School Board Capacity*

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Abstract

School boards have statutory authority over most K–12 policies, but longstanding concerns about members' limited expertise and weak incentives raise questions about their ability to drive policy. Using new data on board members' priorities and actions, combined with a regression discontinuity design that provides quasi-random variation in board composition, we show that board members have large effects on district outcomes across many policy domains. On average, electing a member aligned with a given priority shifts related outcomes by 0.21 standard deviations in the expected direction. Directly observing policy priorities, rather than weakly correlated demographic or professional proxies, is crucial for understanding board capacity.

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1 Introduction

On paper, local boards of education are among the most important policy actors in the United States. School boards are the governing bodies with ultimate responsibility for policy choices in most school districts. Their typical purview includes—but is not limited to—hiring and firing top district management, negotiating with teachers' unions, and setting budgets. Collectively, these budgets are large: the U.S. spent $813 billion on public schools in the 2020-21 school year, more than the federal government spent on Medicare or defense (National Center for Education Statistics, 2023; Congressional Budget Office, 2021).

Despite their statutory importance, school boards receive relatively little attention from researchers. This lack of interest is consistent with a view that local education governance in the U.S. is broken: voters do not hold board members accountable to students, board members lack relevant expertise, and boards are captured by organized interests (Chubb and Moe, 1988; Kogan, 2022). Education reforms, such as accountability systems and state takeovers, represent efforts to mitigate the consequences of school boards' dysfunction (Land, 2002). In this view, while an individual board member might conceivably be able to affect district outcomes, including student academic performance, such impacts are likely to be idiosyncratic and small in magnitude. Whether this view is justified, however, is ultimately an empirical question.

A fundamental challenge in assessing the importance of school boards is the absence of data on the central causal mechanism: the goals and actions of members. This challenge extends beyond education, reflecting a broader difficulty with studying board governance. As Adams et al. (2010) observe in the context of corporate boards, researchers are generally forced to study observable characteristics of boards or board members—i.e., their *identities*—that are "presumed to correlate" with differences in behavior. In the case of school boards, prior work has generally examined district-level impacts of electing board members with particular backgrounds, such as educators (Shi and Singleton, 2023) or Hispanic candidates (e.g. Kogan et al., 2021). This approach speaks directly to the effects of group representation and can establish that boards *can* matter. However, if identities are imperfect proxies for policy actions and priorities, i.e., *ideologies*, identity-based analyses may understate the magnitude and scope of board capabilities.

This paper uses new data on the viewpoints and policy actions of board members, coupled

with a regression discontinuity design, to establish that school boards are much more important policy actors than previously recognized. The priorities of board members shape district policy across many domains, with effect sizes similar to those of large shifts in better-studied inputs such as teacher or school quality. We further show that, because the correlation between identity and ideology is often weak, observing policy priorities is crucial for understanding the importance of school boards. Though identity predicts ideology in some cases, the effects of electing candidates of a given identity are generally small and are fully explained by differences in policy priorities.

Our analysis centers on school board elections in California. Our main dataset contains information on 22,426 candidates competing in 5,782 elections held in 761 districts between 1998 and 2022. We use large language models (LLMs) to create data on board members' policy priorities from the text of political platforms compiled by the League of Women Voters, available for 38% of all candidates. We also use LLMs to construct data on members' actions from the text of board meeting minutes. Our coding of these data sources focuses on topics that are the subject of previous research on school boards (equity, fiscal conservatism, and support for teachers), central to debates on education policy (capital investments, safety and health, dropout prevention, student enrollment, career and technical education, and achievement), core board responsibilities emphasized in the corporate board literature (executive accountability; here, the superintendent), or the frequent subject of candidate discussion (parent engagement and the presence of controversial views in district curricula or practices, which we term "agenda bias").

Benchmarking exercises show that machine codes are as accurate as human hand-codes and provide comprehensive coverage of board debate. Yet whether these texts actually capture the underlying mechanisms of governance is not obvious. Candidate platforms may just reflect cheap talk, and minutes may record little of substance. The substantive validation comes from estimating causal impacts of apparent shifts in board ideological composition on policy discussion and district outcomes, for which we use public datasets describing financial, workforce, and student outcomes.

Understanding whether board members achieve their policy goals requires a mapping between candidate types and district outcomes. For priority groups, we choose outcomes that correspond to candidates' stated goals. For example, for equity-focused candidates we take the test score performance of a district's low-income students and Hispanic students as the outcomes of interest, while for fiscally conservative candidates we take per-pupil spending. For identity groups, we consider

hypotheses from the school board literature on the relationship between identities and outcomes and augment them with additional hypotheses based on cross-group differences in policy views.

We begin our empirical work by describing the relationship between identity and policy priorities, with the main finding being that this relationship is weak. We focus on identity groups central to previous studies of school boards: gender, ethnicity (specifically Hispanicity, given the large Hispanic population in California), experience working as a teacher, and political affiliation. As expected, some policy priorities are correlated with identity characteristics. For example, 34% of Hispanic candidates prioritize trimming school budgets, compared to 44% of non-Hispanic candidates, and 41% of female candidates cite equity as a priority, compared to 27% of male candidates. However, these are extreme cases. Views on most issues are more balanced across most groups. Further, even on the most polarized topics, substantial within-group variation remains. A random forest model that includes all background variables predicts whether a candidate prioritizes a given issue only slightly better than a correctly weighted coin flip.

We then turn our attention to the close elections that are the focus of our causal analysis. We estimate regression discontinuity models that compare board composition in the years following close elections where a candidate with a given attribute either just beats or just loses to a candidate without that attribute. We focus our exposition on three leading candidate types—equity-focused candidates, fiscally conservative candidates, and Hispanic candidates—while also presenting aggregate analyses of the full set of identity and priority types.

We first show that electing a candidate with a given priority shapes the distribution of priorities on the board, but electing a candidate with a given identity does not. For example, electing a candidate who prioritizes equity raises the share of board members who prioritize equity by about 21 percentage points from a base of 25%. In contrast, changes in the identity composition of boards lead to small or null shifts in board priorities. This is because ideological differences between identity groups are simply not that big in the cross-section, not because marginal candidates from different identity groups are more ideologically similar to one another than average candidates.

We then turn to our main results on how school boards affect district outcomes. Focusing first on our leading examples, we find that electing an equity-prioritizing board member raises test scores for low-income students in the district by an average of 0.088 student-level standard deviations (henceforth SDs; 95% CI=[0.062, 0.134]) in the five years following the election. Electing a fiscally

conservative candidate reduces per-pupil spending by $1,212 per year (13% of the baseline mean; 95% CI=[-$1,455, -$1,086]) over that same period.

These effects are large in magnitude. For example, the effect of electing an equity-focused board member on low-income students' test scores is roughly equivalent to assigning *every* such student a teacher who is about 0.8 SDs higher in the distribution of teacher value-added for one year or sending *every* such student to a school that is about 0.4 SDs higher in the distribution of school-level value added for one year (Chetty et al., 2014; Angrist et al., 2017). The decline in school spending due to electing a fiscally conservative board member is similar in magnitude to the gains in spending for beneficiaries of school finance reforms reported in Lafortune et al. (2018).

The effects of electing a Hispanic board member are much smaller. Electing a Hispanic board member has a modest positive effect on test scores for Hispanic students (0.021 SDs; 95% CI=[0.000, 0.046]), consistent with Kogan et al. (2021) and Fischer (2023). The effect of electing a Hispanic member on spending is about one-tenth as large as the effect of electing a fiscal conservative.

This pattern is representative of the broader sample of ideology and identity types. Pooling across all ideology types, we find that the average effect of electing a candidate with a given view is to raise the value of the associated outcomes by 0.21 SDs (95% CI=[0.19, 0.24]). In contrast, the average effect of electing a candidate of a given identity is much smaller: 0.04 SDs for identity-outcome hypotheses studied in previous literature and 0.01 SDs for other hypotheses based on identity differences in policy beliefs. Moreover, the identity effects we observe are explained by ideological differences: holding ideology fixed eliminates or reverses identity effects, while holding identities fixed only slightly reduces our estimates of ideology effects. Taken together, these results imply that identity impacts systematically understate board capacity.

Having established that school boards succeed in changing district policies and student outcomes, we next ask how they do it. The goal is not to document all the channels through which boards can shape policy, but to shed light on mechanisms and to test our results by verifying that a) intermediate outcomes point in similar directions, b) changes in policy are consistent with the scope of board authority, and c) members take action in the boardroom to pursue their policy goals. We focus on our leading case studies to permit more detailed analysis.

We first show that board members advance their goals in ways consistent with their statutory authority. One type of authority boards have is budget allocation. This seems to be an important

channel through which equity effects operate: we find that equity-focused board members shift facilities funding and non-teacher staffing (e.g., paraprofessionals) towards schools with more low-SES students. Boards in California also have the authority to raise revenue for capital projects and therefore to set capital budgets, but not operations budgets. Consistent with this point, we find that fiscally conservative board members realize cost savings entirely through reduced capital spending, and that declines in capital spending coincide with declines in capital revenues. Finally, boards place accountability pressure on management. Electing a fiscally conservative board member dramatically raises the chances of superintendent turnover.

We then turn to the actions members take in meetings. Existing work highlights two channels: board members may change consensus views through debate or agenda setting, or they may cast pivotal votes on contentious topics (Black, 1948; Baron and Ferejohn, 1989; Kogan et al., 2021).

We use our data on meeting records to show that board members effect change through both channels. We find evidence that equity-focused candidates cast pivotal votes on contested issues. When an equity-focused candidate wins a closely contested election, the winner of that election casts an average of 0.18 (159% of the baseline mean; 95% CI=[0.09, 0.27]) more pivotal votes on equity topics each year, and 1.28 (100% of the baseline mean; 95% CI=[0.69, 1.86]) more pivotal votes across all topics. We do not see much evidence that equity candidates change the type or quantity of votes held. In contrast, fiscally conservative candidates appear to effect change through agenda-setting. When a fiscally conservative candidate is narrowly elected, they are no more likely to cast pivotal votes on budget issues or other issues. Instead, the board proposes and passes 15 fewer motions per year on budget topics (34% of the baseline mean; 95% CI=[-21.12, -9.25]) and 51 fewer motions overall (33% of the baseline mean; 95% CI=[-68.85, -33.47]).

The main contribution of this analysis is to show that school boards are important policy actors: changes in board membership have large effects on district performance across a broad set of outcomes. This finding matters because many education interventions with strong micro-level evidence are difficult to scale. Teacher quality, for example, is central to student outcomes (Rivkin et al., 2005; Kane and Staiger, 2008; Chetty et al., 2014), yet district-wide efforts to improve teacher quality often disappoint (e.g., Stecher et al. 2018). By contrast, the governance changes we study translate directly to district-level outcomes since that is the level at which boards operate.

Our second contribution is to illustrate the importance of measuring policy priorities and ac-

tions when evaluating the efficacy of boards. Our analysis of identity effects synthesizes and replicates key prior findings from the school board setting (e.g. Kogan et al. 2021; Fischer 2023; Shi and Singleton 2023), which similarly rely on quasi-experimental variation; that is, when we study the same identity–outcome pairings, we obtain effects with the same sign and, often, statistical significance.[1] But our results also show that identity is a poor guide to the magnitude and scope of board influence. Observable background characteristics are only weakly related to candidates' policy priorities, and identity effects are largely explained by those priorities. Thus, analyses that rely on identity miss much of what boards do and understate how broadly they can matter.

The closest precursor to our work is Stemper (2022a), which uses candidate-platform text to study fiscally conservative "budget hawks." We reproduce the core finding of Stemper (2022a) using LLM-based text analysis and innovate in several ways: we measure many policy priorities rather than a single dimension, jointly analyze priorities and identities, and study mechanisms using intermediate outcomes and meeting minutes. These new approaches generate our central conclusions about the magnitude and scope of board influence and the importance of measuring policy priorities directly. Our analysis of boardroom actions also connects to uses of school board records in other contexts (Karpowitz and Mendelberg, 2014; Anglin et al., 2025; Mellon, 2025).

The point that it is important to measure policy priorities and actions directly when assessing the performance of decision-making bodies extends beyond school boards. In the corporate boards literature, researchers have long noted that the central object of interest is board behavior, but that behavior is difficult to observe (Adams et al., 2010). Empirical work thus often studies characteristics such as gender, professional background, or worker status that are presumed to correlate with decision-making (e.g., Matsa and Miller, 2013; Bertrand et al., 2019; Jäger et al., 2021). Similar issues arise in a broader literature on group decision-making (e.g., Chattopadhyay and Duflo, 2004; Bagues et al., 2017; Budde et al., 2024; Brito Rebolledo et al., 2024). While this work sheds light on the effects of *representation*, we show that identity-based estimates can understate institutional *capacity*. By using LLMs to code the policy views of thousands of local decision-makers, our paper connects group decision-making with a political science literature on whether national parties deliver on stated priorities (Budge and Hofferbert, 1990; Thomson et al., 2017).

[1] Note that the school board literature also considers candidate characteristic-outcome pairs that we do not. Macartney and Singleton (2018), for example, show that Democratic-registered members reduce student segregation across schools.

2 Background and Data

2.1 Boards

The economic role of governing boards is to mitigate agency problems that often plague organizations (Hermalin and Weisbach, 2003). Boards are composed of members, generally elected or appointed by the organization's stakeholders, who meet regularly to recruit, monitor, and, at times, replace management, and to make strategic decisions vital to the functioning of the organization. Across sectors, the existence and some of the characteristics of boards are often mandated by regulation, although organizations typically retain some flexibility over design. Public and private corporations in most countries must have boards of directors. Most non-profit organizations similarly have boards of trustees. Boards are also common among local governments, such as cities, towns, and school districts. The latter are the focus of this paper.

2.1.1 School Boards

Functions Nearly all of the 14,000 school districts in the United States are governed by boards of education, commonly referred to as school boards. These governing bodies, generally composed of 3 to 11 members, are responsible for the functioning of each school district. School board responsibilities include drafting curricula, developing the district's strategies, managing the budget and allocating funds to schools, engaging with the community, and implementing federal and state programs as well as court orders (Hochschild, 2005; Maeroff, 2010; Shi and Singleton, 2023). A key responsibility of school boards is recruiting and evaluating district and school management (such as the superintendent, top administrators, and school principals). Boards in most states also collectively bargain with the local teachers' union over employment contracts, influencing terms of employment such as pay, hours, and working conditions.

Elections In approximately 95% of U.S. school districts, board members are elected through local elections.[2] While the timing of these elections varies across states, they tend to take place during general elections in November or during local elections in the spring or fall. Each member's term

[2]Exceptions include Hawaii's single statewide district, certain districts in Indiana, and city school systems like Washington, D.C.

lasts 3 to 4 years. Terms generally do not expire at the same time, with many districts using a staggered election system to ensure continuity in the board's membership. Typically, about one-third of the board's seats are filled in each election.

Meetings Most school boards in the U.S. operate through regular meetings, which occur at least once a month.[3] Due to so-called "sunshine laws," most states have rules requiring that school boards be accessible to the public. The calendar and agenda of each meeting are thus made public in advance and meetings are open to the general public, who can participate in the discussion. The content of the meetings is also disclosed in the form of minutes, typically published on each district's website.

School Boards in California Boards in California share many features with boards across the country, but a few California-specific characteristics are important to highlight. Like most other school boards, elections are staggered and members are elected to four-year terms. Additionally, school boards in California must bargain with teachers' unions at least once every three years (Shi and Singleton, 2023). Since the passage of Proposition 13 in 1978, California school boards do not have the power to set local property tax rates, a common source of funds for school operations (Loeb, 2001). However, they retain the authority to initiate local bond measures to raise funds for capital projects, subject to voter approval, and can decide how to allocate additional state funds for these projects (Cellini et al., 2010; Biasi et al., 2025). In sum, California school boards have three main channels through which they can directly influence district policy: 1) choosing or influencing district leadership; 2) allocating funds from a fixed budget to different uses, including through negotiations with teachers' unions; and 3) raising and spending additional funds for capital projects.

2.2 Data

2.2.1 School Board Elections

Our analysis combines data from several sources. Data on school board elections come from the California Elections Data Archive (CEDA) of California State University, Sacramento and cover the years 1998-2022. These records include the name of the school district, the type of election (at-large

[3] See records of the National School Boards Association (NSBA), available at https://www.nsba.org.

or by trustee area, full- or short-term), the date, the name of each candidate, the number of votes received, and each candidate's occupation.[4] A unique election contest identifier groups candidates running against one another for the same board seat(s). We limit our sample to board elections for K-12 local education agencies (LEAs) and exclude elections of college boards, county boards of education, and the state board of education, as well as uncontested elections and elections without a recorded winner.

We predict candidate gender and race/ethnicity based on names as reported in election records.[5] We additionally link each candidate to their political affiliation as reported in the L2 Historical Voter Record, a proprietary database containing the history of voter registrations for the near population of the U.S. We obtain gender predictions for 89% of candidates, race predictions for 86%, and political affiliations for 75%. Appendix C provides more detail on data construction.

Panel A of Table 1 describes the school board candidates in our data. In total, we observe 22,426 candidates running in 5,782 elections. Overall, 22% of the candidates are Hispanic and 44% are female. Among those with a matched political affiliation, 49% are registered Democrats, with the remaining 51% of politically affiliated candidates registered as either Republicans or independents. Teachers are the most common listed occupation, comprising 17% of all candidates. Column 2 reports the same statistics, weighting districts by their enrollment in the year before the election. Enrollment weights raise the share of candidates who are Hispanic (27%), teachers (29%), and registered Democrats (53%).

