No Spending Without Representation: School Boards and the Racial Gap in Education Finance*

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Abstract

This paper provides causal evidence that greater minority representation on school boards translates into greater investment in minority students. Focusing on California school boards, I obtain causal effects by instrumenting for minority (specifically, Hispanic) school board representation using the random order in which candidates appear on election ballots. Given the dearth of school-level expenditure data, I introduce detailed records from California's School Facility Program (SFP), a capital investment program for which I observe how school boards allocate the marginal dollar within-district. Instrumental variables estimates show that an additional Hispanic school board member increases SFP-funded investments at high-Hispanic schools within the district by 66 percent, with significantly lower effects at low-Hispanic schools. High-Hispanic schools also exhibit gains in student achievement of 0.08 standard deviations. I attribute this improved performance to increased spending alongside decreased teacher churn: new hiring at high-Hispanic schools decreases by 16 percent. These results provide the first causal evidence that school board politics—and, specifically, school board ethnic composition—shapes education finance policy at the local level. I conclude that enhancing minority representation on school boards could help combat racial disparities in education finance and achievement.

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1 Introduction

Despite six decades of initiatives to improve racial equity in education finance, nonwhite students continue to face a resource gap relative to their white peers. Nationwide, the average white student benefits from as much as $2,200 in additional spending per year relative to the average nonwhite student (EdBuild 2019); nonwhite students are taught by relatively inexperienced teachers (Lankford, Loeb, and Wyckoff 2002; Scafidi, Sjoquist, and Stinebrickner 2007) and are more likely than whites to attend dilapidated schools (Alexander, Lewis, and Ralph 2014; Filardo, Vincent, and Sullivan 2019). As a result, nonwhite students experience high marginal returns to additional resources: school desegregation benefits these students mostly by exposing them to higher spending (Reber 2010), while higher spending mitigates the consequences of re-segregation (Billings, Deming, and Rockoff 2013). So why do policymakers not spend more on minority students?

It is not for lack of trying, at least at the federal and state levels. But while federal and state redistribution reduces *inter*-district funding gaps (Card and Payne 2002; Papke 2005; Roy 2011), these policies do not reduce *intra*-district achievement gaps (Cascio, Gordon, and Reber 2013; Hyman 2017; Lafortune, Rothstein, and Schanzenbach 2018). That disconnect could arise because locally-elected school boards—rather than legislators—allocate the marginal dollar within districts. School board members may represent voters who do not value investments in high-poverty or minority-majority schools, which limits the political impetus for action and could further marginalize those students. Yet, despite their remit to manage district budgets, there is no rigorous evidence showing how school boards' composition affects intra-district resource allocations.

In this paper, I investigate whether school boards with greater minority representation devote greater resources to minority students. Nonwhite board members may be more cognizant of inequitable spending than their white counterparts, and thus more willing to direct resources towards nonwhite students. Such behavior is consistent with a political model in which a candidate's core constituency comprises voters sharing her race/ethnicity, analogous to Glaeser, Ponzetto, and Shapiro (2005).[1] Conversely, if the median voter decides the election, then board members might adopt similar spending policies, regardless of their own ethnic identities. Greater minority representation could even reduce spending on minority students if ethnic diversity on the school board paralyzes decision-making, as Beach and Jones (2017) observe among city councils. Evidence supporting any of these hypotheses would clarify whether a lack of minority political representation helps drive the racial resource and achievement gaps in education.

To identify the causal effect of school board ethnic composition on intra-district spending outcomes, I need to address two empirical challenges. First, school-level spending data are often restricted to a single district, and there is scant variation in finance policy to identify how boards apportion the marginal dollar.[2] I therefore leverage data from California's School Facility Program (SFP), which since 1998 has provided matching funds for districts that choose to construct new schools or renovate existing ones.[3] Although capital spending accounts for only six percent of the median California district's budget, the program provides an opportunity to track the marginal discretionary dollar within districts, since SFP take-up and allocations depend on school board initiative. Using detailed, school-level data from the SFP, I illustrate how school board ethnic composition affects intra-district spending

1. The Civil Rights-era South highlights an extreme example of this kind of race-based electoral model. Cascio et al. (2013) argue that white-dominated school boards withheld Title I funding from *de facto* segregated Black schools, presumably to appease the (mainly white) electorate. While the authors' inference makes sense given their historical setting, my goal is to rigorously estimate the effect of school board ethnic composition on district resource allocation in a modern context with a racially diverse electorate.

2. Descriptive analyses of intra-district resource allocation using individual school districts include Jimenez-Castellanos (2010) and Rubinstein et al. (2007). Comparable descriptive studies using data from federal surveys of multiple districts include Heuer and Stullich (2011) and Ejdemyr and Shores (2017). All of these analyses find evidence that intra-district racial gaps in spending are just as prevalent as inter-district racial gaps in spending. However, none of these data sets include the universe of schools or districts (even within a given state); the nationwide data also suffer from inconsistencies in reporting standards that raise concerns about data quality and comparability across survey waves. On the other hand, Lane, Linden, and Stange (2018) use statewide data from Texas, and the authors report substantial within-district gaps in school facility quality and teacher turnover, two themes that feature prominently in this article as well. However, as with Hyman (2017)—who employs comprehensive, statewide data from Michigan (and causal identification)—the lack of recent variation in education finance policy in many states limits the usefulness of most existing data for my purposes.

3. Some of the SFP projects likely also appear in the samples of school facility investments used by Cellini, Ferreira, and Rothstein (2010) and Lafortune and Shönholzer (2018), although my work is, to my knowledge, the first to study the SFP specifically.

patterns.

The second hurdle for causal identification is that school board composition is endogenously determined via elections. Comparing SFP outcomes for districts with high and low minority school board representation will thus confound composition effects with factors that jointly drive the board's investments and election outcomes. For example, districts where minority candidates win school board elections might have low tax bases, which would limit a board's ability to make capital investments and lead to a spurious negative correlation between SFP spending and minority representation.[4]

To overcome this endogeneity concern, I utilize California's "random alphabets," which sort candidates on election ballots and exogenously impart high-ranked candidates with an electoral benefit. Echoing Shi and Singleton (2019), I instrument for Hispanic school board representation with a measure of Hispanic candidates' rankings on recent election ballots.[5] I show that my ballot order instrument is strongly correlated with post-election Hispanic school board representation but uncorrelated with other district characteristics, which bolsters my causal claim.[6]

I estimate my instrumental variables model using an original data set combining California school board election results from 1996-2007 with information on candidate ethnicity,

4. Such concerns raise doubts about the conclusions in Meier and England (1984) and Fraga et al. (1986), who take up the same topic as this paper, but rely on descriptive statistical techniques. Still, those studies—which show a correlation between Black and Hispanic school board representation, minority teacher hiring, and minority student college-going—provide a fitting backdrop for my work which, as I discuss below, confirms using causal inference that minority representation improves minority student outcomes, and goes one step further by showing how resource re-allocations underpin these gains.

5. I focus on Hispanic representation because Hispanics easily comprise California's largest ethnic minority group. Furthermore, from a practical perspective, identifying Hispanic school board candidates is easier than identifying, for example, Black school board candidates, as I briefly discuss in Section 3.

6. The ballot order IV has two advantages over the more popular regression discontinuity (RD) approach. First, a RD identifies an effect "local" to close elections that also have ethnically diverse candidate pools; the IV approach still relies on diverse candidate pools, but does not require a narrow election margin. Since competitive school board races are rare—and ethnic minority candidates are even rarer—I argue my estimates uncover a broader treatment effect. Second, a RD design might pose a challenge given my focus on race, since identification in a RD framework depends on the quasi-randomization of winning and losing candidates across close elections. Vogl (2014) demonstrates that winning minority candidates often increase voter turnout on election day, which undermines the assumption that close elections with different outcomes are comparable. By contrast, the ballot order instrument relies on randomization that occurs prior to election day, but after candidates must enter the race, limiting the potential for gamesmanship.

district SFP spending, and student demographic and performance data. Two-stage least squares estimates imply that, in the short term, a 20 percentage point increase in Hispanic board representation (equivalent to substituting one Hispanic member for one non-Hispanic member) spurs 35 percent more SFP investment ($146 per student) among schools with relatively high Hispanic enrollment within a district. Spending increases by an insignificant 15 percent ($64 per student) at low-Hispanic schools. I present evidence that this difference is statistically meaningful, while the magnitude of the gap underlines how school board composition shapes the investment decisions. This finding suggests that the lack of minority representation on school boards contributes to the racial gap in education finance.

I then examine how Hispanic board representation affects student achievement. 2SLS results imply that an additional Hispanic board member causes math scores to improve by 0.08 standard deviations at high-Hispanic schools, and by (an insignificant) 0.05 standard deviations at low-Hispanic schools; less precise estimates point to smaller but positive effects on reading scores. I argue that these achievement gains stem from increased investment in disadvantaged schools, including but not limited to improved facilities. I first provide evidence that schools receiving new SFP investment experience larger test score gains than schools without SFP investment, which suggests that improved facilities in themselves raise test scores, in line with similar evidence in the literature.[7] But I still find improvement among schools that do not receive SFP funds, which suggests other factors are at play. While I am again limited by the absence of school-level spending data, I show that, among high-Hispanic schools, Hispanic board members decrease teacher turnover, which leads to a 16 percent reduction in new teacher hiring and a 4 percent increase in average tenure. This result points to pecuniary or non-pecuniary investments in teacher retention, particularly at high-Hispanic schools, which reinforces my claim that minority board allocate resources to benefit under-served students.

7. Namely, Lafortune and Shönholzer (2018) and Neilson and Zimmerman (2014). Of course, in other policy settings, researchers have found no benefits to facility investments, notably Cellini et al. (2010) and Martorell, Strange, and McFarland (2016).

This paper makes two contributions to the literature. First, I provide new causal evidence that school board composition matters for intra-district spending allocations, which highlights school boards' importance in the education production function. This analysis builds upon a nascent literature which describes how school board members' identities—namely, their partisanship (Macartney and Singleton 2018) and professional backgrounds (Shi and Singleton 2019)—shapes local education policymaking. I also substantiate speculation from Cascio et al. (2013), Gordon (2004), and Hyman (2017) that racial equity in education finance depends on school boards, and that the success of redistributive education policies hinges on how local school boards choose to implement them. Moreover, my findings suggest that schools boards can re-allocate spending towards high-need students and promote student achievement, in contrast with the (relatively ineffectual and costly) Title I program.[8]

Second, this paper speaks to a growing literature which explores how strengthening racial and ethnic minorities' political voices affects the distribution of public resources.[9] Specifically, I show that racial gaps in political representation translate into racial gaps in public resource allocations, a result which is implicit in the literature but can be difficult to establish explicitly since the intended beneficiaries of public dollars often remain unclear. Above all, my findings indicate that improving minority political representation on school boards could help combat persistent racial disparities in education finance and achievement.

2 Policy Background

2.1 California School Boards

California has over 950 elected school boards, which makes it an ideal setting to look at the relationship between school board composition and intra-district spending outcomes. School boards in the state consist of three, five, or seven community members elected to administer a public school district. Board members' responsibilities include managing a

8. See, for example, van der Klaauw (2007) and Matsudaira, Hosek, and Walsh (2012).

9. In particular, Ananat and Washington (2009), Beach and Jones (2017), Cascio and Washington (2013), Hopkins and McCabe (2012) stand out.

district superintendent, negotiating staff and service contracts, and implementing a district budget (Hochschild 2005). Surveys report that boards spend most of their time on budget matters, allocating spending across different priorities and schools (Grissom 2007).[10]

Most California school board elections occur biennially, with a fraction of seats contested in each cycle. These contests are non-partisan: the ballot lists only candidates' names and self-described occupations. Most districts use an "at-large" election format in which voters district-wide select candidates to fill multiple seats. For example, in an election with two contested seats and five candidates, every voter can choose up to two candidates to support, and the candidates with the highest and second-highest vote shares will both win. All other districts use a "ward" model in which every school board seat corresponds to a constituency within the district, and only candidates who receive a plurality of the vote in their constituencies win. Once elected, school board members serve four-year terms and do not face term limits; they often receive nominal compensation for their time.

2.2 California's School Facility Program (SFP)

Introduced in the Leroy F. Greene School Facilities Act of 1998 (SB 50), the School Facility Program (SFP) provides state financial support for California school districts that choose to undertake capital improvement projects. Four statewide general obligation bonds—issued in 1998, 2002, 2004, and 2006—provided $35.4 billion in state funding for K-12 school construction and modernization. For most projects, the state contributes between 50 and 60 percent of the total cost; school boards usually issue bonds to pay for their share.

School boards must apply for SFP financing. Eligibility for new construction funds depends on the district's 5-year enrollment projections and its current classroom capacity. The state allots funds for each projected student in excess of the district's current capacity. For instance, a district is eligible for $12,197 per projected elementary school student in excess

10. Modern school boards have little discretion over tax rates, following a nationwide shift towards state-based funding in the 1990s and 2000s. In California, parcel taxes levied directly by school boards account for less than 10 percent of average district revenues; over 75 percent of funding comes from state transfers and property taxes, set by the legislature and county assessors, respectively. Even if school boards lobby counties for additional tax revenue, California's Proposition 13, passed in 1978, prohibits them from substantially raising property tax rates.

of capacity. Modernization funds are only available for schools that are at least 25 years old; the SFP provides funds for each student enrolled in the school being modernized ($4,644 per elementary school student, for example). Projects can also receive supplemental funding for, among other goals, relieving chronic overcrowding, installing earthquake safeguards, and acquiring land. Upon its completion, a project undergoes an independent audit, and the district submits an updated expense report to the state. These final expense reports form the basis of my SFP data, which I describe in the next section.

3 Data

In this paper, I evaluate the impact of minority school board representation on intra-district SFP spending patterns and student outcomes. Specifically, since my policy setting is California, I focus on Hispanic school board representation, since Hispanics easily comprise the state's largest ethnic minority group. The core of my data consists of school board election records, matched to information about candidates' and school board members' ethnicities. I link these election and school board composition data to school- and district-level SFP spending; school and district demographics; and student achievement outcomes.

3.1 School Board Elections Data

Data on California school board elections held between 1996 and 2007 come from the California Election Data Archive (CEDA). The CEDA records each candidate's name, vote total, incumbency status, and whether she won the race. I calculate candidates' vote shares as well as the number of contested seats and the number of candidates by election. The first panel of Table 1 describes these 3,738 elections. The median election outcome is nontrivial, with a 5 percentage-point victory margin. But the number of candidates ranges from 1 (i.e., an uncontested race) to 28, with a median of 4, which indicates that election competitiveness varies substantially.

3.2 Hispanic School Board Representation and Hispanic Candidates

For every district-year between 1996 and 2008, I measure Hispanic school board representation using lists of Hispanic school board members provided by the National Association for Latino Elected Officials (NALEO). The annual directories include school board members who confirm to the NALEO that they identify as Latino.[11] I define Hispanic school board representation in a given district-year as the number of Latino school board members (according to the NALEO) divided by the board size.[12]

To infer which school board candidates most likely identify as Hispanic, I use data from two sources. First, I match candidates' names to the NALEO directories for 1996 through 2018. I classify any candidate who appears in the NALEO directory (in any year) as Hispanic. Second, since the NALEO data do not include Hispanic candidates who never won a school board election, I also classify a candidate as Hispanic if more than 50 percent of respondents to the 2000 decennial Census with her surname self-identify as Hispanic.[13] I find that 84 percent of candidates in the NALEO directory also appear in my list of Census-inferred "Hispanic" surnames, which suggests my name-matching approach does a good job identifying losing candidates who would self-identify as Hispanic.[14] I prefer using both the NALEO and Census records—instead of just Census name matching—since the NALEO

11. I use the terms "Hispanic" and "Latino" interchangeably in this paper. As such, I assume that all of the candidates who self-identify as Latino also identify as Hispanic, and vice versa.

12. I assume that all school boards have 5 total members. In a sample of 1,168 district-years, I find that 90 percent of districts have 5 serving school board members while 6.7 percent have 7 members. The sample consists of 100 school districts whose board composition between 1998 and 2018 I researched using archived district web pages. (Due to data quality concerns and the impracticality of scaling this approach to remaining 850-odd California school districts, I do not use this manual search method to find all boards' compositions.) If I find more than 5 Hispanic members listed in the NALEO directory for a given district-year, I assume the board size is 7 since California school boards can only have 3, 5 or 7 members. While not ideal, I show that an alternative "treatment" which does not rely on the imputed board size—whether any Hispanic member holds a school board seat—also has a strong first first stage and yields quantitatively similar results. Since NALEO data from 1998 and 1999 are missing, I calculate Hispanic representation during these years by interpolating data from 1997 and 2000.

