associated with English, including U.S. political knowledge and Latino identity. As further corroboration of this trend, English interviewees report a *lower* level of national origin identity, which makes sense since this attachment is more strongly linked to Spanish, thereby yielding a negative coefficient for English interviewees. Thus, by making some concepts more accessible, language appears to intensify the political opinions that individuals report.

Second, notice that most of these estimates are also reliably different from zero via one-tailed tests.[17] For example, English interviewees report knowing five percentage points more facts about U.S. politics (.053) than Spanish interviewees. This gap grows to more than seven percentage points (.075) when I drop the Latino-themed items and strictly analyze traditional civic facts ("Whose responsibility is it to determine if a law is constitutional or not?"). This implies that a language-knowledge gap will grow when facts are more clearly and strongly tied to English. In further support of this interpretation, the knowledge-language gap drops to insignificance when I analyze the subscale of Latino-centered knowledge items, which I claim perform as mixed stimuli. In other words, these items are strongly linked to English *and* Spanish.

I find further supporting evidence for my main hypothesis when I look at the results for ethnic and national identities. First, I find that those subjects assigned to interview in English report a stronger level of American identity and Latino identity than those interviewing in Spanish, though only the latter effect is statistically reliable (.041, $p < .073$, one-tailed). Critically for my framework, I also find that subjects assigned to interview in English express a national origin identity level that is about three percentage points *less* ($-.034$, $p < .048$, one-tailed) than Spanish interviewees, which is expected since English interviewees are retrieving a concept—national origin identity—that is more associated with Spanish.[18] The substantive impact of all these effects are visually displayed down the left column in Fig. 1, highlighting that all of them are in the expected direction and most of them reliably different from zero at a 10 % level or better (one-tailed), denoted by asterisks.

I further substantiate my accessibility hypothesis by analyzing response times (RTs) and refusal rates (RRs) for my knowledge questions. I focus only on these items because, unlike my group attachment items, there are correct answers to knowledge queries. Hence, I can be more confident that any reliable variation in RTs is due to the "right" information being mentally accessible. Study 1 timed each

[17] I use one-tailed tests given the explicitly directional nature of my main hypothesis (H1), as well as accumulated lab work showing accessibility effects in specific directions on cognitive outcomes, such as answering questions.

[18] Reflecting prior research (Ross et al. 2002, p. 1041), these findings are largely inconsistent with a social desirability explanation. For example, if social desirability was responsible for these opinion gaps, we should have observed English interviewees express weaker (not stronger) Latino identity than Spanish interviewees.