2.2.2 Political Platforms

To describe candidates' policy viewpoints, we link their records to data from the now-defunct Voter's Edge website. Voter's Edge was a partnership between the League of Women Voters of California and Maplight, a non-profit organization that provided voters with an online guide to federal, state, and local races in California.[6] On the Voter's Edge website, each candidate had a page with

[4] Among all elections in our data, 59% are at-large, accounting for 73% of all candidates. 96% of candidates in our data ran for a full-term position.

[5] For race/ethnicity, we use the Python package *ethnicolr*, which uses Census data to predict the probability that a name belongs to a race/ethnicity group. We classify a candidate as Hispanic if the predicted probability exceeds 0.5. For gender, we use *gender_guesser*, which predicts gender probabilities using a database of 40,000 name-gender pairs.

[6] The League of Women Voters of California is a non-partisan organization that promotes voters' rights; its website can be accessed at https://lwvc.org/. Maplight is a non-profit organization that provides campaign finance, lobbying, and ethics disclosure solutions to state and local governments. We accessed the Voter's Edge site at https://www.maplight.org/ between October 5, 2023 and December 15, 2023.

their name, picture, the position they were running for, their occupation, any endorsements, and their political platform. Figure A1 shows the profile of a candidate running for a board member position in the San Leandro Unified School District, Area 2. We scrape the profiles for 10,069 school board candidates running in California races between 1998 and 2022. We link 38% of all candidates to a Voter's Edge profile (Table 1, Panel A). Platform coverage is better in larger districts: the share of candidates linked to platforms rises to 51% when weighted by district enrollment.[7]

2.2.3 District Outcomes

We quantify the effects of board elections on a range of outcomes measured at the district level. Our main analysis focuses on enrollment, funding, and achievement. Enrollment and finance data come from the Common Core of Data (CCD) for the academic years 1994-1995 through 2021-2022 (from now on, we reference academic years with the calendar year of the spring semester and use calendar years for elections). The CCD reports counts of students in grades K-12 attending schools in each district, broken down by race and ethnicity. Additionally, it lists total expenditures; expenditure on instruction, teacher wages, and capital projects; total revenues; and revenues earmarked for specific programs such as bilingual education, compensatory and basic skills, gifted and talented students, vocational programs, and safety programs. Starting in 2019, the CCD reports funding information on charter schools separately from that of their main districts in California. Because the size of the charter sector is potentially affected by board elections, our main analysis focuses on school finance outcomes for the 2017-18 academic year and earlier, though as we discuss below our results are not sensitive to this restriction. To better understand how board members adjust capital budgets, we supplement the CCD data with annual financial data from the California Department of Education (CDE), which allow us to track revenue sources and spending uses within facility-related funds. We express all finance variables in 2019 U.S. dollars.

Our achievement data come from the Stanford Education Data Archive (SEDA), which provides mean standardized scores in statewide Math and English assessments by district, grade, subgroup (Hispanic vs. non-Hispanic and eligible vs. ineligible for free- and reduced-price lunch, or FRPL), and year. These data, described in Fahle et al. (2024), are available for the years 2009-2024. We report results in student-level standard deviations (SDs), normalizing the state-by-year mean to zero.

[7] Table B1 compares candidate, election, and district characteristics for candidates with and without a profile. Candidates with a platform are less likely to be Hispanic, more likely to be teachers, and more likely to work in larger districts.

Panel B of Table 1 describes funding, enrollment, and educational outcomes for school districts in our sample in the year before each election. Average spending is about $9,700 per pupil, and average enrollment is about 11,900.[8] Average test scores are 0.16 SDs below the state-level mean; mean scores for Hispanic students and low-income students (defined as those eligible for FRPL) are -0.47 and -0.43, respectively.[9]

We use four other datasets to measure priority-relevant district outcomes. The first is information on student suspensions from the Civil Rights Data Collection (CRDC) of the U.S. Department of Education, a measure of behavior available biannually for the years 2010-2020. The second includes teacher personnel information, such as turnover rates and salary schedules from the California Department of Education, available for the years 2013-2018. The third is data on district superintendents from Stemper and The Broad Center (2025). The fourth is data on dropout count for grades 9-12, combining EDFacts files for 1998 through 2010 with California Department of Education dropout data for 2011 through 2017. See Online Appendix C for further details on the construction of district outcome data.

2.2.4 School Board Meeting Minutes

We measure board actions using a new dataset compiled by manually collecting PDF files of meeting minutes for the years 2016 through 2023 from each district's website. In total, we collected information on 74,056 school board meetings from 573 school districts. School board meetings typically follow Robert's Rules of Order.[10] Minutes record start and end times, attendance, motions proposed, individual votes, and vote outcomes. Figure A2 shows an example of a minutes file. Online Appendix E describes how we parse the minutes files to recover both meeting-level data on motions proposed and passed and board-member level voting records.

Panel C of Table 1 reports descriptive statistics from meeting minutes data in the year prior to the election. On average, districts hold 22 meetings, with 5.25 members present per meeting, and

[8] Because districts with more frequent elections, more trustee-area seats, or more contested races appear in our cleaned election sample in more years than other districts, these averages differ from averages across all district-years in the CCD and SEDA data, which are $13,666 for per-pupil spending and 5,593 for enrollment.

[9] As of 2024, California defines "economically disadvantaged" in Education Code §54026 as students counted under Title I of the federal *No Child Left Behind Act*, or, for some districts, students eligible for free meals or those living in households with income at or below the federal poverty guideline. Statutory and administrative criteria have changed over time. We use the term *low-income* throughout the paper.

[10] Robert's Rules of Order is a guide for conducting meetings and making group decisions. It is the most widely used manual among a wide range of organizations in the U.S. See Robert's Rules Association (n.d.) for details.

propose 153 motions per year, almost all of which are approved.

3 Measuring Board Members' Ideology and Actions

3.1 From Political Platforms to Priorities

We use candidates' political platforms to extract information on the policy items they care about the most. We refer to these as their priorities. Platforms vary greatly in detail. Table 2 provides some examples. Some platforms, such as the first one listed in Table 2, include a detailed description of the candidate's views. Others, such as the last one listed, include only a short list of items. The average candidate has a platform of 253 characters in length, with a standard deviation of 141.

To assign priorities from platforms, we feed the text of each platform into an LLM and pose a set of queries about twelve priorities, specified ex ante, which we identify as follows. We start with three policy types studied by existing research on school board decision-making: expanding equity in access to educational resources (*equity*), reducing school spending (which we label *fiscal conservatism*), and supporting teachers and teachers' unions (*teacher support*). We then add six additional topics that are the subject of debate in the economics literature and for which board decisions are important: investments in *school facilities*, declines in *enrollment* and competition for students, reduction in *dropout* rates, the health and safety of students in schools (*safety*), career and technical education (*CTE*), and improvement in academic achievement (*test scores*). Because one of the school board's core responsibilities is to select the superintendent, we also consider each candidate's positions on superintendent hiring and monitoring (*superintendent accountability*). We complete the list with two priorities that are the subject of public debate and which candidates frequently mention: *community engagement* and the presence of undesirable views or biases in academic curricula, policies, and practices (*agenda bias*).

For each priority, we ask the LLM to rate each platform on a scale indicating the relevance of each priority. To help with accuracy, we provided the LLM with annotated examples illustrating the scoring rules. We code candidates as having a given priority if their platform receives a score above a threshold value. The full list of queries, scales, and thresholds can be found in Online Appendix D.

3.2 Validating Priority Codes

We test the validity of our priority codes along two dimensions. The first is accuracy, defined as the extent to which our priority codes reliably capture human intuition about platform content. The second is completeness, defined as the degree to which our priority codes provide a reasonably full picture of candidates' stated priorities.

A first step in assessing accuracy is to ensure that the LLM codes seem intuitively reasonable. The right columns of Table 2 show the codes assigned to each platform. The first candidate mentions maintaining a balanced budget (which alludes to *fiscal conservatism*), a high-quality education (*test score*), and a highly qualified workforce (*teacher support*); he also stresses gains for all students (*equity*) and mentions building new schools (*facilities*) and the need to prepare for a declining enrollment (*enrollment*). The second candidate, with a much more concise platform, mentions student *safety*, service to parents and students (the *community*), opposition to "social experimentation" in the classroom (*agenda bias*), and a financial plan to cut wasteful spending (*fiscal conservatism*). The last candidate only lists three items, two of which ("parent involvement" and "school safety") connect directly to our priorities. To further illustrate how platform text translates into priorities, Appendix Figure A3 displays word clouds for the platforms of candidates who prioritize equity (panel (a)) and fiscal conservatism (panel (b)). Common terms for fiscal conservatism include "fiscal responsibility," "budget," and "accountability." Common terms for equity include "achievement gap," "equity," and "access."

To systematically assess the accuracy of the LLM classification, we compare LLM decisions to those of multiple human raters. We asked two research assistants to answer each query for a random sample of 200 platforms and compute the correlation between their responses and those of the LLM as a measure of inter-rater reliability (IRR). Across all questions and platforms, the correlation between the responses of the two human raters is 0.77. The correlations between the LLM responses and those of each human rater are 0.83 and 0.73, similar to the human-human IRR. Appendix Table B2 provides the details of this calculation. We conclude that machine-generated codes accurately capture the perceptions of human raters.

We assess completeness in two ways. First, we show that our approach allows us to categorize nearly all platforms (97.3% of our sample) by assigning them at least one priority. Furthermore,

the median number of assigned priorities per platform is 3 (with a mean of 2.9), exactly equal to the number of priorities candidates are supposed to list. Second, we extract priorities directly from the text of each platform using both a Latent Dirichlet Allocation and an LLM. In both cases, the extracted priorities closely match those assigned by our primary method (see Appendix Tables B3 and B4 for details).

Although our priority codes closely approximate what a human reader would conclude from reading the platform text, candidates' true policy priorities may still differ from those they explicitly list. Some candidates may strategically emphasize certain issues or omit others, despite holding strong views. For many others, we have no priority information. This might raise concerns about statistical misclassification. In this regard, it is important to emphasize that, by construction, we are not asking the LLM to infer latent priorities. Under our definition, treatment is intentionally tied to what candidates *chose to highlight publicly* and not to their unobserved underlying beliefs. Discrepancies between stated and latent priorities therefore do not reflect misclassification.

Another question is how to treat candidates who do not submit a platform. In our main regression discontinuity analysis, we include these candidates and treat them as not having a priority of a given type. Another way to interpret the absence of a platform is as censoring: these candidates might have stated a given priority had they submitted a platform. To capture this alternate interpretation, we report results that restrict the sample to the set of elections where both marginal candidates report platforms. We prefer the former approach because it is more comprehensive and, if anything, is likely to dilute true effects by weakening contrasts in stated priorities. As we show below, results from the two approaches are very similar.

3.3 From Priorities and Identities to Outcomes

We use the priorities data to evaluate whether board members deliver on what they claim to prioritize. Doing so requires mapping priorities to one or more district-level outcome variables related to that priority. In some cases, the mapping is clear. For example, candidates who want to reduce spending succeed if district spending falls relative to the counterfactual. Other mappings are reasonably clear but potentially multi-dimensional. For example, candidates who want to promote educational equity presumably succeed if achievement for disadvantaged groups improves, but there is more than one plausible definition of "disadvantaged group." Finally, two priorities—

community engagement and agenda bias—lack clear mappings to district outcome measures.

Our approach is to consider all relevant outcome measures, using multiple variables when there is ambiguity and excluding the two priorities for which there are no straightforward mappings from the outcome analysis. We thus assess the success of candidates with an equity priority using measures of achievement for both low-income students and Hispanic students. Similarly, we measure the success of candidates who support teachers using teacher exit rates (which we expect to decline) and the salaries of entering and experienced teachers (which we expect to rise). Appendix Table B5 reports the full mapping between priorities and outcome variables.

For identity groups, we evaluate causal impacts of changes against expectations grounded in the school board literature. To this end, we begin by cataloging identities and outcomes—and the hypothesized relationships between them—that have been the focus of prior work on school boards. For example, Kogan et al. (2021) point out that non-white voters typically express more support for school spending than white voters while also representing different (and non-white) constituencies, so electing more Hispanics may change school finance decisions and educational outcomes for Hispanic students. Similarly, Shi and Singleton (2023) make the point that teachers on school boards may try to shift policies in ways that help teachers.

We augment this list with new hypotheses that arise from the systematic differences in priorities across identity groups that we uncover with the platform data. The idea is to parallel the approach to hypothesis generation taken by previous authors. For example, we see that Hispanics disproportionately prioritize CTE, which suggests that an additional Hispanic elected to the board may cause spending on CTE programs to increase. We detail the relationships between identities and priorities in Section 4. Appendix Table B6 provides a list of identity-related hypotheses we test.

3.4 Measuring Members' Actions from Meeting Minutes

To extract data on the topics, votes, and passage or failure of motions discussed in board meetings, we follow a similar approach to the one used to extract priorities from platforms. Specifically, we feed each set of meeting minutes through an LLM and ask it to extract (i) a list of all motions and their outcomes, (ii) each board member's vote on each motion, and (iii) the topic of each motion. Two differences between the motions and the priorities are important to note. First, while we are able to capture the topic of a motion, we do not capture its stance. For example, meeting minutes

typically make clear when a motion relates to a budget proposal, but do not always make clear whether that proposal raises or lowers spending relative to some alternative. Second, many motions are purely procedural. For example, 33% of all motions are raised to approve the minutes of the previous meeting or to adjourn. We exclude these from our analysis. Appendix E explains the minute classification process in more detail.

Appendix Table B7 shows three examples of our minutes data and illustrates our topic assignment process. Overall, our process appears both accurate and complete: human-machine measures of IRR are high and approximately equal to human-human IRR (Appendix Table B8), and all non-procedural motions are assigned at least one of our twelve priorities.

4 Identity and Ideology Among Board Members

We begin our analysis by exploring the joint distribution of group identity and ideology on school boards. To describe how candidates' stated priorities vary across identity groups, Table 3 reports the share of candidates from each identity who have a platform (top rows) and who have different views (lower rows). Bolded entries are those where the difference in viewpoint shares across an identity group split (e.g., between Hispanic and non-Hispanic candidates, or between male and female candidates) is statistically significant at the 5% level. The rightmost columns report out-of-sample prediction accuracy for a random forest that attempts to predict the characteristic listed in the row using the identity variables. This captures the degree to which identity variables jointly predict platform attributes.

A preliminary observation is that the probability that a candidate has a platform differs by identity. For example, non-Hispanic candidates are more likely to have platforms than Hispanic candidates, and female candidates are more likely to have platforms than male candidates. However, many candidates in each identity group have platforms, and, taken together, identity variables accurately predict whether a candidate will have a platform only 65% of the time, a modest improvement over the 53% rate that can be obtained with a random prediction with knowledge of the variable mean.[11]

Turning to the platform content, we find that candidates' identities and ideologies are corre-

[11] Since 38% of candidates have a platform, the probability of correctly guessing whether any candidate has a platform or not based purely on chance is $0.38^2 + (1 - 0.38)^2 = 0.53$.

lated in expected ways. For example, candidates who are Hispanic, teachers, or Democrats are less likely to be fiscally conservative than their counterparts, while Hispanics, females, and Democrats are more likely to prioritize equity. Differences by gender and political affiliation are pronounced: women are 15 percentage points more likely than men to prioritize equity (55% of the average for men), while Democrats are 16 percentage points more likely to do so than Republicans (61% of the Republican average).

However, substantial ideological variation persists within identity groups. One way to see this is simply to note that, although some of the differences in Table 3 are statistically significant, many are relatively small. More systematically, the joint predictive power of all identity variables for viewpoints is weak. For example, the random forest model correctly predicts whether a candidate prioritizes fiscal conservatism 58% of the time, only a modest improvement over the 51% accuracy one could obtain through random guessing. For equity, random forest predictions are correct only 66% of the time, compared to 55% for random guessing. The observation that the correlation between identity and ideology is not that strong proves to be important for understanding the effect of election outcomes.

5 Identity, Ideology, and School District Outcomes

5.1 The Regression Discontinuity Design

We now ask how altering the composition of the board shapes outcomes for school districts. To estimate causal effects of board composition, we must address the potential endogeneity of elected members' characteristics. The selection of school board members is not random; it may reflect electorate preferences that also influence the outcome variable. For example, boards with more equity-focused members may represent districts with better outcomes for low-income students because non-board actors in the district prioritize equity in many ways, including but not limited to voting in school board elections.

