Translating into Votes: The Electoral Impacts of Spanish-Language Ballots

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This article investigates the impact of one election procedure designed to enfranchise immigrants: foreign-language election materials. Specifically, it uses regression discontinuity design to estimate the turnout and election impacts of Spanish-language assistance provided under Section 203 of the Voting Rights Act. Analyses of two different data sets—the Latino National Survey and California 1998 primary election returns—show that Spanish-language assistance increased turnout for citizens who speak little English. The California results also demonstrate that election procedures can influence outcomes, as support for ending bilingual education dropped markedly in heavily Spanish-speaking neighborhoods with Spanish-language assistance. Small changes in election procedures can influence who votes as well as what wins.

Holding an election involves hundreds of seemingly minor decisions, from the location of polling sites to the registration procedure and the design of the ballot. These decisions matter: there is considerable evidence that election procedures can influence voters' decisions at the polling place (e.g., Berger, Meredith, and Wheeler 2008; Ho and Imai 2006), sometimes decisively (e.g., Wand et al. 2001). Yet past research also shows that large-scale policy interventions to increase voter turnout are not always successful (e.g., Knack 1995). Even when they are, they do not typically have substantial impacts on election outcomes because the people they influence have preferences similar to other voters (Citrin, Schickler, and Sides 2003; Franklin and Grier 1997; Highton and Wolfinger 2001). Considered jointly, past research suggests that the procedural changes that increase turnout are likely to be distinct from the procedural changes that influence election outcomes. One set of procedures influences who votes while another influences how they vote.

This article studies an election procedure that is a potential exception, one that might influence turnout and election outcomes jointly. That procedure is the provision of Spanish-language ballots and voting assistance. In 2008, the United States was home to roughly 38 million immigrants, the majority of whom come from Spanish-speaking countries (U.S. Census Bureau 2008). Language differences make it challenging to incorporate these newcomers into American politics. As of 2000, 14% of U.S. households spoke a language other than English at home (U.S. Census Bureau 2000a). Even among U.S. citizens, there were an estimated 8 million people who spoke little or no English in 2000 (U.S. Census Bureau 2000b). Given these barriers, it is not surprising that voter turnout among Latinos and Asian Americans—two heavily immigrant ethnic groups—lags that among non-Hispanic

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whites (Citrin and Highton 2002; Ramakrishnan 2005; Tam Cho 1999; Verba, Schlozman, and Brady 1995).[1]

One straightforward approach to immigrant political incorporation is to translate voting materials into foreign languages. Since 1975, officials in areas with many non-English speakers have been required by Section 203 of the federal Voting Rights Act to provide ballot materials and voting assistance in certain other languages (Government Accountability Office 2008; Jones-Correa 2005; Jones-Correa and Waismel-Manor 2007; Tucker and Espino 2007). The core assumption underpinning this law is that the language skills required for citizenship are not always sufficient for meaningful political participation.[2] Unlike many election procedures, Spanish-language assistance is deliberately targeted to a specific ethnic group. To the extent that it increases turnout only among Spanish speakers, it also has the potential to influence election outcomes in cases where their political preferences are distinctive. Spanish-language election assistance might thus represent an exceptional case where electoral procedures matter through their influence on turnout.

Empirically, past research on the impact of Spanish-language ballots and assistance has reached inconsistent conclusions (e.g., de la Garza and DeSipio 1997; Jones-Correa 2005; Ramakrishnan 2005). Yet it has faced a common methodological challenge, since it is difficult to separate the treatment effect of Spanish-language ballots from the selection effect of voting in areas with many Spanish speakers. Here, this article innovates by *exploiting the discontinuities in coverage to identify the causal impact of Section 203.* As amended, Section 203 mandates that counties provide language assistance if they cross thresholds such as having a language minority that constitutes more than 5% of the citizenry or includes more than 10,000 citizens.[3] Since researchers know the exact process by which units were assigned to treatment or control, we are in the rare position of being able to eliminate concerns about selection into treatment.

To test the impact of Spanish-language assistance on turnout and election outcomes, this article considers two different types of data from elections held in different years and jurisdictions. The first data set, the Latino National Survey (LNS), provides self-reported voter turnout in the 2004 presidential election for 4,330 Latino citizens living in 495 separate U.S. counties. The analyses show that federally mandated language assistance does not have a strong impact on Latino voters overall. But it does influence those with limited English skills, increasing turnout by 11 percentage points on average. Small, targeted changes in election procedures can have marked impacts on turnout.

The article then turns to California's Proposition 227, a 1998 initiative that restricted bilingual education. The influence of using, seeing, or hearing Spanish at the polls will hinge on the specific question before the voters, so by focusing on a measure about bilingual education, we can develop clear expectations about the direction of the impact for Latino voters. But this clarity is not the only reason to study Proposition 227. California's size means that even looking only at the northern half of the state, researchers can make use of data for thousands of block groups from 35 counties. Analyses of Hispanic neighborhoods show that the availability of Spanish-language materials influenced turnout on Proposition 227, *but only in block groups where there are many monolingual Spanish speakers.*[4] This finding closely mirrors the survey-based estimates above. Also, heavily Spanish-speaking neighborhoods with Spanish assistance are up to 9.4 percentage points less supportive of Proposition 227 on average than their counterparts without such assistance. Propositions are commonly reading intensive, so effects of this magnitude in areas with many non-English speakers are not surprising. The instrumental impacts of Spanish-language assistance are marked. And they can influence election outcomes as well.

Hypotheses

For citizens who do not speak English well, the availability of ballots in their native language could increase the chance of turning out or of casting a vote on a particular ballot question once at the polls. Past work finds that language fluency correlates with political participation

[1] This article uses two primary data sets: the Latino National Survey and voting data from California's 1998 Proposition 227. The first data set is available from the Inter-University Consortium for Political and Social Research at http://www.icpsr.umich.edu/icpsrweb/RCMD/studies/20862. All other replication materials, including R code, U.S. Census data, Section 203 determinations, and the California election data are available at http://dvn.iq.harvard.edu/dvn/dv/DJHopkins. The appended California election data set was posted with the permission of its author, the California Statewide Database.

[2] Applicants for U.S. citizenship can be exempt from language requirements based on age, length of residence, and disability.

[3] For the purposes of the law, people are considered to be members of a "language minority" if they are voting-age citizens who speak a single non-English language and are not proficient in English. The designation is thus based on language ability, not ethnicity.

[4] For simplicity, "monolingual" will denote Spanish speakers who are not proficient in English, although some Spanish speakers obviously speak other non-English languages.

(Barreto and Munoz 2003; Cain and Doherty 2006; Tam Cho 1999), so lowering language barriers should expand the electorate (Reilly and Richey 2011; Tucker and Espino 2007). Still, most procedural changes that expand the electorate have little impact on election outcomes (Citrin, Schickler, and Sides 2003; Franklin and Grier 1997; Highton and Wolfinger 2001). The question is whether the impact of this particular election procedure is sufficiently large and targeted to shape outcomes as well. Effect size is also relevant when considering the extent to which Spanish at a polling place influences native-born Hispanic voters. This section develops hypotheses, identifying the subgroups likely to be influenced and the conditions necessary for such influence.