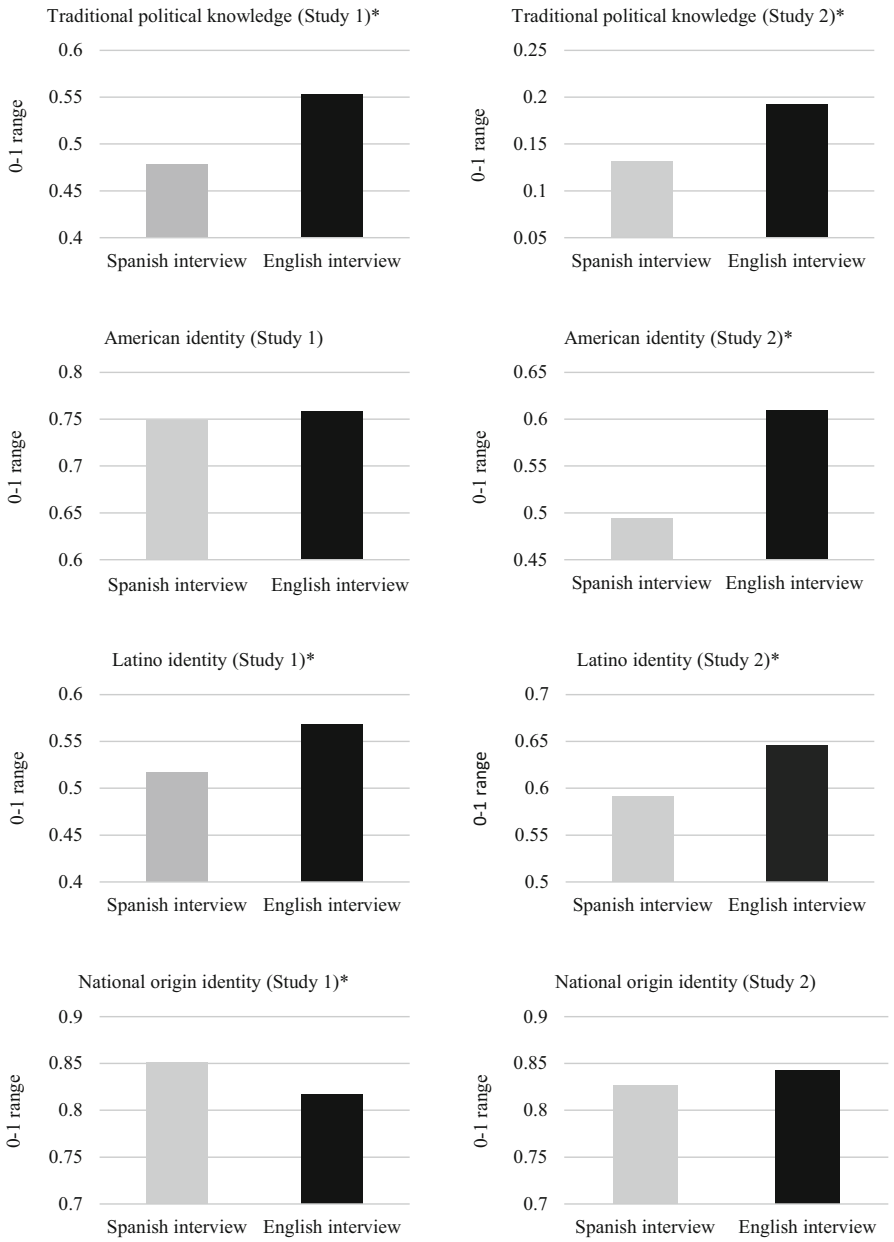

Fig. 1 The Substantive Impact of Interview Language on Opinions (Studies 1 and 2)

knowledge question. Since these RTs are indexed in seconds, and not milliseconds as is typical (e.g., Fazio et al. 1986), it will be harder to find any accessibility effects here: a problem compounded by the large amount of measurement error in RTs

(Fazio et al. 1995). Using the RTs for the five traditional civic knowledge items, I create a scale that takes the log of each RT, adds these up, and divides by the number of RTs. The log-transformation of these items comports with prior work (Greenwald et al. 1998), which uses it to ensure normality in RTs. I then predict scores on this scale as a function of interview language, plus the number of items subjects completed in more than 30 s, age, education, and preferred language for taking surveys. These covariates help to validate my RT scale and allow me to conduct an interactive analysis to gauge the extent to which RTs are affected by slower responders (i.e., those answering more questions in more than 30 s).[19]

Columns A and B in Table 2 contain the relevant results. In column A, interviewing in English produces a roughly 6 % decrease in my RT scale. That is to say, English interviewees answer these U.S. knowledge questions more quickly than Spanish interviewees, which supports my accessibility claim. The results under column B further show this effect is unmoderated by the number of knowledge questions answered in more than 30 s. Finally, the covariates in each model reassure us that my scale is valid since, for example, older subjects and subjects who prefer to take their surveys in Spanish are both more likely to respond much slower.

To lend more credence to my interpretation of these results, I also examine refusal rates (RR) on the five traditional civic knowledge items.[20] Once again, I focus strictly on knowledge questions because in comparison to my group identity items, these have correct answers. This means that refusal of these items can be taken as a strong sign that the "right" information is mentally inaccessible. Given the relatively few number of refusals on these items, the dependent variable here is a dichotomous outcome where '1' indicates refusal of any knowledge item and '0' reflects no refusals. I predict RRs as a function of interview language and the same covariates as in the preceding analysis. The entries under column A in Table 3 reveal that subjects who interview in English are marginally less likely to refuse these knowledge items, which bolsters my language-accessibility claim. The results under column B further show that this effect is unmoderated by subjects who answer a greater number of items in more than 30 s.

Language does not consistently affect the accessibility of all political concepts, however. My theory anticipates that it will be harder to replicate language effects on concepts that are highly crystallized or labile. In Study 1, I consider partisanship to be a very stable trait (Green et al. 2002), while anti-Obama affect (Brader and Marcus 2013) and ideology (Ellis and Stimson 2009) serve as very labile constructs. The last three columns in the top panel of Table 1 reveal that those subjects assigned to interview in English report more Republican identification than those

[19] I used a 30 s threshold for answering knowledge items in Study 1 in order to keep this analysis consistent with an identical one using knowledge items from Study 2, where subjects were explicitly given 30 s to answer each knowledge item. As my analysis will show, knowledge item RTs in Study 1 are largely unaffected by slow responders.

[20] I study refusal rates rather than "don't know" (DK) rates because people offer DKs for many reasons, including (a) not fully knowing an answer (b) incompletely knowing an answer (Gibson and Caldeira 2009); (c) refraining from guessing an answer (Mondak 2001); and (d) being unmotivated to try answering correctly (Prior and Lupia 2008). Refusal rates are less affected by these problems, so I use them to test my accessibility hypothesis.