To overcome this issue, we exploit quasi-experimental variation arising from close elections using a regression discontinuity (RD) framework. Continuing with the working example, suppose we are interested in the causal effect of electing an equity-focused candidate on some district outcome. We estimate the following model in the sample of election contests where one of the two marginal

candidates (i.e., the winner with the lowest vote share and the loser with the highest vote share) prioritizes equity and the other does not:

$$y_{i,k} = \beta_0 + \beta D_i + f(m_i) + \theta_{t(i)+k} + \varepsilon_{i,k}, \tag{1}$$

where $y_{i,k}$ is an outcome in the district where election i is held k years after the election takes place, with k ranging from 0 to 4. To increase precision, we generally specify the dependent variable $y_{i,k}$ as the difference from the year preceding the election.[12] The variable D_i equals one if the equity candidate wins election i, and zero otherwise. The running variable m_i is the vote share margin, defined as the difference between the vote share of the equity candidate and either (i) the closest losing candidate (if the equity candidate wins) or (ii) the closest winning candidate (if the equity candidate loses).[13] We assume f to be piecewise linear, allowing its slope to differ on either side of the cutoff, and use a quadratic function in robustness checks. We control for outcome year fixed effects in the vector $\theta_{t(i)+k}$, where $t(i)$ denotes the year in which election i is held. As in Shi and Singleton (2023), election i and the year relative to the election year k uniquely identify observations in the resulting dataset. We code candidates with missing platform information as not having priorities of a given type.[14]

We estimate equation (1) on the subsample of close elections, selected using the mean-squared error optimal bandwidth of Calonico et al. (2020). For statistical inference, we cluster standard errors at the election level and use Calonico et al. (2020)'s robust bias-corrected confidence intervals, which we note need not be centered precisely at the point estimate.

Under the assumption that any unobservable determinants of outcomes are continuous around the cutoff, estimates of β correspond to the average treatment effect of electing a candidate with a given attribute on a given district-level outcome, among districts with close elections. This approach can be used to estimate the effects of electing members from any identity or ideology group by appropriately redefining D_i, m_i, and the estimation sample. We estimate equation (1) for each

[12] Prior-year outcomes are balanced across the cutoff. We obtain similar results if we use them as controls rather than differencing them out (Appendix Tables B13 and B14, column 6).

[13] The sample pools at-large and trustee-based contests. Note that whether the marginal candidates differ in type, treatment indicator, and vote share margin are each contest-specific; we are never using candidates running for the same board but in different trustee areas or where one is from an at-large contest and the other is not to define these variables.

[14] Our findings are robust to restricting the sample to elections where we observe platforms for both marginal candidates. See Section 5.6

identity and ideology type described above.

The presence of many candidate types creates a practical challenge for exposition. Our approach is to choose three leading examples of candidate types for detailed discussion in the main text, while summarizing other findings more briefly. The three focal types are: equity-focused candidates, fiscally conservative candidates, and Hispanic candidates. We choose the two priority types because educational equity and school spending are central topics for education researchers and because data on policy mechanisms underlying the headline effects are relatively rich. We choose Hispanic candidates because that group is a common focus of empirical school board research.

In order to summarize impacts across multiple candidate attributes and outcomes, we also estimate "stacked" regression discontinuity specifications (Pop-Eleches and Urquiola, 2013). These models stack the individual estimation samples defined by attribute-outcome pairs into a single estimation sample. We then estimate equations of the form

$$y_{i,k,s} = \beta_0 + \beta D_{i,s} + f(m_{i,s}) + \theta_{t(i)+k} + \tau_s + \varepsilon_{i,k,s} \tag{2}$$

where s indexes stacks—unique candidate attribute and outcome variable combinations. We add fixed effects for each stack (τ_s). To combine multiple outcome types in stacked specifications, we standardize the dependent variables to have a standard deviation of one and a sign corresponding to the hypothesized effect direction, as reported in Tables B5 and B6.

Our design relies on close elections between candidates with differing policy priorities or identities. Fortunately, many school board elections fit this description. Columns 3-6 of Table 1 describe our sample of close elections, defined here as those where the gap between the marginal winner's and the marginal loser's vote share is five points or fewer. As reported in column 3, 2,519 elections (44%) are close, and attributes of the candidates, districts, and board meetings in these elections are similar to those for the full sample. Columns 4-6 describe close elections where the marginal candidates differ in terms of the listed priority or identity. Up to differences due to variation in choice of optimal bandwidth, these are the samples we use in the regression discontinuity analysis. There are 905 marginal elections where one candidate is equity-focused and the other is not and 68% of candidates in these elections have platforms. This higher rate occurs mechanically: the equity-focused candidate in each marginal pair by definition has a platform. Candidates in these

elections are somewhat less likely to be Hispanic and more likely to be teachers or women, and the elections take place in districts that spend more and are slightly higher achieving than in the full sample. Fiscally conservative-marginal elections are also somewhat less likely to feature Hispanic candidates, and take place in higher-spending and higher-scoring districts. Hispanic-marginal elections are mechanically more likely to include Hispanic candidates but otherwise closely resemble the full sample of candidates.

The design passes standard tests of RD validity. Table 4 reports seven checks: (i) McCrary tests for bunching on either side of the cutoff; balance around the cutoff in (ii) log enrollment and (iii) low-income share in the year before the election; (iv) selection into the outcome sample; balance in (v) outcome levels in the year before the election and (vi) outcome changes in earlier pre-election years; and (vii) selection into the meeting-minutes sample.[15] Column 1 presents results that pool across all candidate types. Each pooled test fails to reject the null of no discontinuity or imbalance. We also conduct separate tests for our leading case-studies (columns 2-4) and for stacked identity and ideology specifications (columns 5-7). In total, the table contains the results for 49 tests of the null of no imbalance, of which three reject the null at the 5% level. These are also the only three tests that reject the null at the 10% level. Two tests that fail are for the density of the running variable in specifications focused on candidate identity; possible concerns about validity of the design as applied to identity types are mitigated by the observation that lagged outcomes are balanced. The third is a marginally significant difference in pre-treatment outcome changes for the pooled ideology specification. Overall, findings from these balance tests are consistent with the idea that election outcomes are hard to manipulate (Lee, 2008) and that the RD design is working as expected. We provide further evidence in support of design validity below by showing that treated and untreated districts follow similar trends in terms of board composition and district outcomes until the election takes place, at which point gaps emerge.

5.2 Changes in Identity Group Representation and Boards' Ideological Composition

We first explore how electing candidates from different ideology and identity groups affects the distribution of priorities on the board. Figure 1 displays regression discontinuity results for our three leading examples. The left plot in each set of two panels is a standard regression discontinuity

[15] Online Appendix Figure A4 provides graphical representations of the McCrary test results.

graph where the horizontal axis is the (relative) vote share of the reference-group candidate and the vertical axis is the mean share of board members for whom a view of the listed type is observed, relative to the share in the year before election. The plot uses data for the four years following the election ($k = 0, 1, 2, 3$ in Equation 1)—the typical length of a school board term. The right plot in each pair displays RD effects separately from three years before to four years after the election ($k = -3$ to $k = 4$). Light gray diamonds are below-threshold mean values. Darker circles with confidence intervals are above-threshold means, computed here as the below-threshold value plus the estimated RD effect.

Our first finding is that marginally electing members with a specific priority substantially shifts the share of board members who hold that view. Panels (a) and (b) of Figure 1 show results for the elections on the equity margin. As expected, there is a discontinuous jump in the share of board members prioritizing equity when an equity candidate beats a non-equity candidate. The size of the jump is 0.21, an 84% increase on the baseline share of 0.25. Effects of this approximate magnitude are what we expect to see given the distribution of board sizes in our data. At the average board size of 5.87, exchanging a member without a given viewpoint for a member with that viewpoint raises the share of members holding the view by $0.17 = 1/5.87$, economically similar to and not statistically different from the effect we observe. Panel (b) shows that gaps in the share of equity-focused board members open up precisely in the year the equity-focused candidate wins (i.e., there is no correlation between marginal wins and board composition in prior years) and stay open for four years, the length of a typical term. The gap closes once the four-year term has ended. Panels (c) and (d) describe the effects of electing a fiscally conservative candidate. The picture is similar to what we found for equity, with an increase at the cutoff equal to 0.17 (85% of the baseline share of 0.20) that is sustained for four years.

Our second finding is that electing a candidate with a given identity does not shift a board's ideological composition much, even for priorities that are correlated with the identity in the cross-section. The bottom panels of Figure 1 show how electing a Hispanic candidate (relative to a non-Hispanic candidate) affects the share of equity-focused board members. Table 3 shows that Hispanic members are 3 percentage points more likely than non-Hispanic members to list equity as a priority. However, as reported in the bottom panels of Figure 1, electing a Hispanic candidate does not affect the share of equity-focused board members. Appendix Figure A5 shows that the effects of

electing a Hispanic candidate on the share of fiscally conservative board members—the priority on which Hispanic and non-Hispanic candidates differ the most in our data—are similarly small and statistically insignificant.

Panel (a) of Figure 2 summarizes these results across all priority and identity categories using the stacked specification in equation (2). Each bar reports the average effect of electing a candidate from a given category on the share of board members with the associated priority. The contrast between priority and identity groups is stark. Electing a candidate with a given priority raises the share of board members with that priority by 0.17, while electing a candidate from an identity group raises the board share with the associated priority by only 0.01. The impact of identity on the board's ideological composition is minimal both for identity-priority links emphasized in prior school board studies and for links generated from cross-sectional correlations between identities and priorities in our data.

A natural question is whether the small effects of identity on the ideological composition of school boards are due to our focus on close elections. Candidates with different identities may be more likely to hold similar views when they are competing against one another in a close election. We explore this possibility in panel (b) of Figure 2. The vertical axis plots the RD estimate of the effect of electing a candidate of a given type, defined by identity or priority, on the share of board members with an associated priority, as in Figure 1. The horizontal axis reports the RD estimates we would obtain if, instead of using the actual ideology of the focal candidate to compute the board priority share, we used the population average ideology of candidates of the same type. For example, since the average share of candidates who prioritize equity is 0.37 among Hispanics and 0.34 among non-Hispanics and the average board size is 5.87, the x-axis value of Hispanic-Equity is $(0.37-0.34)/5.87 = 0.005$. By contrast, because all equity candidates, by definition, prioritize equity while non-equity candidates do not, the predicted effect of electing an equity candidate is $1/5.87 = 0.17$. Triangles show effects of electing candidates with a stated priority; squares and circles show effects of electing candidates from identity groups, where the identity-priority relationship is either drawn from prior school board studies (squares) or generated from the cross-sectional correlations in our data (circles). We highlight previously shown RD estimates for equity, fiscally conservative, and Hispanic candidates, as well as those for two identity-ideology pairs examined in prior work (Teachers-Teacher Support and Hispanic-Equity).

The results in Figure 2 show that the small effects of identity on board ideology do not arise because cross-identity ideological differences are smaller in close elections. All of the identity estimates cluster near the origin and close to the 45-degree line, indicating that the small ideological shifts induced by identity in close elections are approximately what one would expect given the weak identity-priority correlations observed in the cross-section. In contrast, electing candidates with stated priorities produces large changes in board ideology. The triangles lie above all other points along the vertical axis, with each estimate falling mechanically along the 45-degree line.

5.3 District Outcomes

Next, we examine the effect of electing board members from different identity and ideology groups on district outcomes. We again begin with our leading examples. Figure 3 displays standard RD plots for each candidate type-by-outcome pairing on the left within each row, with year-by-year effects on the right. The pooled RD plot includes data from the full five-year window after the election ($k = 0$ to $k = 4$).

Panel (a) of Figure 3 shows that electing an equity-focused candidate raises the test scores of low-income students by 0.088 SDs. The discontinuity is visible in the RD plot and highly statistically significant ($p<0.001$).[16]

Panel (b) of Figure 3 shows how score gains evolve over time. As expected, scores are balanced in the pre-election period. Gains for low-income students then appear in the year following the election and persist for at least four years. Board members are typically elected in November and seated in December, near the beginning of the academic year at event time $k = 0$. Tests for that academic year are then administered the following spring. This means that test score gains first observed at event time $k = 1$ occur about 1.5 years after new members are seated on the board and begin shaping policy.

Panel (c) of Figure 3 shows that fiscally conservative candidates succeed in reducing district spending: electing a candidate with this priority reduces per-pupil district spending by $1,212, equal to 13% of the below-threshold average spending level. As shown in panel (d), this occurs

[16] An interesting feature of the RD plot is that the slope of the relationship between low-income test scores and the vote margin—the running variable— is negative. This suggests that equity-focused candidates tend to get higher vote shares in districts where scores for low-income students are declining relative to the year before the election. One possible interpretation is that voters react to increased challenges for low-income students by voting for equity candidates.

via a reduction in the *growth* of district spending, rather than a reduction in spending levels. These effects first appear at event time 0, i.e., the first year the member is seated on the board.

The effects of electing equity-focused or fiscally conservative board members are large relative to other education interventions. The effect of electing an equity-focused board member on low-income students' test scores is similar to the effect of assigning every such student to a teacher who is about 0.8 SD higher in the distribution of teacher value-added (Chetty et al., 2014) or to a school that is about 0.4 SD higher in the distribution of school value-added (Angrist et al., 2017) for one year. The reduction in spending from electing a fiscally conservative board member is similar in size to the increase in spending experienced by low-income districts following state-level school finance reforms (Lafortune et al., 2018).

The effects of electing equity-focused or fiscally conservative board members are also large relative to the effects of electing a Hispanic board member on outcomes tested in previous work. Panels (e) and (f) of Figure 3 examine how electing a Hispanic board member affects test scores for Hispanic students. We find positive and marginally significant effects: electing a Hispanic board member raises test scores for Hispanic students by 0.021 SDs (95% CI=[0.000, 0.046]). This finding is consistent with Kogan et al. (2021) and Fischer (2023), but less than one quarter of the size of the estimated effects of electing an equity-focused candidate on scores for low-income students. Appendix Figure A6 presents parallel estimates of the effect of electing a Hispanic candidate on school spending, another link hypothesized in Kogan et al. (2021). Here, results are mixed. We find an economically small effect on total per pupil spending ($124 per student; 95% CI=[-$42, $235]) and an economically modest increase in the amount of bonds passed per student ($475 per student; 95% CI = [$154, $796]). These effects are one tenth and one quarter as large, respectively, as the effects of electing a fiscally conservative candidate on the same set of outcomes (see Table 5 below).

We take two lessons from these case studies. First, for at least some views, electing board members with a given view can have economically large effects on associated outcomes. We explore the channels through which board members achieve these outcomes in Section 6. Second, though shifts in the identities of board members impact district outcomes, these effects are far smaller than those of electing an issue-focused member, and therefore provide an unreliable guide to school boards' governance capabilities.

5.4 Aggregating Multiple Priorities and Identities

We now turn to the full set of priorities and identities. For ease of interpretation and comparison, we first normalize all outcome variables within each year to have a standard deviation equal to one and a sign consistent with the hypothesized direction of the effect (see Appendix Tables B5 and B6 for the full list of hypotheses). We then consider three stacked specifications: one where each focal candidate attribute is an ideology and we consider the associated outcomes, one where each candidate attribute-outcome pair comes from an identity-related hypothesis tested in prior literature, and one where each attribute-outcome pair comes from a novel identity-related hypothesis.

We find that the effects on outcomes of changes in a board's ideological composition are larger than the effects of changes in identity group composition. As reported in Figure 4, the average effect of electing a candidate with a given priority is to shift the associated district outcome by 0.21 SDs in the expected direction (95% CI=[0.185, 0.239], 14 ideology-outcome pairs). In contrast, the average standardized effect from identity-outcome hypotheses tested in prior literature is 0.04 SDs (95% CI=[0.032, 0.044], 6 identity-outcome pairs) and the average standardized effect from novel identity-outcome hypotheses is smaller at 0.007 SDs (95% CI=[0.003, 0.011], 23 identity-outcome pairs). These findings are consistent with the hypothesis that ideology is a key factor driving outcomes and with our previous finding that the effects of shifts in identity on ideology are small.

Table 5 reports the estimates for each priority-outcome pair that we use to construct the aggregate estimate in both raw and standardized units. The main observation to take from this table is that the large effect of board ideology on district outcomes arises from effects across many domains. For the standardized outcomes in column 5, 12 of the 14 tested hypotheses have signs in the expected direction, and we can reject the null of no effect at the 5% level for 10 of those 12 cases.[17] Looking at specific outcomes, a number of interesting results emerge. For example: candidates who prioritize facilities raise capital spending; candidates who prioritize teacher support raise entry salaries and reduce teacher exit; and candidates who prioritize superintendent accountability increase the probability of superintendent turnover relative to the base period. A notable null result is that candidates who prioritize academic achievement do not raise, and perhaps even lower, test

[17] For the non-standardized specifications in column 4, we reject the null in nine cases. Slight differences between standardized and non-standardized results may arise because we standardize variables within each year, so the standardized value is not simply a rescaling.

scores. We see similar null or wrong-signed results for career and technical education (CTE).

Appendix Tables B9 and B10 report the individual estimates underlying the identity-based averages. While effects are null for many identity-outcome pairs, we largely confirm findings from prior school board studies. In addition to replicating the Kogan et al. (2021) and Fischer (2023) findings discussed above, we show that electing a teacher raises spending and entry salaries and reduces teacher turnover, as in Shi and Singleton (2023).

5.5 Identity vs. Ideology in Determining District Outcomes

The evidence presented thus far shows that changes in boards' ideological composition have large impacts on district outcomes, while changes in the representation of identity groups have small impacts. However, these findings rely on analyses that consider one dimension of identity or ideology at a time and do not hold other attributes constant. A limitation of this approach is that, because identity and ideology are correlated, the univariate estimates of identity impacts include both direct effects of representation and effects that operate through changes in the board's ideological makeup. Similarly, univariate estimates of ideology impacts include both direct effects and effects that operate through changes in identity (though identity effects appear to be small).