Language Assistance and Mobilization

Past empirical studies of the impact of Section 203 have not reached a consensus. One initial study found little impact, noting that most Hispanic *citizens* speak English at home. It argued that with respect to Latino voting, "[t]he characteristic that was most important to policymakers in 1975, language, is less an impediment to participation than are education and age" (de la Garza and DeSipio 1997, 95). Given that voting requires citizenship, this null finding is quite plausible. Immigrant voters are a selected group that has chosen to naturalize and to vote, and this highly motivated subset may well be able to cast ballots in English.[5]

Two more recent studies, however, report a positive relationship between Section 203 coverage and voter turnout. Ramakrishnan (2005, 105) finds that third-generation immigrants in areas covered by language access provisions are more likely to turn out, and Jones-Correa (2005) finds that Latinos and first-generation immigrants in covered counties are more likely to vote. In either case, one persistent alternative explanation is that the findings actually reflect geographic differences in Latino political organization or mobilization. Section 203 comes into force in areas with large concentrations of Spanish speakers. In those areas, there are also more concerted efforts to mobilize Latino voters, more organizations trying to do so, more Spanish-language media and advertising, and a higher probability of a Latino on the ballot (Barreto 2007; Barreto, Segura, and Woods 2004; Leighley 2001; Oberholzer-Gee and Waldfogel 2009; Panagopoulos and Green 2010; Parkin and Zlotnick 2011). These recruitment efforts and organizations would confound estimates of the direct impact of Section 203. So too would any unobserved differences between the Latino citizens living in heavily Latino areas and those living elsewhere.

Since past research has not reached firm conclusions about Section 203's impact, it is not surprising that it has yet to identify the mechanisms underpinning the potential impact. Still, there are two broad ways in which Section 203 could influence Spanish speakers instrumentally. One is by reducing the difficulty that voters with limited English *anticipate prior to their arrival at the polls,* a mechanism that would induce higher turnout for all ballot questions among Spanish speakers. Here, it is worth keeping in mind that Section 203 covers preelection mailings, absentee ballots, and other contacts with voters. These contacts could provide voting information to Spanish-language voters and also signal that Spanish-language assistance is available at the polls. Since Section 203 designations are public information, parties might also adjust their mobilization strategies to emphasize the availability of Spanish-language materials. This anticipatory mechanism would be especially likely to operate in neighborhoods with large concentrations of Spanish-speaking voters, as word of mouth might increase knowledge about the availability of Spanish-language assistance. Similarly, it is plausible that the impact of Section 203 coverage in a jurisdiction might grow over time, as people vote and then report back to their friends and neighbors about the availability of Spanish-language materials.

The second mechanism is more subtle and operates *after Spanish speakers arrive at their polling place.* Perhaps Spanish-language ballots encourage Spanish speakers who are already at the polls to vote on more of the ballot questions, reducing "fall-off" as voters move down the ballot. Such a mechanism might be especially likely for ballot propositions, which lack heuristics like partisan identification and can involve substantial reading (Reilly and Richey 2011).[6] To the extent that this mechanism operates, we should observe changes in turnout on down-ballot questions without seeing significant changes in turnout overall.

When we move from considering voter turnout to considering election outcomes, there is an additional condition necessary for Section 203 to have instrumental impacts. On average, Spanish speakers and English speakers must have different voting patterns. This condition of differing preferences explains why even significant changes in levels of turnout typically have limited impacts on election outcomes (Citrin, Schickler, and Sides 2003; Highton and Wolfinger 2001): the marginal nonvoter has preferences similar to those of other voters. Yet

[5]For ongoing work on Section 203's impact, see Fraga (2009).

[6]Proposition 227 itself was 1,662 words, and even its summary used 170 words.

Spanish-language ballots target a specific ethnic group, and in doing so, they might be an unusual election procedure which incorporates voters with systematically different preferences.

Certainly, ethnicity proved a strong predictor of support for several California ballot measures in the 1990s (Barreto and Ramirez 2004; Cain, Citrin, and Wong 2000; Campbell, Wong, and Citrin 2006). In the case of Proposition 227, although preelection polls showed Latinos supporting the proposition, a *Los Angeles Times*/CNN exit poll found that 63% of Latino voters rejected the measure (Locke 1998). A related hypothesis holds that the electoral context matters and that language assistance's impact will vary from election to election. For example, language assistance might matter more in contested, high-salience elections with significant mobilization efforts, as more first-time voters are encouraged to participate and as voters become educated about Spanish-language ballots. California in the 1990s met this description as well. Hispanic immigrants in California were mobilized by the threat of anti-immigrant ballot measures such as Proposition 187 (Pantoja, Ramirez, and Segura 2001; Pantoja and Segura 2003), making it a case where we should observe effects on both turnout and election outcomes.

Symbolic Impacts

The discussion to this point has emphasized the impact of Spanish in facilitating voting among those with little or no English. There, the impact of Spanish is primarily *instrumental*: it enables voters to cast their ballots. But Spanish might also have impacts that are more subtle and *symbolic*. Here, "symbolic" is used to mean evocative of "values, customs, and habits distinctive to a . . . group" (Gusfield 1986, 16).[7] For instance, for an English-speaking Hispanic voter, the sight of Spanish might prime Hispanic identity. To the extent that Spanish-language ballots are especially influential for later immigrant generations (e.g., Ramakrishnan 2005), this is potentially the mechanism at work.

Certainly, there is evidence that subtle primes seen while voting can shape voting decisions. Berger, Meredith, and Wheeler (2008) find that voting in schools increased support for a school funding initiative in Arizona in 2000. Also, Ho and Imai (2006) find that ballot order effects matter for minor-party candidates, a situation which might parallel that of a voter who finds herself making decisions on ballot propositions. Such voters, entering the polling booth without a fixed decision on the particular ballot question, are susceptible to environmental primes. Priming effects are typically time-limited, meaning that they appear in decisions made almost immediately after exposure. This fact in turn suggests that symbolic priming effects are unlikely to be accompanied by turnout effects.[8] We now turn to testing these possibilities.

Research Design and Methods

This section details the research design used to study the impact of Spanish-language assistance. It first explains the changes in election procedure that Section 203 requires and then contends that regression discontinuity design is especially well suited to detect the impacts of those changes.

Section 203 mandates that covered jurisdictions provide several forms of language assistance, including the translation of written materials (such as voter guides or sample ballots) and the availability of live translation at election bureaus and at polling sites. Counties differ in their levels and forms of compliance, meaning that the subsequent analyses are "intent-to-treat" analyses that gauge the impact of the federal mandate as it was actually implemented. The impacts we observe could come from the written materials, the presence of Spanish-speaking officials, or even from parties' differing mobilization strategies in covered jurisdictions.[9]

At the same time, we know that compliance is highest for the requirements pertaining to translated *written* materials such as ballots and signs at polling places (Jones-Correa and Waismel-Manor 2007; Tucker and Espino 2007). In a survey of jurisdictions, Tucker and Espino (2007, 189) found that only 13% of Spanish-covered jurisdictions provided neither written nor oral assistance to non-English-speaking voters. We know, too, that Californian jurisdictions exhibit higher-than-average levels of compliance (Jones-Correa and Waismel-Manor 2007, 172–74). Past audits do find substantial noncompliance, but they also suggest that Spanish materials are likely to be available in some form in covered counties. Albeit briefly, voters in covered jurisdictions were likely

[7]For a detailed discussion of the evocative potential of political symbols, see Sears (1993).

[8]This hypothesis rests on the assumption that preelection efforts to publicize the availability of Spanish-language ballots are not sufficiently salient as to mobilize Hispanic voters. Tucker and Espino (2007, 197) show that compliance with Section 203 is highest for Election Day materials such as ballots and sample ballots, so this assumption seems plausible.

[9]This strategic response among political parties creates no bias in the estimates below, but it does potentially impact their interpretation and generality.

to have seen Spanish signs and sample ballots prior to voting.