Table 2 Interview Language Influences Response Times on Knowledge Items in Study 1 and Study 2

	A. Study 1 U.S. knowledge	B. Study 1 U.S. knowledge	C. Study 2 U.S. knowledge	D. Study 2 U.S. knowledge	E. Study 2 Latin American knowledge	F. Study 2 Latin American knowledge
English interview	−.063**	−.066*	−.040**	−.029	.020	.037
	(.037)	(.049)	(.022)	(.027)	(.032)	(.044)
>30 s	2.134**	2.120**	1.74**	1.775**	1.622**	1.645**
	(.088)	(.116)	(.041)	(.068)	(.040)	(.060)
English x >30 s	–	.024	–	−.070	–	−.046
		(.188)		(.094)		(.091)
Age	.270**	.270**	.291**	.289**	.005**	.004**
	(.088)	(.083)	(.049)	(.049)	(.001)	(.001)
Education	.101*	.101*	−.113**	−.112**	.011	.011
	(.061)	(.079)	(.033)	(.038)	(.016)	(.018)
Prefer Spanish interview	.141**	.141**	.107**	.109**	.151**	.152**
	(.041)	(.038)	(.024)	(.023)	(.035)	(.034)
Constant	2.174**	2.176**	2.464**	2.458**	2.304**	2.296**
	(.056)	(.078)	(.033)	(.042)	(.067)	(.076)

All coefficients are from OLS models. The dependent variables here are mean logged response times as described in the text. The variable >30 s captures how many knowledge items subjects answered in more than thirty seconds. All covariates are rescaled to run from 0 to 1. For all Study 1 analyses, N = 530. For all Study 2 analyses, N = 1,127–1,128

** $p < .05$, * $p < .10$, one-tailed tests

interviewing in Spanish (.022), though this positive shift is not reliably different from zero. This pattern makes sense as far as the stability of partisanship is concerned. And inasmuch as partisanship is highly impervious to change, I should find another null coefficient in the same direction when I attempt to replicate it. In addition, English interviewees express marginally higher levels (.033, $p < .097$, one-tailed) of anti-Obama affect and ideology (.029, $p < .090$, one-tailed). The direction and significance of these coefficients will anchor my expectations for these constructs in Study 2. Insofar as anti-Obama affect and ideology are unstable, I should find either null effects or reliable changes in the opposite direction of these coefficients when I attempt to replicate them.

Results: Study 2

How much of what I find in Study 1 can I replicate in Study 2? Moreover, can I show that language shapes more Spanish-tied concepts than just national origin identity? These were the goals of Study 2, which had the same design as Study 1, save for three details. First, to rule out that poor measurement yielded language's

Table 3 Interview Language Influences Refusal Rates on Knowledge Items in Study 1 and Study 2

	A. Study 1 U.S. knowledge	B. Study 1 U.S. knowledge	C. Study 2 U.S. knowledge	D. Study 2 U.S. knowledge	E. Study 2 Latin American knowledge	F. Study 2 Latin American knowledge
English interview	−.341*	−.426**	−.026	−.163*	.052	.132*
	(.227)	(.236)	(.107)	(.123)	(.079)	(.100)
>30 s	−1.667**	−2.278**	−.126	−.585*	−1.051**	−.925**
	(.875)	(1.398)	(.202)	(.315)	(.106)	(.145)
English x >30 s	–	1.348	–	.895**	–	−.262*
		(1.580)		(.428)		(.208)
Age	.036	.037	−.664**	−.645**	−.004**	−.005**
	(.517)	(.442)	(.250)	(.245)	(.002)	(.002)
Education	−.860**	−.869**	−.554**	−.557**	−.236**	−.235**
	(.338)	(.366)	(.162)	(.163)	(.040)	(.040)
Prefer Spanish interview	.035	.038	−.192*	−.222**	−.162**	−.155**
	(.240)	(.226)	(.122)	(.121)	(.087)	(.088)
Constant	−1.065**	−1.027**	−.681**	−.606**	1.029**	.992**
	(.296)	(.275)	(.156)	(.149)	(.171)	(.174)

All coefficients are from probit models. The dependent variable in each model is dichotomous, capturing whether a respondent refused any knowledge question or not. To reduce clutter, all model cutpoints are not shown. The variable >30 s captures how many knowledge items subjects answered in more than thirty seconds. All covariates are rescaled to run from 0 to 1. For all Study 1 analyses, N = 530. For all Study 2 analyses, N = 1,131

** $p < .05$, * $p < .10$, one-tailed tests

null effect on American identity in Study 1, Study 2 gauged this attachment with four (4) items, three (3) of which were reverse-worded to make it harder to agree across items that American identity is important to oneself.[21] Second, to further show that interviewing in Spanish makes some political concepts more accessible than interviewing in English, I fielded two items that I designed to measure knowledge about Latin American history. The questions were: "Who is known as an effective military and political leader that led various South American nations to independence from Spain?" (i.e., Simón Bolivar); and "Who is known as a friar that advocated for the rights of indigenous peoples in the Americas?" (i.e., Bartolomé de las Casas). Finally, both the U.S. and Latin American knowledge items set an explicit 30-second threshold for subjects, who were expected to answer these question within this range of time. I did this in order to better identify which subjects likely used the Internet to answer these questions.

[21] Indeed, in Study 1, the two items measuring American identity were positively worded. Not surprisingly in hindsight, the estimated intercept for American identity in Study 1 was about .75, which suggests a high degree of this attachment among Spanish interviewees, making it harder for English interviewees to report an even higher level of this attachment.

