

Language Access and Initiative Outcomes:
Did the Voting Rights Act Influence Support for Bilingual
Education?

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Abstract

This paper investigates one tool designed to enfranchise immigrants: foreign-language election materials. Specifically, it estimates the impact of Spanish-language assistance provided under Section 203 of the Voting Rights Act. Focusing on a California initiative on bilingual education, it tests how Spanish-language materials influenced turnout and election outcomes in Latino neighborhoods. It also considers the possibility of an anti-Spanish backlash in non-Hispanic white neighborhoods. Empirically, the analysis couples a regression discontinuity design with multilevel modeling to isolate the impact of Section 203. The analysis finds that Spanish-language assistance increased turnout and reduced support for ending bilingual education in Latino neighborhoods with many Spanish speakers. It finds hints of backlash among non-Hispanic white precincts, but not with the same certainty. The turnout finding gains additional support from multilevel regression discontinuity analyses of 2004 Latino voter turnout nationwide. For Latino citizens who speak little English, the availability of Spanish ballots increases turnout and influences election outcomes as well.

1 Introduction

In 2008, the U.S. was home to roughly 38 million immigrants (U.S. Census Bureau, 2008). Compared to previous generations, today's immigrants are dispersed across the country, as they have increasingly bypassed the traditional gateway cities in favor of new immigrant destinations (Frey, 2006; Jones-Correa, 2006). Together, these trends mean that many U.S. states and localities are becoming involved in immigrant political incorporation for the first time in generations. The challenge of incorporating newcomers into American politics is made tougher still by language differences: as of 2000, 14% of U.S. households spoke a language other than English at home, and 3% had no one who speaks English (U.S. Census Bureau, 2000*a*). Even among U.S. citizens, there were an estimated 8 million potential voters who spoke little or no English as of 2000 (U.S. Census Bureau, 2000*b*). 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 whites (Verba, Schlozman and Brady, 1995; Citrin and Highton, 2002).

One straightforward approach to immigrant political incorporation is to provide voting materials in foreign languages. Since 1975, voters in areas with concentrated populations of 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 (Jones-Correa, 2005; Jones-Correa and Waismel-Manor, 2007; Tucker and Espino, 2007). This paper considers the impact of Spanish language assistance on Hispanic voters. Can language access provisions influence election outcomes by increasing turnout and expanding the electorate?

Still, language assistance is as contentious as it is common. Consider the example of Carpentersville, Illinois, where local activists circulated a flyer asking, “are you tired of having to punch 1 for English?” just prior to a local election (Kotlowitz, 2007). In recent years, language has become a central point of contention in immigration debates, with native-born Americans commonly expressing concern or even anger about the public use of foreign languages (see also Bach, 1993; Horton, 1995; Schildkraut, 2001; Grey and Woodrick, 2005; Schildkraut, 2005; Paxton, 2006; Hopkins, Tran and Williamson, 2009). In attempting to incorporate immigrants into American politics, public policymakers face a potential dilemma. To the extent that they use

English, their attempts might not be understood. And to the extent that they use the immigrants' native languages, they risk provoking a Carpentersville-style backlash. Thus alongside its analysis of the mobilizing impact of language assistance on Spanish speakers, this paper also explores the possibility of a backlash by native-born voters.

To be sure, past research has considered the impact of Section 203 on voter turnout among Latinos, albeit with inconsistent conclusions (e.g. de la Garza and DeSipio, 1997; Jones-Correa, 2005; Ramakrishnan, 2005).¹ Still, this appears to be the first research to *exploit the discontinuities in coverage to identify the impact of Section 203 as separate from the impact of living in areas with more Spanish speakers*. Also, this seems to be the first research exploring the possibility of an unintended influence on non-Hispanic whites or an impact on election outcomes. There is good reason to suspect that the sight of a foreign language at a polling place might influence English-speaking voters as well. As Section 2 details, research has documented the influence of seeing or hearing foreign languages on attitudes that are expressed immediately afterward (e.g. Barreto, Merolla and Ramírez, 2007; Barreto et al., 2008; Hopkins, Tran and Williamson, 2009). Given how commonly the use of Spanish provokes negative reactions, it is quite plausible that seeing Spanish as one votes could sway voters' support on related ballot measures.

The ballot measure of primary interest here is California's Proposition 227, an initiative that restricted bilingual education in 1998. It passed with 61% support. The influence of using or seeing Spanish at the polls will hinge on the specific question before the voters, so by focusing on a measure about bilingual education, researchers can develop clear expectations about the direction of the impact for different ethnic groups. But this clarity is not the only reason that Proposition 227 provides such a valuable starting point in assessing the impacts of Spanish-language voter assistance. California's size means that even looking only at the northern half of the state, researchers can make use of data for tens of thousands of block groups from 41 counties. In the 1990s, some but not all of California's counties were required to make voting materials available in Spanish, allowing for a within-state comparison of block groups. Additionally, the California Statewide Database provides a rich set of covariates at low geographic levels of

¹For research on the implementation of Section 203, see Tucker and Espino (2007), Jones-Correa and Waismel-Manor (2007), and Government Accountability Office (2008).

aggregation. This richness allows researchers to separately investigate impacts on Hispanic and non-Hispanic white block groups, and it also significantly improves the precision of the resulting estimates.²

The California data set, research design, and statistical methods are detailed in Section 3. As amended, Section 203 mandates that counties provide language assistance if they cross key thresholds such as having a language minority that constitutes more than 5% of the citizenry or includes more than 10,000 citizens. Since researchers know the exact process by which units were assigned to treatment or control in this case, we are in the rare position of being able to eliminate concerns about selection into treatment. Specifically, *the main analyses below make use of these legal discontinuities to identify observably identical block groups that did or did not provide materials in Spanish*. Regression discontinuity designs are becoming increasingly common in the social sciences (Green et al., 2009; Imbens and Lemieux, 2008), due in large part to their ability to retrieve causal estimates from observational data (Cook, Shaddish and Cook, 2005). The analyses below also use multilevel modeling to accurately estimate uncertainty (Schochet, 2009; Gelman and Hill, 2006) given that Section 203 coverage is determined by county, not by block group. Separately, the core results are confirmed by matching precincts in covered and uncovered counties.