13. Unfortunately, neither the Census nor any other data set I am aware of provides a breakdown of ethnicity by first (given) name. This limitation means that, for example, I have no way to comment on the prevalence of Hispanic married women with non-Hispanic married names. By the same token, though, I note that non-Hispanic married women may take Hispanic surnames. The relative sizes of these groups is unknown, and so is the direction of the resulting bias.

14. The referenced match results appear in Appendix Table A2.

directory provides the more accurate snapshot of individuals who self-identify as Hispanic, which is a population of greater interest than just those with Hispanic surnames.[15,16]

The second panel of Table 1 summarizes Hispanic candidates' prevalence and performance in California school board elections. The average California school board election features less than one Hispanic candidate (0.66) and Hispanics comprise only 15.6 percent of all candidates in the average race. Hispanic representation on school boards (measured in the year of the election) is only 11 percent, which amounts to less than one member on the modal, five-seat school board. In the next subsection, I elaborate on the differences between districts where Hispanic candidates do and do not run for school board.

3.3 Student Demographic and Achievement Data

For academic years 1998-1999 and 2015-2016, I obtain student enrollment and demographic data from the Common Core of Data (CCD). The data contain total enrollment by year for every school within a district as well as enrollment by demographic subgroups (Hispanic, white, Black, Asian, and students who are eligible for free or reduced-price lunches [FRL]). The CCD censors school-year-subgroup cells with five or fewer students, which leads to missing observations in my final data set.

I also incorporate math and reading (ELA) exam data from grades 2-7 for academic years 1997-1998 through 2011-2012, provided by the California Department of Education (CDE).[17] The data contain the average school- and district-level exam scores by subject-grade; the

15. In Appendix Table A3, I show that I obtain similar, albeit quantitatively smaller and less precise, estimates when I use only the Census records to identify Hispanics. The smaller estimates and smaller sample under this approach suggest my NALEO directory approach helps me properly categorize candidates with non-Hispanic surnames who nevertheless identify as Hispanic.

16. Whether a candidate has a Hispanic-sounding surname and whether she self-identifies as Latinx are in fact distinct and deeply personal concepts. In Appendix Table A2, I show that 16 percent of self-identified Latinx school board members have surnames that might not be considered "Hispanic," which attests to the potential divergence between personal identity and family name. However, that 84 percent of officials in the NALEO directory *do* have majority-Hispanic surnames suggests that those candidates with majority-Hispanic surnames strongly correlate to those who self-identify as Latinx. For deeper qualitative and quantitative discussions (respectively) of this issue, see Donella (2017) and Lopez, Gonzales-Barrera, and López (2017). Also see Grofman and Garcia (2015) and Harris (2017) for short explanations of why using the majority ethnicity for a given surname is suitable to identify individuals' ethnic backgrounds.

17. I only consider grades 2-7 in because students begin tracking into different math classes, with different associated math exams, in 8th grade.

CDE censors school- and district-subject-grade-year cells with fewer than ten students, which again leads to missing observations in my final data set. In part to minimize the incidence of missing data, I aggregate test scores to obtain composite averages by school- and district-subject-year.[18] I normalize test scores using the statewide mean and standard deviation of district-by-subject scores, so the final units are district-subject-year standard deviations.[19]

In Table 2, I describe my district-level demographic and achievement variables during election years. The first column includes all elections in my sample, which I summarize in Table 1. The remaining columns decompose the sample into those elections with no Hispanic candidates (column 2), those with only Hispanic candidates (column 3), and those with a mix of Hispanic and non-Hispanic candidates (column 4). Comparing the first two columns indicates that districts whose elections feature no Hispanic candidates are positively selected: they have lower poverty rates (as measured by the share of FRL-eligible students) than the full sample as well as student achievement that exceeds the state average by 22-29 percent of a standard deviation. Conversely, districts whose elections feature any Hispanic candidates have higher shares of FRL-eligible students than the whole sample, and test scores there lag the statewide mean by 30-35 percent of a standard deviation. These comparisons demonstrate that Hispanic school board candidacies are negatively correlated with district affluence and student performance.

3.4 SFP Spending Data

My project-level SFP spending data come from final expense reports that district officials submit to the state. The data contain 7,641 unique project records including the name of the school district and school that secured SFP funds; categorized spending (for example, the amount spent on modernization and the amount spent on earthquake preparedness);

18. Beginning in academic year 2002-2003, the data also contain student performance by demographic subgroup. I do not use those data due to large volumes of missing observations and the fact that these outcomes are unavailable for most election years in my sample period. As such, I will not report achievement by Hispanic students per se.

19. I make this choice to ensure that all test score outcomes, no matter the level of aggregation, are comparable. School-level estimates measured in school-subject-year standard deviations show virtually identical results and are available upon request.

and the date when the project received its first funds from the state.[20] I define a project's starting date as the day the state first released funds. For each school- and district-academic year, I sum up total SFP spending on projects started in that year, as well as spending by project type.[21] I inflate all costs to 2016 dollars and normalize spending outcomes at the school- and district-level by total student enrollment in that academic year. To correct for implausibly low enrollment in certain observations, I winsorize school- and district-level SFP spending variables at their ninety-ninth percentiles.[22]

3.5 Comparing SFP Spending by Hispanic School Board Representation

I summarize my district-level SFP outcomes in Table 3. In the first column, I report average SFP spending among all district-academic years between 1999-2000 and 2015-2016. This full sample contains 13,900 district-academic year observations. In the remaining columns, I parse the sample into district-years based on the level of Hispanic school board representation. Interestingly, only 27 percent of district-years have any Hispanic board representation whatsoever, despite the fact that 38 percent of students in the average district are Hispanic.

Moreover, the data exhibit a negative correlation between school board Hispanic representation and district SFP spending per student. Among district-years with no Hispanic school board members, the board invests $365 per student in all SFP construction, while among boards with more than 20 percent Hispanic representation, total SFP spending falls to $300 per student. Similar patterns hold for SFP spending by project type (modernization, new construction, and supplemental). This negative relationship could be causal, or could be a function of the negative correlation between district affluence and Hispanic school board candidacies visible in Table 2. Disentangling the causal effects of Hispanic board representation on SFP take-up and spending from confounding factors—particularly school district size and affluence—will be the primary goal of my research design.

20. Summary statistics describing the raw data appear in Appendix Table A1.

21. For my purposes, an academic year begins on July 1, which is the beginning of the fiscal year for California school districts. Thus, for example, a project that begins on July 2, 2001 counts towards a district's SFP spending in academic year 2001-2002, while a project that begins on June 30, 2001 counts towards the district's SFP spending in academic year 2000-2001.

22. Alternative winsorization thresholds yield similar findings.

3.6 Final Sample and Panel Data Set Construction

My final sample contains 1,314 competitive school board elections that have both Hispanic and non-Hispanic candidates. I restrict the sample to these ethnically diverse elections so that I can compare elections in which Hispanic and non-Hispanic candidates each had a chance at winning. In so doing, however, I acknowledge that the "treatment" effect I uncover will be local to these ethnically-competitive elections, since districts that hold these elections differ substantially from the universe of California districts (see Table 2).

Around each of these 1,314 elections, I create a panel of data that spans eight years prior to the election through eight years after the election. Each observation in the panel is an election-period, which corresponds to a district-academic year.[23] The panel data set has an unbalanced structure, where periods may be missing for some elections but not others (e.g., for elections prior to 1997, I have no pre-election data). For a given election, all period observations share time-invariant data, such as the share of Hispanic candidates in the race and the number of contested seats. For each period, I also include time-varying district-academic year data, such as SFP spending and student achievement. Together, the election panels have an overlapping "stacked" structure, where a district-academic years can appear in multiple elections' panels. For example, all data from San Jose Unified School District in 1998 will appear in the panels for its 1996 election (as period +2), its 1998 election (as period 0), and its 2000 election (as period -2).[24] The district-level data set contains 22,595 district-period observations from 489 unique districts.[25]

I construct a similar school-level panel data set using the same time-invariant elections data but with school-year-level SFP and student performance outcomes. That is, rather than

23. For districts that hold "ward" elections, I treat each individual ward race as a separate election, since there is no way *a priori* to determine which of the multiple races is the most interesting for my purposes. Comparable estimates that drop ward elections altogether yield similar findings, and those results are available upon request.

24. I diagram this example in Appendix Figure A1.

25. My stacked data structure mirrors that in Cellini et al. (2010) and Shi and Singleton (2019). This setup allows me to explore dynamic election effects (unlike Ferreira and Gyourko [2010]) and means I do not have to take a stand on which post-election period will show the most meaningful election effects (as in Macartney and Singleton [2018]).

matching an election uniquely to a school district, I match every election to potentially many individual schools within the district. This school-level panel contains 168,507 school-period observations from 3,293 unique schools.

4 Research Design

My goal in this paper is to deliver a causal estimate of how greater Hispanic school board representation affects intra-district spending patterns, measured in terms of SFP investment. However, an OLS estimate of the effect of Hispanic representation on SFP spending may be downward-biased since districts with more Hispanic school board candidates are relatively high-poverty and likely have lower tax bases from which to finance capital improvements (see Table 2). In this section, I discuss the instrumental variables (IV) strategy I use to overcome this endogeneity hurdle, the specifications I employ, and the validity of my instrument.

4.1 Randomization on California Ballots

To obtain plausibly exogenous variation in election results and Hispanic representation on school boards, I leverage California's random ordering of candidates on election ballots. On the eighty-second day prior to an election, the Secretary of State's office draws all twenty-six letters from a (figurative) hat that has been "shaken vigorously" (California Election Code §13112). The order in which the official draws those letters creates a random alphabet that counties use to sort candidates on their ballots.[26] The alphabetization process first sorts candidates by last name and then first name; the drawing take place after candidates must declare their intentions to run (a deadline which varies by county but falls at least 88 days prior to an election), which precludes endogenous entry into the race.[27]

26. All school board elections in the same cycle across the state use an identical random alphabet; elections for state officials introduce another layer of randomization across constituencies.

27. While new candidates cannot join the race in response to the alphabet drawing, it is possible that, upon learning their ballot position, existing candidates alter their campaign strategies. For example, a low-ranked candidate may either put in less effort, convinced he will lose anyways, or redouble his efforts, convinced he can overcome that disadvantage. However, candidates' effort levels are unobservable and, in any event, survey evidence (notably, Hess [2002]) suggests school board campaigns neither garner much public interest nor entail serious campaigning, with most candidates spending less than $1,000. Since high- and low-effort reactions to the ballot drawing are equally plausible *ex ante*, and neither will likely matter much much on

4.2 Randomized Ballot Order: Candidate-level Variation

Ample evidence suggests the resulting ballot rankings could matter for school board election outcomes, as voters minimize the cost of participating in these low-profile races by arbitrarily picking high-ranked candidates.[28] I verify this ballot order effect on candidates' performance in my setting by first compiling the random alphabets drawn between 1996 and 2007 and combining them with my elections data to determine candidates' rankings on election ballots.[29] To confirm that my alphabetization works, I compare my imputed ballot ordering with actual ballots from several elections. I then construct a binary variable equal to one if a candidate is randomly assigned to one of the top K spots on the ballot, where K is the number of contested seats. I refer to the top K spots as the "top tier" of the ballot. This approach echoes that of Shi and Singleton (2019).[30]

I next evaluate the effect of being assigned a top-tier ballot spot by comparing top-tier candidates to their competitors. For candidate c running in district-election e, I regress her election outcome Y (either her vote share or an indicator for whether she won the election) on the top-tier ballot indicator as well as district-election fixed effects, θ_e:

$$Y_{ce} = \alpha_0 + \alpha_1 TopTier_{ce} + \theta_e + \epsilon_{ce}. \quad (1)$$

The coefficient of interest, α_1, captures how being assigned to the top tier affects a candi-

the margin, I argue that the net bias is negligible.

28. See, for example, Ho and Imai (2006, 2008), Koppell and Steen (2004), and Meredith and Salant (2013).

29. For all statewide elections since 2004, I use the alphabets provided in press releases published by the California Secretary of State. For on-year elections prior to 2004, I use Ho and Imai's (2008) alphabet table. And for off-year November elections between 1997 and 2003, I use internal memos provided by the Secretary of State's office. Meredith and Salant (2013) use the same data, but also include March elections from 1997-2003. Shi and Singleton (2019) do not include off-year elections from 1997-2003, but their data are otherwise identical.

30. The authors use an indicator equal to one if a former educator is randomly assigned to the first ballot position, since the ballot order literature finds that the top candidate receives the largest electoral benefit. By contrast, I remain relatively agnostic over how exactly the ballot order effect is conferred. For example, voters who want to minimize the effort costs of voting might select the top K names on the ballot, which could benefit any well-placed Hispanic candidate, even one not listed at the very top of the ticket. Note that the two instruments will be identical in elections with a single contested seat.

date's performance relative to candidates outside of the top tier in the same election.[31] The identifying assumption is that a candidate's top-tier status is randomly assigned and thus uncorrelated with unobserved determinants of her election performance (represented by ϵ).

Results from this candidate-level analysis appear in the top panel of Table 4. The first and second columns use the full sample of candidates described above (N=6,469); the third and fourth columns use the subsample of Hispanic candidates (N=2,140). For the average candidate, being randomly assigned a top-tier ballot spot increases her vote share by 1.2 percentage points (6.5 percent of the control mean) and her probability of victory by 6.2 percentage points (15 percent of the control mean). The effects are larger, although less precisely estimated, among Hispanic candidates. These coefficients are quantitatively similar to estimates of the "top-of-the-ticket" effect (i.e., the effect of being assigned to the very first ballot position) in the ballot order literature, and confirm that a top-tier ballot assignment has a meaningful effect on candidates' electoral prospects.

Additionally, to provide suggestive evidence that candidate ballot ordering is truly random, I estimate the correlation between a candidate's top-tier status and her political and ethnic identities using Equation 1, with candidate characteristics as outcome variables. The second panel of Table 4 shows these results. Encouragingly, I find that whether a candidate receives a top-tier ballot spot is uncorrelated with her political ideology, her ethnicity, and her incumbency status.[32]

31. Including election fixed effects generates a causal estimate of how much better a candidate fared relative to her peers who chose to run the the same race. Leaving the fixed effects out generates a causal estimate of how much better the average top-tier candidate (across all elections) performed relative to the average candidate outside the top tier (across all elections). Since the magnitude of the ballot effect may depend on the number of contested seats, the size of the candidate pool, voter turnout, or any number of election-specific factors, I prefer the fixed-effects approach. I report results from a specification without election fixed effects in Appendix Table A4. I find that top-tier status is closely correlated with incumbency, which I interpret as a byproduct of the fact that, even when board elections are nominally competitive, most incumbents run for re-election, face relatively few challengers, and are thus likely to appear in the top tier. I also find some evidence that top-tier candidates are more likely to be Hispanic. Since my preferred models presented below control for these types of district- and election-based confounding influences, I do not consider these correlations to be a threat to identification.

32. I observe candidate incumbency directly in my elections data. To infer candidates' party affiliations, I match the candidates in my sample to the California Voter Registration Database using candidate name and county of residence. The fuzzy match process leaves many candidates without political parties—either because I cannot uniquely match their names to individuals in the voter roll, or because they have moved

4.3 Randomized Ballot Order: Constructing an Election-level Instrument

The ballot order randomization described above operates at the candidate level, placing some candidates into the top tier and not others. However, my "treatment" of interest—Hispanic school board representation immediately after an election—depends on election-level results. Therefore, to convert my candidate-level variation into election-level variation, I create an indicator equal to one if any Hispanic candidate received a top-tier ballot position in a given election. Intuitively, a Hispanic candidate near the top of the ticket is more likely to win an election than a candidate who is buried in the pack (see Table 4), and her victory could subsequently increase Hispanic representation on the school board.[33]

For the top-tier Hispanic indicator to be a valid instrument, however, it must be uncorrelated with other district and election characteristics that determine both school board composition and district outcomes. That assumption does not follow from randomized ballot ordering at the candidate level. Any candidate's ballot ranking is random from her point of view, since the assignment mechanism is a lottery. But, from the econometrician's perspective, the probability that at least one Hispanic candidate in an election winds up in the top tier depends upon Hispanics' share of the candidate pool. Elections with larger shares of Hispanic candidates will, on average, have more Hispanic candidates randomized into the top tier of the ballot, and will consequently have larger shares of winning Hispanic candidates (conditional on the number of contested seats). Since Hispanic candidate share is endogenous (see Table 2), the top-tier Hispanic indicator is not excludable on its own. Rather, my top tier IV is *conditionally* exogenous with respect to other determinants of school board

or passed away since they ran for school board and have consequently been struck from the voter roll.