The lower panel in Table 1 reports the relevant results of this larger replication study. There we see, once again, that those subjects assigned to interview in English report reliably more (.252, $p < .001$, one-tailed) knowledge about U.S. politics—which in this study was measured with three (3) rather than five (5) items.[22] Moving over to knowledge about Latin American history, we find what at first appears to be evidence of a coefficient with a wrong sign: namely, subjects assigned to interview in English report more knowledge about Latin American history, though the effect is unreliable. This pattern seems to arise for two reasons. First, these items are novel and arguably hard to answer. Second, subjects were given 30 s to answer these items. Indeed, further analysis reveals that when this estimate is adjusted for the number of items that were answered in more than thirty-seconds by subjects in each language condition, a coefficient in the hypothesized direction emerges (−.144, $p < .083$, one-tailed).[23] This result suggests that when subjects answer these items within 30 s, those assigned to interview in English report marginally less knowledge about Latin American history.

Turning to the estimates for group attachments, the entries in the bottom panel of Table 1 reveal that subjects assigned to interview in English again report higher levels of American identity. This time, though, the effect is larger and reliable (.116, $p < 001$, one-tailed). This effect suggests that subjects interviewing in English report nearly 12 percent more U.S. identity than those interviewing in Spanish. I also find for a second time that interviewing in English yields reliably higher Latino identity (.055, $p < .001$, one-tailed), which I theorize is more linked to this language. The one wrinkle in these results involves national origin identity. I hypothesized, and found in Study 1, that subjects assigned to interview in English report lower levels of this attachment. But I find here that English interviewees report *higher* levels of national origin identity, though this effect is shy of statistical significance. A plausible culprit is measurement error. Perhaps these items make it too easy to agree with them, making it harder to find a meaningful difference by interview language.[24] Overall, however, I am able to replicate three out of my four original findings in Study 1, which can be visually appreciated via a side-by-side comparison of the graphs in Fig. 1. Moreover, I find some evidence that interviewing in English reduces the accessibility of some constructs (i.e., Latin American knowledge), thus showing that language effects also occur in Spanish.

Paralleling my analyses in Study 1, I also examine subjects' response times (RTs) for the U.S. and Latin American knowledge items. The entries under column C in Table 2 indicate that subjects interviewing in English report knowledge about American politics more quickly than those assigned to interview in Spanish, with the entries under column D showing this effect is unmoderated by slow responders. In turn, the entries under columns E and F in Table 2 reveal that subjects interviewing in English are, as expected, slightly slower to respond to the Latin

[22] This estimate is from an ordered probit model. The three knowledge scale items were: "Whose responsibility is it to determine if a law is constitutional or not?"; "How much of a majority is required for the US Senate and House to override a presidential veto?"; and "How long is the term of office for a United States Senator?".

[23] Specifically, I estimated an ordered probit model with the following coefficients: *Latin American knowledge* = −.144 *English* + .110 > *30 s* + .613 *English* x > *30 s* + .464 *Education* + .015 *Age*.

[24] Indeed, in both Study 1 and 2 the intercept for this construct is above .80 on a 0–1 range.

American knowledge items, though this effect is statistically insignificant (and unmoderated by slower responders).[25]

Mimicking Study 1, I then analyze refusal rates (RRs) for the American and Latin American knowledge items, with the relevant results in Table 3 under columns C through F. There we see that slow responders do moderate refusal rates, with those answering more questions in more than 30 s being more likely to refuse the U.S. knowledge questions, but marginally less likely to refuse the Latin American knowledge items. However, after statistically accounting for this moderation, I find that among all subjects who complied with the thirty-second deadline, those assigned to interview in English are, as expected, marginally less likely to refuse the U.S. knowledge items than Spanish interviewees ($-.163$, $p < .093$, one-tailed), but marginally more likely to refuse the Latin American knowledge items than Spanish interviewees ($.132$, $p < .094$, one-tailed).[26]

Finally, the results for partisanship affirm its stability. Across both studies, those assigned to interview in English appear to report slightly more Republican partisanship, but neither coefficient is reliably different from zero. In turn, I find that the effects of interviewing in English on feelings toward Obama and ideology change signs and significance. For example, in Study 1, those assigned to interview in English expressed marginally more negative feelings toward President Obama. In Study 2, however, subjects assigned to interview in English report reliably *less* negative feelings toward this executive. A comparable pattern emerges for ideology. These latter results align with my hypothesis that inconsistent language effects will emerge on constructs that are highly labile. All this implies that language makes some, but not all, concepts more accessible.

Are Language Effects Direct or Mediated?

The evidence so far suggests that language directly shapes opinions. Here I scrutinize two alternative explanations for my results. The first is that my language manipulation induces strong emotions among subjects, which *mediate* my language effects (Baron and Kenny 1986; Bullock and Ha 2011). This would imply that language mostly affects opinions indirectly via the emotion(s) sensed during an interview.[27] In turn, interviewing in English, a dominant tongue, might induce a higher sense of efficacy,

[25] Although limited in number, the two reliable RT differences by interview language in Table 2 weakly impact people's survey responses, further hinting at the role of accessibility in producing these results (see Table E, SI).