The paper separately considers the impact of language assistance on two sub-groups of special relevance: block groups that were over 50% Hispanic and block groups that were over 90% non-Hispanic white. Among the first, Section 4 finds that the availability of Spanish language materials influenced turnout on Proposition 227, *but only in block groups with many Spanish speakers*. Indeed, in a neighborhood with many Spanish speakers, the impact of Spanish-language ballot assistance on turnout grows to 9.7 percentage points. Propositions are commonly reading-intensive, so effects of this magnitude in areas with many non-English speakers are not surprising. The analyses find no overall impacts on vote choices, but they again observe effects on the subset of block groups with many non-English speakers. Here, heavily Spanish-speaking neighborhoods with Spanish ballots are 9.4 percentage points less supportive of Proposition 227

²The California Statewide Database collects and reaggregates election data to the block group level. The unit of analysis in this paper is the block group, although it will use the term “neighborhood” interchangeably.

then their counterparts without Spanish ballots. The instrumental impacts of Spanish-language ballots are marked.

On the symbolic impact of Spanish in non-Hispanic white neighborhoods, the results are suggestive but not conclusive. The analyses uncover a 1.8 percentage point increase in support for Proposition 227 in these neighborhoods, with a 89% probability of a positive impact. This impact proves fragile, although it is evidently concentrated in Republican neighborhoods. The tradeoff between the mobilizing and threatening aspects of foreign languages, so clear in examples like Carpentersville, is difficult to discern with sufficient precision. Spanish-language materials clearly mobilize Spanish-speaking Latinos, but they may threaten others as well.

To reinforce the turnout results, Section 6 considers the impact of Spanish-language coverage on Latino citizens across the U.S. It uses the 2006 Latino National Survey (LNS) to estimate the impact of having Spanish-language ballots on self-reported turnout. This analysis finds no overall effect, but again detects an effect among the small fraction of Latino citizens who speak little English. The fact that the same patterns emerge from aggregate and individual-level data sets—and from data sets from different geographic areas and different years—reinforces their credibility. The concluding Section 7 suggests that the consistent politicization of race and ethnicity in 1990s California might have limited the priming impact of Spanish on non-Hispanic whites. The political conditions under which Spanish-language ballots threaten non-Hispanic whites might be quite different from the conditions that lead them to mobilize Spanish speakers. It also discusses what these results mean for three strains of research: turnout, priming, and immigrant incorporation.

2 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 (Cain and Doherty, 2006; Barreto and Munoz, 2003; Cho, 1999), so lowering language barriers should expand the electorate (Tucker and Espino, 2007). Still, the question is whether the impact is

large enough to influence election outcomes. Effect size is also relevant when considering the extent to which Spanish at a polling place influences native-born voters. This section develops hypotheses, identifying the sub-groups likely to be influenced and the conditions necessary for such influence. It first considers the potential of Section 203 to mobilize voters who are not proficient in English and then considers whether it might threaten voters who are.

2.1 Language Assistance and Mobilization

Past empirical studies of the impact of Section 203 on voters with limited English proficiency 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, pg. 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.

Two more recent studies, however, report a positive relationship between Section 203 coverage and voter turnout. Ramakrishnan (2005, pg. 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.³ 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, and a higher probability of a Latino on the ballot (Leighley, 2001; Barreto, Segura and Woods, 2004). 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.

³For ongoing work on Section 203’s impact, see Fraga (2009).

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. 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. This 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 ballots. 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 have arrived 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. 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 matter. On average, Spanish speakers and English speakers must have different voting patterns. Certainly, this condition held in California during the 1990s, when ethnicity proved a strong predictor of support for several ballot measures (Cain, Citrin and Wong, 2000; Barreto and Ramirez, 2004; Campbell, Wong and Citrin, 2006). In the case of Proposition 227, although pre-election polls showed Latinos supporting Proposition 227, 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.

2.2 The Threat of Spanish

Define threat as a state of heightened concern about potential harm to one's group, interests, or values. The possibility that non-Hispanic whites are threatened by ethnic and racial minorities has long attracted scholarly attention (e.g. Key, 1949; Blalock, 1967). Yet research on threat has emphasized African Americans, and as a likely result, researchers have devoted comparatively little attention to the role of foreign languages in exacerbating threat (but see Schildkraut, 2005). In studies of new immigrant destinations, language emerges repeatedly as a flashpoint of tension (Hopkins, Tran and Williamson, 2009; Kotlowitz, 2007; Grey and Woodrick, 2005; Schildkraut, 2001; Bach, 1993). As the country's shared mode of communication, the English language serves as a potent political symbol for multiple American political traditions (Schildkraut, 2005; Huntington, 2004), and is easily observed in the day-to-day world. Foreign languages may provoke threat especially when used by public officials or as part of public actions. Such uses confer a legitimacy that grocery store signs do not.

But can a few signs at a polling place trigger a sufficient threat so as to influence voting decisions? There is good reason to think that they might. Survey experiments commonly show that subtle primes influence subsequent decisions, even if those primes are outside of people's conscious awareness (Mendelberg, 2001; Wheeler and Berger, 2007). Some such experiments have shown that brief exposures to the Spanish language can influence preferred candidates (Barreto et al., 2008) and expressed attitudes toward immigration (Hopkins, Tran and Williamson, 2009), at least among some sub-groups such as Blacks or Democrats. Survey experiments are commonly criticized for lacking external validity, but seeing a bilingual sign and then voting on multiple ballot questions seems reasonably similar to these experimental manipulations. In both cases, the manipulation is subtle but the opportunity to register one's preferences is immediate.

Also, voters may have been drawn to the ballot box by their interest in a particular candidate, meaning that when asked to vote on a plethora of other people and issues, they might rely on whatever heuristics are readily available (Popkin, 1994).

In a related vein of research, 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. The backlash hypothesis holds that non-Spanish speaking voters will be threatened by seeing Spanish at the polling site, and will be more likely to cast votes that seem to support English or to oppose immigration if such options are available. The impact of seeing Spanish might vary across groups, as those with a particular partisanship or prior level of contact with Spanish react more negatively. Unlike the mobilization hypothesis, however, *this hypothesis is most likely to hold for voters who came to the polls to vote on another ballot question*. Such voters, entering the polling booth without a fixed decision on the particular ballot question, are susceptible to environmental primes. This in turn suggests that backlash effects should not be accompanied by turnout effects.