In this section, we test whether and how much identity matters when ideology is held constant, and vice versa. To do this, we extend the analysis from section 5.4. Specifically, we estimate stacked specifications (equation (2)) that restrict the sample of elections used to test identity-related hypotheses to those where the outcome-relevant priority has the same value for both marginal candidates. For example, if the outcome in question is low-income test scores, and the identity type is Hispanic, this analysis would only consider elections where a) one marginal candidate is Hispanic and the other is not and also b) the two candidates share the same value of the equity priority variable. This allows us to focus on elections where the relevant identity is changing but the relevant ideology is not.

Similarly, for ideology-related hypotheses, we restrict the sample to include only elections where the marginal candidates have similar identities, in an outcome-relevant sense. To operationalize this, we use the random forest predictions described in Section 4 to generate predicted ideologies using the full set of identities. We then restrict the sample to elections where the predicted values of the relevant ideology for the marginal candidates differ by less than 0.01. For example, if the

ideology type we are considering is equity, our analysis would only consider elections where a) one marginal candidate lists an equity priority and the other does not, and b) the two candidates are similar in terms of the identity variables that predict an equity priority, such as Hispanicity, gender, and political affiliation (Table 3). Panel (b) of Figure 4 reports results from these exercises.

We find that, when we hold ideology fixed, the effect of identity on outcomes reverses sign and becomes negative for previously tested hypotheses and remains small for the set of novel identity-outcome hypotheses. In contrast, holding identity fixed only minimally alters our estimates of the effects of ideology on outcomes. In short, these results suggest that differences in ideology explain the identity effects we find, while ideology effects operate independently of identity.

Appendix G considers an alternate approach. We estimate models that simultaneously hold fixed multiple identity or ideology variables, rather than just those closely corresponding to the focal type. Because holding fixed multiple variables at once dramatically raises the dimensionality of the control set, we impose the assumption that effects are separable across attributes, as in Barrios-Fernández et al. (forthcoming). Though this exercise is quite different from our main approach—with the benefit of a more extensive control set at the cost of additional parametric restrictions—the conclusions are the same. We find strong evidence that ideology matters after conditioning on identity, but little evidence that identity matters when ideology is held fixed.

5.6 Robustness

Our findings are robust to alternate approaches to estimation and measurement.

First, our case study and aggregate results are qualitatively unchanged and quantitatively very similar under different values of key tuning parameters and specification choices within the Calonico et al. (2020) framework, including the use of alternate control sets, higher-order polynomial terms, and uniform rather than triangular kernels, as well as to alternate approaches to computing standard errors (Appendix Tables B13 and B14, columns 2-5). We use an optimally chosen bandwidth under each of these modifications. Bandwidths used in these robustness tests vary substantially: for example, when we study the election of an equity candidate, we consider bandwidths ranging from 3.7 to 8.7.

Second, our findings are robust to using the levels of outcome variables rather than changes relative to the year before the election (Appendix Tables B13 and B14, column 6) and to including

harmonized spending data for school years after 2019, when the CCD started to report funding information on charter schools separately from that of public school districts (Appendix Table B13, Panel B, column 7).

Third, our estimated effects of ideology remain robust when we (a) restrict the sample to elections where both marginal candidates have a platform and (b) impute a candidate's missing priorities using platform information from the candidate's other campaigns within a four-year window. This latter approach increases the share of candidates with valid platforms from 38% to 43% in the unweighted sample. Appendix Figure A7 reports results from both these exercises.

As an additional robustness check, we considered an alternative design based on randomized ballot order, as in Shi and Singleton (2023) and Fischer (2023). We find that ballot order has a weak first-stage effect on election outcomes in specifications focused on candidate ideology, and therefore cannot be used to test the hypotheses we explore in this paper.[18]

6 Mechanisms

We have shown that board members succeed in shifting districts in the direction of their policy priorities. We now explore how they do so by examining intermediate inputs and actions taken by members in the boardroom. This exercise has two goals. First, it is a check on the plausibility of our main findings. We expect intermediate inputs to move in ways consistent with both the headline outcomes and board authority, which, as explained in Section 2.1.1, covers budget allocation, capital spending, and superintendent accountability but not the level of the operations budget. Second, it illustrates how board members achieve their goals. This is particularly important for equity-focused board members, because they succeed in raising scores for low-income students—a notoriously challenging task. We continue to focus on our leading case studies.

6.1 Intermediate Policies

We begin by studying how equity candidates affect intermediate outcomes that may be related to low-income students' test scores. A plausible theory is that equity-focused board members move resources towards schools and programs where low-income children are disproportionately repre-

[18] This contrasts with a stronger first stage for candidate identities, reported in previous papers and replicated here.

sented. We would ideally test this theory using data on school- or even student-specific resource allocation. In practice, student-level resource data do not exist and school-level data are limited. We therefore consider several reasonable proxies. The first is funding data from California's School Facility Program, which provides matching grants to individual schools for capital improvements. While a relatively small share of total spending, it is one of few funding sources for which expenditure data are systematically available at the school level (Fischer, 2023). We estimate versions of equation (1) using data at the school-year level, splitting schools by whether their SES-group share exceeds the district-year share, as in Fischer (2023). We find that when an equity candidate is elected, low-income schools receive $767 more funding per pupil through the program (Table 6, Panel A, column 1), compared with $339 for higher-income schools (column 2). Similarly, schools with high shares of Hispanic students receive $908 more per pupil, compared with $295 for schools with lower shares (columns 3 and 4). Cross-group differences in effects are at the margin of statistical significance (p=0.002 for the income split, and p<0.001 for the Hispanic split).

We then turn to school-level staffing decisions. Here we do not observe expenditures, but we do observe counts of staff by type. We find that electing an equity candidate raises the number of classified staff—non-teacher staff, including paraprofessionals—in low-income schools, with no evidence of shifts in staffing for higher-income schools (Table 6, Panel A, columns 5 and 6). We interpret this evidence cautiously because we cannot reject the null that effects for the two groups are equal at conventional levels (p=0.349). Parallel analyses reported in Online Appendix Table B11 show similar results for the high Hispanic/low Hispanic split and for certified staff (i.e., teachers).

Additional analyses explore resource allocation across program types. We find that electing an equity candidate leads to modest district-level increases in revenues earmarked for programs that likely target low-income groups (free lunch programs, basic education), declines in revenues likely earmarked for higher-SES groups (gifted and talented programming) and no change in revenues for program types with no obvious SES valence (career and technical education). These results are reported in Online Appendix Table B11.

Taken together, these findings suggest that resource reallocation is an important mechanism through which equity-focused board members help low-income students.

The rightmost two columns of Panel A of Table 6 test alternate theories of how equity-focused board members might shift district outcomes. One possibility is that members push the board to

select a new superintendent. Estimates in column 7 of Table 6 suggest this does not systematically occur: electing an equity-focused board member has a small and statistically insignificant effect on the probability that a district reports having a different superintendent than the one we observe prior to the election. Another way for equity-focused candidates to achieve the test score gains we observe for low-income students could be by shifting the socio-demographic composition of schools towards higher-performing students. We test for this by predicting each district's average test scores using the observable characteristics of its students and using this prediction as the outcome variable.[19] We do not find any evidence for this mechanism (column 8).

We next consider fiscally conservative candidates. Because school boards in California have substantial authority over capital spending but limited authority over the total operating budget, we expect reductions in capital spending to drive the overall changes in spending we observe. This is precisely what we find. As reported in Panel B of Table 6, capital expenditures fall by $1,227 per pupil after the election of a fiscally conservative member. This category accounts for all of the decline in total spending ($1,212, as reported in Figure 3, panel (c)). Within capital spending, the construction subcategory accounts for most of the decline. By contrast, changes in teacher salaries and staff salaries—two major spending categories in education budgets—are close to zero. Further results reported in Online Appendix Table B12 confirm that changes in formula revenue from the state, the crucial source of districts' operating budgets, are also economically small relative to the mean.

A more detailed analysis of capital spending and revenue generation shows that the *timing* of spending changes is also consistent with board authority. Recall from Figure 3(c) that expenditure effects appear in the year the candidate takes office. This contrasts with the lagged effect of equity candidates, and raises a question: how can fiscal conservatives change spending so quickly, especially when capital spending often uses bond financing and therefore would appear to require substantial lead time? The answer is that it is common for districts to have a reserve of potential bond funding that has been approved by voters but not yet been issued. Medrano (2025) reports that 38.4% of school district bond dollars approved by California voters between 2001 and 2024 had not been issued by 2024. Districts with approved but unissued bonds can choose whether and

[19] We obtain our predictions by running a linear regression of test scores on district shares of black, Hispanic, Asian, free/reduced-price lunch, below poverty-line, special-education, English language learner students, and the log of total enrollment.

when to issue them. Once issued, these bonds appear as revenue in the district's facilities fund. As the CFO of one California school district put it, "you have a credit card, but you don't have to use it" (Eger, 2026). Columns 5 and 6 of Table 6 report RD estimates that take bond revenue and the dollar value of voter-approved bonds as the dependent variables. We observe large declines in both outcomes. Appendix Figure A8 confirms that declines in bond revenue are immediate and account for most of the decline in districts' overall revenue, while declines in bond passage are more noisily estimated and appear to operate at a lag.

Fiscally conservative board members also exert substantial pressure on district leadership. As reported in column 7 of Table 6, electing a fiscal conservative causes a 17 percentage point increase in the likelihood of a superintendent change in the five years following an election (43% of the below-threshold mean).

The large effects of electing a candidate with a given ideology again contrast with the smaller effects of electing a candidate from a given identity group. Panel C of Table 6 asks whether electing a Hispanic candidate leads to a similar reallocation of School Facility Program funds. We find little evidence that it does: point estimates suggest only modest differences in SFP funding across schools with high and low low-income or Hispanic shares, and we cannot reject a null of no effect in any specification. We also fail to rule out null effects on classified staff at schools where the Hispanic share is high. We do find a modest increase in superintendent turnover, but it is smaller than the corresponding effect for fiscally conservative candidates. Overall, these intermediate-outcome results are consistent with the relatively small outcome effects we observe for Hispanic candidates.

6.2 Actions in Meetings

We have thus far shown that board members shape district outcomes and described the types of intermediate policy levers they pull to achieve these results. A remaining question is what exactly board members are doing in meetings to shift policy. This is important to explore because it is potentially surprising that electing a single board member can have such a large effect on policy. In this section, we use voting records from board meetings to examine the actions members take to shift district outcomes and to distinguish between different theories of change.

We consider two main theories about how individual members may succeed in shaping board governance, drawn from the economics and political science literature and summarized by Kogan

et al. (2021) in the school board context. The first is that boards govern by consensus: even if their opinion is not in the majority, board members can shift policies by influencing the behavior of other members or by shifting the board's discussions. The second is that policies are made by the majority, and board members influence policy by casting pivotal votes even without influencing other members of the board.

These theories have implications for voting outcomes. If the majority-rule theory holds, we expect to see members who narrowly win cast pivotal votes on closely contested motions. If the consensus theory holds, we expect to see changes in meeting agendas and the quantity and content of board motions, but not necessarily contentious votes. To test the two theories, we estimate equation (1) using records of the quantity and type of motion passed, as well as candidates' individual voting records, as outcomes. As in the analysis of ideological composition in Section 5.2, we include observations from the election year through three years after the election ($k = 0, 1, 2, 3$ in Equation 1), the typical length of a board term. For concision, we focus on our leading examples: the effect of equity candidates and fiscally conservative candidates.

Our first finding is that equity-focused candidates cast pivotal votes on contentious motions, but do not observably shift meeting agendas. As reported in Panel A of Table 7, we see no evidence that electing an equity-focused candidate leads a school board to propose or pass more motions (counted here at the annual level), either on equity (the focal topic of these candidates, column 1) or overall (column 2). We do see that boards become much more likely to pass non-unanimous motions, which are very rare at baseline. For example, when an equity-focused member is elected, the number of non-unanimous equity-related motions a board passes per year rises by 0.67, or 31% from a base of 2.16 (Panel C, column 1).

Results in Panel D confirm that equity-focused candidates often cast pivotal votes themselves. We measure pivotal votes by counting how many times the winner of the reference election votes with the winning side on a motion decided by one vote in each outcome year, and taking these counts as outcome measures in equation (1). We find that equity-focused board members cast 0.18 more pivotal votes per year on equity measures (159% of a baseline mean of 0.11) and 1.28 (100%) more pivotal votes overall than the candidates they defeat. We find similar results when we define an analogous outcome based on non-unanimous votes (including those decided by more than one vote) in Panel E. In Figure 5, panels (a) and (b) contrast the clear discontinuity in marginal votes

cast on equity motions with the absence of a discontinuity in total equity motions passed.

In contrast, fiscally conservative candidates appear to shape outcomes by shifting board consensus rather than by casting pivotal votes. As reported in Panel A of Table 7, electing a fiscally conservative candidate reduces the number of budget motions proposed by 15.28 per year (34%) and the number of motions proposed on any topic by 51.2 (33%). Effects on passed motions are almost identical. However, in contrast to what we saw for equity-focused candidates, the number of non-unanimous motions does not change at all: there are fewer votes taken, but votes do not become more contentious. Results in Panel D show that, when elected, fiscal conservatives are no more likely to cast pivotal votes on fiscal issues or overall than their electoral competition. In Figure 5, panels (c) and (d) contrast the large reduction in total fiscal motions passed with the absence of an effect on pivotal votes cast.

The last two columns of Table 7 explore how electing a Hispanic candidate—the third of our three leading examples—shapes voting outcomes. We see some evidence that electing a Hispanic candidate leads to boards passing more equity measures. However, when Hispanic candidates win elections they are no more likely to cast pivotal votes on equity issues than the candidate they defeat. This is consistent with the observation that Hispanic board members do not appear to shift district outcomes in a more equitable direction, while equity-focused members both affect equity-related outcomes and cast pivotal votes on equity motions. Though electing a Hispanic candidate does not affect the total number of budget motions proposed or passed, it does raise the likelihood that a board will pass a non-unanimous budget motion. Hispanic board members also appear to be more likely to cast pivotal votes on fiscal issues, although this effect is noisy. Our findings are consistent with the finding from Section 5.3 that electing Hispanic candidates has a small positive effect on per-student funding.

Stepping back, our analysis of meeting minutes supports our results on district outcomes by filling in the last link of the causal chain: from the election to board actions, from board actions to intermediate outcomes such as budget allocation or superintendent choice, and from intermediate outcomes to endline outcomes such as district spending and student outcomes. Further, the analysis supports both the consensus and majority-vote models of board governance, but suggests that their relative importance may be domain-specific. At the same time, while our measure of pivotal voting is exhaustive—whenever a candidate casts a pivotal vote, we see it—and our measures of

the quantity and type of board motions capture important elements of the board agenda, candidates may shape meeting agendas in ways beyond those we examine here, such as by changing the content of a motion within a topic category. This possibility is suggested by meaningful effects of equity-focused members on the number of "no" votes from both the member herself and other board members (Table B15), despite the absence of an effect on the number of equity motions proposed. Our claim is therefore not that the effects we document here are the *only* actions board members take that shift district policies, but rather that these results provide proof of concept: board member actions are consistent with both members' stated policy priorities and district outcomes.

7 Discussion

7.1 External Validity

There are several reasons to believe that our estimates provide a lower bound on board members' ability to affect student and district outcomes.

First, it is possible that narrowly elected candidates may be less willing or able to act on their policy priorities than candidates elected by a wider margin. If so, the effects we estimate, which come from narrow victories, would form a lower bound on the average effect of electing a board member with a given priority. That said, evidence from national politics suggests that narrowly elected politicians do not moderate their policy actions (Lee et al., 2004).

Second, school boards in California have less authority over operations budgets than boards in many other states. The evidence we have on mechanisms indicates that board members pull the policy levers available to them. One might therefore expect school board capacity to be greater in states where boards have more levers to pull.

Third, and finally, we assess board capacity by measuring the effects of electing board members with different stated priorities. This is distinct from approaches that quantify the importance of policy actors by measuring how latent actor-specific effects contribute to the dispersion of a given outcome. For example, a large literature estimates the distribution of teacher effects on student achievement. The movers-based designs that drive this literature are not applicable to the board setting, because members rarely (if ever) change districts. From an external validity perspective, the key point is that dispersion in outcomes generated by observable, stated priorities mechanically

understates the total dispersion generated by latent member fixed effects.

7.2 Policy Tradeoffs

We have focused so far on whether candidates implement their stated priorities, the central issue when evaluating board capacity. Voters might also be interested in another question: what are the tradeoffs? For example, equity-focused candidates may increase achievement for low-income students at the expense of other students or of taxpayers, while fiscally conservative candidates may cut spending at the expense of student outcomes. Continuing to focus on our three leading examples, Online Appendix F discusses the effects of electing equity-focused, fiscally conservative, and Hispanic candidates on leading non-focal outcomes.

The evidence on tradeoffs is mixed. For equity-focused candidates, we see little evidence of tradeoffs across student types. Electing an equity-focused candidate raises scores for high-income students by 0.022 SDs, about 25% of the value we observe for low-income students. Equity candidates also do not appear to raise per-pupil spending; if anything, they decrease it.