[26] Since my RR variables have much more limited variation than my RT scores, I do not further examine whether RR differences subsequently impact knowledge reports.

[27] One might reasonably wonder whether an emotion like pride is conceptually and empirically synonymous with group consciousness, linked fate, or solidarity, since all of these reflect positive feelings to a degree (Dawson 1994; McClain et al. 2009; Sanchez 2006; Leach et al. 2008). True, group attachments can induce pride and other emotions in group members (Mackie et al. 2008). But my conceptualization and measure of pride focus on this feeling among *individuals* and what each person accomplishes during an interview. Still, I would expect, but cannot test here, that language impacts people's sense of group consciousness, linked fate, and/or solidarity insofar as these flow from a specific group identity.

which channels language effects. It is also plausible, however, that interviewing in Spanish spurs greater efficacy as subjects bolster against this tongue's stigmatization. Either way, efficacy might also mediate my language effects.

In the interest of space, table F (SI) reports the distribution of responses to my anxiety, anger, and pride self-reports, as well as my efficacy item. These questions were near the end of the survey, with the emotion items as the last three completed by subjects, in random order. Table F has two punchlines. First, interviewing in Spanish induced more anxiety among subjects than interviewing in English across both studies. This effect was marginally significant in Study 1 and highly significant in Study 2. Second, a greater sense of efficacy was more prevalent among Spanish interviewees, but this effect was statistically reliable only in Study 2. Given these patterns, I formally test whether anxiety and efficacy each mediates the impact of language on individual opinions.

Several mediation tests exist, but the most rigorous framework so far has been proposed by Imai et al. (2011). This approach estimates an average causal mediation effect (ACME), while assuming the absence of pre- or post-treatment effects that might affect the mediator and outcome. The ACME allows me to estimate how much of language's influence on opinion occurs indirectly through a heightened sense of anxiety and efficacy, respectively. I estimate two models for each of these mediators: one where the mediator (e.g., anxiety) is a function of the treatment (i.e., English interview); the other where an opinion is a function of the treatment (i.e., English interview) and mediator (e.g., anxiety). To provide as strong a test as possible, I focus on the five (5) dependent variables that reliably affected people's opinion reports in one or both of my studies: American identity; Latino identity, national origin identity, and U.S. political knowledge with and without Latino-themed items. Table 4 reports the ACMEs, which can be interpreted as the change in an outcome due to a shift in the mediator while keeping the treatment constant.

There we see evidence that the mediating influence of anxiety on these opinions is generally weak and inconsistent. In Study 1, anxiety appears to mediate the impact of language on individual opinions, but these effects are small. Indeed, no more than 15 % of language's effect on individual opinions is mediated by anxiety, suggesting language influences opinions largely in a direct way. Also, of these four mediation effects in Study 1, two lend themselves to re-examination in Study 2, yet neither one is replicated (see entries for anxiety under Latino identity and non-Latino political knowledge). In the case of efficacy in Study 2, this mediator channels some of language's impact on individual reports of American identity and political knowledge. But again, the extent of mediation here is small. In each instance, more than three-fourths of language's direct effect on opinion remains intact after allowing for mediation. All this suggests that the reliable language effects I have uncovered are largely direct ones, whereby language makes some concepts more accessible.

Implications

The results of my language experiments generally suggest that people's opinions intensify when relevant political concepts are retrieved from memory by using the same language these constructs are associated with. For example, English

Table 4 Mediation of Language's Impact on Political Opinions (with 90 % confidence intervals)

	American identity	Latino identity	National origin identity	Political knowledge (all items)	Political knowledge (non-Latino)
Study 1					
English interview (direct effect)	–	.036	−.039	.046	.067
		[−.008, .081]	[−.070, −.006]	[.003, .090]	[.021, .115]
Anxiety (ACME)	–	.005	.005	.008	.008
		[.000, .012]	[.001, .011]	[.001, .015]	[.001, .017]
% of total effect mediated	–	11 %	15 %	14 %	11 %
Study 2					
English interview (direct effect)	.120	.057	–	–	.070
	[.095, .146]	[.038, .078]			[.039, .102]
Anxiety (ACME)	−.005	−.001	–	–	.001
	[−.009, −.002]	[−.004, .001]			[−.003, .005]
% of total effect mediated	5 %	*ns*	–	–	*ns*
English interview (direct effect)	.122	.057	–	–	088
	[.098, .148]	[.037, .077]			[.058, .119]
Efficacy (ACME)	−.005	−.001	–	–	−.012
	[−009, −.002]	[−.003, .000]			[−.020, −.004]
% of total effect mediated	4 %	*ns*	–	–	16 %

Entries are OLS estimates with 90 % confidence intervals in brackets. ns indicates not significant. For all models in Study 1, N = 500–530. For all models in Study 2, N = 1,088 – 1,110. All variables run on a 0-1 range, save for the mediators, which run from 1 to 4, with higher values reflecting greater quantities of the underlying trait.

interviewees, relative to Spanish interviewees, report higher levels of opinions based on concepts more strongly tied to English, such as U.S. political knowledge and American identity. Subjects also reply faster to, and are less likely to refuse answering, questions gauging information linked to English or Spanish (e.g., "Whose responsibility is it to determine if a law is constitutional or not?"). Thus, whereas prior work has centered on detecting a link between interview language and mass opinion, regardless of its direction, I clarify whether and how language intensifies people's political attitudes, beliefs, and identities. This should be useful for scholars studying linguistically diverse populations, such as U.S. Latinos (cf. Pérez 2009; Schildkraut 2005; Wallace 2014), by delineating one mechanism behind some language effects.