3 Research Design and Data

To test the relative influence of language assistance and the threat it might induce, we focus primarily on a single California ballot proposition. This section justifies that choice, and outlines the specific methods employed. Proposition 227 passed with 61% support, and its passage curtailed the use of bilingual education in California public schools. In the 40 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 the counties of interest here, 33.1% of 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.⁴

Certainly, compliance with federal election law is not perfect, and specific forms of language assistance vary across jurisdictions. Given that, the analyses below are best thought of as “intent-to-treat” analyses that measure the impact of the federal policy as it stands rather than the impact given perfect implementation. However, the Department of Justice does contact covered jurisdictions to remind them of their legal responsibilities, and it does bring enforcement lawsuits as well. Moreover, two separate studies have found high levels of compliance with the requirements pertaining to translated written materials such as ballots and signs at polling places (Tucker and Espino, 2007; Jones-Correa and Waismel-Manor, 2007). In a survey of jurisdictions, Tucker and Espino (2007, pg. 189) found that only 13% of Spanish-covered jurisdictions provided neither written nor oral assistance to non-English speaking voters. The treatment is meaningful and observable: covered counties do in fact provide significant language assistance that would be observable to voters.⁵ Political actors are aware of the Section 203 determinations, so the estimated treatment effects might capture the impact of differing mobilization strategies as well. This fact creates no bias in the estimates below, but it does potentially impact their interpretation and their generality.

Research design considerations encourage us to focus on Proposition 227 as well. As the largest state in the nation, California has hundreds of thousands of block groups, including tens of thousands which were covered by Section 203.⁶ Still, we should not use data from every California county. In the 1990s, none of the seven northernmost counties in California were covered by Section 203. These rural, mountainous counties lack counterparts in the treated

⁴Jurisdictions 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.

⁵We 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 estimated 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).

⁶By statute, counties covered by Section 203 are listed in the *Federal Register* by the U.S. Census Bureau. The 1992 determinations are available on page 35,371 of Volume 58(125). The 2002 determinations were published on page 48,871 of Volume 67(144) on June 26, 2002.

population and were discarded. At the same time, all but two of the counties in southern California were covered by Section 203, so the ten 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 eleven were covered by Section 203 during the 1990s. Figure 1 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.⁷



Figure 1: These maps illustrate California counties that are covered by Section 203 (left), not covered by Section 203 (center), and excluded from this study (right).

3.1 Regression Discontinuity Design

Knowing the exact 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 the treatment. In this case, there are two key triggers that lead counties to be required to provide language assistance under Section 203. The first is if more than 5% of the county’s citizens are members of a language minority group and do not speak

⁷There 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, and San Francisco, and San Mateo counties all gained Spanish-language coverage while Lake County lost coverage.

English well. The second is instead if more than 10,000 of the jurisdiction’s citizens are members of a language minority group and do not speak English well. The law thus lends itself to a sharp regression discontinuity design (see Imbens and Lemieux, 2008; Angrist and Pischke, 2009; Green et al., 2009) comparing counties just above and just below the legal thresholds. The core idea of a regression discontinuity design (or RDD) is that observations just above and below the discontinuity can be treated as if they are randomly assigned: units slightly above the discontinuity should be no different on other confounders than units slightly below the discontinuity. When properly applied (Imbens and Lemieux, 2008; Green et al., 2009), *RDD can remove concerns about unobserved confounders, recovering causal estimates 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. Lee, Moretti and Butler, 2004; Leigh, 2007; Meredith, 2009).

In practice, however, RDD analyses face challenges that randomized experiments do not. There are never a sufficient number of data points that are arbitrarily close to the discontinuity, so researchers typically rely on models to estimate the relationship between the underlying continuous variable—sometimes referred to as the “forcing variable”—and the outcome. One practical implication of this is that RDD results can be model-dependent (Green et al., 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 thus require more observations to make inferences with the same level of certainty (Schochet, 2009).

The analyses below make two adaptations to classical RDD estimators. The first 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 the respective threshold. This involves conditioning on both of the forcing variables and their higher-order terms simultaneously.⁸ Yet we also must confront the disjoint between the unit of observation (block groups) and the unit at which Section 203 coverage is determined (counties). This paper does so via a multi-level

⁸For examples of other research exploiting multiple discontinuities, see Gagliarducci and Nannicini (2009) and Ferraz and Finan (2009).

model (Schochet, 2009; Gelman and Hill, 2006), where the county-level impact is a function of Section 203 coverage as well as the forcing variables and their higher-order terms. There are at most 41 counties, so there will inevitably be uncertainty in the modeled relationship between the forcing variables and the outcomes. Given this potential problem, the analyses include a series of robustness checks.

Specifically, the block group-level model for observation i in county j is:

$$y_{ij} = \beta_{0j} + \beta_1 x_{ij}^1 + \dots + \epsilon_{ij}$$

where y_{ij} is a continuous measure of the fraction of the block group's support for Proposition 227 (or its voter turnout) and where ϵ_{ij} is mean zero with a normal distribution. x_{ij}^1 indicates one of the independent, block group-level variables. At the level of counties, we also model:

$$\beta_{0j} = \gamma_1 w_j^1 + \dots + \delta_j$$

where δ_j is similarly mean zero, normally distributed, and uncorrelated with the independent variables w and x . To make this a regression discontinuity design, the county-level variables w_j must include the two forcing variables and the treatment indicator.

The number of California counties is limited, so as one set of robustness checks, the analyses below also include matching estimators as a pre-processing 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 et al., 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 can recover similar estimates of the impact of Section 203, we can be all the more confident in the results. The county-level data are already sparse, so matching tests represent an especially high threshold of confirmation.

3.2 Data Set

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>). 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.⁹ 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 another critical variable, so the availability of multiple measures of ethnicity from different sources is another advantage of this data set.¹⁰ 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 1 in the Appendix lists all of the tract-level 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 45 and 13,962 census block groups with election return information, with a median of 2,029. These are very small units: the average block group has 43 registered voters and 91 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 creates two data sets: one of 27,547 block groups that are more than 90% non-Hispanic white, and one of 6,097 block groups that are more than 50% Hispanic. It focuses in part on non-Hispanic whites because they are by far the largest group in the block groups of interest, constituting on average 61% of the population. Also, past research indicates that threatened responses are most common among this group, making it a valuable starting point.

⁹For details on the process by which precinct-level data were disaggregated to the block group level, see McCue (2008).

¹⁰Among 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.