Turning to fiscally conservative candidates, we find that their election slightly reduces student scores, as reported in Stemper (2022a), although these estimates are indistinguishable from zero. The point estimates of score declines are smaller on a per dollar basis than those found in the literature (see Jackson and Mackevicius, 2024, for a review). This is consistent with our previous finding (Table 6) that the budget cuts come primarily from reductions in construction spending, often ineffective at improving achievement (Biasi et al., 2025).[20]

8 Conclusion

This study provides causal evidence that school board members are important policy actors whose choices have large effects on a wide variety of school district outcomes. Board members implement policy changes both by shifting the board agenda and by casting pivotal votes on controversial issues; these actions shift intermediate outcomes like district spending and executive leadership in ways that are consistent with members' goals but circumscribed by the limits of board authority. We further show that, to understand the importance of school boards for district outcomes, measuring

[20] Biasi et al. (2025) show that school construction projects that involve schools' athletic facilities—among the most expensive ones—have no significant effects on test scores.

the ideologies of board members is crucial. Identity and background variables often used as proxies for policy aims prove to be weakly correlated with those aims.

Our findings push back on claims that local school boards are not important determinants of student achievement. The idea that school boards lack the expertise or authority to improve student outcomes has a long history and substantial support. As Danzberger et al. (1987) put it nearly 40 years ago, "local school boards in most states have been ignored or cast in passive roles as inconsequential reactors rather than as partners in shaping educational improvement." More recently, White et al. (2023) argue that "[m]ost school-board members are not equipped with the educational and experiential background to understand what it takes to improve academic achievement." Our finding that board members who prioritize educational equity succeed in raising scores for disadvantaged students suggests that board composition can indeed have important achievement effects, and it illuminates the high stakes of interventions that shape members' policy priorities, whether through candidate selection, election outcomes, or political pressure.

Looking beyond school boards, our results suggest that researchers seeking to understand the effects of policies that shift the composition of governing bodies such as corporate boards, scientific panels, or city councils would benefit from gathering data on members' policy preferences. Predicted changes in preferences based on aggregate demographic shifts—the basis for much hypothesis generation in the literature on board governance—may be poor proxies for the actual ideological changes that take place.

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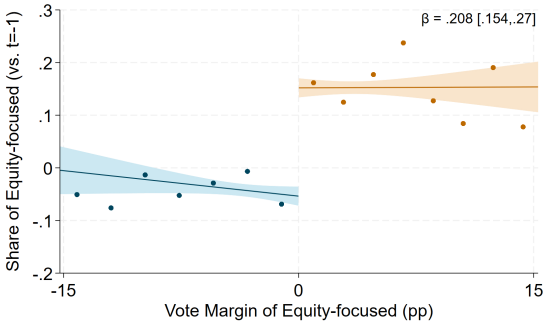
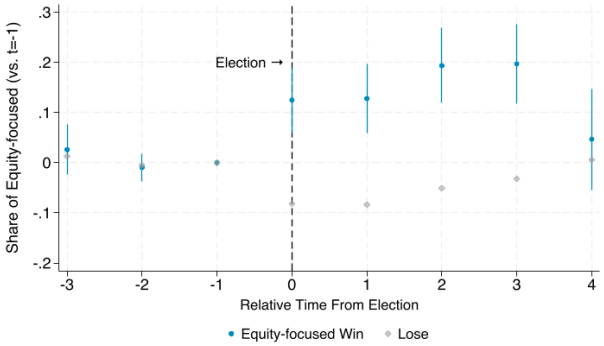
Figures

Figure 1: Elected Candidates' Attributes and the Ideological Composition of School Boards

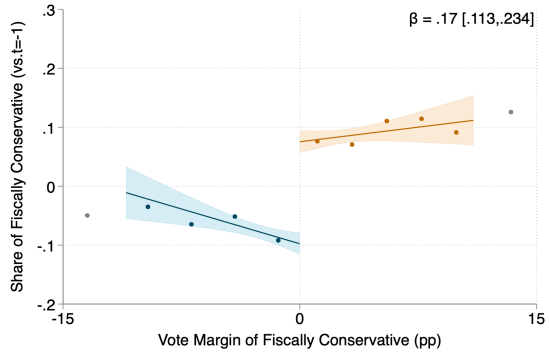
(a) Equity Candidate and Share w/Same Priority



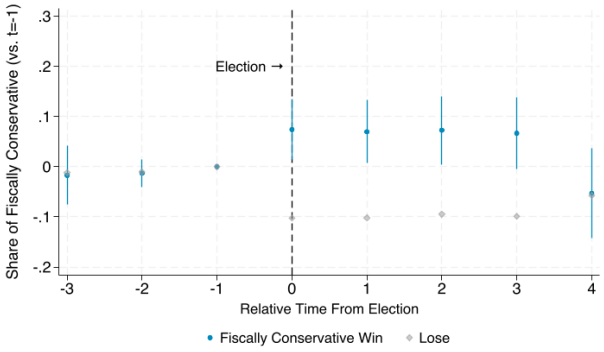
(b) Equity Candidate – Dynamic Effects



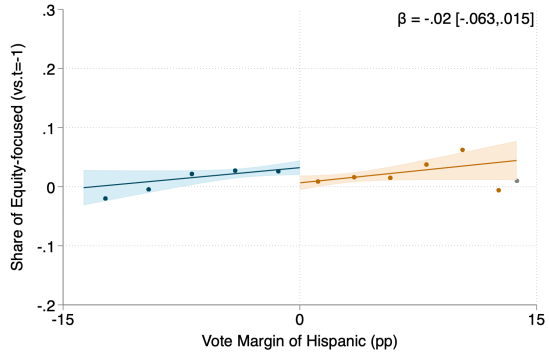
(c) Fiscally Cons. Candidate and Share w/Same Priority



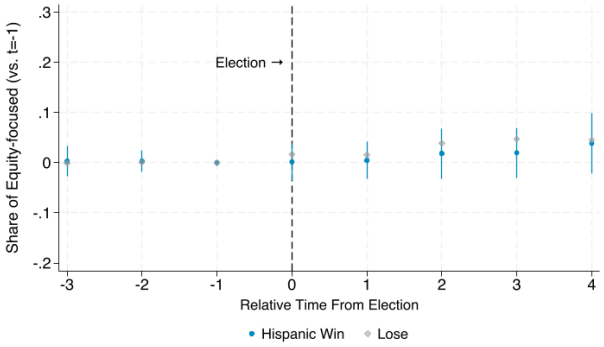
(d) Fiscally Cons. Candidate – Dynamic Effects



(e) Hispanic Candidate and Share w/Equity Priority



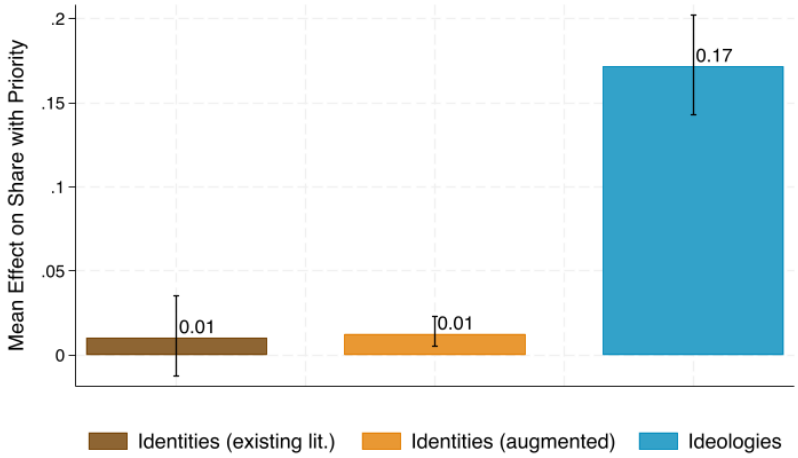
(f) Hispanic Candidate and Equity Priority – Dynamic Effects



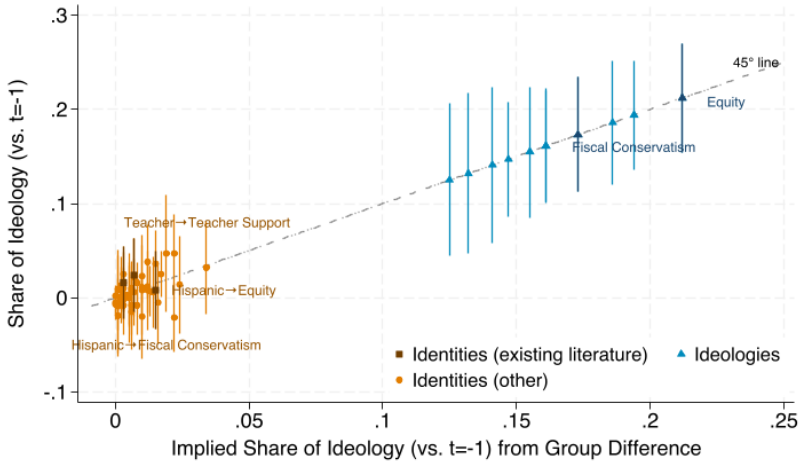
Notes: The left panels display regression discontinuity estimates of the effect of electing a candidate with a given characteristic on the share of board members with a given priority. We pool observations across years 0 to 3 since the election, corresponding to the elected candidate's four-year term. Confidence intervals for the RD point estimates are taken from Calonico et al. (2020). We obtain 95% confidence bands for the local linear fits in the shaded region from separate linear regressions on the RD sample (Stata's lfitci), so the bands for the fitted lines and the intervals for the discontinuity need not coincide exactly at the threshold. The right panels report RD estimates in which the same model is estimated separately for each year relative to the election. "Lose" points are mean below-threshold values. "Win" points are equal to the below-threshold mean plus the RD estimate and include a 95% CI. All estimates are obtained using equation (1), where D_i and m_i indicate the election of a candidate with the corresponding focal characteristic and their vote margin, respectively. The dependent variable in all panels is expressed as the change relative to the year before the election. We use a triangular kernel with MSE-optimal bandwidths from Calonico et al. (2020). We use standard errors clustered at the election level in the left panels and robust standard errors in the right panels.

Figure 2: Elected Candidates' Attributes and the Ideological Composition of School Boards: Summarizing RD Estimates Across Candidate Types

(a) Mean Change in Share w/Priority



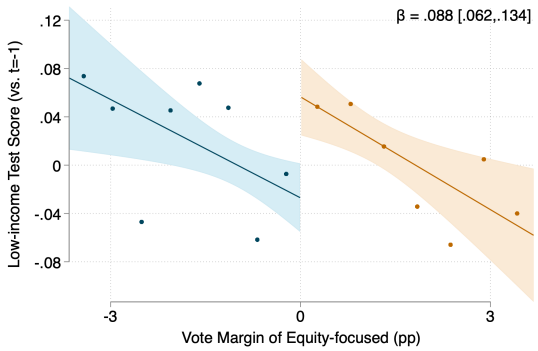
(b) Change in Share w/Priority: RD Estimates vs. Cross-Sectional Predictions



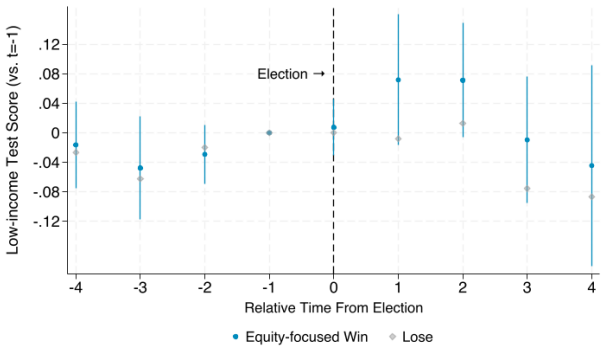
Notes: Panel (a) shows the mean of RD estimates by characteristic group, obtained using equation (2). *Identities (existing literature)* includes three identity-ideology pairs (Hispanic-Equity, Hispanic-Fiscal Conservatism, Teacher-Teacher Support). *Identities (augmented)* includes other identity-ideology pairs where viewpoint difference is significant by the identity group (Table B6). *Ideologies* includes the effects of electing a candidate with a given ideology on the share of that ideology in the board and considers all ideologies. In both panels, the dependent variable is the change in share relative to the year before the election; we pool observations across years 0 to 3 since the election, corresponding to the elected candidate's four-year term. In panel (a), we additionally multiply each outcome by the sign of the corresponding effect, so that all aggregated effects have a positive sign (Appendix Table B6). Panel (b) plots RD estimates of the effect of electing a candidate with a given characteristic (identity or ideology) on the subsequent share of board members with a given priority (y-axis) against the effect of the same characteristic that would be predicted given differences in priorities across identity groups in the raw data (x-axis). The predicted effect is calculated from the average difference in priorities between candidates with and without the focal characteristic. RD estimates are obtained using equation (1), where D_i and m_i indicate the election of a candidate with the corresponding focal characteristic and their vote margin, respectively. The dashed line is the 45-degree line. All models control for year fixed effects; estimates in panel (a) additionally control for identity-ideology pair fixed effects. We use a triangular kernel with MSE-optimal bandwidths from Calonico et al. (2020). Standard errors are clustered at the election level.

Figure 3: Elected Candidates' Attributes and School District Outcomes

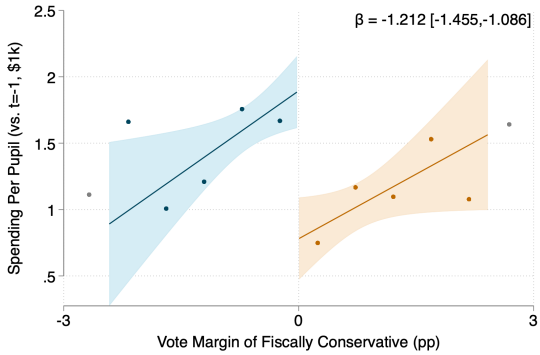
(a) Equity Candidates and Low-Income Test Scores



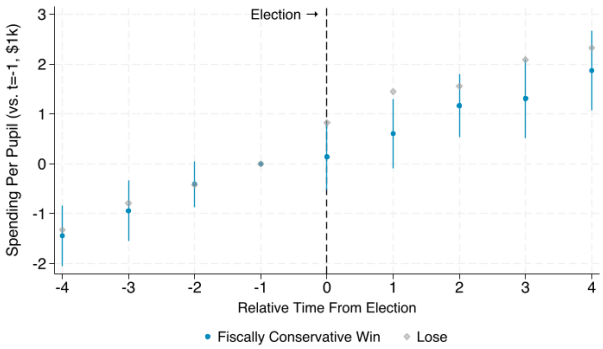
(b) Equity Candidates - Dynamic Effects



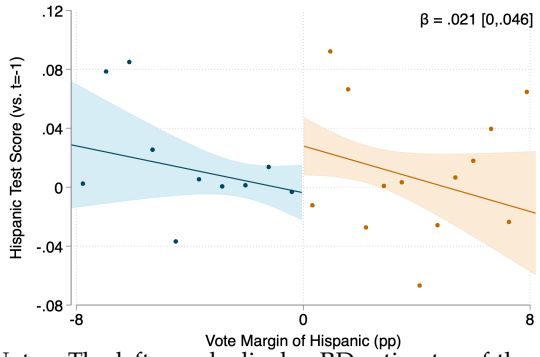
(c) Fiscally Cons. Candidates and Per-Pupil Spending



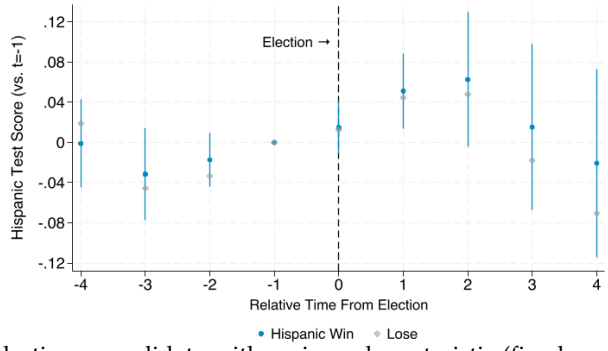
(d) Fiscally Cons. Candidates - Dynamic Effects



(e) Hispanic Candidates and Hispanic Test Scores



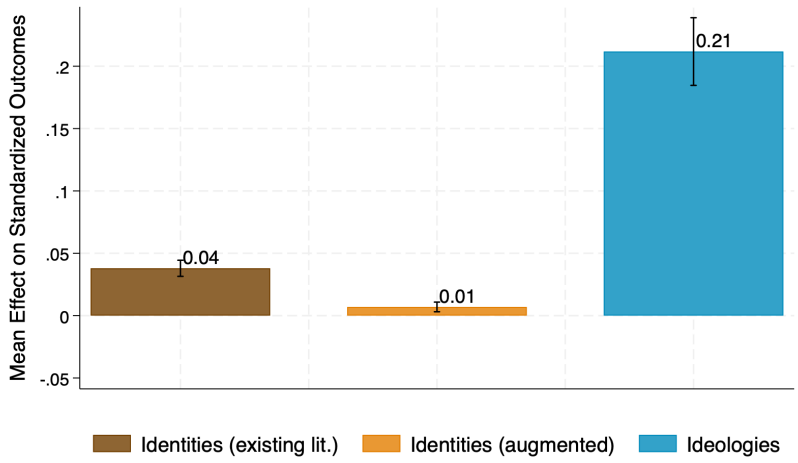
(f) Hispanic Candidates - Dynamic Effects



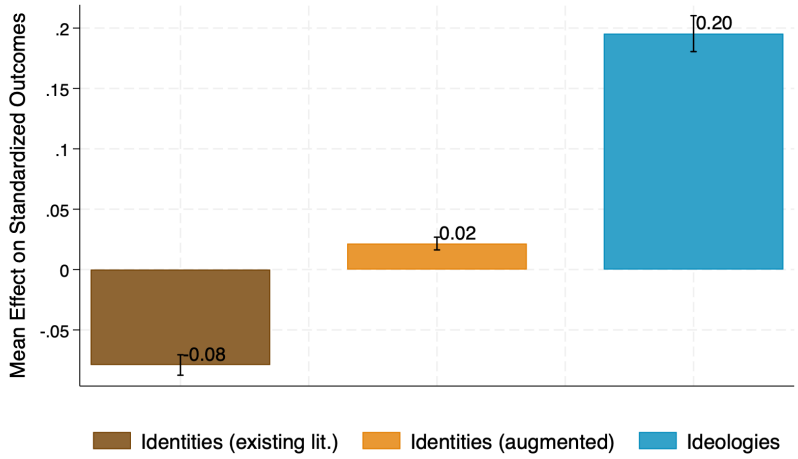
Notes: The left panels display RD estimates of the effect of electing a candidate with a given characteristic (fiscal conservatism, equity focus, or Hispanic identity) on district outcomes. We pool observations across years 0 to 4 since the election. Confidence intervals for the RD point estimates are taken from Calonico et al. (2020). We obtain 95% confidence bands for the local linear fits in the shaded region from separate linear regressions on the RD sample (Stata's lfitci), so the bands for the fitted lines and the intervals for the discontinuity need not coincide exactly at the threshold. The right panels report RD estimates in which the same model is estimated separately for each year relative to the election. "Lose" points are mean below-threshold values. "Win" points are equal to the below-threshold mean plus the RD estimate and include a 95% CI. All estimates are obtained using equation (1), where D_i and m_i indicate the election of a candidate with the corresponding focal characteristic and their vote margin, respectively. The dependent variables are standardized test scores for low-income students (panels (a) and (b)), per-pupil spending (in thousands of 2019 U.S. dollars, panels (c) and (d)), and standardized test scores for Hispanic students (panels (e) and (f)). These outcomes are all expressed as the change relative to the year before the election. All models include year fixed effects. We use a triangular kernel with MSE-optimal bandwidths from Calonico et al. (2020). We use standard errors clustered at the election level in the left panels and robust standard errors in the right panels.