33. A potential concern is that the ballot effect is concentrated among top-of-the-ticket candidates, which would limit the relevance of the top-tier effect. Indeed, Meredith and Salant (2013) argue that this is the case. In Appendix Table A6, I compare my preferred first stage specification (see below) to alternatives, one using an indicator for whether a Hispanic appeared at the top-of-the-ticket, the other using the top-tier indicator, but omitting all top-of-the-ticket candidates. I do find that the first stage effect is larger for top-of-the-ticket Hispanic candidates, but that omitting these candidates still yields a reasonably precise, economically meaningful effect on post-election Hispanic board representation. The persistence of the ballot effect among lower-ranked candidates could be tied to race: voters may actively seek out minority candidates on election day, even ones not listed at the very top of the ballot. As such, I continue to use my top-tier instrument, since this variable captures the most information about the ballot order's effect on post-election school board composition.

composition once I control for the share of Hispanic candidates in the race and the number of contested seats. I include these controls in all specifications except where otherwise noted.[34]

After controlling for Hispanic candidate share and the number of contested seats, I should find no relationship between district i's characteristics χ in the election year t and the presence of a top-tier Hispanic candidate on the ballot. I examine these correlations using the following specification:

$$\chi_{it} = \phi_0 + \phi_1 HispTopTier_{it} + \phi_2 X_{it} + \lambda_i + \gamma_t + \epsilon_{it}, \tag{2}$$

where *HispTopTier* is a binary variable which equals one if a Hispanic candidate appears in the top tier of the ballot. I include election year and district fixed effects (γ and λ, respectively) to control for unobserved factors across school districts and election cycles that determine ballot composition. I cluster my standard errors at the district level to correct for repeated sampling of districts.

The results appear in Table 5.[35] Crucially, I find that whether a Hispanic candidate appears on the ballot is uncorrelated with Hispanic school board representation on the outgoing board, which means my instrument is unrelated to pre-election district treatment status. Nor do I find any correlation between whether a top-tier Hispanic candidate appears on a ballot and district observables in the election year, which suggests the instrument is unrelated to these potential confounding variables.[36]

34. In the final column of Appendix Table A8, I show that my results are robust to including flexible versions of these covariates—specifically, a quadratic in the share of Hispanic candidates and a fixed effect for the number of seats. In alternative,"event-study" specifications, which I discuss below, I employ election fixed effects, which control for all observable and unobservable election characteristics, as well as any functional forms of the two covariates described here, that could stand in the way of random assignment—not just the (linear) number of seats and (linear) Hispanic candidate share. My results are also robust to this approach.

35. In Appendix Table A7, I consider a range of other election-year outcomes, including SFP spending, SFP take-up, proxies for SFP eligibility, and alternative election outcomes (e.g., whether a Democrat wins the election). None of these outcomes are significantly correlated with whether a top-tier Hispanic candidate appeared on the ballot.

36. One might be concerned that these null results follow entirely from the inclusion of district fixed effects in my specification. In Appendix Table A5, I show that I reach the same conclusion using an alternative specification in which I omit district fixed effects. Though I find a slight correlation between my instrument and Black enrollment, the estimate is both small, imprecisely estimated, and unaccompanied by any effect on other enrollment measures. The largely null results confirm that my identification strategy does not depend

4.4 Estimating the Causal Effect of Hispanic School Board Representation

Using my top-tier Hispanic instrument, I analyze how differences in Hispanic school board representation influence district outcomes over time using a two-stage least squares (2SLS) system. Since the consequences of school board decisions likely take years to play out, I evaluate how the level of Hispanic board representation immediately following an election affects outcomes Y over the next $\tau = 1, ..., T$ periods. In other words, I do *not* estimate the contemporaneous effect of school board composition in period τ on outcomes in period τ—nor do I want to, since those estimates will capture effects of policies enacted by prior school board members.

In the first stage, I compare post-election Hispanic school board representation from districts that did and did not have a top-tier Hispanic candidate on the ballot. In the second stage, I use my first-stage estimate of differences in Hispanic board representation (driven by top-tier Hispanic candidates) to infer the average effect of greater Hispanic school board membership on district outcomes over the T years after the election. Consistent with my goal of identifying within-district effects, I estimate this model using both my school- and district-level data sets; for conciseness, I only describe the school-level model here, but the district-level model operates similarly.[37]

on district fixed effects.

37. The primary difference between the school- and district-level specifications is that I use school-level fixed effects in the former and district-level fixed effects in the latter. The other, more subtle distinction concerns the first stage: the district-level model will estimate the first stage specification (which, again, is cross-sectional and does not vary with respect to τ) $T + 9$ times, once for each district-by-period (see below where I note that I include 8 pre-election periods, plus period 0); the school-level model will estimate the first stage $(T + 9) \times s$ times, once for each school-by-period. Note that the district- and school-level specifications yield very similar first stage coefficients, which suggests this re-estimation does not bias my results (compare the first stage estimates given in Tables 7 and 8, for example).

For each school s in district i, I estimate the following specification:

$$\begin{aligned} HispBoardShare_{ista} \times post_\tau &= \pi_0 + \pi_1 HispTopTier_{it} \times post_\tau + post_\tau \\ &\quad + \rho_\tau + \lambda_s + \gamma_t + \psi_a + \pi_2 X_{it\tau} + \epsilon_{1,ista\tau} \\ Y_{ista\tau} &= \beta_0 + \beta_1 HispBoardShare_{it} \times post_\tau + post_\tau \\ &\quad + \rho_\tau + \lambda_s + \gamma_t + \psi_a + \beta_2 X_{it\tau} + \epsilon_{2,ista\tau}, \end{aligned} \tag{3}$$

where *post* is an indicator for whether the period $\tau > 0$; t denotes an election year; and a denotes an academic year.[38,39] The vector X includes the share of Hispanic candidates and the number of contested seats in the election, which I include to identify my first stage effect π_1, following the discussion above. I index X by τ to allow election covariate effects to vary across periods.

I estimate the model using observations from $\tau = -8, ..., T$. I include pre-election data from periods $\tau \leq 0$ to identify school, election year, period, and academic year fixed effects (λ, γ, ρ, and ψ, respectively). Those fixed effects do not aid with identification—rather, I include them to improve the precision of my estimates.[40] I cluster my standard errors at the district level since treatment statuses are correlated across time within districts.

Intuitively, by limiting my first stage to post-election Hispanic board representation, I assume that there are no confounding differences in pre-election Hispanic board share, con-

38. Academic years are a function of the election year t and the period relative to the election, τ. For example, if I observe an election in November 2005, the election year is set to 2005, which is the academic year 2006. The first post-period ($\tau = 1$) is academic year 2007, the second ($\tau = 2$) is academic year 2008, and so on.

39. This fixed effects specification with pre-election data is comparable to that of Beach and Jones (2017), Cellini et al. (2010), and Shi and Singleton (2019).

40. In Appendix Table A9, I show that my results are robust to using school-by-election fixed effects or district fixed effects instead; I also show that I obtain significant, though quantitatively smaller results when I exclude school- or district-level fixed effects altogether. In Appendix Table A8, I show what happens when I omit both pre-period data and fixed effects. Quantitatively, the magnitudes and standard errors of my estimates of effects on student achievement and teacher composition change once I exclude both fixed effects and pre-election data; spending data are quite robust. That is to be expected: omitting fixed effects and pre-election data will fail to correct for the fact that I observe district- and school-academic years multiple times, oftentimes as both "treated" and "untreated" observations. Consequently, the post-election data will reflect not only the impact of the focal election, but prior contests as well, which I ought to control for to recover a meaningful treatment effect. Once I include pre-period data, I obtain qualitatively similar results, albeit smaller than in my preferred specification and less precisely estimated.

sistent with Table 5. As such, the first stage coefficient, π_1, delivers the regression-adjusted, cross-sectional difference in post-election Hispanic school board representation between districts that did and did not have a top-tier Hispanic candidate. The 2SLS coefficient of interest, β_1, then captures how the level of Hispanic school board representation causally affects the outcome Y on average over the T years after an election.

4.4.1 Relevance of the First Stage

It remains for me to show that the first stage described in Equation 3 generates meaningful variation in Hispanic school board representation, my treatment of interest. To make this case, I leverage my school-level panel and estimate a representative first stage using my full sample of observations spanning the eight years before and after an election.[41] Table 6 presents these results. The causal parameter in the first row suggests that having a top-tier Hispanic candidate on the ballot raises Hispanic school board representation by 8.6 percentage points (48 percent) immediately after the election.[42]

While I focus exclusively on Hispanic board representation in this paper, given its straightforward interpretation, other measures of Hispanics' presence on a board could also be of interest, and so I highlight alternative first stage outcomes in Table 6 as well. I find that top-tier Hispanic candidates lead to a 10 percentage-point higher probability of any Hispanic member sitting on the board, another treatment that could in itself affect decision-making. This finding helps allay concerns that non-linearities in my preferred treatment—the marginal Hispanic victory may not affect boards with already-high Hispanic representation—sway my results: the binary "any Hispanic" treatment is by definition monotonic, is strongly correlated with my instrument, and will yield a comparable Wald estimator as my preferred "Hispanic representation" treatment. Similarly, I find an 8.8 percentage point increase in the probability of a Hispanic majority on the school board, although the F-statistic for this

41. This sample composition is identical to the "All Schools" sample described in Table 10. The samples used in Tables 8 and 9 differ due to constraints on post-election timing and missing test score data, respectively, both of which result in slightly different first stage coefficients than the ones presented here.

42. To put this number in perspective, on the modal 5-seat school board, a new Hispanic member will increase Hispanic representation by 20 percentage points.

model is low, likely due to the rarity of these majorities. All of these estimates confirm that top-tier Hispanic candidates lead to meaningful increases in Hispanic school board presence.

4.4.2 Estimating the Reduced-Form Effect of Top-tier Hispanic Candidates

I supplement my 2SLS model with a reduced-form framework, which evaluates the effect of a top-tier Hispanic candidate on post-election district outcomes. Admittedly, because this framework removes the "structural" dimension of my 2SLS approach, the magnitudes of the reduced-form coefficients will not be very meaningful in themselves. But, in view of the strong first stage relationship apparent in Table 6, I use the reduced-form model to highlight overall patterns of outcomes (rather than levels) that I attribute to having more Hispanic members on the school board, in whatever capacity.

As with my 2SLS design, my reduced form specification uses a panel approach in which I include pre- and post-election observations and interact the treatment assignment (the top-tier Hispanic candidate indicator, *HispTopTier*) with an indicator for post-election periods:

$$Y_{ista\tau} = \delta_0 + \delta_1 HispTopTier_{it} \times post_\tau + post_\tau + \rho_\tau + \lambda_s + \gamma_t + \psi_a + \delta_2 X_{it\tau} \epsilon_{ista\tau}. \qquad (4)$$

I once again include school, election year, period, and academic year fixed effects (λ, γ, ρ, and ψ, respectively), as well as controls for the share of Hispanic candidates and the number of contested seats. The coefficient of interest, δ_1, captures the causal effect of having a Hispanic candidate randomized to the top tier of the ballot on the average outcome Y over the $T.$ periods following the election.

4.5 Estimating the Dynamic Effect of Top-tier Hispanic Candidates

Both my 2SLS design and corresponding reduced form model capture average outcomes over post-election periods. While parsimonious, this approach obscures the dynamic effects of school board ethnic composition on district outcomes. For example, any student performance effects may take time to materialize, in which case the average post-election treatment effect will appear small, even when the treatment effect in the medium term is substantial.

I employ an "event study"-type specification to explore such dynamic effects, as well as to test for the presence of confounding pre-election trends in my outcome variables. As with my reduced-form specification, this event study approach estimates the "top-tier Hispanic" effect, not the impact of Hispanic school board representation per se. As such, I use this framework to qualitatively illustrate how Hispanic school board members' impact on district outcomes evolves over time.

I estimate my dynamic model using the same school-level panel data set as in my 2SLS and reduced form specifications, but now I treat every school-by-election as a separate "event." The specification is a pooled regression, including observations from periods $\tau = -8$ through $\tau = 8$, that isolates period-specific treatment effects:

$$\begin{aligned} Y_{esa\tau} =& \delta_0 + \sum_{j=-8}^{j=8} \Big(\delta_\tau HispanicTopTier_e \times \mathbb{1}(\tau = j)\Big) \\ &+ \kappa HispanicTopTier_e + \rho_\tau + \psi_a + \theta_{e\times s} + \epsilon_{esa\tau}, \end{aligned} \tag{5}$$

where e refers to an election and $e \times s$ refers to a school-by-election. By including school-by-election fixed effects, θ, and a dummy denoting elections with top-tier Hispanic candidates, I measure changes in school outcomes relative to their election-year levels.[43] That is, each event-study coefficient δ_τ reflects the change in Y between the election year (period 0) and period τ attributable to a top-tier Hispanic candidate. Note that because I incorporate school-by-election fixed effects (which control for all observable and unobservable election characteristics), I do not have to separately control for the share of Hispanic candidates and number of contested seats in the election. To this extent, the dynamic effects model also

43. I do not include school-by-election fixed effects in any other specification because these fixed effects will preclude a 2SLS approach, as the first stage of that specification relies on cross-sectional variation at the election level. Rather, to estimate the effect of Hispanic representation, I prefer to use school fixed effects, as discussed above, and I only introduce (potentially more intuitive) school-by-election effects in this event study (reduced-form) model. Full reduced-form results using school-by-election fixed effects appear in Appendix Table A9, and show precisely-estimated results similar to those I uncover using my preferred specification.

functions as a robustness check for my main specification.

5 Impact of Hispanic School Board Representation on SFP Spending

I begin my empirical analysis by establishing how Hispanic representation affects SFP spending along the extensive margin—the total amount of SFP funds the school board invests district-wide. The effect of having more Hispanic school board members on SFP investment is theoretically ambiguous. Beach and Jones' (2017) findings suggest ethnic diversity on a school board may increase discord and reduce total spending; on the other hand, Hispanic board members may take up additional funds to renovate or build more schools to benefit marginalized students that their non-Hispanic counterparts pass over.

5.1 District-level Regression Analysis

Using my top-tier Hispanic instrument, I estimate the causal effect of Hispanic school board membership on district-wide SFP spending. Since most school board members serve four-year terms, I focus on spending during the four years after an election.[44] I include as outcomes SFP spending by project type (modernization, new school construction, or supplemental), as well as total SFP spending. My results appear in Table 7.

Reduced-form coefficients in columns 2-4 reflect the impact of a randomly-assigned top-tier Hispanic candidate on average SFP spending outcomes over the four years after an election. The specification follows Equation 4. Point estimates in the first row reveal that annual SFP modernization spending per student increases by 26-29 percent relative to the post-election mean ($30-34 per student) for districts with a top-tier Hispanic candidate on the ballot. These effects are precisely estimated and robust to including additional election and district demographic controls. I find no appreciable effects on new construction or

44. Appendix Figure A2 shows that modernization spending effects largely dissipate by the fifth year after the election. The figure also provides evidence that, in the lead-up to the election, districts with and without top-tier Hispanic candidates do not have systematically different levels of SFP modernization spending. Spikes in modernization spending likely reflect the fact that projects receive state approval in bunches, which tend to occur semi-annually and coincide (particularly in the early 2000s) with new bond issues (see Section 2).

supplemental SFP spending.[45] While imprecise, point estimates indicate that total SFP spending increases by roughly the same amount as modernization spending.

2SLS estimates, presented in columns 5 and 6 of Table 7, capture the effect of Hispanic representation per se on SFP spending. The IV specification follows Equation 3. Each coefficient captures how a 100 percentage-point increase in Hispanic school board representation immediately after the election would affect the given outcome. But my preferred interpretation considers how a 20 percentage point increase in Hispanic board representation—equivalent to substituting one Hispanic for one non-Hispanic member on the modal board—influences SFP spending. Second-stage coefficients in the first row imply that a 20 percentage-point increase in Hispanic representation causes the board to raise SFP modernization spending by $71 per pupil per year, or 61 percent of the sample mean. Again, point estimates in the fourth row suggest that total SFP spending rises by a similar amount, although these coefficients are imprecisely estimated.

5.2 Robustness of District-level Spending Results

I perform two robustness checks to confirm that the results presented thus far capture causal effects. First, I address the possibility that districts with top-tier Hispanic candidates also have greater SFP eligibility, which would mechanically lead to a positive correlation between SFP spending and my treatment assignment. Reassuringly, reduced-form results in Appendix Table A7 show no correlation between whether a top-tier Hispanic candidate appears on the ballot and various proxies of SFP eligibility criteria.[46]

45. That Hispanic board composition only affects modernization spending is not surprising. In Appendix Table A1, I show that 75 percent of all SFP projects involve a modernization component, which suggests these grants are the most popular investments for school boards to make. Whereas brand new school construction is costly, and requires a suitable location within the district to put a new facility, modernization projects improve existing sites, which might limit their pecuniary and non-pecuniary costs. Furthermore, school boards have little sway over which supplemental grants they can access, since those largely depend on pre-existing district characteristics (for instance, susceptibility to earthquakes). All of these features likely make modernization the most attractive project type for school boards and the one most responsive to school board composition.