4 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 variables $x_{ij}^1, \dots, x_{ij}^{31}$ 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.). At the county level, the independent variables w_j^1, \dots, w_j^7 include an indicator for federally mandated language assistance. They also include the two continuous variables which determine treatment status, the county’s percent and number of limited English voters.¹¹ In RDD terminology, these variables are called “forcing” variables, as their values force the county into a given treatment status. The county-level independent variables also include higher-order terms for these variables.¹² 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.

The first question is whether voter turnout is higher in Latino block groups with federally mandated language assistance. With thousands of neighborhoods, the models can condition on a wide variety of neighborhood-level variables and improve precision with little downside. However, at the county level, we have to be especially attentive to issues of specification given

¹¹The 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 are employed in Section 6 below. From those data, we know that the correlation between 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.

¹²The 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.

the threat of model dependence. 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.¹³ Overall, the estimated increase in Proposition 227 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 caveat of de la Garza and DeSipio (1997) that most Latino *citizens* speak English, and wouldn't need Spanish ballots to cast votes. A second model interacts Section 203 coverage with a tract-level measure of English ability.¹⁴ Figure 2 depicts the results, with county-level covariates indented and denoted by a "C."¹⁵ An "R" indicates variables measured at the block group level, while "T" indicates those measured at the census tract level.¹⁶ The figure shows that Section 203 coverage positively predicts turnout *in those neighborhoods with many Spanish speakers*: the interaction term is strongly positive and substantively large. First consider a neighborhood where all of the residents are proficient in English. There, the expected turnout increase on Proposition 227 given Spanish language election materials is 2.0 percentage points, but with a wide 95% confidence interval from -5.2 percentage points to 9.1 percentage points. Now consider a neighborhood where 34% of residents speak Spanish but little or no English, which is the maximum value in the observed precincts. In that neighborhood, we expect a turnout impact that is 3.2 percentage points *greater*, with a 95% confidence interval from 0.7 percentage points to 5.8 percentage points. Alternately, if we estimate a model with only on the 30% of neighborhoods where more than 15.3% of residents spoke only Spanish, we recover a treatment effect of 9.7 percentage points (SE=4.9). Section 203 did not appear to have an overall influence in Latino precincts, but it had a marked influence in precisely those

¹³ 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, its level of geographic mobility, and its number of labor unions.

¹⁴In 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.

¹⁵Some of the county-level coefficients have been divided by factors of ten to put them on a comparable scale.

¹⁶The intra-class correlation is 0.11, indicating that most of the variation is at the level of neighborhoods rather than counties.

precincts where there are many Spanish speakers.¹⁷ To be sure, these are neighborhood-level variables: the results do not tell us if the Spanish speakers were actually the individuals driving up turnout. But that is a reasonable inference, and will be confirmed with individual-level data below.

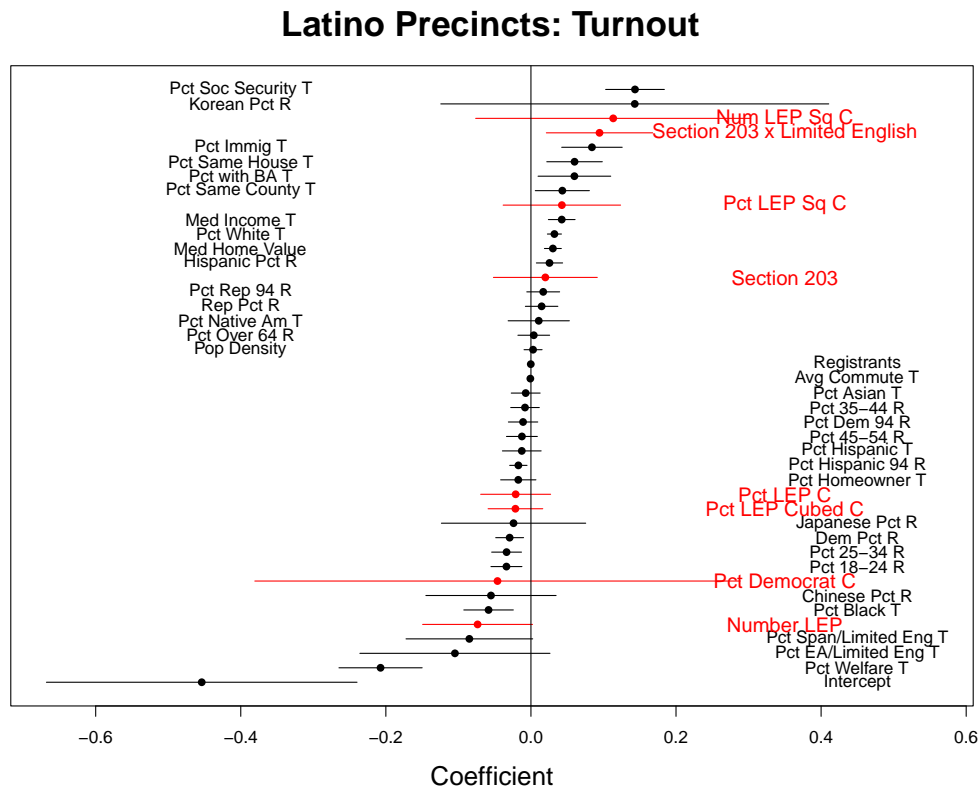


Figure 2: This figure presents the coefficients from a fitted multilevel model of turnout in majority Latino neighborhoods. The county-level covariates of primary interest are indented.

In probing the mechanisms through which Spanish-language ballots might operate, it is valuable to ask about voter “fall-off” as well. Did the Spanish-language assistance lead a greater proportion of voters at the polls to continue voting on other ballot measures? The answer is yes. Using the same model as in Figure 2, 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 percent in these block groups, while turnout on Proposition 225

¹⁷Heavily 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).

averaged 26.5 percent. When modeling the fall-off, we find a significant negative interaction of -0.066 with a standard error of 0.029. 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 availability of Spanish-language ballots keeps people voting down 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. Figure 3 tests this claim, using the same model specification as above. The dependent variable is now the block group's share of votes in support of Proposition 227. The figure 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 ($\beta=-0.18$, $SE=0.07$). In neighborhoods with no monolingual Spanish speakers, Section 203's impact was just -3.1 percentage points, with a 3.4 percentage point standard error. But in heavily Spanish-speaking neighborhoods, Section 203 had an impact that was stronger by -6.3 percentage points on the share of support for ending bilingual education. The 95% confidence interval runs from -10.9 percentage points to -1.8 percentage points. Having Spanish-language ballots can influence election outcomes as well as turnout.¹⁸

One potential concern is the assumption, embedded in the models above, that the coefficients are constant for Spanish-speaking neighborhoods and English-speaking neighborhoods. To relax that assumption, the analyses again considered only at the 1,811 neighborhoods where the share

¹⁸Given 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 13 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.