To better understand actual exposure to Spanish during the 1998 primary, we contacted every California county in the data set and requested its 1998 sample primary ballot. In all, we were able to visually inspect the ballots for 95% of the treated neighborhoods. In doing so, we detected only one case of noncompliance: San Francisco provided Chinese and Spanish directly on its ballots although its 1998 mandate applied only to Chinese.[10] Other uncovered counties consistently provided English-only ballots, with examples including Merced County, San Mateo County, and San Joaquin County. Covered counties such as Fresno provided fully bilingual ballots, with English and Spanish appearing together. However, most covered counties provided their actual ballots separately in English and Spanish (e.g., San Benito, Monterey), typically alongside a bilingual sample ballot (e.g., Kings County, Santa Clara County, Tulare County). The online appendix provides examples of each type and makes it clear that while Spanish-language materials were available, the extent to which Spanish was visible to English speakers did vary by county.[11]

Regression Discontinuity Design

Knowing the mechanism whereby units are assigned to treatment gives researchers tremendous leverage in isolating the impact of the treatment itself, as opposed to spurious relationships that come from selection into treatment (Achen 1986). In this case, there are two triggers that lead to federally required language assistance. The first is if more than 5% of the county's citizens are members of a single language minority group and do not speak English well. The second is if more than 10,000 of the jurisdiction's citizens meet the same criteria. The law thus lends itself to a sharp regression discontinuity design (Angrist and Pischke 2009; Green, Leong, Kern, Gerber, and Larimer 2009; Imbens and Lemieux 2008) comparing counties above and below the legal thresholds. The core idea of a regression discontinuity design (RDD) is that observations just above and below a discontinuity should not differ on any variables except the treatment itself. When properly applied (Green, Leong, Kern, Gerber, and Larimer 2009; Imbens and Lemieux 2008), RDD can remove concerns about unobserved confounders, recovering the estimated local average treatment effect from observational data. In a meta-analysis, RDD approaches consistently recovered experimental benchmarks (Cook, Shaddish, and Cook 2005), a strong testament to their value. For that reason, they have seen increasing use in political science in recent years (e.g., Eggers and Hainmueller 2009; Lee, Moretti, and Butler 2004; Leigh 2007; Meredith 2009).

In practice, however, RDD analyses face challenges that randomized experiments do not. Some discontinuities may induce systematic sorting, although in this case, it is implausible that counties would make systematic efforts to influence their Census counts to avoid Section 203. More relevant is the fact that there is rarely sufficient data arbitrarily close to the discontinuity, so researchers must rely on models to estimate the relationship between the underlying continuous variable and the outcome. One practical implication is that RDD results can be model-dependent (Green, Leong, Kern, Gerber, and Larimer 2009), especially if the data are sparse near the discontinuity. A second implication is that RDD analyses have less power than their randomized counterparts and require more observations to make inferences with the same level of certainty (Schochet 2009).

The analyses below extend classical RDD estimators in two ways. The first extension allows for multiple forcing variables, since counties are covered if either their number of Spanish-only citizens or their percentage of Spanish-only citizens crosses its respective threshold. One approach, adopted in the initial models below, is to model each continuous forcing variable (x_i, z_i) along with the associated higher-order terms (x_i^2, x_i^3, $z_i{}^2$, etc.) and binary treatment indicators for each threshold ($I[x_i > \tau_x]$, $I[z_i > \tau_z]$).[12] This is a straightforward generalization of a standard regression discontinuity design which incorporates multiple discontinuities (see also Ferraz and Finan 2009; Gagliarducci and Nannicini 2009). It, too, should return unbiased causal estimates given that the assumptions underpinning RDD hold. Yet this approach can be inefficient, as it involves estimating coefficients for two

[10] In the analyses below, this impacts 31 Hispanic neighborhoods out of 6,097, or 0.5%. Removing the San Francisco observations or reclassifying their treatment status has no impact whatsoever on any estimates below.

[11] We should also inquire about the potential for contamination in the control group. To the extent that Spanish materials are available *outside* of the covered jurisdictions, the control group will be exposed to the treatment, and treatment effects will be underestimated. Under the 2002 determinations, California was covered by Section 203 at the state level, but this was not true under the 1992 determinations, meaning that there was still significant variation in language access policies across jurisdictions as of 1998. A 1994 California law provides additional language access provisions (SB 1547), but they are far more limited than Section 203. Those provisions mandate Spanish-speaking deputy registrars (Section 2103) and the posting of sample ballots in foreign languages in select precincts (Section 14201).

[12] Here, i indexes observations, x_i and z_i are continuous forcing variables, and τ_x and τ_z are the associated thresholds.

correlated binary variables (e.g., $I[x_i > \tau_x]$ and $I[z_i > \tau_z]$) that indicate the same treatment. Most of the models thus make the simplifying assumption, justified below, that the treatment effect is fixed irrespective of which threshold was crossed. In practical terms, this means conditioning on a single treatment indicator $I(x_i > \tau_x \cap z_i > \tau_z)$.

We also must confront the disconnect between the unit of observation (individuals or block groups) and the unit at which Section 203 coverage is determined (counties). The results below indicate that this clustering has no notable impact on the national survey data, where there are only 8.7 respondents per county. For the California election data, clustering is a more serious challenge, as the 6,097 Hispanic neighborhoods are located in only 35 counties. The analyses incorporate this clustered structure via multilevel models (Gelman and Hill 2006; Schochet 2009), where the county-level impact is a function of Section 203 coverage as well as the forcing variables and their higher-order terms.

The number of California counties is limited, so robustness checks are especially important. In the appendix, the analyses also include matching estimators as a preprocessing step. Matching is a tool which improves balance on observed covariates by reweighting observations (Rubin 2006). Its use means that data sets will have better overlap on the covariates and that any results will be less dependent on subsequent modeling choices (Ho, King, Imai, and Stuart 2007). Matching itself is no substitute for randomization: it relies on an assumption of ignorability. Yet in this case, the analyses can couple matching with the regression discontinuity design to reduce the threat of model dependence. *To the extent that different methods relying on different assumptions and even different data sets recover similar estimates of the impact of Section 203, we can be still more confident in the results.* The county-level data are already sparse in the California case, so matching tests which further reduce the data set represent an especially high threshold of confirmation.

2004 Latino Turnout

To assess the influence of Spanish in American election procedures, this article analyzes two data sets with quite different advantages. It begins in this section with the Latino National Survey. The LNS provides individual-level data, eliminating any threat of unseen aggregation effects.[13] Its sampling frame covered 19 U.S. states and 495 separate counties, limiting the potential problems of model dependence and clustering. This broad sampling frame also enables us to see whether the results are specific to some states or regions. Still, the LNS voter turnout information is self-reported, a fact which might induce measurement error. And there is no clear way to assess the impact of Spanish-language assistance on election outcomes using the LNS. The LNS analyses here thus provide initial estimates that will inform the analyses of California's 1998 primary election in the sections to come.

Data and Analyses

The years 2005–2006 saw the largest political survey of Latinos to date, as the LNS completed phone interviews with 8,634 Latinos. Here, we focus on the 4,330 respondents who reported being U.S. citizens as of 2004 and whose county of residence was known. Sixty-seven percent of respondents lived in counties that were federally mandated to provide Spanish-language ballots and assistance. Of the two legal thresholds, the numerical one influences more Latinos nationwide: 47% of respondents in counties with Spanish-language assistance were covered due to the numerical threshold alone. For the percentage-based threshold, the figure is just 6%, with another 38% living in counties covered by both thresholds.[14] The dependent variable is a binary LNS question that asked respondents if they voted in the 2004 presidential election. Of that sample, 70.7% reported that they did.[15] Many respondents failed to provide their income or other demographic information, so to avoid eliminating 33% of the respondents, the analyses below use multiple imputation (King, Honaker, Joseph, and Scheve 2001).