It is plausible, though, that my language effects are heterogenous. For example, Abrajano and Panagapolous (2011) and Binder et al. (2014) find that the impact of political communications varies by Latinos' primary language, with stronger effects for English dominant speakers. It is also plausible that immigrant generation plays a similar role (Abrajano and Alvarez 2010; Portes and Rumbaut 2006), with weaker effects among those who are further removed from the immigrant experience. Although my studies measured subjects' preferred language (PL) and immigrant generation (IG), I did so to gain a sense of each sample's composition, not to explore heterogeneous effects. In fact, conducting such an analysis dramatically reduces my sample size, limiting my detection of true effects, if they exist. With low power, any reliable effect is more likely to be a fluke, making it harder to replicate (Cohen 1992). Tables G and H (SI) provide evidence in this vein.[28]

My findings suggest that the contours of public opinion can sometimes be distinctly affected by the language people use to express their political views. My results for political knowledge illustrate this dynamic. Prior work finds that Latinos report much less political knowledge than other groups, such as Whites (Abrajano 2010; Abrajano and Alvarez 2010). However, in both of my studies, Latinos who interviewed in English reported more knowledge about U.S. politics than those interviewing in Spanish—despite the fact that both sets of interviewees were alike in all respects, save for their interview language. This implies that some of what people know about politics is not solely due to possessing facts, but also to whether those facts are tied more to a specific tongue and whether one uses that language to recall facts from memory. Beyond political knowledge, however, I also detected and replicated comparable results for American identity and Latino identity, suggesting that language-opinion effects are not restricted to political information or concepts resembling it. The most appropriate conclusions to draw from these findings, then, are that language can sometimes affect public opinion; such effects are most likely to emerge when reporting political knowledge, group attachments, or qualitatively similar constructs; and, subsequent research can do well by further theorizing about what other types of concepts lend themselves to language-opinion effects like the ones I have uncovered.

[28] A better strategy here is to design a sufficiently powered experiment that blocks subjects' assigned interview language on their PL or IG, thus retaining the experiment's causal leverage. I leave this possibility open for future research.

Of course, there is plenty of room for improvement here. Most of the language effects I unearthed reflect a better fit between interviewing in English and concepts tied to this tongue (i.e., U.S. political knowledge, American identity, and Latino identity). In comparison, the evidence for Spanish-linked concepts is weaker, though not absent. Specifically, I expected to observe English interviewees reporting weaker national origin identity than Spanish interviewees, a pattern I did detect once. I also expected to find English interviewees reporting less knowledge about Latin America than Spanish interviewees, a result that marginally emerges among subjects who answered both items within the 30-second response window I set. I interpret these patterns as indicating there is "something" to the connection between interviewing in Spanish and Spanish-linked constructs, yet subsequent work can improve upon this by measuring these constructs more precisely, while identifying other concepts tied to Spanish (e.g., beliefs about U.S. colonialism in Latin America).

By randomly assigning the interview language in my studies, I isolated its influence while holding constant all other (un)observed differences between subjects. Thus, in contrast to prior observational studies (e.g., Garcia 2009; Lee and Pérez 2014), I resolved the challenge of omitted variable bias and exposed the unique effects of language on mass opinion. As a result, my studies suggest that the association between political opinion and interview language *is* due to language and not differences in, say, education, cognitive ability, language proficiency or any other (un)observed individual difference that we fear might be conflated with interview language. My studies also suggest the mechanism behind these language effects *is* accessibility. True, as I acknowledged above, some of my reported evidence for this mechanism can arguably be construed as weak (e.g., finding that some English interviewees report marginally less knowledge about Latin America). But these are blemished results, not contradictory ones. They still lend some support to my proposed mechanism. Going forward, one way to buff out these imperfections and bolster my accessibility claim is to exert more control over the language-concept link: for instance, by manipulating the encoding of concepts in specific languages, rather than making informed assumptions about which concepts are tied to which tongues, as I have done here (see Pérez 2015a).

Besides offering evidence in favor of my accessibility mechanism, my study also casts doubt on two alternative explanations for my language effects, thus bolstering confidence in the internal validity of my study and others like it (Marian and Neisser 2000; Marian and Kaushanskaya 2007). The first of these explanations suggests that random assignment to an interview language makes one anxious, angry, or proud, with any of these emotions mediating the impact of language on opinions. Yet by and large, these expectations were dashed. Of these three emotions, English and Spanish interviewees reliably differed only with respect to anxiety—a pattern that emerged across both studies. Nevertheless, differences in anxiety weakly and inconsistently mediated the influence of language on people's opinions, which reassures us that language largely impacts people's opinions directly by making relevant political concepts more accessible.