Latino Precincts: Vote For 227

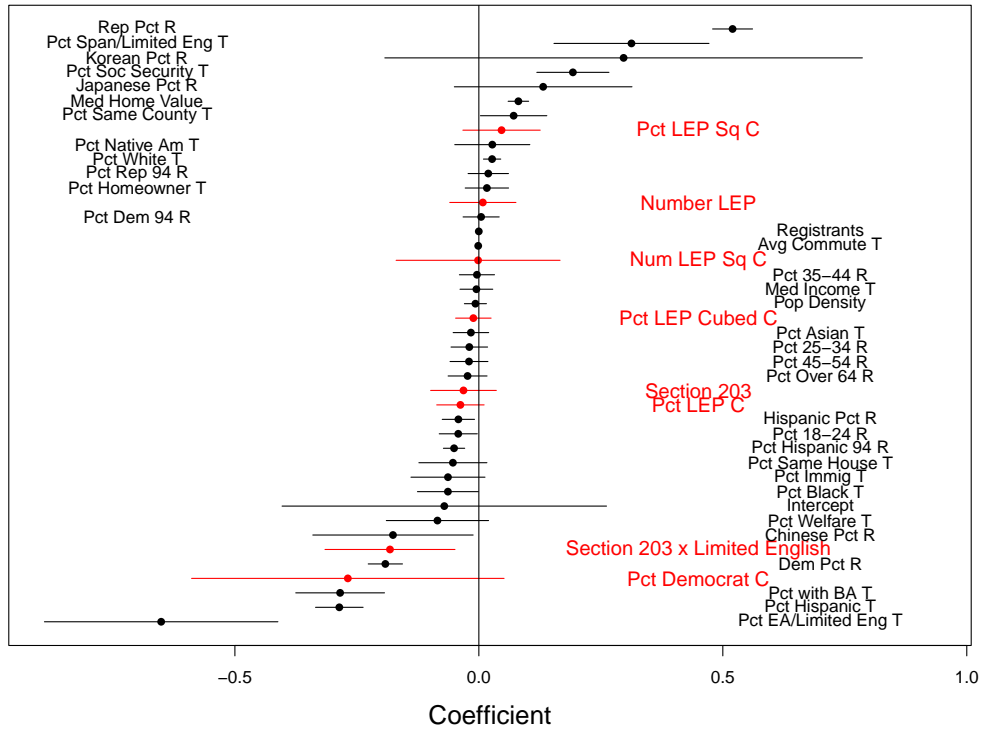


Figure 3: This figure presents the coefficients from a fitted multilevel model of support for Proposition 227 in majority Latino neighborhoods in 1998.

of Spanish-only residents is above the 70th percentile.¹⁹ In that subset, the same model with no interaction term recovers an estimated treatment effect of -9.4 percentage points (SE=4.2). This finding indicates that 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. 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. At the same time, no such interaction appears when predicting support for Proposition 225 or 226 (on unions’ political contributions), providing valuable placebo tests.

As a final robustness test, the analyses use either Coarsened Exact Matching (Iacus, King and Porro, 2008) or Genetic Matching (Diamond and Sekhon, 2005) to reduce the data set of Latino

¹⁹The 70th percentile neighborhood is one where 15.3% of residents speak Spanish but little or no English.

neighborhoods and improve balance on key covariates. This is an especially difficult test given the limited number of observed counties in the initial data set. Appendix A presents descriptive statistics for the two matched data sets, and shows the high levels of balance achieved. Coarsened Exact Matching reduces the data set by 56%, leaving 1,805 neighborhoods with bilingual ballots and 908 without. Even so, estimating a multilevel model uncovers the same robust interaction, with Spanish-speaking neighborhoods reducing their support of Proposition 227. The coefficient on the interaction is -0.82 (SE=0.26). Genetic matching leaves 1,028 neighborhoods without bilingual ballots and 4,414 with bilingual ballots, for a total reduction of 12%. It, too, returns a strong interaction: -0.45 (SE=0.20). Using Genetic Matching, Spanish language ballots have an impact that is -15.2 percentage points greater in Spanish-speaking neighborhoods than in English-speaking ones. The 95% confidence interval runs from -2.2 to -28.5 percentage points. The sight of Spanish impacts neighborhoods with many Spanish-speakers, and that finding holds on well balanced subsets of the data.

5 Majority White Block Groups

The paper now considers the impact of Spanish-language ballots on non-Hispanic whites, and the possibility of native-born backlash. The analyses employ the same research design as above, but instead focus on block groups that are more than 90% non-Hispanic white. The 90% figure makes these homogeneous neighborhoods, and also bounds the threat of ecological inference. For this subgroup, the expectation is that Spanish-language polling places will not influence turnout, but that they might just influence election outcomes.

The first expectation proves true: in a multilevel model with the same 41 total covariates as the main models above, we find no impact whatsoever on turnout overall ($\beta=1.2$ percentage points, SE=3.5) or on turnout on 227 ($\beta=0.8$ percentage points, SE=3.3). There is a hint that falloff on 227 might be *higher* in counties with bilingual ballots, but the standard error (0.39 percentage points) eclipses the estimated impact (0.41 percentage points). The Spanish-language ballots and signs did not appear to keep voters in non-Hispanic white neighborhoods at the polling station.