Figure 4: Elected Candidates' Attributes and School District Outcomes: Summarizing RD Estimates Across Candidate Types

(a) Main Effects



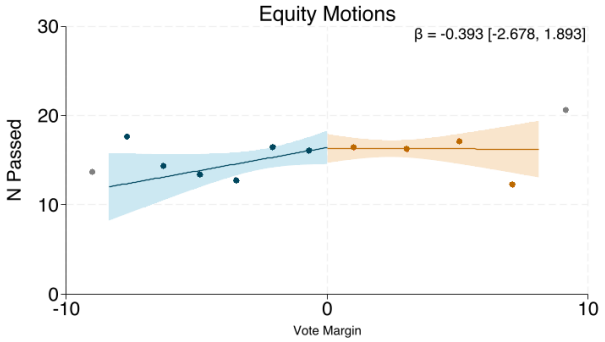
(b) Effects Holding Other Characteristics Constant



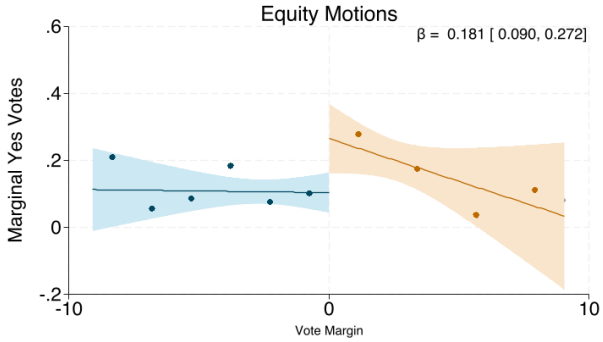
Notes: Mean RD estimates on district outcomes by characteristic group, obtained by estimating equation (2) on a stacked dataset where each stack is constructed by considering a focal characteristic and its corresponding outcome, and where D_i and m_i indicate the election of a candidate with the relevant characteristic and their vote margin, respectively. We include six characteristic–outcome pairs for the *Identities (existing literature)* category, 23 for the *Identities (augmented)* category, and 14 for the *Ideologies* category (see Table B6 and B5 for the full list of characteristics-outcome pairs). The dependent variable in each stack is standardized to have mean zero and variance equal to one and expressed as the change relative to the year before the election; we pool observations over the five years following an election. For ease of interpretation, we multiply each outcome by the hypothesized direction of the effects (for ideology-outcome pairs, expected signs are taken from Table B5; for identity-outcome pairs, expected signs are taken from Table B6). All models control for year and stack fixed effects. We use a triangular kernel with MSE-optimal bandwidths from Calonico et al. (2020). Standard errors are clustered at the election level. In panel (b), we restrict attention to races where the two marginal candidates have the same ideologies associated with a given identity (left and center bar) and to races where the two marginal candidates have similar predicted ideologies given identities (i.e., with a difference smaller than one percentage point, right bar). We predict ideologies using the random forest shown in Table 3.

Figure 5: Candidates' Ideology and Board Actions

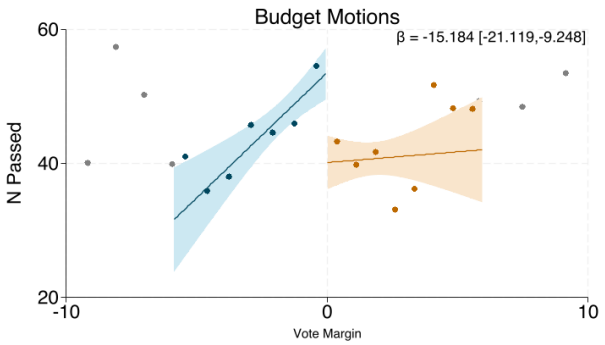
(a) Equity Members and # Passed Motions



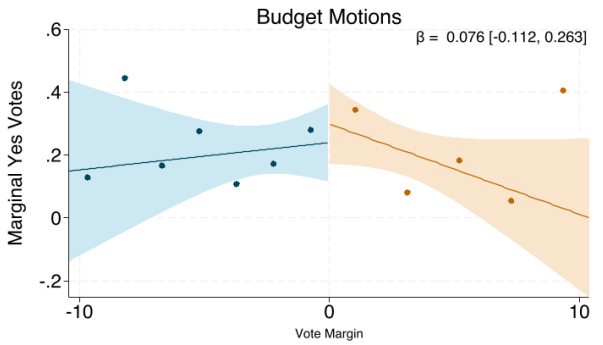
(b) Equity Members and Marginal Votes



(c) Fiscally Conservative Members and # Passed Motions



(d) Fiscally Conservative Members and Marginal Votes



Notes: The figures show RD estimates of equation (1), where D_i indicates the election of either an equity candidate (panels (a) and (b)) or a fiscally conservative candidate (panels (c) and (d)) and m_i is their respective vote margin. The outcome variables are the number of motions related to the focal priority passed by the board (panels (a) and (c)) and the candidates' marginal votes on the focal priority, i.e., yes votes that shift the majority (panels (b) and (d)). We pool observations across years 0 to 3 since the election, corresponding to the elected candidate's four-year term. All models include year fixed effects. We use a triangular kernel with MSE-optimal bandwidths from Calonico et al. (2020). Standard errors are clustered at the election level. Confidence intervals for the RD point estimates are taken from Calonico et al. (2020). We obtain 95% confidence bands for the local linear fits in the shaded region from separate linear regressions on the RD sample (Stata's lfitci), so the bands for the fitted lines and the intervals for the discontinuity need not coincide exactly at the threshold.

Tables

Table 1: Summary Statistics

	All Elections		Close Elections (< 5pp)			
	All (1)	All (weighted) (2)	Any Close (3)	Equity (4)	Fiscally Conservative (5)	Hispanic (6)
Panel A: Candidate Characteristics						
Share Hispanic	0.22	0.27	0.22	0.18	0.16	0.42
Share Female	0.44	0.43	0.44	0.48	0.45	0.43
Share Teacher	0.17	0.29	0.18	0.23	0.21	0.18
Share Registered Democrat	0.49	0.53	0.50	0.56	0.52	0.53
Share Having a Platform	0.38	0.51	0.39	0.68	0.65	0.31
N Candidates	22,426	22,426	5,038	3,602	4,458	3,988
N Elections	5,782	5,782	2,519	905	1,087	1,004
Panel B: Educational Outcomes in the Year Before the Election						
CCD Spending Per Pupil ($1k) (1998-)	9.72	9.74	9.51	10.92	10.06	9.75
N Districts x Years	3,654	3,654	1,973	705	907	813
CCD District Enrollment (1998-)	11926		11797	17756	16460	16455
Share White (1998-)	0.35	0.26	0.36	0.34	0.38	0.23
Share Hispanic (1998-)	0.45	0.51	0.45	0.41	0.38	0.59
N Districts x Years	1,673	1,673	867	367	461	410
SEDA Test Scores (2009-)	-0.16	-0.21	-0.18	-0.04	0.00	-0.32
Hispanic Test Scores (2009-)	-0.47	-0.48	-0.48	-0.42	-0.41	-0.51
Low-Income Test Scores (2009-)	-0.43	-0.45	-0.43	-0.37	-0.34	-0.49
Share Low-Income Students (2009-)	0.56	0.63	0.57	0.50	0.48	0.67
N Districts x Years	1,673	1,673	867	367	461	410
Panel C: Meeting Characteristics in the Year Before the Election						
Total Number of Meetings (2016-)	21.92	23.62	21.60	23.28	23.32	21.83
Present Members per Meeting (2016-)	5.25	5.53	5.08	5.22	5.20	5.33
Proposed Motions per Year (2016-)	153.31	160.34	152.47	158.51	154.37	157.71
Passed Motions per Year (2016-)	151.44	158.08	150.51	156.67	152.55	155.68
N Districts x Years	1,316	1,316	411	300	251	247

Notes: Summary statistics for school board elections and district characteristics. Column 1 reports averages across all elections. Column 2 reports the same averages using district enrollment in the year before the election as weights, so these estimates can be interpreted as student-weighted averages. Column 3 reports close elections with a vote margin smaller than 5 percentage points. Columns 4–6 report close elections where the marginal candidates differ in the focal characteristic listed in the column header: equity-focused, fiscally conservative, or Hispanic. Panel A reports candidate characteristics. Panel B summarizes educational outcomes in the year before the election, including per-pupil spending from the Common Core of Data (CCD) and average test scores from the Stanford Education Data Archive (SEDA), disaggregated for Hispanic and *low-income* subgroups. While the statutory criteria have changed over time, California has used Title I status of the federal *No Child Left Behind Act*, free/reduced-price lunch eligibility, or poverty status following the federal guideline to define economically disadvantaged, or *low-income* students. Years of data availability are shown in parentheses. For data provided at the academic-year level, we report the spring semester of the corresponding year. If not indicated, variables span the entire analysis period (1998–2022). Spending is reported in thousands of 2019 U.S. dollars. Panel C reports average school board meeting characteristics in the year prior to the election.

Table 2: Examples of Platforms and Priorities

Example of Candidate Platform	Equity	Fiscal Conservatism	Agenda Bias	Community Engagement	CTE	Dropout	Enrollment	Facility Improv.	Safety	Sup Accountability/Hiring	Teacher Support	Test Scores
Maintain a balanced budget to meet the needs of providing a high-quality education for all students while maintaining a prudent reserve. **Maintain and support a highly-qualified workforce** to provide a rigorous academic program, top-notch extracurricular activities, up-to-date technology, and continuous improvement for all students. To develop a sustainability plan to ensure that we are providing **best possible education for all students** as our district reaches its capacity to **build new schools** and we possibly move to a **declining enrollment**. (2016, Rocklin Unified)	✓	✓					✓	✓			✓	✓
Safety of Students, Serve Parents and Students – **no social experimentation in the classroom**, **Sound Financial Plan** for the District – more dollars to the classroom and no wasteful spending (2008, Grossmont High)		✓	✓	✓					✓			
School Safety, Smaller Class Sizes, **Parent involvement** (2018, El Rancho)				✓					✓			

Notes: Examples of candidate platform statements and their corresponding priorities. Each row presents a platform portion, along with indicators for coded priorities. We highlight in bold language indicative of assigned priorities.

Table 3: Priorities by Demographic Group

	Hispanicity		Gender		Occupation		Political Affiliation		Prediction Accuracy	
	Hispanic (1)	Non- (2)	Female (3)	Male (4)	Teacher (5)	Non- (6)	Democrat (7)	Republican (8)	R-forest (9)	Mean Only (10)
N Candidates	4444	15607	8491	10890	3896	18530	8312	5787		
Have a Platform	**0.26**	**0.40**	**0.41**	**0.36**	**0.56**	**0.34**	**0.44**	**0.31**	0.65	0.53
Share with Priority Among Candidates with Valid Platform										
Equity	**0.37**	**0.34**	**0.41**	**0.27**	0.35	0.34	**0.42**	**0.26**	0.66	0.55
Fiscal Conservatism	**0.34**	**0.44**	0.43	0.43	**0.40**	**0.43**	**0.41**	**0.48**	0.58	0.51
Agenda Bias	0.03	0.04	0.04	0.04	0.04	0.04	**0.03**	**0.08**	0.96	0.92
Community Engagement	**0.46**	**0.42**	**0.48**	**0.38**	0.43	0.43	**0.45**	**0.40**	0.57	0.51
Career and Technical Education	**0.18**	**0.16**	0.16	0.15	**0.18**	**0.15**	0.16	0.15	0.84	0.73
Dropout	**0.15**	**0.10**	**0.11**	**0.09**	**0.12**	**0.10**	**0.13**	**0.08**	0.89	0.81
Enrollment	0.06	0.07	0.07	0.07	0.07	0.07	0.07	0.07	0.93	0.87
Facility Improvement	0.27	0.26	0.25	0.27	**0.24**	**0.27**	**0.25**	**0.28**	0.74	0.61
Safety	**0.40**	**0.32**	**0.36**	**0.30**	0.33	0.34	0.35	0.34	0.66	0.55
Superintendent Accountability	0.05	0.05	0.05	0.05	0.06	0.05	0.06	0.05	0.95	0.90
Teacher Support	0.22	0.24	0.25	0.23	**0.26**	**0.23**	**0.25**	**0.20**	0.76	0.64
Test Scores	**0.39**	**0.36**	**0.38**	**0.35**	0.37	0.36	0.36	0.35	0.64	0.54

Notes: Number of candidates, share of candidates with a valid platform, and share of candidates with each stated priority among those with a platform. Columns 1–2 compare Hispanic and non-Hispanic candidates; columns 3–4 compare female and male candidates; columns 5–6 compare teachers and non-teachers; and columns 7–8 compare Democrats and Republicans. Values in bold indicate that the difference in the share of candidates stating a given priority between identity groups (e.g., Hispanic vs. Non-Hispanic) is statistically significant at the 5% level. Column 9 shows the share of all candidates for whom we can accurately predict a priority using all the identity variables in a random forest classification with 1,000 iterations, calculated using out-of-bag errors. Column 10 shows the share of candidates for whom we accurately predict priorities using only the share of each priority in the sample, i.e., purely out of chance (for example, since 35% of candidates prioritize equity, the probability of correctly predicting a candidate's priority based purely on chance is $0.35^2 + 0.65^2 = 0.55$).