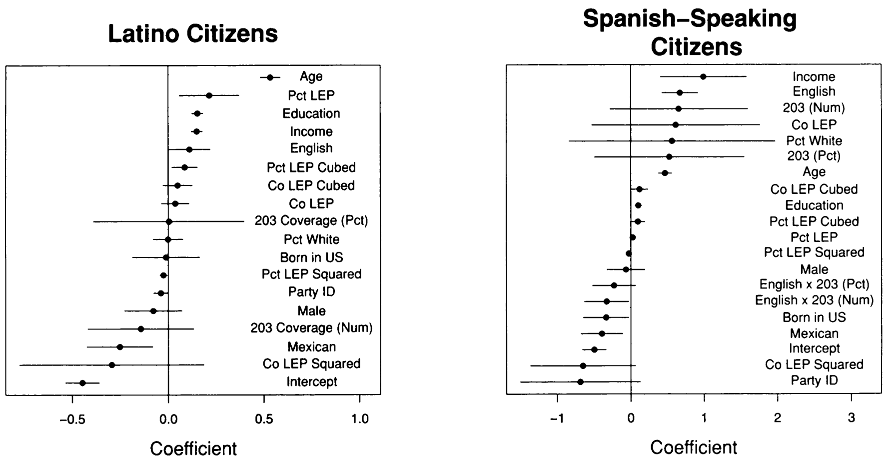
The first analysis models voter turnout with logistic regression. Using the logic of regression discontinuities, we can recover the impact of Section 203 coverage by conditioning on the county-level percentage of citizens who have limited English skills as well as the number

[13]The LNS provides the county of residence for all respondents. The Current Population Survey's November Supplements include a much larger number of Latinos, but they report county of residence only for those living in larger U.S. counties.

[14]The remaining 9% lived in counties covered at the state level. This 9% is a potential source of bias, as its selection into treatment is not accounted for by the continuous forcing variables. Yet in the initial analyses, its presence can only create a downward bias, since this small subset was actually treated but considered as if it were not. The core results below grow slightly *stronger* and remain statistically significant when we remove the 60 Spanish-speaking respondents in this category.

[15]Like other Americans, Latinos overreport their actual levels of turnout (Bernstein, Chadha, and Montjoy 2001; Cassell 2002), a fact which has the potential to bias our inferences. Yet there is no reason to believe that such misreports are more or less likely in counties on either side of the thresholds. Also, the analyses below confirm the results using California precinct-level turnout data.

FIGURE 1 Logistic Regressions Predicting Self-Reported Voter Turnout, 2005–2006 Latino National Survey



Note: This figure reports the results of logistic regressions predicting self-reported 2004 voter turnout. The left figure uses 4,330 Latino citizens, while the right figure uses a smaller set of 1,510 Spanish-speaking Latino citizens.

of such citizens. Following past practice in RDD estimation, this model also conditions on higher-order terms for both variables such as squared and cubed terms. As in Ferraz and Finan (2009), these initial models estimate the treatment effects induced by the two discontinuities separately. This procedure allows Section 203's impacts to vary based on the operative threshold. The other independent variables include those standard in turnout analyses (e.g., education, age, gender, income, and partisan identification) as well as variables that are specific to immigrants (e.g., English-language skills, birthplace in the United States, Mexican ancestry, and the county's percentage non-Hispanic white).[16]

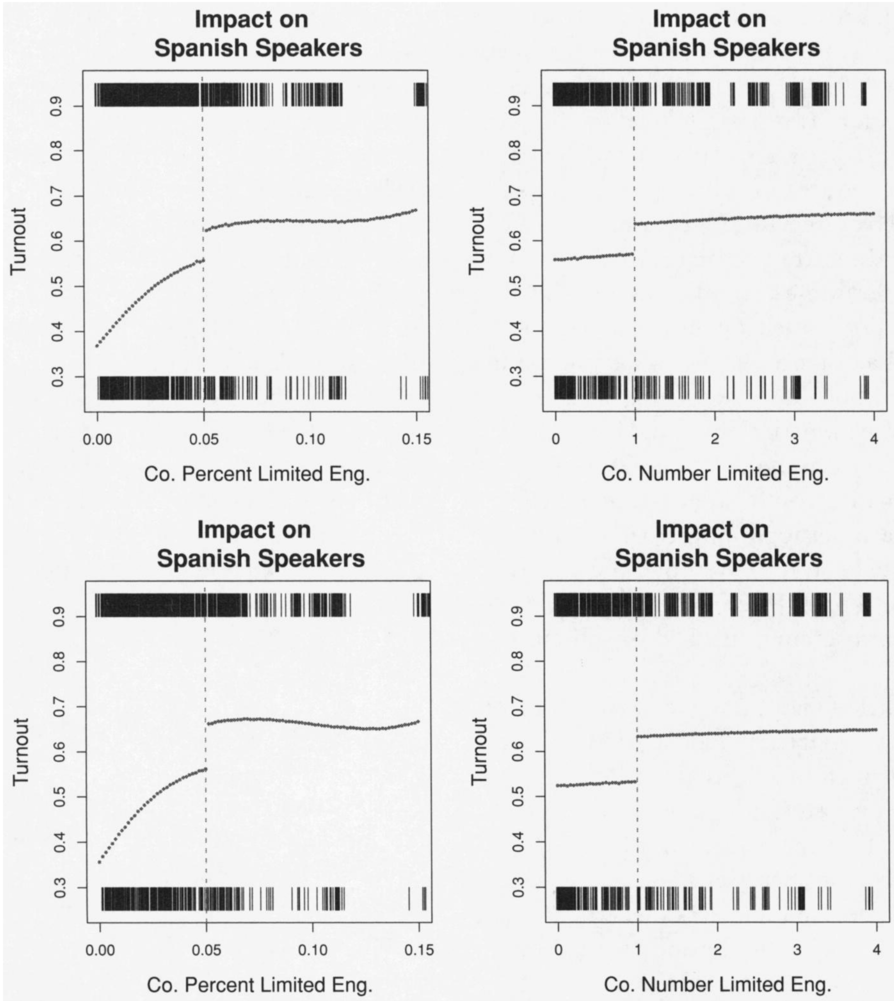
The fitted logistic regression is reported on the left side of Figure 1. Each dot represents an estimated coefficient, and the surrounding line indicates the 95% confidence interval. The results show little impact of Section 203 coverage across the population of Latino citizens, with a negative but insignificant point estimate for triggering the numerical threshold ($\beta = -0.15$, $se = 0.14$) and a near-zero estimate for the percentage-based threshold ($\beta = 0.02$, $se = 0.20$). These null results make some sense. Among all Latino respondents to the survey, 56% report speaking English very well or fluently. Yet among Latino citizens, the comparable figure is 85%. A significant majority of the voting-eligible Latino population doesn't need Spanish-language ballots, and we should expect little impact of Section 203 on that group. The core claim of de la Garza and DeSipio (1997, 95) is correct that demographics such as age and income are more reliable predictors of Latino turnout.

At the same time, the results on the left side of Figure 1 indicate that English-language ability is a significant positive predictor of turnout, even conditional on a host of other demographics. To explore the impact of language further, we removed those respondents who opted to answer the survey in English, leaving us with 1,510 Spanish-speaking citizens. We then estimated a logit model similar to that above but including interaction terms between English-language ability and the two indicators for crossing the Section 203 thresholds. The right side of Figure 1 presents the fitted model, again representing the coefficients with dots and the 95% confidence intervals with lines.[17] Both interactions are negative. The interaction for respondents in counties triggering the numerical threshold is statistically significant ($\beta = -0.32$, $SE = 0.16$), and the interaction for counties triggering the percentage threshold is nearly so ($\beta = -0.24$,

[16]The data are dense near the continuity. For example, 43% of respondents live in counties where the share of monolingual Spanish citizens is between 2.5% and 7.5%. Thirty-two percent of respondents live in counties where the number of such citizens is between 2,500 and 25,000.

[17]The 1,510 respondents live in 287 separate counties, and there appears to be very little county-level clustering. For instance, when adjusting the standard errors for county-level clustering (Wooldridge 2003), the standard error for the interaction between English ability and the numerical threshold changes imperceptibly, from 0.1522 to 0.1529. The results reported in this section use normal standard errors, but are essentially identical when using clustered standard errors.