Still, given that voters arrived at the polls, did the sight of Spanish at certain polling stations lead to more support for Proposition 227? Figure 4 illustrates an answer. It shows that block groups with Spanish-language materials on average were more supportive of Proposition 227 by 2.1 percentage points, with a standard error of 1.8 percentage points. Simulations show that the impact is positive in 89% of simulations, for a one-sided p-value of 0.11. *The impact is positive—Spanish at the polls appears to induce more support for ending bilingual education—but we cannot rule out a negative impact with sufficient precision.* Based on this finding, the data indicate that a “backlash” effect is more probable than its opposite. But they do not do so with much precision. Perhaps not surprisingly, the results grow smaller and still less statistically significant when using either variant of matching as a pre-processing step.²⁰

Non-Hispanic White Precincts: Vote For 227

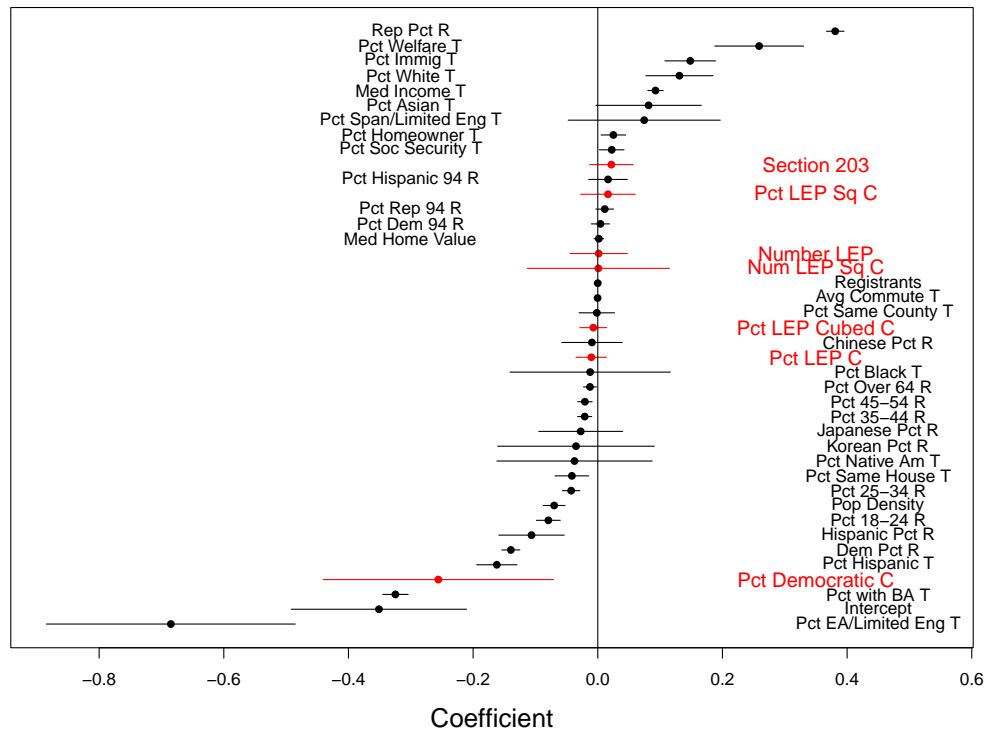


Figure 4: This figure presents the coefficients from a fitted multilevel model of support for Proposition 227 in 90% non-Hispanic white neighborhoods in 1998.

²⁰Specifically, Genetic Matching preserves the 5,871 treated block groups and finds 3,382 block groups without bilingual balloting to serve as control units. Its estimated treatment effect is 0.018—almost identical to that above—but with a 0.022 standard error.

Looking for a backlash across all heavily white precincts might be unreasonable. Hopkins, Tran and Williamson (2009) find that a brief line of Spanish on an exit poll led to more anti-immigration views only among Democrats. It is plausible that the Spanish language cue would be particularly influential on those who might otherwise have opposed Proposition 227. Given that, a subsequent analysis considered the interaction between Section 203 coverage and neighborhood-level partisanship as a predictor of Proposition 227 support. Figure 5 depicts the results. The estimated interaction is quite strong ($\beta=6.3$, $SE=0.8$), and is positive. Among the heavily white neighborhoods, Section 203 has more of a positive impact in Republican neighborhoods than in Democratic ones. Compare a neighborhood where 16.6% of the registrants are Republican (10th percentile) with a neighborhood where 69.0% are Republican (90th percentile). Given that change, the estimated increase in the impact of bilingual ballots is 3.3 percentage points. The 95% confidence interval runs from 2.5 percentage points to 4.1 percentage points, indicating a robust positive effect. In contrast to the overall backlash effects above, these results *do* hold for matched subsets of the data.²¹

Like those above, the data are aggregated, making any individual-level inferences tentative. With only aggregate data, we cannot observe whether it is the Democrats or the Republicans who are actually responding to the Spanish-language materials within Republican neighborhoods. We can, however, look at the Republican gubernatorial primary, in which only Republicans could vote. There, too, we observe an interaction effect indicating that Spanish-language ballots are more influential in heavily Republican areas ($\beta = 0.035$, $SE = 0.016$). This result suggests that in a political environment that was ethnically and racially charged (Pantoja and Segura, 2003; Pantoja, Ramirez and Segura, 2001), Spanish might well have acted as a partisan cue in the uncontested Republican primary, and in the voting on Proposition 227 as well. For Spanish-speaking Latinos, the use of Spanish at polling stations is largely instrumental, while for white Republicans it appears entirely symbolic. Confirmation of this possibility will have to await individual-level testing.

²¹In fact, this finding proves highly robust: it holds when looking only at majority Republican neighborhoods, when dropping any one county, when using either form of matching discussed above, or when conditioning sequentially on fifteen county-level covariates. At the same time, we find no impact or a markedly smaller impact on other ballot propositions.