46. Specifically, I use district cumulative SFP spending (which captures potential eligibility that has been exhausted); total enrollment (which approximates an enrollment projection); full-time equivalent teachers (a proxy for the number of classrooms in the district); and facility age. Note that my data on school facility ages are poor: the CDE assigns a default opening date of July 1, 1980 for all schools built prior to that

Second, I check that my estimator captures just the effect of Hispanic candidates—and not, for example, the effects of incumbent re-elections or elections of Democratic candidates. Based on results in Appendix Table A7, I find no evidence that top-tier Hispanic candidacies are correlated with other election outcomes, which supports my claim that my results only capture Hispanic school board members' influence.

6 How Do Hispanic School Board Members Allocate SFP Funds?

To help explain why Hispanic board members invest more in SFP modernization projects, as I show in the previous section, I analyze how school board ethnic composition affects the distribution of SFP funds within a district. I hypothesize that school board elections follow a "core-constituency" voting model in which a candidate garners support from voters of her own ethnic background. Thus, Hispanic board members might invest the marginal SFP dollar in schools with large shares of Hispanic students in order to cater to nearby Hispanic voters; they take up additional SFP funds in order to pay for those marginal projects. If, on the other hand, school board elections hinge on the median voter, then Hispanic board members may invest the marginal dollar without considering schools' demographics, in which case I would see no correlation between school demographics and SFP allotments. These competing hypotheses have contrasting implications for the role of minority voices on school boards, and the potential for minority officials to reallocate district resources to benefit minority students.

6.1 School-level Regression Analysis

Using my school-level panel data set, I assess how Hispanic officials allocate the marginal dollar within school districts. Specifically, over the four years after an election, I quantify the impact that Hispanic board members have on modernization and total SFP spending among

date (67 percent of schools ostensibly opened that day). I therefore include an alternative measure of school age—the share of district schools listed as having opened in 1980—and again find no evidence of a correlation with my top-tier Hispanic indicator. Since the SFP only began in 1998, it is reasonable to assume that most schools with the placeholder opening date are at least 25 years old during my sample period, which means all these schools are eligible for SFP modernization funds.

schools that have relatively "high" and "low" shares of Hispanic and impoverished students within a district (which I define below). I describe patterns of intra-district SFP spending by estimating the reduced-form model given in Equation 4, which efficiently conveys which types of school within "treated" districts receive relatively more SFP funds. Likewise, I use the 2SLS model given in Equation 3 to quantify how Hispanic school board representation per se affects spending levels across schools within a district.

I first show that my results from Section 5 translate into comparable effects at the school level by estimating my reduced-form and IV models using the full sample of schools. Every specification includes controls for election characteristics, school demographics, and election year and academic year fixed effects. I also use school enrollment weights to facilitate comparisons in per-pupil spending outcomes across schools. The results appear in the second column of Table 8. Estimates in the first panel show that top-tier Hispanic candidates lead to 30 percent ($51 per student) more SFP modernization spending, and a statistically insignificant 14 percent more total SFP spending ($59 per student), on the average school relative to the post-election mean. IV results in the second panel imply that a 20 percentage-point increase in Hispanic board representation leads to 58 percent ($100 per student) more SFP modernization spending on average across all schools in a district. These findings are similar to the district-level results shown in Table 7.

6.2 Do Hispanic School Board Members Invest in Hispanic Students?

I now consider whether schools boards with more Hispanic members invest in schools within the district that have high Hispanic enrollment, which I interpret as investment intended for Hispanic students. To distinguish "high Hispanic" schools within a district, I first compute the median Hispanic enrollment share among all schools by district-year and then divide schools into two types: those with above-median Hispanic enrollment within a district, and those with below-median Hispanic enrollment within a district.[47] I then separately estimate reduced-form and 2SLS spending effects for schools that have high (above-median)

47. Schools with exactly-median Hispanic enrollment are implicitly dropped; including those median schools in either the above-median or below-median subsamples has no bearing on my results.

and low (below-median) Hispanic enrollment within their districts, using the specifications described above.

The results from those regressions appear in the third and fourth columns of Table 8. Each reduced-form coefficient in the first panel captures how a top-tier Hispanic candidate (in the district-wide election) affects the level of SFP spending per student among schools of the given type. The estimates indicate that students in schools with above-median Hispanic enrollment within a district receive \$56 in additional modernization funds (33 percent), whereas schools with below-median Hispanic enrollment receive a marginally significant \$36 per student. Similarly, schools with above-median Hispanic enrollment within their district receive \$73 more total SFP funds per student (17 percent), while those with below-median Hispanic enrollment receive a statistically insignificant \$32 per student.[48]

2SLS coefficients in the second panel of Table 8 underscore the magnitude of this difference. IV estimates imply that a 20 percentage-point increase in Hispanic board representation raises total SFP spending by a marginally significant \$146 per student, and SFP modernization spending by \$113 per student, among high-Hispanic schools within the district. Imprecise estimates point to considerably smaller increases of \$64-\$70 per student among schools with relatively low Hispanic enrollment. This comparison indicates that Hispanic voices on the school board substantially increases relative spending on Hispanic students.[49]

48. The data structure precludes a formal test to determine whether these estimates are statistically different. Since each school only appears in one subsample at a time (not by design, but because relative Hispanic enrollment is quite sticky over the sample period), I cannot test for equality using my school fixed effects approach. Still, in Appendix Table A10 I present results from an alternative "pooled" specification to argue the differences in spending across school types are meaningful. That model resembles Equation 4 but includes an indicator for whether a school is high-Hispanic, and interacts that term with my top-tier Hispanic dummy. The coefficient on that interaction term denotes the treatment effect of a top-tier Hispanic candidate among high-Hispanic schools relative to the treatment effect on low-Hispanic schools.

49. See Appendix Figure A3 for event study estimates of SFP spending across high- and low-Hispanic schools. The figure shows no consistent evidence of pre-election trends, although the data are noisy, in part because SFP investments are cyclical, responding to increases in fund availability following new statewide bond approvals (see Section 2 and a similar footnote in Section 5 above). Post-election data point to relatively sustained increases in modernization spending among high-Hispanic schools over the eight years following an election, with especially significant increases in the four years after the election.

6.3 Do Hispanic School Board Members Invest in Disadvantaged Students?

In the remaining columns of Table 8, I consider how the treatment effect varies by school poverty level. I divide schools within a district-year into those with above- and below-median shares of FRL-eligible students, as well as those schools that do and do not qualify for Title I funding. Once again, I separately estimate the reduced-form impact of a top-tier Hispanic candidate, and the 2SLS impact of Hispanic board representation, on SFP spending by school type, and compare the resulting coefficients.

I find that top-tier Hispanic candidates lead to \$65 more modernization spending per student among schools with high FRL-eligible student enrollment, compared to \$37 more per student on schools with low FRL-eligible enrollment. 2SLS estimates suggest these differences in spending by school poverty level are large. Each Hispanic board member raises total SFP spending by \$152 per student at high-FRL schools and \$178 at Title I-eligible schools (versus \$72 more and \$62 *less* spending at low-FRL and non-Title I schools, respectively). Given the strong correlation between Hispanic enrollment and school poverty level, I interpret these results only as a further indication that Hispanic board members direct funds towards marginalized students in general.[50]

6.4 Robustness of School-level Spending Results

In Appendix Table A11, I probe the robustness of these school-level results. First, I consider whether SFP spending across school types differs prior to the election—in other words, whether there are differential pre-election trends at the school level.[51] Importantly, I find that school-level SFP spending over the three years prior to the election is uncorrelated

50. Specifically, 77 percent of high-Hispanic schools are also high-FRL and 85 percent are Title I-eligible. These strong correlations would complicate any cross-type analysis (e.g., the effect on high-FRL/high-Hispanic schools).

51. Note that I employ district-level fixed effects in this specification, not school-level effects (Appendix Table A9 shows that the choice of fixed effects makes little difference for my main findings). I make that choice so I can easily compare coefficients across sub-samples without having to rely on my pooled specification described below Appendix Table A10; p-values from chi-squared tests of equality appear in every third column. I do not use this procedure to test for equality of my main treatment effects because, in that case, the pooled specification provides a more reliable indication of which schools drive my results. By contrast, in this robustness test, I am essentially comparing regression-adjusted, cross-sectional sample means.

with whether a Hispanic candidate will appear in the top tier of the future ballot (which follows from a similar result in the district-level data, shown in Appendix Table A7).

I also check that different school types have similar SFP eligibility. In particular, I want to ensure that an above-median Hispanic schools do not have greater SFP-eligibility than below-median Hispanic schools in treated districts. Using election-year data, I estimate the correlation between the top-tier Hispanic candidate indicator and my proxy measures for SFP eligibility shown in Appendix Table A7. I find no evidence that my treatment assignment indicator is correlated with these measures of SFP eligibility among any school type.[52]

A related concern is that my above- and below-median school types are not comparable across treated and control districts. For example, schools with above-median Hispanic enrollment in the treated group may have systematically higher Hispanic enrollment than above-median Hispanic schools in the control group. Although I find that above-median Hispanic and FRL-eligible schools do have higher Hispanic enrollment in treated districts, these differences amount to at most 1.5 percent of the sample mean. I find no other demographic differences that would bias my estimates. Altogether, these null results indicate that my sample delineations are not themselves driving my results, and support a causal interpretation of my intra-district spending results.

7 Hispanic School Board Representation and Student Achievement

My analysis thus far has established how Hispanic school board members raise district spending on minority students. It remains to be seen whether minority representation on a school board affects students' academic performance, either directly or via SFP spending; these student outcomes are of first-order interest to both policymakers and related research on education finance. As such, in my final analyses, I assess how Hispanic board members affect within-district test scores in the years following an election.

52. I find some evidence that non-Title I schools in treated districts are older than non-Title I schools in untreated districts. But, if anything, that means my point estimate for non-Title I schools is upward-biased (and it is already effectively zero).

7.1 School-level Regression Analysis

I estimate the impact of Hispanic school board representation on school-level math and reading (ELA) scores. I consider a medium-term horizon—the eight years after an election—to allow sufficient time for test score effects to materialize. Reduced-form and 2SLS results appear in Table 9. The reduced-form specification is Equation 4, with additional controls for election characteristics as indicated, while the 2SLS specification is Equation 3 with full election controls.

The first panel indicates that top-tier Hispanic candidates lead to significant gains in math achievement among high-Hispanic schools. Reduced-form results suggest these schools see math scores improve by 0.04 standard deviations on average over the eight years after an election, while low-Hispanic schools improve by an insignificant 0.02 standard deviations.[53] Imprecise point estimates support a similar conclusion for ELA scores. 2SLS coefficients in the second panel imply that a 20 percentage-point increase in Hispanics' share of school board seats improves math test scores by 0.08 standard deviations at high-Hispanic schools over the eight years after an election, and by a statistically insignificant 0.05 standard deviations at low-Hispanic schools. This gap is not large, but it does indicate test score gains are being driven by improvement at high-Hispanic schools.[54]

7.2 School-level Event Study Analysis

Since test score effects appear over time, the estimates in Table 9 likely understate the true effect of Hispanic board representation on student achievement. To document how these student achievement effects play out over time, I use Equation 5 to illustrate the dynamic

53. See Appendix Table A9 for estimates across different types of schools besides high-/low-Hispanic. I find comparable results when I compare high- and low-FRL schools, but less economically and statistically meaningful results when I compare Title I and non-Title I schools. That latter result likely stems from the fact that most schools qualify for Title I in my sample, whereas Hispanic board members target funds towards only those neediest, highest-minority schools within the district. On average, then, Title I schools show no gains.

54. As I note in the previous section, I cannot formally test equality of my estimates due to my fixed effects specification and data structure. But in Appendix Table A10, I show some evidence that the reduced-form gap is significant using a pooled specification in which I interact my top-tier Hispanic indicator with an indicator for whether a school is high-Hispanic.

effect of a top-tier Hispanic candidate on student performance.

Figure 1 visualizes these dynamic test score effects. Importantly, I find no evidence of differences in math scores prior to the election, which once more supports my claim that the estimates deliver causal effects. I do find that ELA scores among high-Hispanic schools are lower in districts with top-tier Hispanic candidates during the year prior to the election, though it is not clear what is causing this pre-election difference. Reassuringly, the effect is exclusive to ELA scores among high-Hispanic schools, and the fact that only one coefficient (out of twenty-four pre-period coefficients tested here) shows significant pre-treatment effects suggests that this significant correlation might be due to chance.

After the election, gains in math scores become apparent within two years. The visual evidence suggests that the eight-year average reduced-form effect of around 5 percent of a standard deviation (see Table 9) masks substantial improvements of up to 20 percent of a standard deviation 6-8 years after the election among high-Hispanic schools. ELA scorse—the problematic pre-election coefficient notwithstanding—show smaller but positive effects of up to 0.10 standard deviations at high-Hispanic schools. That these gains in achievement persist over time suggests that Hispanic board members' influence outlasts their tenure on the school board. Furthermore, the fact that I find test score effects only among high-Hispanic schools mirrors my SFP spending results, and reinforces my conclusion that minority board representation primarily benefits minority students.

Altogether, these results point to an economically meaningful reduced-form effect on student achievement. The effects documented here appear large, yet the post-election mean is -0.17 standard deviations for math and -0.32 standard deviations for ELA scores (see Table 9). The substantial impact on student performance, then, means that an additional Hispanic board member leads to improvement upon a low baseline. Moreover, the coefficients shown here will implicitly capture cohort effects: a student in kindergarten in period 0 will have the most exposure to Hispanic board members' policies, while students in higher grades will have less exposure, and will matriculate out of the sample. Therefore, math scores' upward trajectory reflects not only accumulating gains from district policies but also increasing sample selection in favor of students with long tenures in the district. Given the limited data at my disposal, I do not attempt to separately identify these cohort effects, but I note that they are part and parcel of the Hispanic representation effect that I want to estimate, and support my overall conclusion that minority representation on the school board promotes minority student achievement.

7.3 Exploring the Mechanism

Given my policy context, the mechanism behind these achievement gains is especially difficult to disentangle. My claim throughout this paper has been that Hispanic board members devote greater resources to Hispanic students. I provide evidence to that effect using SFP spending data, but since many policy shifts likely occur at once, and I do not directly observe *which* resources (aside from SFP funds) the school board chooses to expend on high-Hispanic schools, the exact cause for this uptick in achievement will remain elusive.

In light of this limitation, I discuss four straightforward mechanisms that help clarify the roles of the SFP on student performance as well as non-SFP policy shifts prompted by Hispanic school board members. I first discuss the extent to which new facilities themselves improve student test scores. I then present new results showing how Hispanic board members reduce teacher churn among high-Hispanic schools. Finally, I present null results showing no effects on overall district budgeting and no student sorting within, into, or out of the school district. I conclude that capital improvements likely raise test scores, but that Hispanic board members, as hypothesized, pursue myriad investments that benefit high-Hispanic schools.

7.3.1 Mechanism: Does SFP Investment Raise Test Scores?

I first analyze the extent to which the direct and indirect effects of school facility investments explain my findings. To do so, I compare test scores across schools that do and do not begin an SFP modernization project after the focal election. I use my event study framework given in Equation 5, but include only four years of pre-election data, since most SFP projects begin early in the sample period and have few years of pre-construction data.

Event study coefficients, plotted in Appendix Figure A4, indicate that schools with a post-election modernization project exhibit greater gains in student performance those without additional SFP investment. At the same time, schools that did not receive modernization funds show improvement—albeit slight—in math scores post-election. This comparison points suggest that my results partly capture positive returns to new school facilities, but also that achievement increases independent of SFP investment.

7.3.2 Mechanism: How Does Hispanic Board Representation Affect Teachers?

I next analyze how Hispanic board representation affects teachers within the district. Teachers represent the most costly and high-profile educational input, and changes in teacher composition, particularly among high-Hispanic schools, would provide further evidence that increasing Hispanic board representation ushers in new policies. Using anonymous, individual-level teacher data from the CDE, I construct four school-level outcomes: average full-time equivalent (FTE) experience; average FTE tenure in the school district; the number of "new hires" (teachers with ≤ 1 year of tenure in the district); and the share of Hispanic teachers.

Using Equation 5, I examine how teachers' experience and tenure evolve after the election. I find that top-tier Hispanic candidates lead to increases in teacher tenure and experience of up to half a year in the medium term (five years after the election) at high-Hispanic schools, with much smaller and imprecisely estimated gains at low-Hispanic schools.[55] Point estimates in Table 10 indicate that an additional Hispanic board member raises average teacher experience by 0.38 years (2.7 percent of the post-election mean) and average teacher tenure by 0.48 years (4 percent) at high-Hispanic schools. Likewise, I find that these schools have 0.59 (17 percent) fewer newly hired teachers. The data also suggest that, when more-Hispanic school boards do hire new teachers, they tend to hire Hispanics, manifest in a 1.2 percentage-point increase in the share of Hispanic teachers across all schools (6 percent).