I also ruled out that efficacy largely mediates my language effects. I reasoned that among bilingual subjects, interviewing in a majority tongue (e.g., English)

heightens their sense of efficacy, thus spurring better recall of relevant concepts. I also considered whether interviewing in a minority tongue (i.e., Spanish) heightens efficacy as a result of bolstering against this language's implied lower social status, thereby triggering a deeper memory search. Yet my analysis of a standard political efficacy item revealed scant evidence to support either contention. English and Spanish interviewees reported uniform levels of political efficacy in Study 1, but reliably different levels in Study 2. The distinct levels of efficacy in Study 2, however, weakly mediated language's impact on opinions.

My experiment enhances the external validity of prior language effects studies (cf. Marian and Neisser 2000; Marian and Kaushanskaya 2007). By revealing the effect of language on *political* thinking in two heterogeneous samples of bilingual adults, I show that language has "real world" effects outside tightly controlled lab settings, thus strengthening the empirical foothold of this larger research area. Even so, additional questions merit further research. For example, my experiment took place in a non-lab setting using an online mode. Thus, even after relaxing some of my control over subjects and stimuli, language effects on people's political opinions still broke through, bolstering confidence in survey experiments like these (Mutz 2011). Still, this online setting likely provides a lower bound to estimates of language-opinion effects. By completing their surveys via the Internet, subjects did not interact with interviewers in person or on the phone, some of whom might be co-ethnics and/or co-language speakers. On one hand, this is a "good thing" because it eliminates the need to grapple with these additional complicating factors. On the other hand, despite the virtues of online studies like mine, many surveys still take place either in-person or via phone. This is simply another way of saying that interviewer characteristics might provide useful leverage over theoretical questions, such as "when are language effects intensified (Pérez 2015a)?"

In the end, my results make it more difficult for political scientists to dismiss the association between language and mass opinion as spurious or atheoretical. They also make it easier for political scientists to better appreciate the degree to which language leaves its imprint on individual opinions. To be sure, these results arise from a comparison of two languages in a specific setting: English and Spanish among U.S. Latino bilinguals. Yet I have developed a theoretical framework capable of explaining similar effects in comparable cases. Indeed, laboratory studies from cognitive psychology already tell us that effects similar to mine emerge between other language pairings (see Pérez 2015b for an overview). Still, only the accumulation of research can determine how broadly applicable this framework is. But given the polyglot nature of so many societies across the globe, I think it behooves public opinion scholars to evaluate this sooner rather than later.

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Appendix: Question Wording for Study 1 and Study 2

Unless otherwise noted, all items below appeared across both studies. Underlined items appeared only in Study 1. Italicized items appeared only in Study 2.

Bilingual Identification Items

(1) Would you say you can carry on a conversation in Spanish, both understanding and speaking—(1) very well, (2) pretty well, (3) just a little, or (4) not at all?
(2) Would you say you can read a newspaper or book in Spanish—(1) very well, (2) pretty well, (3) just a little, or (4) not at all?
(3) Would you say you can carry on a conversation in English, both understanding and speaking—(1) very well, (2) pretty well, (3) just a little, or (4) not at all?
(4) Would you say you can read a newspaper or book in English—(1) very well, (2) pretty well, (3) just a little, or (4) not at all?

[ORDER OF LATINO, AMERICAN, AND NATIONAL ORIGIN IDENTITY BATTERIES WAS RANDOMIZED].

Latino Identity Items

(5) Being Latino is unimportant to my sense of what kind of person I am.

(1) Strongly disagree
(2) Somewhat disagree
(3) Somewhat agree
(4) Strongly agree

(6) Overall, being Latino has very little to do with how I think about myself.

(1) Strongly disagree
(2) Somewhat disagree
(3) Somewhat agree
(4) Strongly agree

(7) *Identifying with other Latinos is central to who I am as an individual*

(1) *Strongly disagree*
(2) *Somewhat disagree*
(3) *Somewhat agree*
(4) *Strongly agree*

American Identity Items

(8) How strongly do you identify as American?

(1) Not at all
(2) Slightly
(3) Somewhat
(4) Very much

(9) To what extent do you see yourself as a typical American?

(1) Not at all
(2) Slightly
(3) Somewhat
(4) Very much

(10) *In general, being American has very little to do with how I think of myself*

(1) Strongly disagree
(2) Somewhat disagree
(3) Somewhat agree
(4) Strongly agree

(11) *Being American is unimportant to who I am as a person*

(1) Strongly disagree
(2) Somewhat disagree
(3) Somewhat agree
(4) Strongly agree

(12) *Identifying as American is central to how I see myself*

(1) Strongly disagree
(2) Somewhat disagree
(3) Somewhat agree
(4) Strongly agree

(13) *Overall, American identity is not important to my self-image.*

(1) Strongly disagree
(2) Somewhat disagree
(3) Somewhat agree
(4) Strongly agree

National Origin Identity Items

[ITEMS 14-16 WERE CALIBRATED ACCORDING TO NATIONAL ORIGIN GROUP]

(14) I am pleased to be [e.g., Mexican].