Non-Hispanic White Precincts: Vote For 227

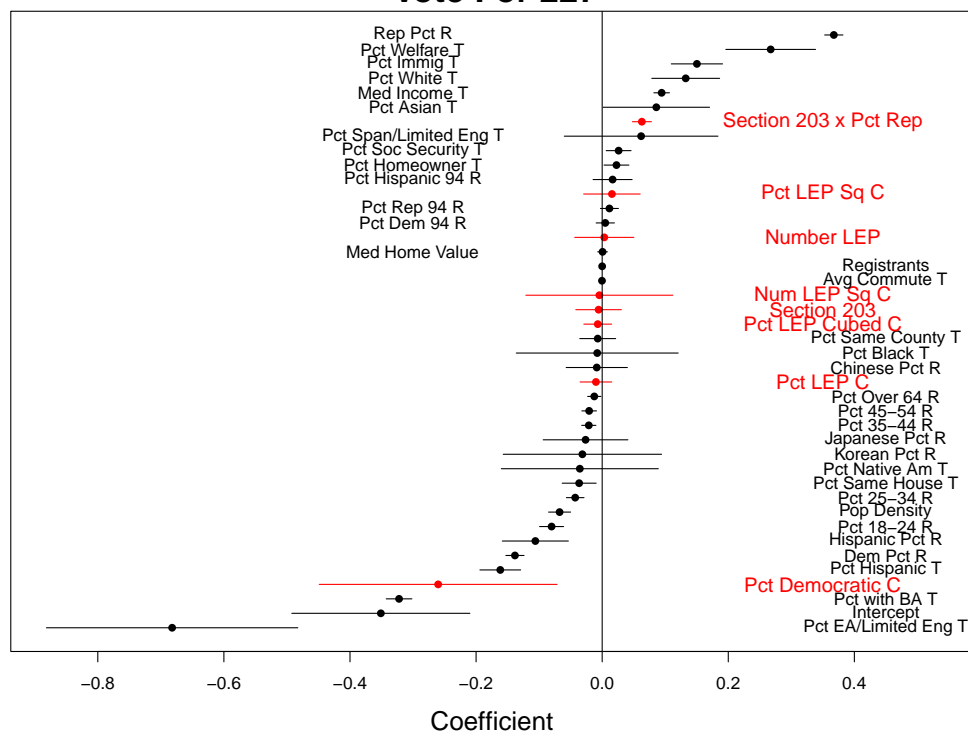


Figure 5: This figure presents the coefficients from a second fitted multilevel model of support for Proposition 227 in 90% non-Hispanic white neighborhoods in 1998.

6 2004 Turnout

The analyses of California are highly instructive, and they provide a rare opportunity to consider the impact of Spanish-language election materials on election *outcomes* and on non-Hispanic whites. Still, the estimates reported above are at the aggregate level, leaving us to wonder if they are the outcome of unseen aggregation effects. At the same time, the RDD analyses above are necessarily model-dependent, as there simply are not enough counties in California to avoid such dependence entirely. This section relaxes these assumptions, and again uses variants of regression discontinuity design to estimate the impact of Section 203 on self-reported voter turnout in the 2004 election. Does the finding of increased Latino turnout concentrated among immigrant-heavy block groups hold when considering the 2004 Presidential election? Does it apply outside of California? In both cases, the answer is yes.

6.1 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 in 19 U.S. states. Here, we focus on the 4,394 respondents who reported being U.S. citizens as of 2004. These respondents lived in 496 separate counties in 19 states, with 67% of the respondents' counties being mandated to provide Spanish-language ballots. The dependent variable is a binary LNS question that asked respondents if they voted in the 2004 Presidential election. 70.7% of the sample reported that they did. 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 (Schafer, 1997; King et al., 2001).

The first analysis of the LNS data models voter turnout using a logistic regression. Using the logic of regression discontinuities, by conditioning on the county-level percentage of citizens who have limited English skills as well as the number of such citizens, we can recover the impact of Section 203 coverage. Following past practice in RDD estimation, this model also conditions on higher-order terms for both variables such as squared and cubed terms. 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 U.S., Mexican ancestry, and the county's percentage non-Hispanic white).

The fitted logistic regression is reported graphically in Figure 6. It shows little impact of Section 203 coverage across the population of Latino citizens, with a negative point estimate. However, this makes 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, pg. 95) is correct that demographics such as age and income are more reliable predictors of Latinos' turnout.

Yet Figure 6 also indicates 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

Latino Citizens

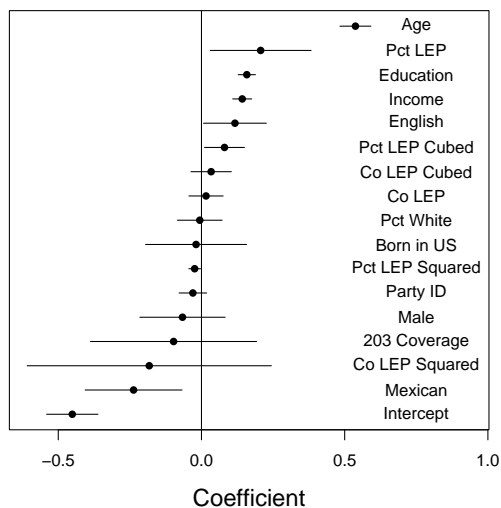


Figure 6: This figure reports the results of a logistic regression predicting self-reported 2004 voter turnout among 4,394 Latino citizens.

further, we remove those respondents who opted to answer the survey in English, leaving us with 1,574 citizens. We then estimated a separate logit model similar to that above but including an interaction term between English-language ability and Section 203 coverage, as presented in Figure 7. The standard errors are clustered by county due to the clustering of respondents (Wooldridge, 2003). The interaction is negative and significant ($\beta = -0.41, SE = 0.16$) indicating that as English language skills improve, the impact of Spanish-language ballots declines. This result also proves robust to the use of multi-level models with random county-level effects or to models with state-level fixed effects or random effects.²² Those findings should ease concerns about unobserved heterogeneity at higher geographic levels.

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, under the logistic regression model depicted in Figure 7, we should expect her to report turning out to vote 50.8% of the time. In a covered county, however, that same figure is 62.0%, for a treatment effect of 11.2 percentage points on average. There are relatively few citizens with such limited English, so the effect has considerable uncertainty: the 95% confidence interval runs

²²Multi-level models were fit using R’s `glmer()` function (Bates and Maechler, 2009). To enable convergence, the highly collinear cubed terms and the county’s percent non-Hispanic white were dropped.

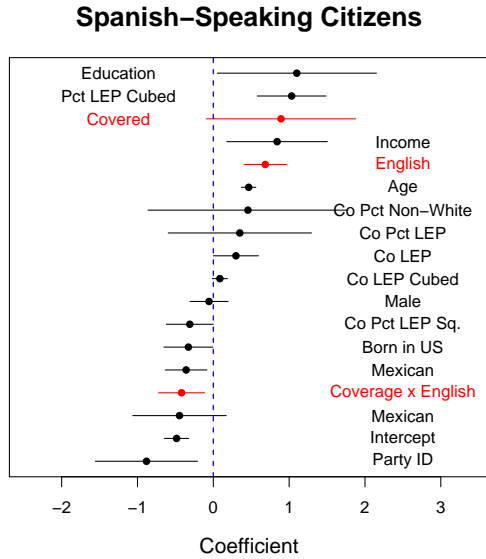


Figure 7: This figure reports the results of a logistic regression predicting self-reported 2004 voter turnout among 4,394 Latino citizens.

from -6.3 to 29.5 percentage points. Still, the p-value for the test that the impact on turnout is positive is 0.10. Section 203 does not influence the significant majority of Latino citizens who are fluent in English. But it does appear to increase self-reported turnout markedly among the smaller subset not fluent in English. This finding replicates what we uncovered in California in 1998 at the individual level. For Latinos, the impact of Section 203 is practical rather than symbolic: it hinges on language ability.