FIGURE 2 Predicted Voter Turnout from Logistic Regressions, Spanish-Speaking Citizens, 2005–2006 Latino National Survey



Note: These figures report the results of two logistic regressions predicting self-reported 2004 voter turnout among 1,510 Spanish-speaking citizens. In each case, the model's predicted probability that a Spanish-only citizen votes is plotted as a function of a continuous, county-level variable. The legal discontinuities are shown with dashed vertical lines. Each figure also presents the distribution of voting for actual respondents using thin vertical lines near its borders. For the model presented at top, the treatment effect can vary based on the type of trigger. At bottom, it cannot.

$SE = 0.15$). As English-language skills improve, the impact of Spanish-language assistance declines.

The two panels on the top of Figure 2 give substantive meaning to these estimates by plotting a Spanish-only citizen's predicted probability of voting as a function of the continuous, county-level variables. At left, the forcing variable is the county's percentage of citizens who speak only Spanish, while at right, it is the number of such citizens. Along the top and bottom, each figure uses thin vertical lines to show the actual distribution of voters and nonvoters as the county composition changes. It is clear that the data are quite dense near both of the discontinuities. It is also clear that for Spanish-only citizens, the probability of voting increases markedly at the discontinuity, which is depicted by a dashed vertical line. For the percentage-based threshold, the estimated treatment effect is an increase of 6.8 percentage points in voter turnout when the percentage of the county's citizens that speak only Spanish moves from 4.99% to 5.01%. For the numerical threshold, the effect is almost identical, at 7.7 percentage points. Still, these estimated effects are very uncertain. The p-value for the hypothesis that crossing the numerical threshold has a negative impact on turnout is 0.25.

We might have expected this uncertainty ex ante. Even with a single treatment indicator, RDDs are known to be low in power due to the built-in correlation between the continuous forcing variables and the binary treatment indicator (Schochet 2009). Here, that problem is exacerbated by the inclusion of two separate treatment indicators with a polychoric correlation of 0.66. Yet we can easily modify the model to reduce this collinearity and improve efficiency. The legal requirements for counties covered by Section 203 are identical irrespective of which threshold led to coverage. In both cases, counties are mandated to provide the same set of written, Spanish-language materials as well as oral assistance. We can thus impose the restriction that conditional on the forcing variables, the impact of crossing the numerical threshold is the same as crossing the percentage threshold. Practically, this means that instead of estimating two separate treatment effects, we include a single indicator for whether the respondent lived in a treated county. Given that the estimated treatment effects for the two thresholds are within a percentage point, this restriction is justified by substance as well as statistics.

The bottom panel of Figure 2 replicates the discontinuity plots on the top, but it uses a model with the restriction that the treatment effect is constant. The figures and indeed the underlying results are quite similar to those above. But the restriction does reduce the estimated standard errors markedly. Consider a specific scenario, where a Latino citizen with median values on other independent variables reports little English ability and does not live in a covered jurisdiction. On average, we should expect her to report turning out to vote 55.1% of the time. In a covered county, however, that same figure is 66.2%, for a treatment effect of 11.0 percentage points on average. The treatment effect's 95% confidence interval remains wide, spanning from –7.7 to 31.0 percentage points. Yet this represents a notable improvement in efficiency: in 87% of simulations, the treatment effect is positive, indicating a one-sided p-value of 0.13.[18] As detailed in the appendix, these results prove quite robust to the removal of respondents far from the discontinuity and the use of multilevel models. At the same time, placebo tests do not detect discontinuities where they do not actually exist.

Section 203 does not influence the significant majority of Latino citizens who are fluent in English. Its symbolic impacts appear limited. But it does appear to increase self-reported turnout markedly among the smaller subset not fluent in English. For the 15% of surveyed Latino citizens who are not proficient in English, the impact of Section 203 is instrumental rather than symbolic. And it is an impact on par with other powerful turnout interventions, such as the 8 percentage point impact of the application of local social pressure (Gerber, Green, and Larimer 2008). Still, these results have considerable uncertainty: we would not want to draw policy conclusions from the LNS analyses alone. They are based on self-reported voting in an election one to two years prior to the survey, leaving open the possibility of measurement error. They also tell us little about whether these turnout effects influence election outcomes. To address these issues, we now turn to California's June 1998 primary election.

California's Proposition 227

To study the relative influence of language assistance and the threat it might induce, we now focus on a single California ballot proposition. This section provides background on the proposition before outlining the data set and the results. Proposition 227 passed with 61% support, and its passage curtailed the use of bilingual education in California public schools. In the 35 counties of interest here, more people voted on Proposition 227 than on any other ballot measure in that election, including in the gubernatorial primary that took place at the same time. In these counties, 33% of all registered voters cast ballots on Proposition 227. Since the vote took place in June 1998, all counties' federally mandated language policies had been fixed since 1992, allowing time for this information to diffuse.[19]

Research design considerations encourage us to focus on Proposition 227. As the largest state in the nation, California has hundreds of thousands of block groups, including tens of thousands which were covered by Section 203.[20] Still, given its tremendous social and

[18] Spanish-language respondents were asked, "How good is your spoken English? Would you say you could carry on a conversation in English?" The basic models include this variable as an ordinal, four-category variable ranging from four ("very well") to one ("not at all"). When we separately interact Section 203 coverage with each level of English-language ability, the model suggests that the positive impact is strongest for those in the second category ("just a little")—and that it is insignificant but *negative* for those in the third category ("pretty well"). However, because only 113 citizens put themselves in the "not at all" category, we do not have sufficient precision to identify whether the effect is truly nonlinear.

[19] Jurisdictions can also be covered under Section 4(f)(4) of the Voting Rights Act, but this applies primarily to Texas based on voter registration triggers from 1972.

[20] By statute, counties covered by Section 203 are listed in the *Federal Register* by the U.S. Census Bureau. The 1992 determinations

economic diversity (e.g., Baldassare 2000), we should not necessarily use data from every California county: we do not want regional variation to confound our estimated treatment effects.[21] In the 1990s, none of the seven northernmost counties in California was covered by Section 203. These rural, mountainous counties lack counterparts in the treated population and were discarded. At the same time, all but two of the counties in Southern California were covered by Section 203, so the 10 counties including and south of San Luis Obispo, Kern, and San Bernardino were dropped as well. This leaves us with 41 counties in the central region of the state, of which 11 were covered by Section 203 during the 1990s. These are counties for which the counterfactual—a change in Section 203 coverage—is most plausible. Figure A5 in the appendix illustrates which counties are excluded from the study, as well as those that provide block groups for the treatment and control groups. It is counties in the southern part of the Central Valley, such as Fresno and Tulare, that have the largest number of covered block groups.[22]

California has unparalleled election return data available through the Statewide Database maintained by the University of California at Berkeley (http://swdb.berkeley.edu/index.html).[23] Block group-level results from the 1994 general election and the 1998 primary election were combined with registration statistics from the same years, which provide aggregate voter ages, party registration statistics, and ethnicity imputed by last name.[24] Together, these data sets allow the analyses to condition on a rich battery of measures of neighborhood partisanship, one central covariate. Certain core census-based measures are available at the block group level, primarily racial and ethnic demographics. Block group-level ethnicity is a critical variable, so the availability of multiple measures of ethnicity from different sources is another advantage of this data set. At the census tract level, we can use the U.S. Census Bureau's Gazetteer to learn the location of each block group's corresponding census tract. A wide range of other census variables were matched from the tract level, including 2000 Census measures of race and ethnicity, language use, socioeconomic status, and population density. Table A1 in the appendix lists all of the tract- and neighborhood-level covariates. The availability of an extensive set of covariates is critical, as it can dramatically increase the precision of the resulting RDD estimates (Schochet 2009).