These facts support the conclusion that Hispanic board members reduce teacher churn

55. An important caveat is that I find some evidence of differential pre-trends three years prior to the election across all schools; it is unclear what is driving this effect, although I note that no other pre-period coefficients are statistically different from zero.

at high-minority schools and favor hiring Hispanic teachers. That effect likely explains a non-trivial share of the test score effects I observe: per Ronfeldt, Loeb, and Wyckoff (2013), teacher turnover has a large impact on student achievement among high-minority schools. Since underprivileged schools witness especially high turnover, this effect suggests Hispanic board members help alleviate a structural hurdle preventing low-performing schools' advancement. Equally, these findings provide indirect evidence that Hispanic school board members give other pecuniary or non-pecuniary incentives to teachers in these schools to encourage retention—in other words, these results provide further evidence that minority board members invest in minority students.

7.3.3 Mechanism: Do Students Re-sort Within or Between Districts?

I return to my district-level data set to assess potential sorting mechanisms. In-migration could increase test scores if the election or new facilities entice higher-ability students into the district. Appendix Table A12 shows a marginally significant increase in white enrollment share and concomitant decrease in Hispanic enrollment share. The demographic shift is not economically large, amounting to around 1 percent of the sample mean for white enrollment.

I also evaluate the degree of intra-district sorting, which could improve test scores if, for example, high-ability students switch into modernized schools from elsewhere in the district, and positive peer effects improve overall student achievement. As a measure of within-district sorting, I use intra-district dissimilarity indices, a common heuristic to quantify student segregation across schools.[56] Intuitively, if students re-sort within a district—for example, if white students concentrate in renovated schools—then the relevant dissimilarity index would be affected (in my example, the dissimilarity index for white enrollment would increase). Appendix Table A12 shows no evidence of this intra-district sorting response.

56. The general formula for a dissimilarity index is as follows: $\frac{1}{2}\sum_s \mid \frac{z_s}{Z} - \frac{n_s - z_s}{N-Z} \mid$, where s denotes schools within a district, z is the number of students at a school with a given demographic characteristic (e.g., Hispanic students), N is total enrollment in the district, and Z is the total enrollment of type z in the district (e.g., the number of Hispanic students in the district).

7.3.4 Mechanism: Do Hispanic Board Members Reallocate General Funds?

Finally, I analyze how top-tier Hispanic candidates affect districts' non-SFP budget outcomes. It could be that Hispanic candidates manage to raise revenues, potentially by securing other state transfers, or reallocate existing spending across budget priorities, which could improve the return on the marginal dollar.[57] I also estimate changes in student-teacher ratios as a measure of teacher hiring and class sizes. Event study estimates, plotted in Appendix Figure A5, show no relationship between the top-tier Hispanic indicator and subsequent district budget outcomes or student-teacher ratios. In this setting at least, school board ethnic composition does not appear to affect districts' overall budgets.[58]

8 Concluding Discussion

The well-known racial gap in education achievement coexists alongside a racial gap in education finance. State and federal efforts to remedy this imbalance have been unsuccessful at reducing intra-district disparities in white and nonwhite students' performance (Cascio et al. 2013; Lafortune et al. 2018). Researchers have posited that school boards lie at the heart of this discrepancy, but have not previously linked school board composition to intra-district gaps in spending and student achievement.

My analysis confirms that school boards play an integral role in education finance. Using spending data from the SFP, I find that an additional minority (Hispanic) school board member increases spending on school renovations using state transfer grants. In particular, SFP spending on high-Hispanic and high-poverty schools within the district increases by up to 66 percent, which juxtaposes smaller, insignificant changes among low-Hispanic and relatively affluent schools. These findings corroborate indirect evidence, presented in Cascio

57. I use district-wide budget data from the Census of Governments. I include data on total district expenditures and revenues, along with spending on teachers' salaries (which comprises 35 percent—a plurality—of total spending). For each district-year, I divide all three outcomes by total enrollment to obtain spending per pupil, and winsorize at the first and ninety-ninth percentiles.

58. Given the limitations that California school boards face from Proposition 13, it is not surprising that total district revenues do not depend on school board composition. It is possible that in another setting minority representation could affect tax policy and total district spending, but I leave this question for future research.

et al. (2013), Gordon (2004), and Hyman (2017), that school board composition matters for the take-up of state transfers. By extension, the efficacy of "carrot" policies meant to incentivize local school boards to address inequality clearly depends on alignment between state and local politics. Whereas state legislators pass financial reforms and spending bills meant to bridge gaps between rich and poor, white and nonwhite students, this paper suggests that the modal (white) school board member responds to political incentives which discourage investment in marginalized students.

Moreover, these differences in investments translate into differences in student achievement. A combination of SFP spending, implicit incentives to encourage teacher retention, and (likely) other, unobservable extra inputs into high-Hispanic schools result in moderate gains in student test scores. These results re-affirm that spending on traditionally disadvantaged students can maximize the returns to the marginal education dollar.

This article contrasts with prior research on education finance and the racial achievement gap by focusing on how local school board politics can perpetuate unequal outcomes. Building upon the nascent school boards literature, the results in this study link school board politics with changes in intra-district spending patterns and improvement in student outcomes for the first time. More broadly, the education setting provides an apt context to see how minority voices in local government can improve the flows of public resources to minority constituents. These findings provide room for further investigation of the role school boards play in the United States' federalized education system, with a particular emphasis on how local officials shape the practical effects of statewide or even national policies. Overall, my analysis suggests that the success of state and federal redistribution initiatives relies on school boards' cooperation—and that a lack of minority political representation could stymie future top-down initiatives to improve racial equity in education.

Table 1: Summary of California School Board Elections, 1996-2007

	Mean	Median	Std Dev	Min	Max
I. Elections Data					
# Seats	2.15	2	0.84	1	7
# Candidates	4.24	4	2.02	1	28
# Incumbents	1.53	1	0.98	0	5
# Ballots Cast	17,733	6,531	31,934	15	319,754
Election Margin	0.09	0.05	0.11	0.00	0.81
Election Year	2001	2001	3.40	1996	2007
Ward Election?	0.26	0	0.44	0	1
II. Hispanic Candidate Prevalence					
# Hisp Candidates	0.66	0	1.11	0	9
Share Hisp Candidates	0.16	0	0.25	0	1
Any Hisp Candidate?	0.38	0	0.49	0	1
Share Hisp Winners	0.16	0	0.31	0	1

The sample contains 3,738 statewide primary and general elections between 1996 and 2007. The election margin reflects the difference in vote shares between the marginal winning candidate and marginal losing candidate.

Table 2: Summary of Election-Year District Demographics and Student Achievement

	(1) All Elections	(2) Elections w/o Hisp	(3) Elections w/ Only Hisp	(4) Elections w/ Both Hisp and non-Hisp
I. Election-Year District Demographics				
Total Enrollment	8,718	7,291	16,396	10,479
	(16,342)	(12,856)	(60,158)	(12,658)
Share White	0.46	0.58	0.10	0.30
	(0.28)	(0.24)	(0.13)	(0.24)
Share Hispanic	0.38	0.26	0.78	0.54
	(0.27)	(0.21)	(0.21)	(0.26)
Share Asian	0.08	0.07	0.07	0.08
	(0.10)	(0.10)	(0.09)	(0.11)
Share Black	0.05	0.04	0.04	0.05
	(0.07)	(0.07)	(0.07)	(0.07)
Share FRL-eligible	0.47	0.39	0.74	0.56
	(0.26)	(0.24)	(0.20)	(0.25)
N:	*3,155*	*1,913*	*92*	*1,150*
II. Election-Year Student Achievement				
Composite Math Score	-0.02	0.22	-0.79	-0.35
	(0.91)	(0.91)	(0.68)	(0.80)
N:	*2,962*	*1,795*	*85*	*1,082*
Composite ELA Score	0.04	0.29	-0.87	-0.30
	(0.97)	(0.94)	(0.69)	(0.88)
N:	*3,251*	*1,968*	*92*	*1,191*

The table describes the mean of demographic characteristics and student achievement outcomes listed in the left-hand column. Standard deviations appear in parentheses. The unit of observation is a district-election. I divide the sample based on the share of Hispanic candidates on the election ballot: column 1 describes all district-elections that I successfully match to enrollment or test score data; column 2 describes elections without any Hispanic candidates; column 3 describes elections with only Hispanic candidates; and column 4 describes elections with both Hispanic and non-Hispanic candidates. Sample sizes vary within column because of missing test score and demographic data, as discussed in the text.

Table 3: Summary of District-year SFP Spending Outcomes, by Hispanic School Board Representation

		Hispanic School Board Representation			
	(1)	(2)	(3)	(4)	(5)
	Full Sample	No Hisp Members	(0%, 20%]	(20%, 60%]	(60%, 100%]
Total SFP Spending per Pupil	352	365	340	297	303
	(1196)	(1244)	(1127)	(962)	(1036)
Modernization Spending per Pupil	99	105	86	83	70
	(382)	(403)	(321)	(322)	(296)
New Constr. Spending per Pupil	44	44	47	41	56
	(226)	(231)	(221)	(182)	(245)
Supplemental SFP Grants per Pupil	190	195	192	164	167
	(699)	(723)	(679)	(571)	(627)
N:	*13,900*	*10,161*	*1,712*	*1,455*	*572*

Each cell provides mean of the outcome variable in the left-hand column by subsample, with standard deviations in parentheses. The full sample in column 1 contains 13,900 district-academic years between 1999-2000 and 2015-2016. The remaining columns describe subsamples, broken down by Hispanic representation on the district's school board in a given year.

Table 4: Relevance and Excludability of Ballot Order: Candidate-level Analysis

	All Candidates		Hispanic Candidates	
	(1) Control Mean	(2) Top-tier Effect	(3) Control Mean	(4) Top-tier Effect
I. Impact of Top-tier Assignment on Candidate Outcomes				
Vote Share	0.185	0.012	0.207	0.021
	(0.116)	(0.002)	(0.129)	(0.007)
Wins?	0.401	0.062	0.404	0.105
	(0.490)	(0.015)	(0.491)	(0.055)
N:	*3,418*	*6,469*	*1,088*	*2,140*
II. Correlation of Top-tier Assignment with Candidate Traits				
Democrat?	0.473	-0.020	0.599	-0.021
	(0.499)	(0.027)	(0.491)	(0.187)
Republican?	0.374	0.014	0.230	0.051
	(0.484)	(0.026)	(0.422)	(0.123)
N:	*1,448*	*2,724*	*421*	*847*
Hispanic?	0.348	-0.007	—	—
	(0.476)	(0.016)		
N:	*3,130*	*5,943*		
Incumbent?	0.315	0.008	0.321	-0.010
	(0.465)	(0.014)	(0.047)	(0.055)
Missing Ethnicity?	0.084	-0.006	—	—
	(0.278)	(0.008)		
Missing Party?	0.576	0.015	0.613	0.013
	(0.494)	(0.013)	(0.487)	(0.052)
N:	*3,418*	*6,469*	*1,088*	*2,140*

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Columns 1 and 3 show the mean of the outcome variable in the left-hand column among candidates who are not assigned a top-tier ballot spot (my control group). Standard deviations appear in parentheses. Columns 2 and 4 present regression estimates of the top tier effect in the relevant sample, following Equation 1. Robust standard errors clustered at the district level appear in parentheses. Sample sizes vary within columns due to missing ethnicity and political party affiliation data, as discussed in the text.

Table 5: Excludability of Top-tier Hispanic IV: Election-level Analysis

	Control Mean (1)	Top-tier Hispanic Effect (2)
Hisp Board Share, Outgoing Board	0.13	0.01
	(0.21)	(0.01)
N:	*486*	*1,314*
Total Enrollment	11448	75
	(12538)	(129)
Share White	0.36	-0.00
	(0.23)	(0.00)
Share Hispanic	0.48	-0.00
	(0.24)	(0.00)
Share Asian	0.08	0.00
	(0.11)	(0.00)
Share Black	0.05	0.00
	(0.06)	(0.00)
Share FRL-eligible	0.51	0.01
	(0.23)	(0.01)
N:	*409*	*1,149*
Composite Math Score	-0.25	-0.03
	(0.80)	(0.04)
N:	*390*	*1,081*
Composite ELA Score	-0.19	-0.01
	(0.86)	(0.04)
N:	*429*	*1,190*

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The data consist of district-election year observations (N=1,314). Column 1 reports the mean of the outcome variable in the left-hand column among district-election years without a top-tier Hispanic candidate (my control group). Standard deviations appear in parentheses. Column 2 presents regression estimates of the top-tier Hispanic effect, following Equation 2, with controls for the share of Hispanic candidates, the number of contested seats in the election, and district fixed effects, as discussed in the text. Robust standard errors clustered at the district level appear in parentheses. Sample sizes vary within columns due to missing enrollment and student performance data, as discussed in the text.

Table 6: Relevance of Top-tier Hispanic IV: Representative First Stage Results Using School-level Panel

		Baseline Model		Addtl. Controls	
	Post-Election Control Mean	Top-tier Hispanic Effect	F-stat	Top-tier Hispanic Effect	F-stat
	(1)	(2)	(3)	(4)	(5)
Hisp Board Share	0.183	0.086***	9.15	0.086***	17.31
	(0.230)	(0.015)		(0.015)	
Any Hisp on Board?	0.509	0.115***	23.95	0.115***	34.78
	(0.500)	(0.032)		(0.032)	
Hisp Majority?	0.119	0.088***	2.62	0.090***	2.31
	(0.324)	(0.026)		(0.026)	
N:	*36,381*	*141,329*		*135,308*	
School FEs		Y	Y	Y	Y
Control for Share Hisp Cand		Y	Y	Y	Y
Control for # Seats		Y	Y	Y	Y
Election/Academic Yr, Period FEs		Y	Y	Y	Y
Other Election Controls		N	N	Y	Y
Demographic Controls		N	N	Y	Y

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The sample includes school-period observations from pre-election period -8 through post-election period +8. Column 1 reports the mean of the first stage outcome in the left-hand column among observations without a top-tier Hispanic candidate over the 8 periods after an election (my control group). Standard deviations appear in parentheses. Column 2 presents regression estimates of the top-tier Hispanic effect, following the first stage given in Equation 3, with controls for the share of Hispanic candidates, the number of contested seats in the election, as discussed in the text, along with school, election year, academic year, and period fixed effects. Column 4 reports estimates using additional election and demographic controls. Election covariates include the total number of candidates in the race, the number of Hispanic candidates, and an indicator for whether the district holds ward elections. Demographic covariates include the shares of white, Black, Hispanic, Asian, and FRL-eligible students. Sample sizes vary due to missing enrollment data, as discussed in the text. Robust standard errors clustered at the district level appear in parentheses.

Table 7: How Does Hispanic Representation Affect Overall SFP Spending? District-level Analysis

	Post-Election Mean	Reduced-Form			2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Modernization Spending per Pupil	116.0	34.2***	34.2***	29.9***	356.6***	327.3***
	(368.8)	(10.8)	(10.7)	(10.5)	(122.6)	(122.0)
New Construction Spending per Pupil	57.9	-3.0	-2.9	-4.9	-31.2	-53.7
	(239.3)	(7.6)	(7.6)	(7.2)	(77.6)	(77.5)
Supplemental SFP Spending per Pupil	200.6	8.8	8.8	1.9	91.6	20.6
	(619.1)	(16.7)	(16.7)	(16.2)	(172.6)	(173.3)
Total SFP Spending per Pupil	386.6	35.9	36.0	24.9	374.5	272.0
	(1,080.8)	(30.3)	(30.2)	(29.3)	(315.3)	(316.6)
N	*5,009*	*11,590*	*11,590*	*11,584*	*11,590*	*11,584*
District FEs		Y	Y	Y	Y	Y
Control for Share Hisp Cand		Y	Y	Y	Y	Y
Control for # Seats		Y	Y	Y	Y	Y
Election /Academic Yr, Period FEs		Y	Y	Y	Y	Y
Other Election Controls		N	Y	Y	N	Y
Demographic Controls		N	N	Y	N	Y
First Stage F-stat		—	—	—	11.06	12.02
First Stage Coefficient		—	—	—	0.10	0.09
					(0.01)	(0.01)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The sample includes district-period observations from pre-election period -8 through post-election period +4. Column 1 presents the sample mean of the outcome variable in the left-hand column in periods 1-4 after the election. Standard deviations appear in parentheses. Regression results reported in Columns 2-6 use panel data spanning eight years before the election through four years after the election. The specification in columns 2-4 is Equation 4. The specification in columns 5 and 6 is Equation 3. All specifications include election year, academic year, and district fixed effects, as well as controls for the number of contested seats and the share of Hispanic candidates. Additional election controls are described below Table 6. Demographic covariates include the shares of white, Black, Hispanic, Asian, and FRL-eligible students in the district. Robust standard errors are clustered at the district level.