Table 4: Specification Checks for the Regression Discontinuity Design

	Pooled Tests	Case-Study Characteristics			Aggregates Priorities or Identities		
		Equity	Fiscally Conservative	Hispanic	Identity (Literature)	Identity (Other)	Ideology
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: McCrary Test							
p-value	0.191	0.719	0.445	0.146	0.028	0.032	0.607
N	5327	557	692	616	1025	2257	3485
Panel B: Pre-Treatment District Covariate Balance							
Log Enrollment in t=-1							
Estimate	0.009	-0.056	0.037	0.159	0.057	0.017	-0.014
	[-0.150, 0.169]	[-0.392, 0.281]	[-0.300, 0.374]	[-0.193, 0.510]	[-0.196, 0.310]	[-0.187, 0.220]	[-0.216, 0.188]
N	8353	639	775	764	1627	3603	4731
Low-Income Share in t=-1							
Estimate	0.000	0.011	-0.006	-0.012	-0.000	0.001	-0.002
	[-0.011, 0.012]	[-0.012, 0.033]	[-0.029, 0.016]	[-0.038, 0.014]	[-0.020, 0.019]	[-0.013, 0.015]	[-0.015, 0.012]
N	8430	680	725	658	1505	3482	5217
Panel C: Selection into Outcome Sample							
Estimate	-0.002	0.007	0.012	-0.000	0.043	-0.017	-0.000
	[-0.024, 0.019]	[-0.188, 0.202]	[-0.022, 0.046]	[-0.021, 0.021]	[-0.017, 0.104]	[-0.042, 0.008]	[-0.053, 0.053]
N	80840	1403	2145	1644	13753	45182	17592
Panel D: Pre-Treatment Outcome Balance							
Outcome Level in t=-1							
Estimate	-0.043	0.005	0.139	0.030	-0.028	-0.023	-0.189
	[-0.090, 0.005]	[-0.618, 0.628]	[-0.025, 0.303]	[-0.128, 0.187]	[-0.164, 0.107]	[-0.081, 0.034]	[-0.331, -0.047]
N	18845	275	537	655	2896	10815	3577
Outcome Change from $k = -4, -3, -2$ to $t = -1$							
Estimate	-0.028	0.022	0.037	-0.024	-0.029	-0.001	-0.098
	[-0.067, 0.010]	[-0.120, 0.164]	[-0.043, 0.117]	[-0.109, 0.060]	[-0.137, 0.078]	[-0.044, 0.042]	[-0.196, -0.000]
N	46537	491	2252	1982	8549	27701	10588
Panel E: Selection into Minutes Sample							
Estimate	-0.003	0.124	-0.057	0.019	-0.031	-0.047	0.034
	[-0.075, 0.069]	[-0.026, 0.274]	[-0.316, 0.203]	[-0.151, 0.190]	[-0.146, 0.085]	[-0.132, 0.039]	[-0.063, 0.132]
N	14171	1290	258	1039	2229	5360	8193

Notes: Results of RD specification tests. Each column reports results for a (set of) candidate types. Column (1) aggregates across all focal characteristics, including both identity and ideology characteristics. Columns 2-4 correspond to candidate characteristics: Equity (column 2), fiscal conservatism (column 3), and Hispanic ethnicity (column 4). Columns 5-7 consider aggregated identity characteristics tested in the literature (column 5), other identity characteristics (column 6), and ideology characteristics (column 7). Panel A shows p-values from a McCrary test for discontinuities in the density function of each characteristic around the electoral cutoff. Panel B tests for balance in pre-treatment district characteristics measured in the year before the election. Panel C tests for selection into the outcome sample using an indicator for non-missing outcomes as the dependent variable. Panel D tests for balance in associated outcomes (see Appendix Tables B5 and B6), using both outcome levels in the year before the election and pooled changes from the baseline year to earlier pre-election years. The dependent variable in the outcome change panel is the difference between outcomes in year $t-1$ and outcomes in year $t-k$ for $k = -4, -3, -2$. Panel E tests for selection into the minutes data using an indicator for non-missing minutes records as the dependent variable. In Panels B-E we control for year fixed effects and show estimates and 95% confidence intervals (in brackets) obtained using a triangular kernel with MSE-optimal bandwidths from Calonico et al. (2020) and clustering standard errors at the election level.

Table 5: Effects of Electing Candidates with Different Policy Priorities

(1) Priority	(2) Outcome (Unit)	(3) Expected Sign	(4) RD Estimates Raw Outcome [95% CI]	(5) RD Estimates Standardized Outcome [95% CI]
Panel A: Case studies				
Equity	Low-income test scores (student-level SD)	+	0.088 [0.062, 0.134]	0.360 [0.231, 0.489]
Fiscal Conservatism	Total spending per pupil (2019 USD, thousands)	−	-1.212 [-1.455, -1.086]	-0.267 [-0.294, -0.240]
Panel B: Other Ideology-Outcome Pairs				
Equity	Hispanic test scores (student-level SD)	+	0.016 [-0.004, 0.043]	0.129 [0.007, 0.252]
Fiscal Conservatism	Bond passage per pupil (2019 USD, thousands)	−	-1.765 [-2.015, -1.515]	-0.471 [-0.536, -0.407]
CTE	CTE revenue per pupil (2019 USD)	+	0.044 [-0.260, 0.588]	0.030 [-0.011, 0.071]
Dropout	Log dropout count (log points)	−	-0.160 [-0.433, 0.038]	-0.116 [-0.287, 0.055]
Enrollment	District enrollment (log points)	+	0.041 [0.039, 0.042]	0.001 [0.000, 0.001]
Facility Improvement	Capital spending per pupil (2019 USD, thousands)	+	0.456 [0.373, 0.598]	0.130 [0.049, 0.212]
Safety	Suspended share (Share)	+	0.019 [0.002, 0.044]	0.721 [0.307, 1.135]
Superintendent	Superintendent turnover (Indicator)	+	0.133 [0.111, 0.178]	0.363 [0.278, 0.448]
Teacher Support	Teacher exit rate (Share)	−	-0.042 [-0.058, -0.035]	-0.411 [-0.529, -0.293]
	BA60 salary (log points)	+	-0.002 [-0.020, 0.009]	-0.015 [-0.055, 0.025]
	Entry salary (log points)	+	0.079 [0.069, 0.099]	0.235 [0.194, 0.277]
Test Scores	Average test scores (student-level SD)	+	-0.026 [-0.047, 0.003]	-0.051 [-0.107, 0.005]
Panel C: Aggregate Effects of Ideology				
Ideologies	Aggregate standardized outcome	+		0.212 [0.185, 0.239]

Notes: Estimates of the parameters β in equation (1), where D_i and m_i indicate the election of a candidate with the priority indicated in the first column. Column 2 reports the dependent variable and its unit. Column 3 reports the expected sign of the estimate. Column 4 reports the RD estimate and its 95% confidence interval where the dependent variable is measured in the unit shown in column 2. Column 5 reports the RD estimate and its 95% confidence interval when the dependent variable is standardized to have mean zero and variance one across districts within each year. Panel A lists the two leading case studies from the main text. Panel B reports the remaining ideology-outcome pairs included in the aggregate ideology analysis. Panel C reports the aggregate effect of ideology (the rightmost bar in Figure 4). All dependent variables are measured as changes relative to the year before the election. Observations are restricted to districts offering grade 12 for estimates with dropout outcomes. We pool observations across the five years following each election. All models control for year fixed effects. We use a triangular kernel with MSE-optimal bandwidths from Calonico et al. (2020), and we cluster standard errors at the election level. 95% confidence intervals are shown in brackets.

Table 6: Elected Candidate Attributes and Intermediate Policy Outcomes

Panel A: Equity Candidates

	SFP funds per pupil ($1,000s, 2019 dollars) to Schools with				Classified Staff at Schools with			
	High Low-Income Share	Low Low-Income Share	High Hispanic Share	Low Hispanic Share	High Low-Income Share	Low Low-Income Share	Super Turnover	Predicted Test Scores
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Estimate	0.767	0.339	0.908	0.295	5.234	1.499	-0.008	0.003
	[0.631, 0.904]	[0.126, 0.552]	[0.771, 1.045]	[0.109, 0.481]	[2.668, 7.800]	[-1.353, 4.351]	[-0.035, 0.018]	[-0.001, 0.008]
N	4,744	5,000	4,153	4,995	14,978	12,772	2,300	477
Mean Dep. Var.	3.574	3.200	3.398	3.325	19.904	20.862	0.411	0.070

Panel B: Fiscally Conservative Candidates

	Spending By Sub-Category				Bond Financing		
	Total Capital	Construction	Teacher Salaries	Staff Salaries	CDE Bond Revenue	Bond Passed Amount	Super Turnover
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Estimate	-1.227	-1.205	-0.040	0.001	-1.464	-1.850	0.168
	[-1.315, -1.140]	[-1.288, -1.122]	[-0.071, -0.009]	[-0.015, 0.017]	[-1.656, -1.271]	[-2.129, -1.572]	[0.140, 0.196]
N	924	924	1,625	2,162	1,426	1,293	1,625
Mean Dep. Var.	1.087	0.997	3.251	1.580	1.225	1.033	0.393

Panel C: Hispanic Candidates

	SFP funds per pupil ($1,000s, 2019 dollars) to Schools with				Classified Staff at Schools with			
	High Low-Income Share	Low Low-Income Share	High Hispanic Share	Low Hispanic Share	High Hispanic Share	Low Hispanic Share	Super Turnover	Predicted Test Scores
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Estimate	0.055	-0.054	0.070	-0.089	1.283	-2.449	0.052	0.003
	[-0.100, 0.210]	[-0.272, 0.164]	[-0.115, 0.255]	[-0.320, 0.142]	[-2.860, 5.427]	[-6.710, 1.812]	[0.026, 0.078]	[0.000, 0.006]
N	8,127	6,518	9,099	5,073	16,095	10,835	2,485	666
Mean Dep. Var.	4.293	3.745	4.386	3.617	20.814	21.301	0.401	-0.386

Notes: Estimates of the parameters β in equation (1), where D_i and m_i indicate the election of an equity (Panel A), a fiscally conservative (Panel B), or a Hispanic (Panel C) candidate. In Panels A and C, the dependent variable is the amount of School Facility Program (SFP) funds received by schools with a low-income student share above and below the districtwide low-income share (columns 1-2), and schools with a Hispanic student share above and below the districtwide Hispanic share (columns 3-4); the number of classified staff in schools with a low-income student share above and below the districtwide share (columns 5-6); superintendent turnover (column 7); and test scores as predicted using district student body composition (obtained by estimating a linear regression of test scores on district shares of students who are black, Hispanic, Asian, free/reduced-price lunch, below poverty-line, special-education, or English language learners, and the log of total enrollment; column 8). To account for implausibly low enrollment figures in some schools, we winsorize SFP spending variables at their 2.5th and 97.5th percentiles. In Panel B, the dependent variable is spending on capital investments (column 1), school construction (column 2), teacher salaries (column 3), and staff salaries (column 4); CDE bond revenues (column 5); the amount of bonds passed (column 6); and superintendent turnover (column 7). All spending and revenue amounts are per pupil and expressed in thousands of 2019 U.S. dollars. Dependent variables are expressed as the change relative to the year before the election, except for superintendent turnover. All models control for year fixed effects. We use a triangular kernel with MSE-optimal bandwidths from Calonico et al. (2020), and we cluster standard errors at the election level. 95% confidence intervals are shown in brackets. Subgroup-difference p-values reported in the text are computed from 999 bootstrap replications.

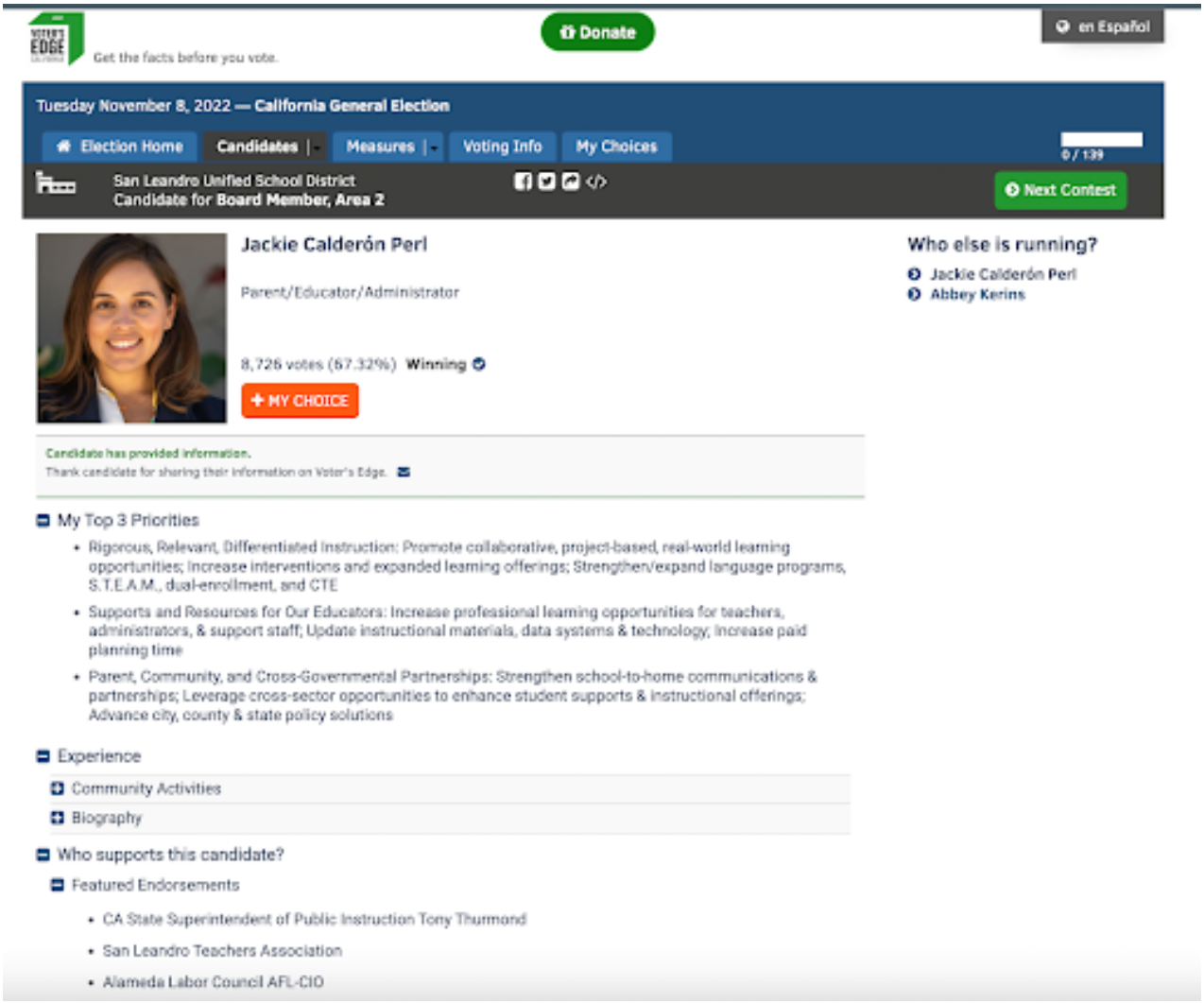
Table 7: Elected Candidate Attributes and School Board Actions

	Equity		Fiscal		Hispanic	
	Focal	All	Focal	All	Equity	Fiscal
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Proposed Motions						
Estimate	-0.284	-5.424	-15.285	-51.162	3.839	0.543
	[-2.623, 2.055]	[-21.929, 11.082]	[-21.174, -9.396]	[-68.850, -33.473]	[1.439, 6.240]	[-3.817, 4.903]
Mean Dep. Var.	15.346	154.376	45.057	157.286	17.296	43.990
N	786	819	548	555	635	610
Panel B: Passed Motions						
Estimate	-0.393	-6.004	-15.184	-51.317	3.527	0.184
	[-2.678, 1.893]	[-22.594, 10.586]	[-21.119, -9.248]	[-69.027, -33.607]	[1.152, 5.902]	[-4.076, 4.444]
Mean Dep. Var.	15.028	152.645	44.565	155.140	16.910	43.510
N	784	816	552	552	645	631
Panel C: Non-Unanimous Motions						
Estimate	0.672	3.021	-0.103	-1.456	1.484	2.007
	[0.134, 1.209]	[0.247, 5.795]	[-1.059, 0.852]	[-4.364, 1.451]	[0.791, 2.177]	[0.786, 3.227]
Mean Dep. Var.	2.164	15.260	3.650	14.203	2.157	4.521
N	827	971	905	876	645	679
Panel D: Pivotal Votes (One-Vote margins)						
Estimate	0.181	1.283	0.076	0.166	-0.011	0.101
	[0.090, 0.272]	[0.698, 1.868]	[-0.112, 0.263]	[-0.640, 0.972]	[-0.072, 0.049]	[-0.039, 0.240]
Mean Dep. Var.	0.114	1.287	0.219	1.265	0.124	0.230
N	819	739	786	823	679	738
Panel E: Pivotal Votes in Non-Unanimous Case						
Estimate	0.494	3.957	0.609	0.951	0.180	0.595
	[0.186, 0.803]	[1.454, 6.459]	[-0.008, 1.226]	[-1.612, 3.513]	[-0.256, 0.617]	[-0.287, 1.477]
Mean Dep. Var.	0.834	11.198	1.770	10.178	1.191	2.554
N	735	732	924	949	645	692

Notes: Estimates of the parameters β in equation (1), where D_i and m_i indicate the election of an equity candidate (columns 1 and 2), a fiscally conservative candidate (columns 3 and 4), or a Hispanic candidate (columns 5 and 6). In Panel A, the dependent variable is the number of proposed motions on each candidate's focal issue (e.g., equity for equity candidates; columns 1 and 3), on all issues (columns 2 and 4), on equity motions (column 5), and on budget motions (column 6). In Panel B, the dependent variable is the number of passed motions on the same issues. In Panel C, the dependent variable is the number of non-unanimous motions, i.e., those passed with at least one negative vote. In Panel D, the dependent variable counts the number of yes votes cast by the focal elected member on motions on each issue that pass with a margin of only one vote. In Panel E, the dependent variable counts the number of yes votes cast by the focal elected member on motions on each issue that pass with at least one negative vote. We pool observations across years 0 to 3 since the election, corresponding to the elected candidate's four-year term. All models control for year fixed effects. We use a triangular kernel with MSE-optimal bandwidths from Calonico et al. (2020), and we cluster standard errors at the election level. 95% confidence intervals are shown in brackets.

A Additional Figures

Figure A1: Example of a Candidate Profile on Voter's Edge



Notes: This figure shows an example of a candidate profile from the Voter's Edge website.

Figure A2: Examples of School Board Meeting Minutes

(a) Date and List of Present Members — Dublin Unified School District, 2021

MINUTES
DUBLIN UNIFIED SCHOOL DISTRICT
BOARD OF TRUSTEES
REGULAR BOARD MEETING
Meeting Held via Zoom Video Conference / Live streaming
Tuesday, February 9, 2021, and Wednesday, February 10, 2021

Members present
Dan Cherrier, Megan Rouse, Gabrielle Blackman, Kristin Pelham, Catherine Kuo, Katherine Cheng, Michael Riley.