Each county has between 1 and 2,003 census block groups with election return information, with a median of 31. These are very small units: the average block group has 21 registered voters and 94 people. To reduce the challenges inherent in ecological inference and to focus our attention on the block groups of primary theoretical interest, the analysis then created a data set of the 6,097 block groups that are more than 50% Hispanic. De facto, this threshold excludes six additional counties, leaving 35. The average block group is 71% Hispanic, meaning that the effects among non-Hispanics would have to be substantial to produce misleading conclusions. By providing an individual-level benchmark, the results from the LNS analysis above also help us avoid ecological fallacies.[25] Future work could productively consider whether the impact of Spanish changes in more integrated neighborhoods.

California Results

We begin with the subset of data which is most likely to be positively influenced by Section 203: the 6,097 fully observed block groups where more than half of the registered surnames are Latino. Here, the independent

are available on page 35371 of Volume 58(125). The 2002 determinations were published on page 48871 of Volume 67(144) on June 26, 2002. In 2002, the Census Bureau's determinations based on the 2000 Census led California to be covered at the state level for the first time. Future studies could profitably consider whether Section 203's impact grows with the length of time a jurisdiction has been covered.

[21]On the methodological value of such data reduction in observational settings, see especially Rosenbaum (2009), which notes that "reducing heterogeneity reduces sensitivity to unmeasured biases"(284).

[22]There are several counties close to the legal thresholds, making the counterfactual quite reasonable. In fact, four years after Proposition 227, Colusa, Contra Costa, Madera, Merced, San Francisco, and San Mateo counties all gained Spanish-language coverage while Lake County lost coverage.

[23]The California Statewide Database collects and reaggregates election outcome data to the block group level. However, much of the demographic data (e.g., Hispanic surnames) and the political data (e.g., voter turnout, number of registrants) are available directly at the block level. The unit of analysis in this article is the block group, although it will use the term "neighborhood" interchangeably.

[24]For details on the process by which precinct-level election outcome data were disaggregated to the block group level, see McCue (2008). Note that this induces some measurement error in one dependent variable (the share supporting Proposition 227), but not in our measure of voter turnout or the independent variables. Among the 41 counties of interest here, the Pearson's correlation between the percent Hispanic as calculated by the 2000 Census and based on voters' last names is 0.67. Last name is an imperfect proxy for Hispanic ethnicity at the individual level, but at the aggregate level, it appears to work well.

[25]Moreover, since separate results not presented here show that the impacts on non-Hispanic whites are likely to be in the opposite direction (see also Barreto, Soto, Merolla, and Ramirez 2008; Hopkins, Tran, and Williamson 2009), to the extent that ecological inference is a problem, the results in the more heterogeneous Hispanic precincts are likely to be *understated*.

variables are 31 neighborhood-level measures, including both measures of block group-level politics (e.g., number of registrants, percent registered Democratic in 1994, percent registered Republican in 1994, percent registered Republican in 1998, percent registered with Korean surnames, etc.) and tract-level demographics (e.g., percent Black, percent Hispanic, percent immigrant, percent homeowner, logged median home values, median household income, percent on Social Security, percent moved in the last five years, etc.). With thousands of neighborhoods, the models can condition on a wide variety of neighborhood-level variables and improve precision with little downside. Yet the core results hold in much more limited neighborhood-level specifications, such as a model that includes only the block group's percent Democratic, its percent Republican, and its percent Hispanic.

At the county level, the independent variables include a single indicator for federally mandated language assistance. As with the LNS analyses above, to improve the efficiency of our estimates, we impose the restriction that the treatment effect is of the same magnitude irrespective of the discontinuity that triggered it. The independent variables also include the two continuous variables which determine treatment status, the county's percent and number of limited English voters.[26] The county-level independent variables also include higher-order terms for these variables.[27] By including the forcing variables and functions of them, *the models can account for any unobserved factors related to the assignment process.* The majority-Latino block groups are located in 35 distinct counties, making it important to ensure that estimates are not sensitive to the county-level specification choices. Still, we have considerably more leverage than would a county-level study: because of within-county heterogeneity, the effective sample sizes for the models of Latino precincts range from 459 to 1,299. Moreover, precinct-level data allow us to explore the possibility of heterogeneous treatment effects.

The first question is whether voter turnout is higher in Latino block groups with federally mandated language assistance. We do not want to attribute to Section 203 what is really the influence of unobserved county-level differences that are not well estimated by the forcing variables. The analyses of turnout thus began by adding a set of 15 possible county-level confounders to the basic model one at a time (not shown).[28] Overall, the estimated increase in turnout in counties with bilingual ballots ranges from 1.8 percentage points to 3.5 percentage points, but never nears statistical significance.

Yet we should keep in mind the LNS results above, as well as the caveat of de la Garza and DeSipio (1997) that most Latino *citizens* speak English and wouldn't need Spanish ballots to cast votes. Language skills might be a critical moderating variable. A second model interacts Section 203 coverage with a tract-level measure of English ability.[29] Figure 3 depicts the fitted model graphically, with county-level covariates indented and denoted by a "C."[30] An "R" indicates variables measured at the block group level, while a "T" indicates those measured at the census tract level. For each variable, the coefficient is represented by a dot. The surrounding line represents the 95% confidence interval.[31] The fourth coefficient from the top shows that Section 203 coverage predicts turnout more positively *in those neighborhoods with many Spanish speakers*: the interaction term is strongly positive and substantively large. The coefficient on the interaction is .093 with a standard error of .037 and a two-sided p-value of 0.01.[32]

[26]The 1990 Long Form Census data include cross-tabulations of people in a given geographic unit who speak Spanish by their level of English proficiency, allowing us to closely approximate the forcing variables that were used to make the 1992 Section 203 determinations. However, the legal thresholds are determined based on *citizens*, not people. In 2000, the Census Bureau did release the figures for citizens' language proficiency, and those data were employed to analyze the LNS data above. From those data, we know that the correlation between the number of Spanish-only citizens and Spanish-only residents is so high (0.97) that this measurement error is of little concern. For the percentages of Spanish-only citizens and residents, the comparable correlation is 0.90. The relevant thresholds do shift upwards as we move from citizens to residents, to a percentage threshold of 10.5% and a numerical threshold of 20,700 people.

[27]The initial specification included squared and cubed terms. The correlation between the squared and cubed terms for the number speaking limited English is 0.993, making the cubed term's inclusion unnecessary. The results are robust to the inclusion of an interaction between the two forcing variables as well.

[28]These county-level measures include the county's percent Hispanic, Democratic share in the 1996 presidential election, 1996 voter turnout, 1997 social capital score (Rupasingha, Goetz, and Freshwater 2005), 1991 crime rate, percent urban, population density, median household income, percentage on public assistance, percent immigrant, logged population, percent Black, percent white, geographic mobility, and number of labor unions.

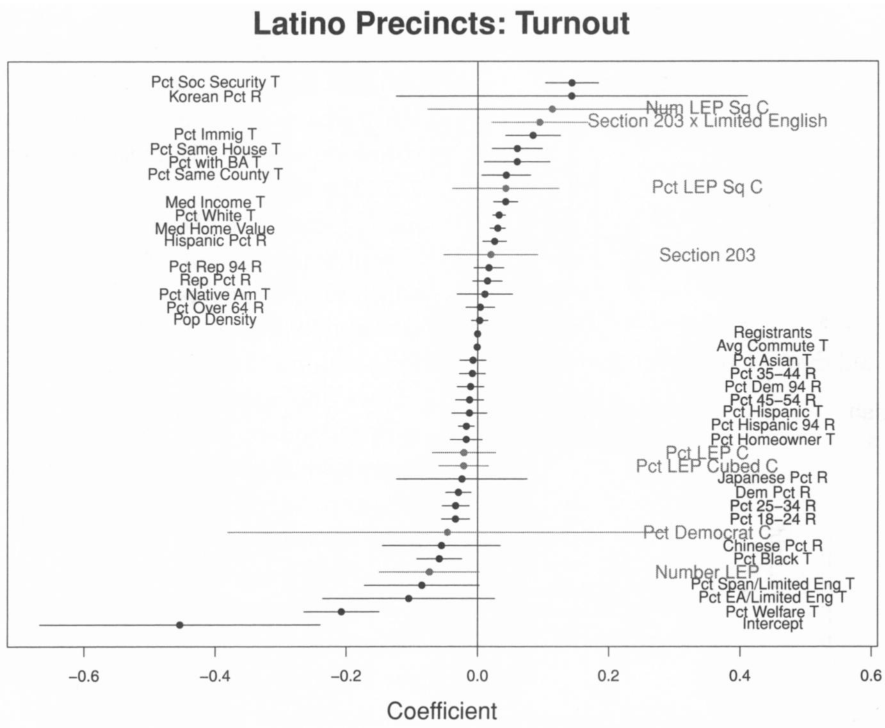
[29]In this case, we observe almost no variability across the 15 possible county-level specifications, and so the running example conditions on the county-level percent Democrat.