Table 8: How Does Hispanic Representation Affect Intra-District SFP Spending? School-level Analysis

		School-level Regression Estimates						
			By Share Hispanic		By Share FRL		By Title I	
	Post-Election Mean	All Schools	Above Median	Below Median	Above Median	Below Median	Title I Eligible	Not Title I Eligible
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
I. Reduced-form Estimates of Top-tier Hispanic Effect								
Modernization Spending per Pupil	172.2	50.8***	56.3***	35.6*	65.4***	37.4**	57.0***	21.3
	(674.2)	(16.4)	(17.1)	(18.2)	(18.7)	(16.6)	(18.0)	(22.6)
Total SFP Spending per Pupil	420.0	59.5*	72.8**	32.3	77.2	35.8	91.8**	-22.2
	(1,458.6)	(34.9)	(36.0)	(38.6)	(47.6)	(36.9)	(39.8)	(44.5)
II. 2SLS Estimates of Hispanic Representation Effect								
Modernization Spending per Pupil	172.2	500.2***	563.6***	352.2*	644.8***	373.5**	552.4**	244.1
	(674.2)	(187.2)	(205.9)	(190.0)	(225.2)	(175.7)	(189.9)	(287.4)
Total SFP Spending per Pupil	420.0	586.0*	728.5*	320.6	761.3**	357.8	890.2**	-308.3
	(1,458.6)	(351.4)	(384.0)	(376.6)	(388.7)	(359.6)	(393.0)	(547.9)
N	*43,446*	*92,601*	*43,610*	*43,635*	*43,510*	*43,510*	*64,827*	*27,768*
First Stage Coefficients	—	0.10	0.10	0.10	0.10	0.10	0.10	0.08
		(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
First Stage F-stat		19.03	17.70	16.93	16.16	19.05	20.52	6.91
Election/Academic Yr, Period FEs	—	Y	Y	Y	Y	Y	Y	Y
School FEs	—	Y	Y	Y	Y	Y	Y	Y
Control for Share Hisp Cand	—	Y	Y	Y	Y	Y	Y	Y
Control for # Seats	—	Y	Y	Y	Y	Y	Y	Y
Other Election Controls	—	Y	Y	Y	Y	Y	Y	Y
School Demographic Controls	—	Y	Y	Y	Y	Y	Y	Y

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The sample includes school-period observations from pre-election period -8 through post-election period +4. The first column shows the mean of the two outcome variables in the left-hand column over the four post-election periods. Estimates in the remaining columns cell comes from separate OLS and 2SLS regressions. The reduced-form specification is Equation 4 and the 2SLS specification is Equation 3. All specifications include election and academic year fixed effects, school fixed effects, and additional election and demographic controls as described below Table 6. All specifications use school enrollment weights. Sample sizes vary across columns due to missing enrollment data and exclusion of schools with exactly median enrollment by category. Robust standard errors are clustered at the district level.

Table 9: How Does Hispanic Representation Affect Intra-District Achievement? School-level Analysis

			By Share Hispanic	
	Post-Election Mean	All Schools	Above Median	Below Median
	(1)	(2)	(3)	(4)
I. Reduced-Form Estimates of Top-tier Hispanic Effect				
Composite Math Score	-0.170	0.030*	0.041**	0.026
	(1.080)	(0.017)	(0.020)	(0.020)
N	*70,260*	*110,613*	*55,460*	*49,476*
Composite ELA Score	-0.318	0.022*	0.028*	0.022
	(1.049)	(0.012)	(0.015)	(0.015)
N	*70,256*	*110,607*	*55,453*	*49,477*
II. 2SLS Estimates of Hispanic Representation Effect				
Composite Math Score	-0.170	0.311*	0.429**	0.263
	(1.080)	(0.165)	(0.197)	(0.194)
N	*70,260*	*110,613*	*55,460*	*49,476*
Composite ELA Score	-0.318	0.229*	0.288*	0.216
	(1.049)	(0.127)	(0.160)	(0.154)
N	*70,256*	*110,607*	*55,453*	*49,477*
Election/Academic Yr, Period FEs		Y	Y	Y
School FEs		Y	Y	Y
Control for Share Hisp Cand		Y	Y	Y
Control for # Seats		Y	Y	Y
Other Election Controls		Y	Y	Y
School Demographic Controls		Y	Y	Y
ELA First Stage Coeff.		0.10	0.10	0.10
		(0.02)	(0.02)	(0.02)
ELA First Stage F-stat		15.14	10.47	15.56
Math First Stage Coeff.		0.10	0.10	0.10
		(0.02)	(0.02)	(0.02)
Math First Stage F-stat		15.04	10.49	15.56

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The sample includes school-period observations from pre-election period -8 through post-election period +8. All test score outcomes are measured in district-subject-year level standard deviations. The first column shows the mean of the two outcome variables in the left-hand column over the eight post-election periods. Estimates in the remaining cells comes from separate OLS and 2SLS regressions given by Equations 3 and 4. Additional controls are described below Table 6. All specifications use school enrollment weights. Sample sizes vary across columns due to missing enrollment and test score data, and due to the exclusion of schools with exactly median Hispanic enrollment, as discussed in the text. Robust standard errors are clustered at the district level.

Table 10: How Does Hispanic Representation Affect Teachers? School-level Analysis

	Post-Election Mean	All Schools		High-Hispanic Schools		Low-Hispanic Schools	
		Reduced-Form	2SLS Effect	Reduced-Form	2SLS Effect	Reduced-Form	2SLS Effect
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mean FTE Experience	13.99	0.14**	1.59**	0.16**	1.88**	0.12**	1.38*
	(4.03)	(0.06)	(0.70)	(0.07)	(0.87)	(0.06)	(0.71)
Mean FTE Tenure	11.85	0.18***	2.07***	0.20***	2.38***	0.15**	1.84**
	(3.85)	(0.06)	(0.70)	(0.07)	(0.84)	(0.06)	(0.71)
# New Hires	3.41	-0.22**	-2.75**	-0.25**	-2.96**	-0.16	-1.87
	(4.65)	(0.11)	(1.29)	(0.12)	(1.39)	(0.11)	(1.26)
Share FTE Hisp	0.190	0.005**	0.061***	0.003	0.038*	0.005**	0.063**
	(0.176)	(0.002)	(0.022)	(0.002)	(0.023)	(0.002)	(0.024)
N:	*91,508*	*135,309*		*63,726*		*63,848*	
School FEs		Y	Y	Y	Y	Y	Y
Control for Share Hisp Cand		Y	Y	Y	Y	Y	Y
Control for # Seats		Y	Y	Y	Y	Y	Y
Election/Academic Yr, Period FEs		Y	Y	Y	Y	Y	Y
Other Election Controls		Y	Y	Y	Y	Y	Y
Demographic Controls		Y	Y	Y	Y	Y	Y
First Stage F-stat		—	16.42	—	14.36	—	16.25
First Stage Coefficient		—	0.085	—	0.084	—	0.084
			(0.017)		(0.017)		(0.018)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The sample includes school-period observations from pre-election period -8 through post-election period +8. Column 1 reports the mean of the first stage outcome in the left-hand column among observations without a top-tier Hispanic candidate (my control group). Standard deviations appear in parentheses. The remaining columns report regression coefficients using Equations 4 (reduced-form) and 3 (2SLS). Additional covariates are described in Table 6. Sample sizes do not add up across columns due to the exclusion of schools with exactly median Hispanic enrollment. Robust standard errors clustered at the district level appear in parentheses.

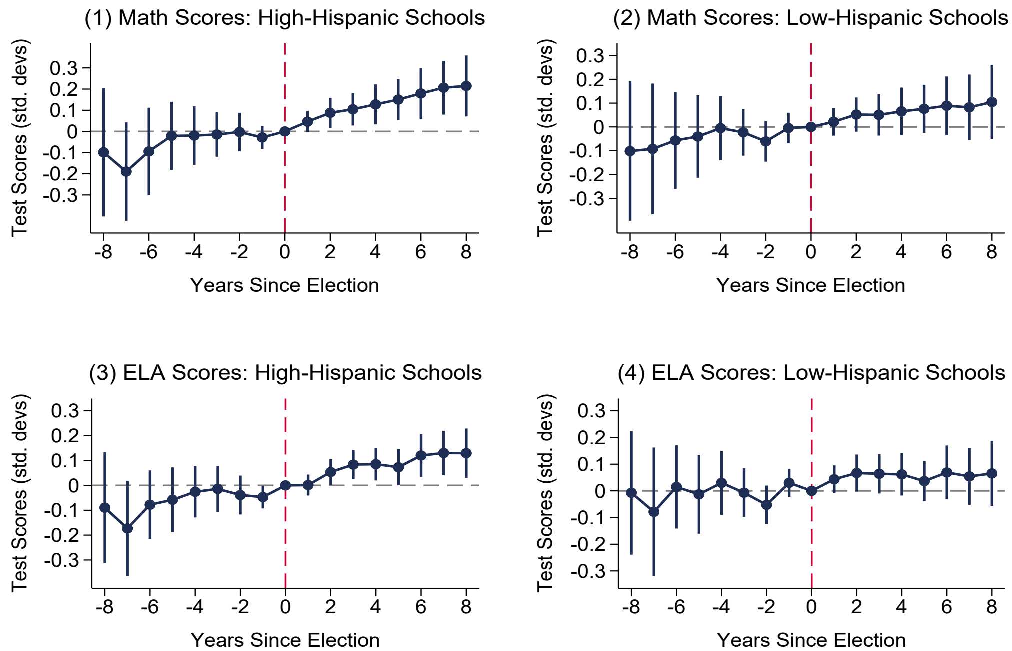

Figure 1: I measure all test score outcomes in district-subject-year-level standard deviation units. The specification is Equation 5. All coefficients are relative to the election year (period 0). Vertical bars denote 95 percent confidence intervals using robust standard errors clustered at the district level. Election-period sample sizes vary by panel due to missing test score and enrollment data as well as exclusion of schools with median Hispanic enrollment: N=56,295 for panel 1; N=50,141 for panel 2; N=56,288 for panel 3; and N=50,182 for panel 4.

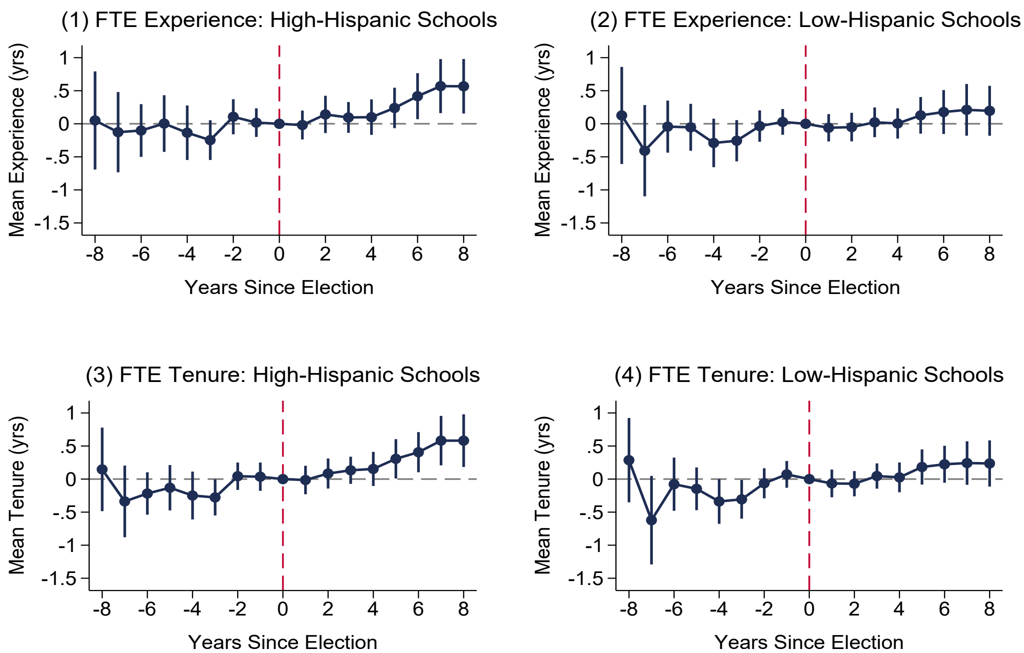

Figure 2: The specification is Equation 5. All coefficients are relative to the election year (period 0). Vertical bars denote 95 percent confidence intervals using robust standard errors clustered at the district level. The sample contains N=64,726 school-period observations in panels 1 and 3, and N=63,848 school-period observations in panels 2 and 4.

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9 Appendix

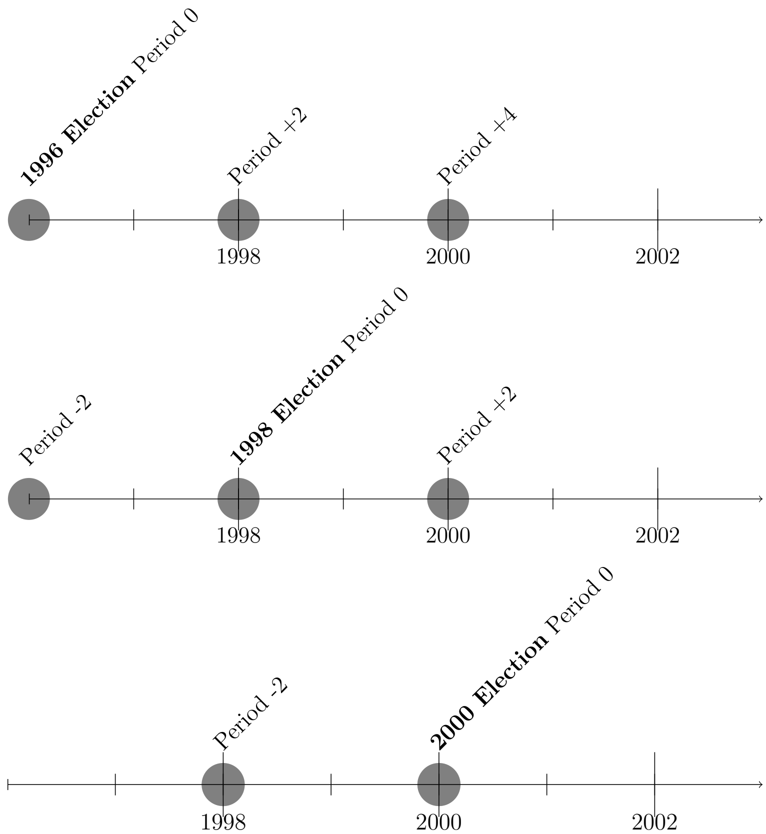

Figure A1: The diagram describes the overlapping panel structure I construct. Data from 1998 appear alternately as post-1996 election period 2 data; the 1998 election period 0 data; and pre-2000 election period -2 data.

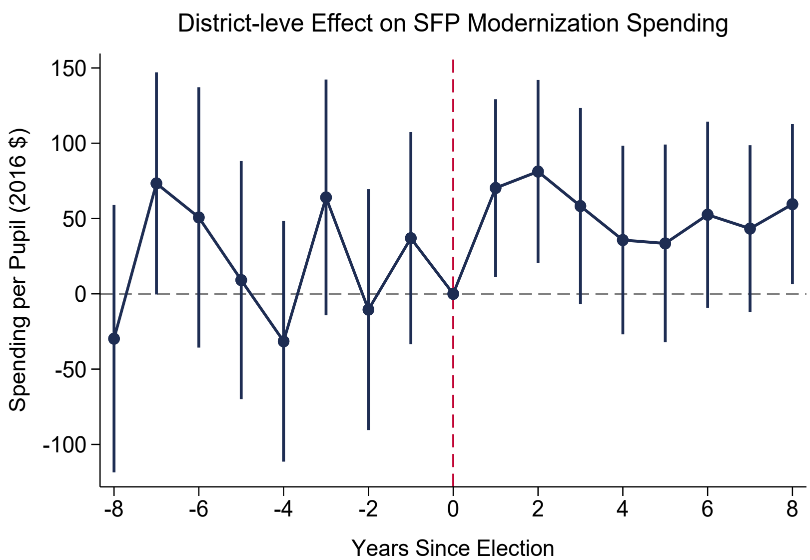

Figure A2: The figure plots event-study estimates of the top-tier Hispanic treatment effect on district-wide SFP modernization spending per pupil by year relative to the election. The specification is Equation 5. All coefficients are relative to the election year (period 0). Vertical bars denote 95 percent confidence intervals using robust standard errors clustered at the district level. The sample includes 16,601 district-by-election-period observations.