(1) Not at all
(2) Slightly
(3) Somewhat
(4) Very much

(15) I identify as [e.g., Mexican].

(1) Not at all
(2) Slightly
(3) Somewhat
(4) Very much

(16) In general, how strongly do you think of yourself as [e.g., Mexican]?

(1) Not at all
(2) Slightly
(3) Somewhat
(4) Very much

Partisanship and Ideology Items

(17) Generally speaking, do you think of yourself as a…

(1) Republican
(2) Democrat
(3) Independent
(4) Another party, please specify:
(5) No preference

[If item 17 = "Republican"]

(17a) Would you call yourself a…

(1) Strong Republican
(2) Not very strong Republican

[If item 17 = "Democrat"]

(17b) Would you call yourself a…

(1) Strong Democrat
(2) Not very strong Democrat

[If item 17 = "Independent" or "Another party" or "No preference" or "Refused"]

(17c) Do you think of yourself as closer to the…

(1) Republican Party
(2) Democratic Party

(18) In general, do you think of yourself as…

(1) Extremely liberal
(2) Liberal
(3) Slightly liberal
(4) Moderate, middle of the road
(5) Slightly conservative
(6) Conservative
(7) Extremely conservative

Political Knowledge Items

[ORDER OF KNOWLEDGE ITEMS AND THEIR RESPONSE OPTIONS WAS RANDOMIZED]

[Completion of 19a or 19b was randomized]

(19a) In general, thinking about the political parties in Washington, would you say Democrats are more conservative than Republicans, or Republicans are more conservative than Democrats?

(1) Democrats are more conservative than Republicans
(2) Republicans are more conservative than Democrats
(3) I don't know

(19b) In general, thinking about the political parties in Washington, would you say Republicans are more conservative than Democrats, or Democrats are more conservative than Republicans?

(1) Republicans are more conservative than Democrats
(2) Democrats are more conservative than Republicans
(3) I don't know

(20) Whose responsibility is it to determine if a law is constitutional or not?

(1) the President
(2) Congress
(3) the Supreme Court
(4) I don't know

(21) How much of a majority is required for the U.S. Senate and House to override a presidential veto?

(1) one half plus one vote
(2) three-fifths
(3) two-thirds
(4) three quarters
(5) I don't know

(22) What job or office is now held by John Roberts?

(1) Secretary of the Treasury Department
(2) Vice-President of the United States
(3) Chief Justice of the U.S. Supreme Court
(4) I don't know

(23) What job or political office does John Boehner now hold?

(1) Vice-President of the United States
(2) Speaker of the U.S. House of Representatives
(3) U.S. Ambassador to China
(4) I don't know

(24) In June 2012, it was announced that illegal immigrants who came to the U.S. under the age of 16 would be able to live and work here for two years if they met various requirements, including a clean criminal record. What government institution was formally responsible for this policy change?

(1) The U.S. Supreme Court
(2) The U.S. Congress
(3) The Office of the President of the United States
(4) I don't know

(25) What job or office does Sonia Sotomayor now hold?

(1) Secretary of Homeland Security
(2) News reporter for Univision News

(3) Justice of the U.S. Supreme Court
(4) I don't know

(26) What job or office does Marco Rubio now hold?

(1) News reporter for Telemundo News
(2) Secretary of Health and Human Services
(3) U.S. Senator
(4) I don't know

(27) *How long is the term of office for a United States Senator? [OPEN-ENDED]*

Political Efficacy and Obama Thermometer Rating

(28) Sometimes politics and government seem so complicated that a person like me can't really understand what's going on.

(1) Strongly disagree
(2) Somewhat disagree
(3) Somewhat agree
(4) Strongly agree

(29) Using what is called a feeling thermometer, we want you to rate how you feel toward each group or individual below. Ratings between 0 and 49 mean that you feel unfavorable or cool toward the group or individual. Ratings from 51 to 100 mean that you feel favorable or warm toward the group or individual. You can rate the group or individual at the 50 degree mark if you don't feel warm or cool toward them

Click on the line below to indicate your opinion. You can slide the indicator to the position that best reflects your opinion. If the indicator does not work, you can enter a number in the number box.

Barack Obama

← →

Unfavorable Neutral Favorable

Self-reported Emotion Items

[ORDER OF ITEMS WAS RANDOMIZED]

(30) How anxious do you feel right now, at this moment?

(1) Not anxious at all
(2) A little anxious
(3) Somewhat anxious
(4) Very anxious

(31) How worried do you feel right now, at this moment?

(1) Not worried at all
(2) A little worried
(3) Somewhat worried
(4) Very worried

(32) How angry do you feel right now, at this moment?

(1) Not angry at all
(2) A little angry
(3) Somewhat angry
(4) Very angry

(33) How proud do you feel right now, at this moment?

(1) Not proud at all
(2) A little proud
(3) Somewhat proud
(4) Very proud

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