[30]Some of the county-level coefficients have been divided by factors of 10 to put them on a comparable scale.

[31]The intra-class correlation is 0.36, meaning that most of the variation is at the level of neighborhoods rather than counties.

[32]This result holds up substantively with a two-sided p-value of less than 0.10 when we systematically drop any of the 35 counties save Tulare. Located in the southern part of California's central valley, Tulare County provides 19% of the treated group, so the dependence of the results on its inclusion is not surprising.

FIGURE 3 Multilevel Model of June 1998 Voter Turnout, Majority Latino Precincts, Northern California



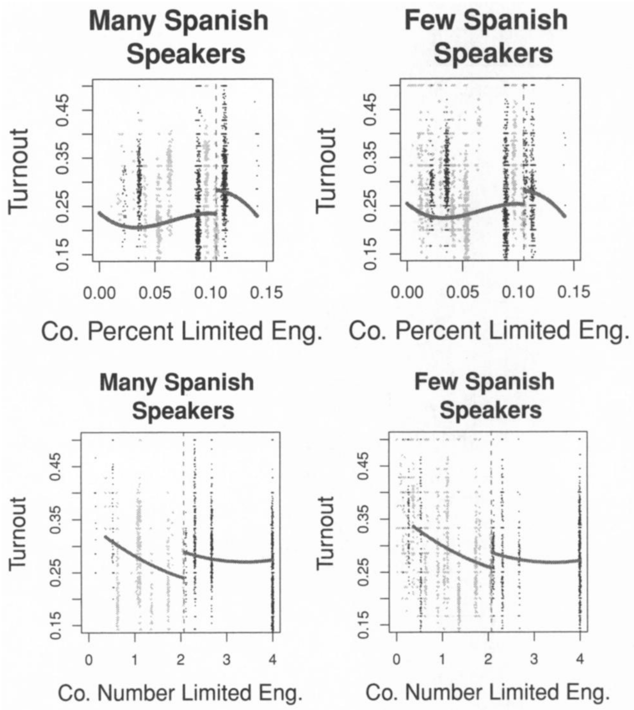
Note: This figure presents the coefficients from a fitted multilevel model of turnout in majority Latino neighborhoods. The county-level covariates of primary interest are indented. "LEP" stands for limited English proficiency.

Figure 4 allows us to understand the substantive magnitude of these effects by using the model to predict turnout as the forcing variables increase. At top, we see the predicted turnout as a function of the percentage of people in the county who speak only Spanish. The left side shows the impact of the discontinuity for a tract where 25.0% of residents speak only Spanish (95th percentile), while the right side uses the same model to show the impact where 2.9% of residents speak only Spanish (5th percentile). The actual discontinuities are plotted as vertical lines. Grey and black dots present the corresponding raw data, with grey dots indicating neighborhoods without Spanish-language assistance and black dots indicating neighborhoods with such assistance.[33] There are multiple ways to trigger federally mandated language assistance, so while every neighborhood to the right of the threshold has Spanish-language assistance, some precincts to the left do as well. At bottom, we replicate this figure using the same model for the numerical forcing variable. In both cases, there is a considerable number of observations in the region of the discontinuity—and that is true for neighborhoods with many Spanish speakers as well as those with few.

The sudden increases in turnout as units cross either the percentage threshold or the numerical threshold indicate that the treatment itself matters, apart from any influence of living near more Spanish speakers. First consider the 5th percentile neighborhood, where all but 3% of residents are proficient in English. With all other variables set to their medians, the expected turnout increase with Spanish election materials is 2.8 percentage points, with a wide 95% confidence interval from –6.0 to 11.7 percentage points. Now consider a neighborhood where 25% of residents speak Spanish but little or no English. There, we expect a turnout increase of 4.9 percentage points, as shown by the larger jumps in the plots on the left side of Figure 4. Put differently, *in counties covered by Section 203, the treatment effect grows by 2.1 percentage points* as the share of neighborhood that is not proficient in English increases. The 95% confidence

[33]For neighborhoods with many Spanish speakers, the plot shows the raw neighborhoods with above-average shares of Spanish-only residents. For neighborhoods with few Spanish speakers, it illustrates neighborhoods below the mean.

FIGURE 4 Predicted and Actual Turnout in June 1998, Majority Latino Precincts, Northern California



Note: These figures present predicted turnout from the model. At top, they show the change in turnout for neighborhoods with many Spanish speakers (left) and for neighborhoods with few (right) as the county's share of Spanish-only residents increases. At bottom, they replicate these results for the number of Spanish-only residents in the county (numbers in thousands). Grey dots (jiggered) illustrate actual neighborhoods without Spanish-language assistance while black dots illustrate neighborhoods with such assistance.

interval on the difference in treatment effects runs from –0.3 to 4.5 percentage points.[34] Section 203 did not have an overall influence in Latino precincts, but it had a marked influence in precisely those precincts where there are many Spanish speakers.[35]

In probing the mechanisms through which Spanish-language ballots might operate, it is valuable to ask about voter "fall-off" as well. To be sure, Proposition 227 was the final of nine initiatives on the ballot (for actual ballot images, see the appendix). But it generated the most attention and the largest number of votes in these Hispanic precincts, making it conceivable that people who were primarily motivated to vote on Proposition 227 then faced a decision about how much time to spend on other initiatives. Did the Spanish-language assistance lead a greater proportion of voters at the polls to continue voting on other ballot measures aside from Proposition 227? The answer is yes. Using the same model as in Figure 3, the analysis also examined the difference between overall voter turnout and turnout on Proposition 225, which sought congressional term limits. Overall turnout averaged 29.8% in these block groups, while turnout on Proposition 225 averaged 26.5%. When modeling the fall-off, we find a significant negative interaction of −0.066 with a standard error of 0.030. In neighborhoods with many Spanish speakers, we should expect the turnout difference to be 2.3 percentage points smaller when Spanish-language materials are available. The same negative interaction appears for Proposition 226, which proposed to restrict unions' political contributions.[36] The availability of Spanish-language ballots keeps people voting on other parts of the ballot in Spanish-speaking neighborhoods.