7 Conclusion

National and local officials can play a key role in immigrant incorporation (e.g. Lewis and Ramakrishnan, 2007). But they must walk a fine line, in that explicit uses of foreign languages risk provoking a backlash. In the case of Section 203 and voting, 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. The paper’s results are consistent with a symbolic impact on non-Hispanic whites as well, but hardly conclusive on that question.

The analyses here are the first to exploit the discontinuities in Section 203 coverage, and are the first to find effects concentrated among Spanish-speakers. Among its findings, the manuscript shows that majority Latino block groups with Spanish-language coverage and many Spanish speakers were 9.4 percentage points less supportive of Proposition 227 than were similar block groups across county lines. We see similar impacts on turnout and fall-off. In this case, the barrier to voting in question has a disproportionate impact on certain populations, meaning that its removal influences election outcomes as well as turnout. 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 as well. Bilingual voter assistance is an effective tool of immigrant political incorporation across states, and its substantive import may grow as more immigrants naturalize.

On the question of backlash among non-Hispanic white precincts, the results are less definitive. The data suggest a backlash effect of 1.8 percentage points in heavily non-Hispanic white precincts, with significantly larger effects in Republican precincts. The possibility that Spanish became a partisan cue is provocative. Still, future work at the individual level is critical to isolate the conditions under which seeing Spanish is likely to produce a Carpentersville-style backlash.

How do the findings on backlash fit with past work on language and priming, such as Barreto et al. (2008) and Hopkins, Tran and Williamson (2009) which both find priming effects of Spanish on non-Hispanic whites? Or the results of Berger, Meredith and Wheeler (2008), where voting in schools influenced support for school spending in the 2000 general election? Perhaps the most critical point is that Proposition 227 was a high-profile issue during the primary election, one that attracted more voters than anything else on the ballot. Thus white voters for and against it are likely to have arrived at the polling place with views on the subject, meaning that only a small subset of those voters were susceptible to priming or persuasion. In the four years from 1994 to 1998, California politics was dominated by issues that were ethnically or racially charged, from Proposition 187 (on public assistance to illegal/undocumented immigrants) and

Pete Wilson's 1994 re-election bid to Propositions 209 (on affirmative action) and 227. Those entering the voting booth in June 1998 might well have been aware of their own concerns about language and immigration issues, putting an upper bound on the impact that seeing Spanish could exert. In short, voters are less likely to be influenced by momentary primes on issues that are chronically accessible in their minds. The same ethnically charged environment might have magnified Section 203's impact on Spanish-speaking Latinos and dampened its impact among non-Hispanic whites.

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A Balance from Matching

	Mean Treated	Mean Control
% Republican R	0.183	0.189
% Hispanic R	0.727	0.685
% Korean R	0.000	0.000
% Japanese R	0.003	0.002
% Chinese R	0.004	0.005
% Hispanic T	0.674	0.596
% Black T	0.019	0.031
% Non-Hispanic White T	0.390	0.408
% Native American T	0.017	0.016
% Asian American T	0.050	0.049
% Immigrant T	0.338	0.328
% Homeowner T	0.530	0.540
% on Public Assistance T	0.108	0.111
% on Social Security T	0.229	0.237
% with Bachelor's T	0.077	0.083
% Same House '95-'00 T	0.529	0.525
% Same County '95-'00 T	0.852	0.835
Population Density T	0.002	0.002
% Democratic R '94	0.679	0.693
% Republican R '94	0.204	0.197
% Hispanic R '94	0.639	0.574
Ages 18-24	0.164	0.149
Ages 25-34	0.208	0.217
Ages 35-44	0.210	0.216
Ages 45-54	0.150	0.148
Above 65	0.140	0.143
% Spanish, Limited English T	0.044	0.034
% Asian Lang., Limited English T	0.002	0.004
Total Registrants R	24.794	22.451
Log, Median Home Val. T	11.584	11.660
Avg. Commute T	25.283	26.372
Log, Median Income T	10.374	10.408

Table 1: This table presents the balance in terms of means when matching 4,4414 Latino neighborhoods with bilingual balloting to 1,028 Latino neighborhoods without using Genetic Matching. “T” indicates tract-level variables from the 2000 census, while “R” indicates block group level variables from the 1998 Statement of the Vote.

	Mean Treated	Mean Control
% Republican R	0.132	0.141
% Hispanic R	0.735	0.690
% Korean R	0.000	0.000
% Japanese R	0.003	0.002
% Chinese R	0.003	0.005
% Hispanic T	0.701	0.624
% Black T	0.021	0.035
% Non-Hispanic White T	0.372	0.374
% Native American T	0.017	0.017
% Asian American T	0.050	0.056
% Immigrant T	0.359	0.356
% Homeowner T	0.518	0.498
% on Public Assistance T	0.113	0.121
% on Social Security T	0.232	0.242
% with Bachelor's T	0.072	0.068
% Same House '95-'00 T	0.536	0.530
% Same County '95-'00 T	0.866	0.836
Population Density	0.002	0.002
% Democratic '94 R	0.746	0.742
% Republican '94 R	0.140	0.147
% Hispanic '94 R	0.662	0.582
% Ages 18-24	0.163	0.142
% Ages 25-43	0.204	0.212
% Ages 35-44	0.198	0.211
% Ages 45-54	0.153	0.152
% Ages Above 64	0.145	0.148
% Spanish, Limited Eng. T	0.043	0.037
% Asian Lang., Limited Eng. T	0.002	0.005
Total Registrants R	26.448	24.054
Log Median Home Values T	11.602	11.602
Avg. Commute T	25.821	26.907
Log Median Household Income T	10.362	10.327

Table 2: This table presents the balance in terms of means when matching 1,805 Latino neighborhoods with bilingual balloting to 908 Latino neighborhoods without using Coarsened Exact Matching.