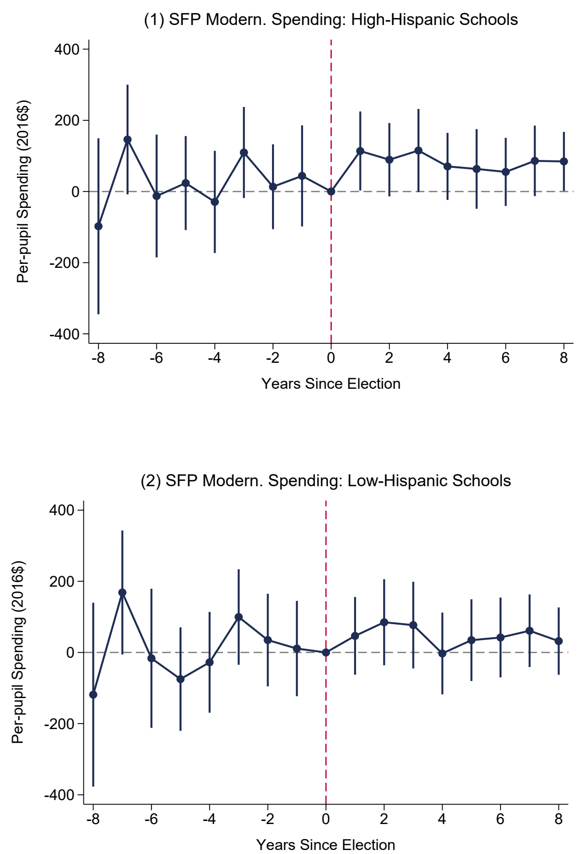

Figure A3: The specification is Equation 5. All coefficients are relative to the election year (period 0). Vertical bars denote 95 percent confidence intervals using robust standard errors clustered at the district level. The sample size is N=43,737 for both panels.

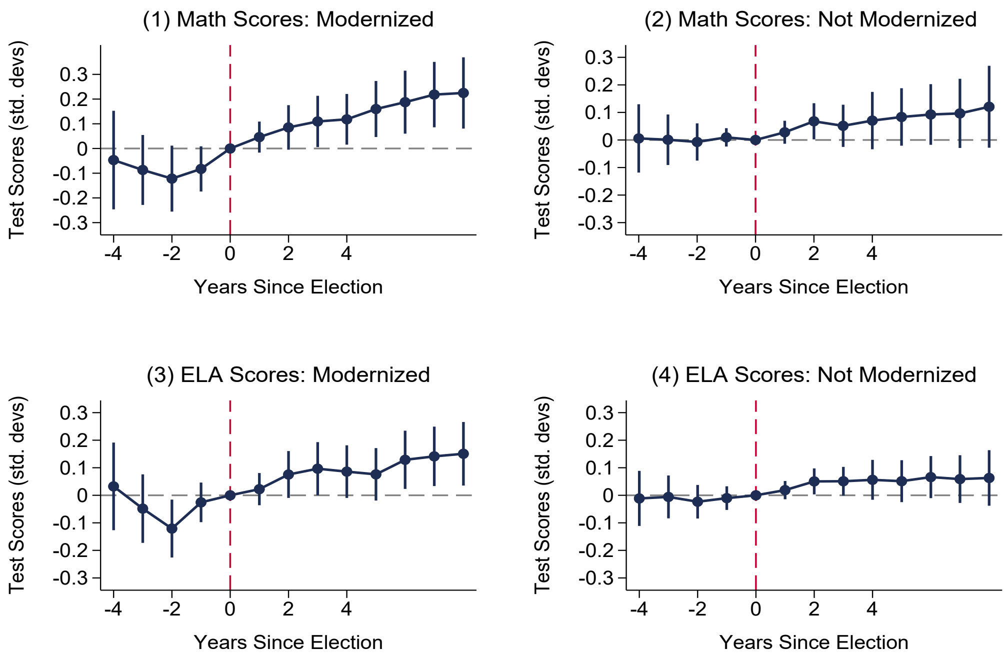

Figure A4: The figure shows event study plots depicting the estimated effect of a top-tier Hispanic candidate on test scores by year relative to the election. The sample is broken down into schools that did and did not initiate an SFP modernization project after the given election. The specification is Equation 5. All coefficients are relative to the election year (period 0). Vertical bars denote 95 percent confidence intervals using robust standard errors clustered at the district level. Sample sizes vary due to missing test score data, as discussed in the text: N=45,379 for panel 1; N=56,384 for panel 2, N=45,376 for pane1 3, and N=56,382 for panel 4.

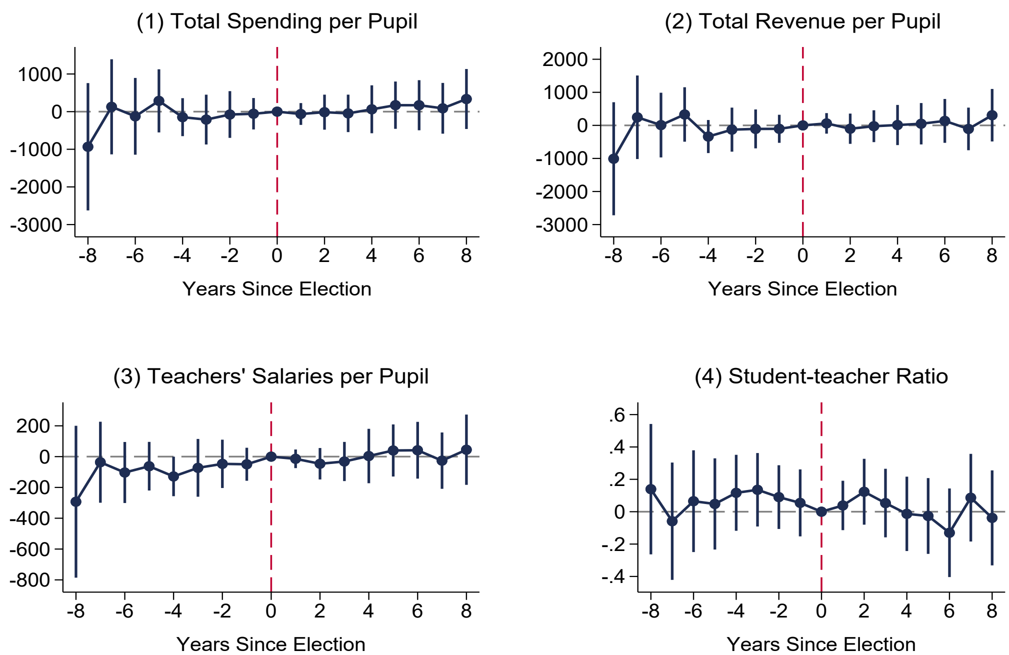

Figure A5: The figure shows event study plots depicting the estimated effect of a top-tier Hispanic candidate on non-SFP budget outcomes by year relative to the election. The data come from the Census of Governments. The specification is Equation 5 with district-level, rather than school-level, data. All coefficients are relative to the election year (period 0). Vertical bars denote 95 percent confidence intervals using robust standard errors clustered at the district level. Sample sizes vary due to missing data, as discussed in the test: N=17,414 for panels 1-3 and N=15,963 for panel 4.

Table A1: Summary of School Facility Program (SFP) Projects

	N	Mean	Median	Std Dev	Min	Max
I. All Projects						
Project Involves New Construction?	7641	0.27	0	0.44	0	1
Project Involves Modernization?	7641	0.75	1	0.43	0	1
Year Construction Began	7641	2005	2004	4.18	1999	2017
Total Funds ('000s)	7641	4212	2381	6427	4.6	128470
Funds from State ('000s)	7641	2131	1648	3910	3.7	73102
Funds from District ('000s)	7641	1480	621	2941	0	64235
Modernization Spending ('000s)	7641	1299	786	1876	0	18823
New Construction Spending ('000s)	7641	587	0	1890	0	34142
Supplemental SFP Grants ('000s)	7641	2326	1031	4541	0	97104
II. Projects w/ Enrollment Data						
Total Funds per Pupil	7412	5093	3699	7887	5.1	219253
School Enrollment	7412	956	700	738	0	5213
School Share FRL	7412	0.52	0.53	0.30	0	1
School Share White	7412	0.33	0.27	0.28	0	0.99
School Share Hispanic	7412	0.46	0.43	0.30	0	1
School Share Other Minority	7412	0.20	0.15	0.18	0	1

The table reports summary data from all 7641 SFP projects begun by 2017. All SFP data come from California's Bond Accountability program. Note that a project can have both modernization and new construction components. All enrollment data come from the Common Core of Data. There are 229 new construction projects that I cannot match to enrollment data because the OPSC only assigns temporary identifying information. I report all costs in 2016 dollars.

Table A2: Summary of Name Matching in California School Board Races

	All Candidates	All Hispanic Candidates	NALEO Match	Hispanic Surname	NALEO and Hispanic Surname
N	16112	2494	1233	2302	1041
Avg Name Hisp Share	0.16	0.84	0.80	0.89	0.90
	(0.32)	(0.20)	(0.27)	(0.08)	(0.06)

The elections sample is identical to the one used in Table 1 and contains 16112 candidates across 3738 elections. The first panel describes the candidate-level data I obtain from matching my list of candidates to the list of most common Census surnames by race and to the NALEO directory of Latino officials. Standard deviations appear in parentheses. "Name Hispanic Share" refers to the share of Census respondents with a given surname who self-identify as Hispanic. Note that because some surnames do not appear in the Census' list of common surnames, sample sizes vary: I match 14510 total candidates and 1176 NALEO-listed candidates to the Census list.

Table A3: Reduced-form Results Using Only Census Data to Identify Hispanics

	(1) NALEO + Census Name Matching	(2) Only Census Name Matching
I. First Stage		
Hisp. Board Share	0.081***	0.094***
	(0.017)	(0.019)
	63,726	*62,216*
II. Reduced-Form Estimates Among High-Hispanic Schools		
Modernization Spending per Pupil	56.3***	39.9***
	(17.1)	(18.0)
	43,610	*42,688*
Total SFP Spending per Pupil	72.8**	50.7
	(36.0)	(38.0)
	43,610	*42,688*
Composite Math Scores	0.041**	0.028
	(0.020)	(0.019)
	55,460	*54,187*
Composite ELA Scores	0.028*	0.022
	(0.015)	(0.015)
	55,453	*54,181*
FTE Experience	0.154**	0.138*
	(0.070)	(0.083)
	63,726	*62,216*
FTE Tenure	0.197***	0.165**
	(0.064)	(0.071)
	63,726	*62,216*

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

The specification is Equation 4 with controls for the share of Hispanic candidates, the number of contested seats, and school-level fixed effects, as well as additional election and demographic covariates, which appear below Table 6. Sample sizes appear in italics. The first column presents point estimates that appear in the main text of the paper and use my preferred definition of Hispanic candidates. The second column identifies Hispanic candidates using only Census name-matching. Robust standard errors clustered at the district level appear in parentheses. Sample sizes vary within columns because of missing enrollment and student performance data. Note that the first stage outcome in both columns is measured using NALEO data, whereas only the first column uses NALEO data to identify candidate ethnicities.

Table A4: Relevance and Excludability of Ballot Order, without Fixed Effects

	All Candidates		Hispanic Candidates	
	(1) Control Mean	(2) Top-tier Effect	(3) Control Mean	(4) Top-tier Effect
I. Impact of Top-tier Assignment on Candidate Performance				
Vote Share	0.185	0.039***	0.207	0.034***
	(0.116)	(0.003)	(0.129)	(0.006)
Wins?	0.401	0.152***	0.404	0.176***
	(0.490)	(0.014)	(0.491)	(0.022)
N:	*3418*	*6469*	*1088*	*2140*
II. Correlation of Top-tier Assignment with Candidate Traits				
Democrat?	0.473	-0.013	0.599	0.000
	(0.499)	(0.019)	(0.491)	(0.034)
Republican?	0.374	0.018	0.230	0.037
	(0.484)	(0.019)	(0.422)	(0.029)
N:	*1448*	*2724*	*421*	*847*
Hispanic?	0.348	0.026*	—	—
	(0.476)	(0.013)		
N:	*3130*	*5943*		
Incumbent?	0.315	0.085***	0.321	0.088***
	(0.465)	(0.013)	(0.047)	(0.021)
Missing Ethnicity?	0.084	-0.006	—	—
	(0.278)	(0.007)		
Missing Party?	0.576	0.005	0.613	-0.018
	(0.494)	(0.012)	(0.487)	(0.021)
N:	*3418*	*6469*	*1088*	*2140*

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

The table is identical to Table 4, except that the specification used does *not* include election fixed effects. Otherwise, the specification is Equation 1, including controls for the share of Hispanic candidates and the number of contested seats in the election. Robust standard errors are clustered at the district level.

Table A5: Excludability of Top-tier Hispanic IV, without Fixed Effects

	(1) Control Mean	(2) Top-tier Hispanic Effect
Hisp Board Share, Outgoing Board	0.13	0.01
	(0.21)	(0.01)
N:	*486*	*1,314*
Total Enrollment	11448	-714
	(12538)	(734)
Share White	0.36	-0.01
	(0.23)	(0.02)
Share Hispanic	0.48	0.00
	(0.24)	(0.02)
Share Asian	0.08	0.00
	(0.11)	(0.01)
Share Black	0.05	0.01*
	(0.06)	(0.00)
Share FRL-eligible	0.51	0.02
	(0.23)	(0.02)
N:	*409*	*1,149*
Math Composite Scores	-0.25	-0.02
	(0.80)	(0.05)
N:	*333*	*1,081*
ELA Composite Scores	-0.19	-0.03
	(0.86)	(0.05)
N:	*333*	*1,081*

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

The table is identical to Table 5, except that the specification does not include any year or district fixed effects. Otherwise, the specification is Equation 2 with controls for the share of Hispanic candidates and the number of contested seats in the election. Robust standard errors are clustered at the district level.

Table A6: Exploring Sensitivity of First Stage to "Top-of-the-ticket" Candidates

	Top-tier Effect	"Top-of-the-Ticket" Effect	Top-tier Effect, Excl. Top-of-the-Ticket
Hisp Board Share	0.086***	0.097***	0.047**
	(0.015)	(0.015)	(0.023)
N:	*141,329*	*141,329*	*87,483*
School FEs	Y	Y	Y
Control for Share Hisp Cand	Y	Y	Y
Control for # Seats	Y	Y	Y
Election/Academic Yr, Period FEs	Y	Y	Y

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

The data consist of school-period observations. The specification follows the first stage of Equation 3, with modifications as indicated in column headers. Column 2 is identical to the second column in Table 6. Column 3 uses as an instrument an indicator for whether a Hispanic candidate appeared in the first ballot position (the "top of the ticket"). Column 4 uses the top-tier Hispanic indicator, but excludes top-of-the-ticket Hispanic candidates (that is, the sample only includes elections with no top-tier Hispanic candidates and those with a top-tier Hispanic candidate who was not in the first ballot position). Robust standard errors clustered at the district level appear in parentheses.

Table A7: Correlation of Hispanic Top-Tier Indicator with SFP Outcomes, Election Characteristics

	(1) Control Mean	(2) Hisp Top tier Effect
I. Election Year SFP Outcomes		
Total SFP Spending per Pupil	348	114
	(988)	(111)
Modernization Spending per Pupil	140	2
	(424)	(41)
New Constr. Spending per Pupil	49	31
	(216)	(30)
Supplemental SFP Grants per Pupil	155	66
	(498)	(62)
Share of Students Treated	0.39	0.02
	(0.38)	(0.03)
N:	*392*	*1097*
II. Election Year SFP Eligibility Proxies		
Cum. SFP Spending per Pupil	2162	160
	(2933)	(311)
N:	*392*	*1097*
Total Enrollment	11448	75
	(12538)	(129)
# FTE Teachers	524	3
	(576)	(6)
Student-FTE Teacher Ratio	21.2	0.09
	(2.35)	(0.12)
Avg. Age of Schools	21.62	0.06
	(7.38)	(0.18)
Share of Schools Opened 1980	0.80	0.01
	(0.24)	(0.01)
N:	*409*	*1149*
III. Election Candidate Composition, Results		
Top-Tier Democrat?	0.58	0.02
	(0.46)	(0.06)
	486	*1314*
Top-Tier Incumbent?	0.29	0.05
	(0.45)	(0.05)
	486	*1314*
Share Missing Ethnicity	0.08	0.01
	(0.13)	(0.01)
	486	*1314*
# Democrat Wins	0.39	0.05
	(0.63)	(0.06)
	486	*1314*
# Incumbent Wins	1.12	-0.04
	(0.90)	(0.08)
	486	*1314*

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

The specification is Equation 2 with controls for the share of Hispanic candidates, the number of contested seats, and district-level fixed effects. Robust standard errors clustered at the district level appear in parentheses. Sample sizes vary within columns because of missing enrollment and student performance data.