Political scientists commonly find that measures to expand the electorate have surprisingly limited impacts on the expressed preferences of the electorate (e.g., Citrin, Schickler, and Sides 2003; Highton and Wolfinger 2001). That is, changes in turnout do not always mean changes in election outcomes. In this case, however, the policy intervention targets a specific ethnic group, and so might influence both turnout and the initiative outcome. The dependent variable is now the block group's share of votes in support of Proposition 227. We use the same model specification as above and present the fitted model graphically in Figure A6 in the appendix. The model shows that more Hispanic neighborhoods were less supportive of Proposition 227, which is in keeping with individual-level survey results (e.g., Locke 1998). So too were neighborhoods with more Democratic registrants, while neighborhoods with more Republican registrants tended strongly in the opposite direction. But the critical finding is the interaction of neighborhood English proficiency and Section 203 coverage ($\beta = -0.19$, $SE = 0.07$, two-sided p-value = 0.006).[37]

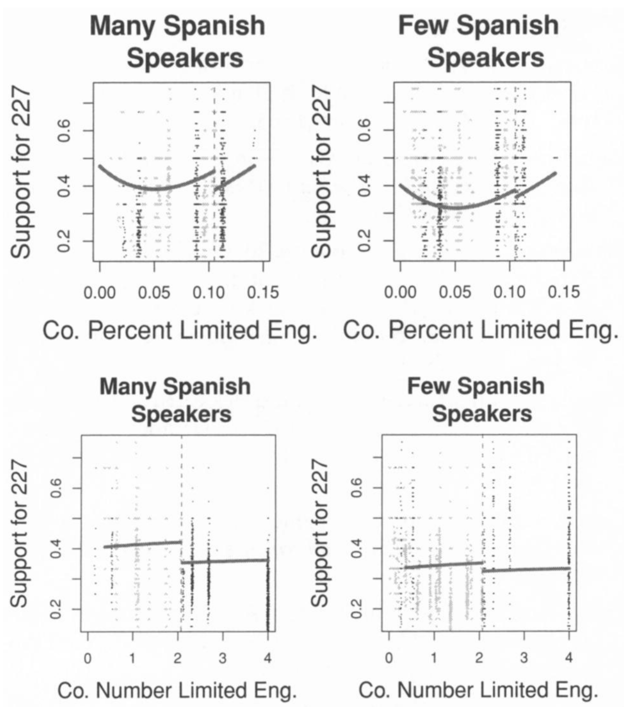
[34]The corresponding one-sided p-value is 0.05. Alternately, if we estimate a model only on the 30% of neighborhoods where more than 15.3% of residents spoke little English, and use no interaction term, we recover an estimated treatment effect of 9.5 percentage points ($SE = 4.9$).

[35]Heavily Spanish-speaking neighborhoods tend to be heavily foreign born as well: the Pearson's correlation between the two measures is 0.73. The interaction between Section 203 coverage and the neighborhood's percent foreign born is similarly robust, at 0.075 ($SE = 0.021$).

[36]Specifically, the estimated interaction when the dependent variable is the difference between overall turnout and turnout on Proposition 226 is –0.067 with a standard error of 0.027.

[37]This result holds at the $p < .05$ level when dropping any one of the 35 counties in the study. Despite overlap with the actual treatment indicator, it does *not* hold when we perform placebo tests by falsely specifying the percentage threshold as 3% or 5% of residents or the numerical threshold as 7,500 or 30,000 residents.

FIGURE 5 Predicted and Actual Support for Proposition 227 in June 1998, Majority Latino Precincts, Northern California



Note: These figures show the impact of increasing a county's percentage (top) or number of residents who speak only Spanish (bottom) on support for Proposition 227. The grey dots (jiggered) show neighborhoods that do not have Spanish-language ballots, while the black dots show neighborhoods that do.

Figure 5 presents the predicted share supporting Proposition 227 as either the county's percentage or number of Spanish-only residents increases. We begin by considering predicted support for 227 in neighborhoods with few non-English speakers, as depicted on the right side of the figure. At the cusp of the two thresholds, in counties with no Spanish-language election materials and few non-English speakers, the expected share in support of Proposition 227 is 38.2%. That number drops by 2.7 percentage points, to 35.4%, in counties with Spanish-language ballots. Looking at neighborhoods where one-quarter of the residents are not English proficient, however, we see a much larger treatment effect of 6.8 percentage points. Again, Spanish-language ballots appear to have a differential impact in neighborhoods with many monolingual Spanish speakers. The two treatment effects differ by 4.1 percentage points, with the 95% confidence interval on the interaction spanning from –0.3 percentage point to 8.5 percentage points ($p = 0.07$, two-sided). The availability of Spanish-language ballots reduces the share of voters supporting an end to bilingual education. And its impact is pronounced in precisely the neighborhoods where it has an instrumental impact: neighborhoods with many people who are not proficient in English. Spanish-language ballots can influence election outcomes as well as turnout.[38]

One potential concern is the assumption, embedded in the model above, that the effects are the same for Spanish-speaking neighborhoods and English-speaking neighborhoods. To relax that assumption, the analyses again considered only the 1,811 neighborhoods where the share of Spanish-only residents is above the 70th percentile.[39] In that subset, the same model with no interaction term recovers an estimated treatment effect of –9.4 percentage points ($SE = 4.8$, $p = 0.05$ two-sided). Thus, the interaction is not driven by the assumption that the forcing variables' relationship to the outcome is identical for Spanish-speaking and English-speaking neighborhoods. The core finding is further reinforced by using two types of matching to pre-process the data and improve the balance across key covariates, as described in Appendix C. Irrespective of specific modeling decisions, Spanish-language election materials appear influential on the subset of neighborhoods with many Spanish speakers. Moreover, the effects on turnout and the actual election outcome are notably similar in magnitude.

Conclusion

When targeted to specific subgroups, small changes in election procedures can influence both who votes and what wins. In the case of Section 203, this analysis finds strong and consistent evidence of the incorporating impacts of language assistance at the polls. Spanish at polling stations clearly has an instrumental use for those who speak little English. Still, future work at the individual level is critical to isolate the conditions under which seeing Spanish is likely to produce symbolic effects among Latinos or backlash effects among other groups.

The analyses here are the first to exploit the discontinuities in Section 203 coverage and are the first to find

[38]Given the limited number of counties in the data, it is important to test these results' robustness and to pay special attention to the county-level specification. One can include any of the 15 county-level covariates named in footnote 28 without any notable change in the interaction. A robust interaction also appears in models that vary the specification of the forcing variables, including models with cubed terms for the county's number of Spanish speakers with limited English or with interactions between the two forcing variables.

[39]The 70th percentile neighborhood is one where 15.3% of residents speak Spanish but little or no English.

effects concentrated among Spanish speakers. Among its findings, the article shows that majority Latino block groups with Spanish-language coverage and many Spanish speakers were up to 9.4 percentage points less supportive of Proposition 227 than were similar block groups across county lines. We see similar impacts on turnout and falloff. One might suspect that the impact of Spanish-language ballots would be especially pronounced in California in the 1990s, where considerable political mobilization occurred on racial and ethnic lines. Nonetheless, the mobilizing impact of Spanish-language assistance holds in a sample of Latino voters from across the United States in 2004 as well. Future studies could consider its impact in other states or other elections, developing our understanding of when and where Spanish assistance influences outcomes.

Bilingual voter assistance is an effective tool of immigrant political incorporation across states, and its substantive importance may grow as more immigrants naturalize. With 8 million U.S. citizens who are not proficient in English, these results provide one important explanation for why immigrants have lower rates of political participation than native-born Americans (Ramakrishnan 2005; Tam Cho 1999). Unlike many barriers to voting, language barriers fall on specific ethnic groups, meaning that their removal can influence election outcomes as well as turnout.

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Supporting Information

Additional Supporting Information may be found in the online version of this article:

Figure A.1: This image is the bilingual ballot for Fresno County, which was covered by Section 203 in 1998.
Figure A.2: This image is the ballot for Contra Costa County, which was not covered by Section 203 in 1998.
Figure A.3: This image is the cover from the sample ballot in Kings County, which was covered by Section 203 in 1998.
Figure A.4: This image is the sample ballot for San Joaquin County, which was not covered by Section 203 in 1998.
Figure A.5: The map at left illustrates California counties that are covered by Section 203 (left) from 1992 to 2001. The map at the right shows the counties which are excluded from the study (in grey), as well as treated counties (black) and control counties (white).
Figure A.6: This figure presents the coefficients from a fitted multilevel model of support for Proposition 227 in majority Latino neighborhoods in 1998.

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