Table A8: Robustness of Main Results to Alternative Regression Specifications, Reduced-form Estimates

	(1) No FEs Post-election Obs	(2) +Elec Yr FEs	(3) +Academic Yr FEs	(4) +Period FEs	(5) +Period 0 Obs	(6) + Periods -1 thru -8 Obs	(7) +School FEs	(8) + Elec Covars	(9) Preferred Model	(10) + Flexible Elec Covars
I. Student Achievement at High-Hispanic Schools										
ELA Composite Score	0.01	0.02	0.02	0.02	0.02	0.05**	0.03*	0.04**	0.03*	0.03*
	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
N	34,416	34,416	34,416	34,416	38,324	55,453	55,453	55,453	55,453	55,453
Math Composite Score	-0.04	-0.06	-0.06	-0.06	-0.06	-0.06	0.05**	0.05**	0.04**	0.04**
	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.02)	(0.02)	(0.02)	(0.02)
N	35,248	35,248	35,248	35,248	39,164	56,295	56,295	56,295	55,460	55,460
II. SFP Spending at High-Hispanic Schools										
Total SFP Spending per Pupil (School-level)	66.18*	73.45**	72.64**	72.61**	74.93**	69.13*	66.59*	73.19**	72.75**	78.24**
	(38.96)	(36.11)	(35.95)	(35.83)	(36.02)	(35.67)	(34.93)	(35.78)	(35.97)	(36.50)
Modernization Spending per Pupil (School-level)	55.34***	49.30***	48.98***	49.25***	53.11***	56.19***	57.52***	58.22***	56.28***	57.52***
	(18.98)	(18.68)	(18.65)	(18.69)	(18.48)	(16.25)	(17.38)	(16.98)	(17.14)	(17.38)
N	20,481	20,481	20,481	20,481	24,822	43,737	43,737	43,737	43,610	43,610
III. FTE Tenure and Experience at High-Hispanic Schools										
FTE Mean Experience	0.20	0.18	0.19	0.19	0.19	0.19	0.18**	0.19**	0.18**	0.18**
	(0.20)	(0.20)	(0.20)	(0.20)	(0.20)	(0.20)	(0.07)	(0.07)	(0.07)	(0.07)
FTE Mean Tenure	0.02	0.05	0.06	0.06	0.08	0.14	0.22***	0.24***	0.22***	0.22***
	(0.14)	(0.13)	(0.13)	(0.13)	(0.12)	(0.12)	(0.07)	(0.07)	(0.06)	(0.07)
N	40,354	40,354	40,354	40,354	44,320	59,553	59,553	59,553	58,669	58,669

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Each cell comes from a separate regression. The sample consist of school-period observations. All samples include only high-Hispanic schools (the subsample of greatest interest in this paper). The base specification in column 1 regresses the outcome on the top-tier Hispanic indicator and controls for the share of Hispanic candidates and number of contested seats. The sample only includes post-election outcomes. Subsequent columns add additional controls and pre-election observations as indicated. Column 9 reports results that appear in the main paper, with full demographic and election controls, which are described below Table 6. Note that sample sizes decline across columns 8 and 9 because of missing demographic variables. Column 10 presents a specification based on the preferred model which adds a quadratic term in the share of Hispanic candidates and uses a fixed effect for the number of contested seats (instead of a linear control as in the remaining columns). Standard errors in parentheses are clustered at the district level.

Table A9: Specification Robustness: Comparing Fixed Effect Choice

	School-level Regression Estimates						
		By Share Hispanic		By Share FRL		By Title I	
	All Schools	Above Median	Below Median	Above Median	Below Median	Title I Eligible	Not Title I Eligible
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
I. School-by-Election Fixed Effects							
Modernization Spending per Pupil	67.4**	79.4***	44.7	93.2***	53.3*	78.2***	31.4
	(26.2)	(28.7)	(32.5)	(31.6)	(29.3)	(27.6)	(44.8)
Total SFP Spending per Pupil	76.2	92.2	39.1	97.3	58.6	106.4*	-10.9
	(57.6)	(65.9)	(70.0)	(66.3)	(67.2)	(62.9)	(90.8)
N	*92,607*	*43,610*	*43,635*	*43,510*	*43,510*	*64,827*	*27,768*
Math Composite Score	0.072**	0.092**	0.079	0.099**	0.053	0.016	0.015
	(0.036)	(0.043)	(0.051)	(0.049)	(0.045)	(0.036)	(0.067)
N	*110,610*	*55,460*	*49,476*	*57,488*	*46,805*	*83,118*	*27,484*
ELA Composite Score	0.052**	0.071**	0.055	0.076**	0.037	0.011	0.027
	(0.027)	(0.034)	(0.038)	(0.036)	(0.035)	(0.030)	(0.056)
N	*110,607*	*55,453*	*49,477*	*57,484*	*46,805*	*83,112*	*27,484*
Mean FTE Experience	0.292**	0.384**	0.249*	0.449***	0.231*	0.241	0.213
	(0.122)	(0.161)	(0.132)	(0.174)	(0.131)	(0.153)	(0.170)
Mean FTE Tenure	0.350***	0.439***	0.310**	0.504***	0.271**	0.319**	0.193
	(0.125)	(0.157)	(0.137)	(0.170)	(0.134)	(0.160)	(0.159)
N	*135,309*	*63,726*	*63,848*	*63,592*	*63,592*	*95,827*	*37,143*
II. District Fixed Effects							
Modernization Spending per Pupil	50.8***	62.3***	38.4**	66.1***	41.1**	64.0***	23.9
	(16.0)	(16.9)	(18.5)	(18.9)	(16.8)	(18.0)	(23.4)
Total SFP Spending per Pupil	58.9*	82.2**	37.7	83.0**	41.1	97.8**	-20.7
	(34.3)	(35.9)	(39.4)	(38.0)	(37.6)	(40.3)	(45.8)
N	*92,607*	*43,610*	*43,635*	*43,510*	*43,510*	*64,827*	*27,768*
Math Composite Score	0.030*	0.041**	0.026	0.038*	0.020	0.006	0.02
	(0.017)	(0.019)	(0.020)	(0.021)	(0.019)	(0.016)	(0.026)
N	*110,610*	*55,460*	*49,476*	*57,488*	*46,805*	*83,118*	*27,484*
ELA Composite Score	0.022*	0.028*	0.022	0.025	0.019	0.003	0.033
	(0.012)	(0.015)	(0.015)	(0.015)	(0.015)	(0.013)	(0.022)
N	*110,607*	*55,453*	*49,477*	*57,484*	*46,805*	*83,112*	*27,484*
Mean FTE Experience	0.143**	0.182**	0.134**	0.177**	0.126*	0.109	0.131*
	(0.063)	(0.080)	(0.067)	(0.077)	(0.068)	(0.072)	(0.075)
Mean FTE Tenure	0.183***	0.223***	0.172***	0.226***	0.160**	0.152**	0.136**
	(0.061)	(0.074)	(0.066)	(0.074)	(0.065)	(0.074)	(0.068)
N	*135,309*	*63,726*	*63,848*	*63,592*	*63,592*	*95,827*	*37,143*
III. Neither District Nor School Effects							
Modernization Spending per Pupil	42.2***	54.9***	32.0***	57.0***	34.8***	55.6***	11.0
	(15.9)	(10.3)	(11.1)	(10.4)	(11.0)	(8.4)	(14.9)
Total SFP Spending per Pupil	61.0*	85.0***	46.6**	86.6***	49.1**	100.0***	-14.9
	(35.0)	(22.3)	(22.2)	(21.5)	(22.9)	(18.1)	(28.7)
N	*92,607*	*43,610*	*43,635*	*43,510*	*43,510*	*64,827*	*27,768*
Math Composite Score	0.060**	0.080***	0.044***	0.077***	0.034***	0.046***	0.002
	(0.027)	(0.010)	(0.011)	(0.010)	(0.012)	(0.008)	(0.016)
N	*110,610*	*55,460*	*49,476*	*57,488*	*46,805*	*83,118*	*27,484*
ELA Composite Score	0.052**	0.056***	0.048***	0.063***	0.033***	0.040***	0.015
	(0.025)	(0.009)	(0.011)	(0.009)	(0.011)	(0.007)	(0.016)
N	*110,607*	*55,453*	*49,477*	*57,484*	*46,805*	*83,112*	*27,484*
Mean FTE Experience	0.093	0.075**	0.140***	0.125***	0.063**	0.039	0.188***
	(0.121)	(0.031)	(0.032)	(0.031)	(0.032)	(0.026)	(0.041)
Mean FTE Tenure	0.103	0.101***	0.150***	0.147***	0.078**	0.044*	0.157***
	(0.106)	(0.029)	(0.030)	(0.029)	(0.030)	(0.024)	(0.040)
N	*135,309*	*63,726*	*63,848*	*63,592*	*63,592*	*95,827*	*37,143*
Election and Academic Yr FEs	Y	Y	Y	Y	Y	Y	Y
Control for Share Hisp Cand	Y	Y	Y	Y	Y	Y	Y
Control for # Seats	Y	Y	Y	Y	Y	Y	Y
School Demographic Controls	Y	Y	Y	Y	Y	Y	Y
Other Election Controls	Y	Y	Y	Y	Y	Y	Y

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

The sample includes school-period observations. Each cell comes from a separate reduced-form regression, following Equation 4, with modifications as indicated in the panel headers. Note that the first panel—using school-by-election effects—does not include any election-level covariates, which are collinear with the fixed effects. See the footnote below Table 6 for descriptions of the election and demographic controls used. All specifications use school enrollment weights. Sample sizes vary within columns due to missing enrollment data and exclusion of schools with exactly median enrollment by category. Robust standard errors are clustered at the district level.

Table A10: Are the Treatment Effects on High- and Low-Hispanic Schools Statistically Different?

	(1) Top-tier Hispanic Effect	(2) High Hispanic Interaction Effect
Total SFP Spending per Pupil	44.13	35.62*
	(35.16)	(21.01)
Modernization Spending per Pupil	43.62***	16.61*
	(16.80)	(9.05)
N	92,607	
Math Composite Score	-0.07***	0.19***
	(0.02)	(0.02)
N	110,613	
ELA Composite Score	-0.05***	0.13***
	(0.01)	(0.02)
N	110,607	
FTE Mean Experience	0.04	0.21***
	(0.06)	(0.06)
FTE Mean Tenure	0.08	0.22***
	(0.06)	(0.06)
N	135,309	

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

The data consist of school-period observations. Each row comes from a single regression. The reduced-form specification is analogous to Equation 4 but includes a fixed effect for whether the school has above-median Hispanic enrollment ("high Hispanic") and interacts this dummy with my main treatment variable ($TopTierHisp \times post \times HighHisp$). Sample sizes vary due to missing test score data and different post-election time frames. All specifications include school, election year, academic year, and period fixed effects, as well as controls for the share of Hispanic candidates and the number of contested seats, as discussed in the text. All specifications also include other election controls and demographic controls, as described below Table 6. Standard errors are clustered at the district level.

Table A11: Pre-election Placebo and Balance Estimates, by School Type

	Sample Mean	All Schools	Share Hispanic: Above Median	Share Hispanic: Below Median	Share Hispanic: p-value	Share FRL: Above Median	Share FRL: Below Median	Share FRL: p-value	Title I: Title I Eligible	Title I: Not Title I Eligible	Title I: p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Total SFP Spending per Pupil	456.3	32.6	6.3	-15.3	0.704	58.2	-0.4	0.054	-20.4	31.9	0.636
	(1482.5)	(60.4)	(72.6)	(74.3)		(69.1)	(65.7)		(74.5)	(95.2)	
N	25179	25179	11867	11867		11826	11826		17512	7664	
Mdernization Spending per Pupil	448.0	19.0	-0.0	1.1	0.965	17.5	17.5	0.279	-10.2	21.6	0.564
	(1130.6)	(27.4)	(29.7)	(34.3)		(30.3)	(32.4)		(28.9)	(50.2)	
N	27079	27079	11867	11867		11826	11826		17512	7664	
Cum. SFP Spending per Pupil	4695.6	129.8	54.6	-119.9	0.369	127.6	5.71	0.245	29.3	76.8	0.85
	(8538.3)	(167.4)	(175.4)	(198.7)		(171.7)	(173.9)		(188.5)	(221.9)	
N	9788	9788	4342	4342		4330	4330		6500	2704	
Total Enrollment	909.0	-0.2	-3.8	14.6	0.52o	-7.6	-3.2	0.973	7.5	-48.8	0.231
	(636.7)	(5.2)	(21.7)	(17.2)		(18.6)	(8.5)		(21.2)	(36.1)	
N	9206	9206	4342	4342		4330	4330		6500	2704	
Total FTE Teachers	41.58	-0.03	-0.42	0.58	0.404	-0.46	-0.12	0.961	-0.07	-1.94	0.268
	(25.47)	(0.26)	(0.83)	(0.82)		(0.75)	(0.43)		(0.87)	(1.30)	
N	9205	9205	4342	4342		4330	4330		6500	2704	
Student-FTE Ratio	21.22	0.00	0.07	0.05	0.878	0.08	-0.02	0.849	0.16	-0.04	0.411
	(2.82)	(0.14)	(0.15)	(0.14)		(0.12)	(0.16)		(0.12)	(0.21)	
N	9204	9204	4342	4342		4330	4330		6500	2704	
School Age	23.3	0.04	0.06	0.17	0.589	0.11	-0.11	0.383	-0.11	0.56**	0.02
	(7.18)	(0.04)	(0.08)	(0.15)		(0.12)	(0.13)		(0.09)	(0.22)	
N	9230	9230	4342	4342		4330	4330		6500	2704	
School Opened in 1980?	0.889	0.002	0.003	0.004	0.962	0.005	-0.006	0.561	-0.005	0.013	0.234
	(0.315)	(0.002)	(0.004)	(0.005)		(0.004)	(0.005)		(0.005)	(0.012)	
N	9237	9237	4342	4342		4330	4330		6500	2704	
Share Hispanic	0.559	0.003	0.006*	0.002	0.103	0.009***	-0.001	0.015	0.008	0.006	0.594
	(0.276)	(0.003)	(0.003)	(0.003)		(0.003)	(0.004)		(0.003)	(0.008)	
Share White	0.242	-0.004*	-0.006**	-0.003	0.484	-0.005**	-0.003	0.526	-0.001	-0.006	0.454
	(0.230)	(0.002)	(0.003)	(0.003)		(0.002)	(0.003)		(0.002)	(0.006)	
N	9205	9205	4342	4342		4330	4330		6500	2704	
Share FRL	0.589	0.002	0.004	0.002	0.685	-0.001	0.002	0.423	-0.001	0.013	0.372
	(0.285)	(0.004)	(0.006)	(0.005)		(0.004)	(0.004)		(0.007)	(0.013)	
N	9186	9186	4342	4342		4330	4330		6500	2704	
Elec Yr FEs	—	Y	Y	Y		Y	Y		Y	Y	
District FEs	—	Y	Y	Y		Y	Y		Y	Y	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The table presents placebo and robustness results for my school-level SFP analysis. The sample contains school-period observations. Column 1 reports the sample mean of each variable in the election year. The remaining columns report OLS coefficients from separate regressions. The specification employs district and election year fixed effects and controls for the share of Hispanic candidates and the number of contested seats. The first panel uses data from the three periods prior to the election. All other models use data from only the election year. All specifications use school enrollment weights. Sample sizes vary within columns due to missing enrollment data. Robust standard errors are clustered at the district level.

Table A12: Reduced Form Effect on District Demographics, 1-6 and 1-8 Years After Election

	Sample Mean	After 6 Year	After 8 Years
	(1)	(2)	(3)
Total Enrollment	10618	-40.1	-53.1
	(13106)	(64.0)	(61.6)
	7515	*10021*	*12313*
Share White	0.27	0.003	0.003*
	(0.23)	(0.002)	(0.002)
Share Hisp	0.57	-0.002	-0.003*
	(0.27)	(0.002)	(0.001)
	7505	*10010*	*12298*
Share FRL-eligible	0.58	-0.001	-0.000
	(0.25)	(0.003)	(0.003)
	7515	*10010*	*12298*
FRL Dissimilarity Index	0.24	0.002	0.002
	(0.16)	(0.003)	(0.002)
White Dissimilarity Index	0.20	0.001	0.001
	(0.15)	(0.002)	(0.002)
Hispanic Dissimilarity Index	0.19	0.000	0.001
	(0.14)	(0.002)	(0.002)
	7515	*10021*	*12313*

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

The sample mean consists of data from years 1-6 after the election. The regression specification follows Equation 4 with district, election year, and test year fixed effects, as well as the same election and demographic covariates described below Table 7. Sample sizes appear in italics. Sample sizes vary due to missing data. Robust standard errors are clustered at the district level.