(b) Example of Meeting Discussion — Dublin Unified School District, 2021

Action: 3. FACILITIES - Facilities Solutions - Short and Long Term Solutions to the Challenges Faced in the Dublin Unified School District Building Program
President Cherrier noted the lateness of the hour, and noted that the remainder of the agenda would take a few hours to complete. He proposed that the Board plan to continue the meeting at a yet-to-be determined date at time, in order to complete all agenda items. He proposed that, prior to recessing the meeting, that the Board receive staff's presentation on Item G-3, ask clarifying questions, and hear the public comment from those wishing to speak who are not able to return at the continuation of the meeting. By general consensus, it was agreed by all to take this approach, and to plan the continuation of the meeting for Wednesday, February 10, 2021 at 6:30 PM.

Interim Superintendent Moirao, along with Chris Hobbs, Interim Assistant Superintendent of Business Services and Thomas Moore, Assistant Superintendent of Facilities, Construction, and Planning presented information on the budget shortfall on the District's facilities projects. The presentation included various options for moving forward, as well as the current state of progress on the projects at Murray Elementary, Emerald High School, and Dublin Elementary. Also included was staff's recommendation to fund the shortfall at Murray with $15M with accumulated interest and additional developer fees; to fund the Dublin Elementary project for $43M, including $10M from Measure J; and to fund the shortfall at EHS Phase 1 from Measure J for $103M. Staff addressed clarifying questions from the Board, then took public comment from those speakers who were not able to return to make comment at the continuation of the meeting tomorrow.

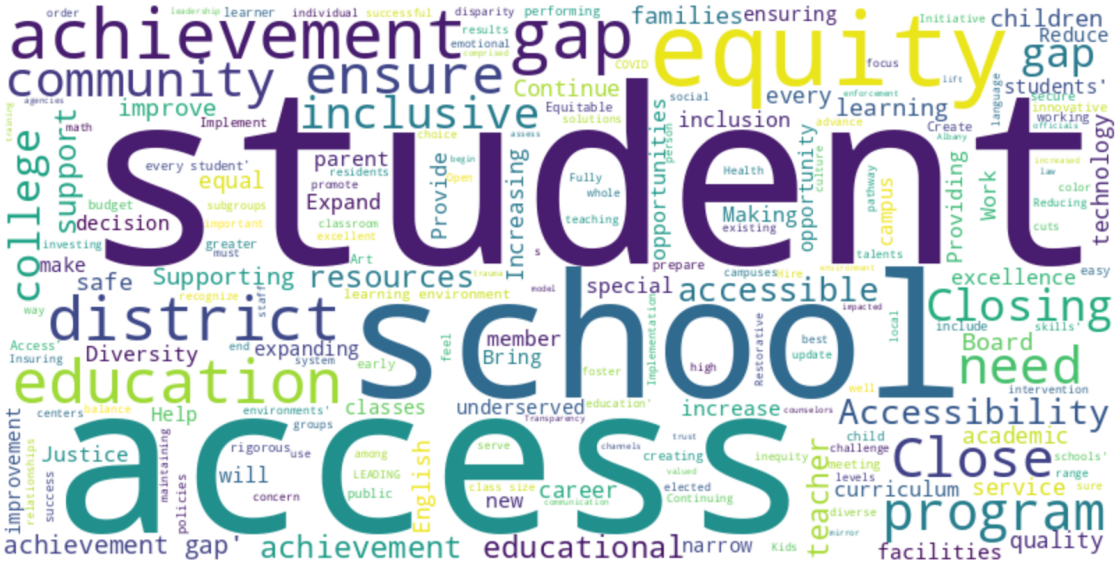
(c) Example of a Motion — Antioch Unified School District, 2016

Motion: Approve tentative settlement agreement of Special Education matter - Case No. 2016070742, Case No. 2016070703, Case No. 2016070134, Case No. 2016070060, and Case No. 2016070266. **Action:** Approve. **Moved by:** Mr. Walter Ruehlig, **seconded by:** Mr. Alonzo Terry. **Vote:** Motion carried by 3-2 vote. **Yes:** Mr. Alonzo Terry, Mr. Walter Ruehlig, Ms. Diane Gibson-Gray. **No:** Mr. Fernando Navarro, Ms. Debra Vinson.

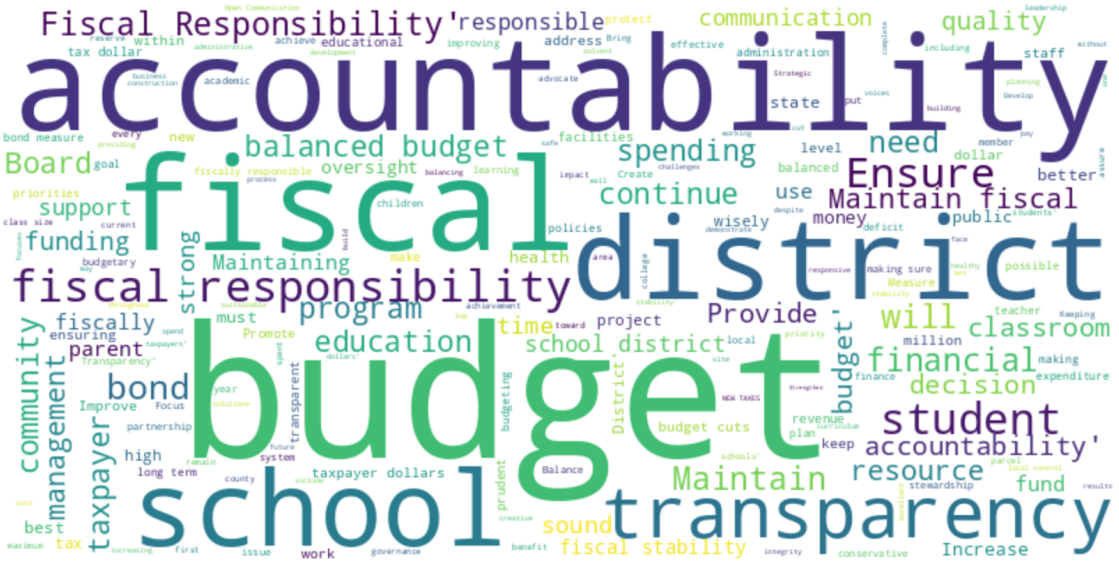
Notes: These figures are excerpts from school board meeting minutes.

Figure A3: Word Clouds from Platforms of Equity and Fiscally Conservative Candidates

(a) Equity Candidates



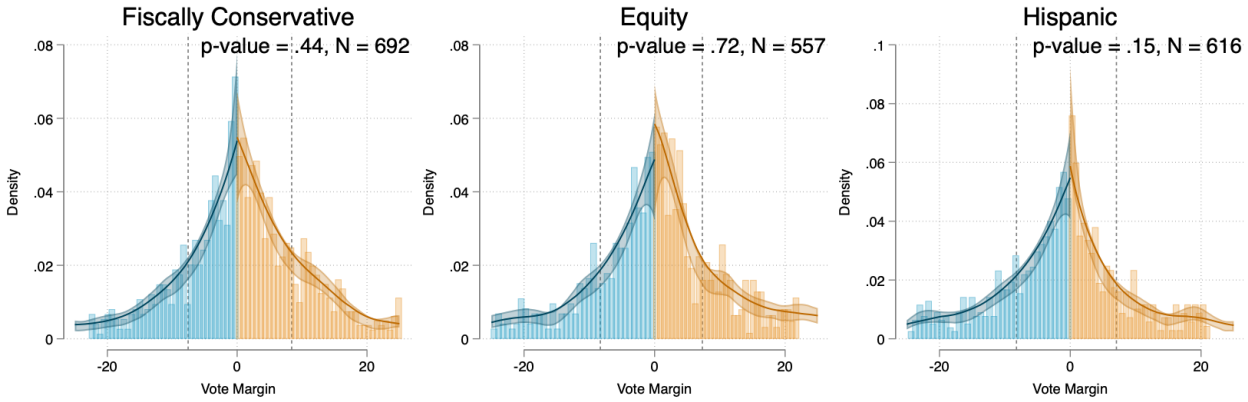
(b) Fiscally Conservative Candidates



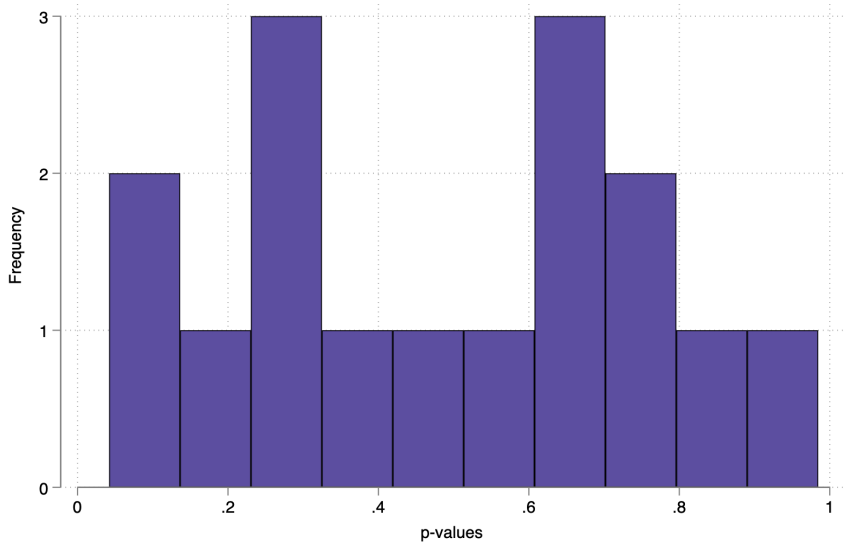
Notes: Word clouds constructed from a 10% sample of candidates' platform priority statements. We split each candidate's platform into priorities and estimated a Latent Dirichlet Allocation model. We show the topics we interpret as equity (panel (a)) and fiscal conservatism (panel (b)); larger words denote higher estimated frequency among the terms in the corresponding topic. The excluded stop words include scikit-learn's English stop word list plus five terms we added, which appear frequently across nearly all platforms: *district*, *school*, *schools*, *student*, and *students*.

Figure A4: Distribution of Candidates' Vote Margins

(a) Case-Study Characteristics



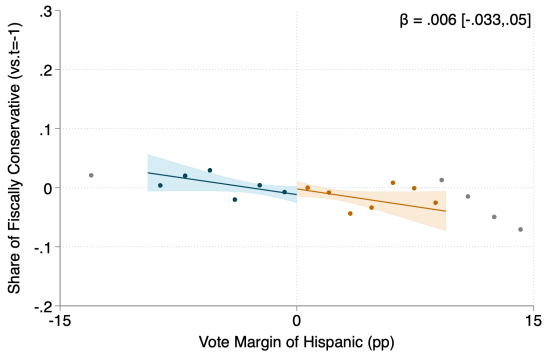
(b) All Ideology and Identity Characteristics – P-Values



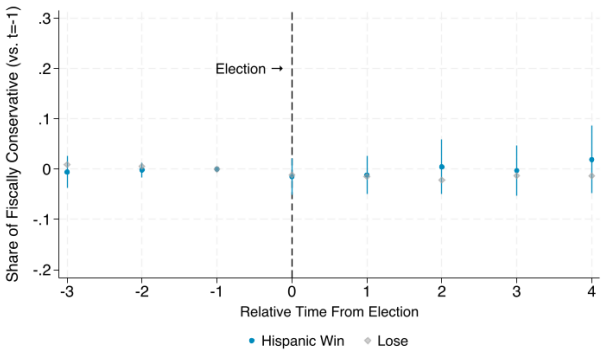
Notes: Panel (a) shows the distribution of vote margins around the electoral cutoff for fiscally conservative, equity, and Hispanic candidates. The vote margin is defined as the difference between the focal candidate's vote share minus the closest opposing marginal candidate's vote share, and it is positive when the focal candidate is the least popular winner and negative when they are the most popular loser. Shaded regions denote confidence intervals for a McCrary test of discontinuity of the density function around the cutoff, for which p-values are reported. The dotted lines mark the optimal bandwidth used for the same test. Panel (b) summarizes the distribution of p-values from McCrary tests for all focal ideology and identity characteristics used in the analysis.

Figure A5: Hispanic Candidates and Share Fiscally Conservative

(a) Pooled Effect



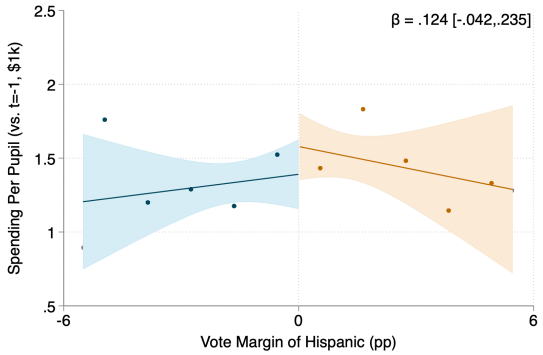
(b) Dynamic Effects



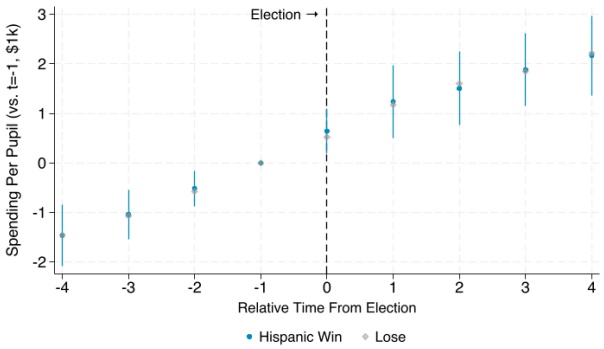
Notes: The left panel displays an RD estimate of the effect of electing a Hispanic candidate on the fiscal conservatism of school boards. We pool observations across years 0 to 3 since the election, corresponding to the elected candidate's four-year term. Confidence intervals for the RD point estimates are taken from Calonico et al. (2020). We obtain 95% confidence bands for the local linear fits in the shaded region from separate linear regressions on the RD sample (Stata's lfitci), so the bands for the fitted lines and the intervals for the discontinuity need not coincide exactly at the threshold. The right panel reports RD estimates in which the same model is estimated separately for each year relative to the election. "Lose" points are mean below-threshold values. "Win" points are equal to the below-threshold mean plus the RD estimate and include a 95% CI. All estimates are obtained using equation (1), where D_i and m_i indicate the election of a Hispanic candidate and their vote margin, respectively. The dependent variable in all panels is expressed as the change relative to the year before the election. We use a triangular kernel with MSE-optimal bandwidths from Calonico et al. (2020). We use standard errors clustered at the election level in the left panels and robust standard errors in the right panel.

Figure A6: Hispanic Candidates, Per-Pupil Spending, and Bond Passage

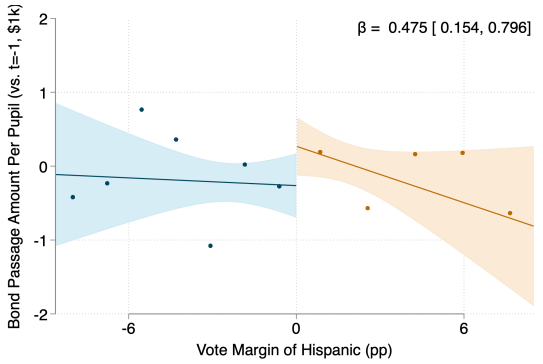
(a) District Spending: Pooled Effect



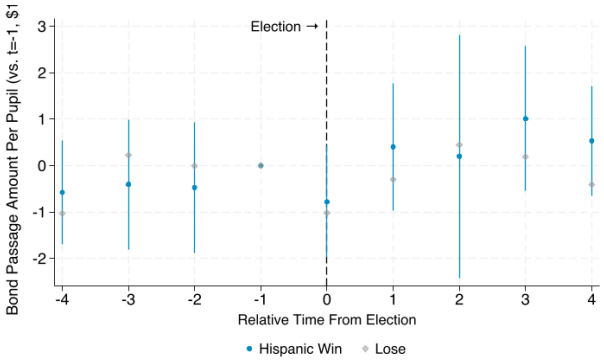
(b) District Spending: Dynamic Effects



(c) Bond Passage: Pooled Effect



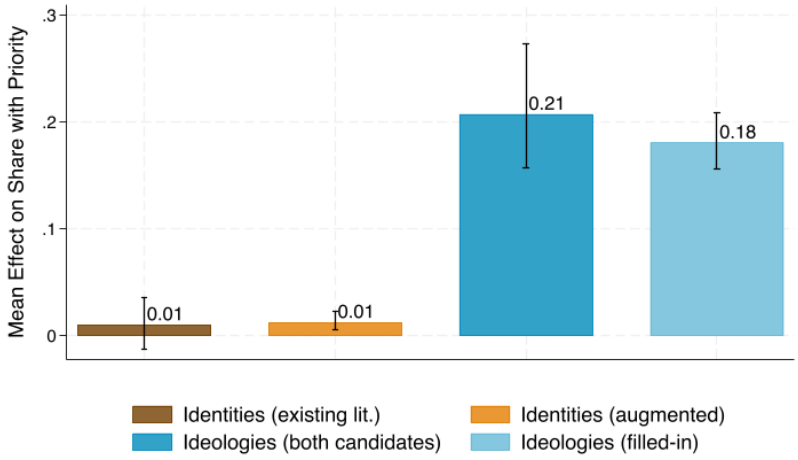
(d) Bond Passage: Dynamic Effects



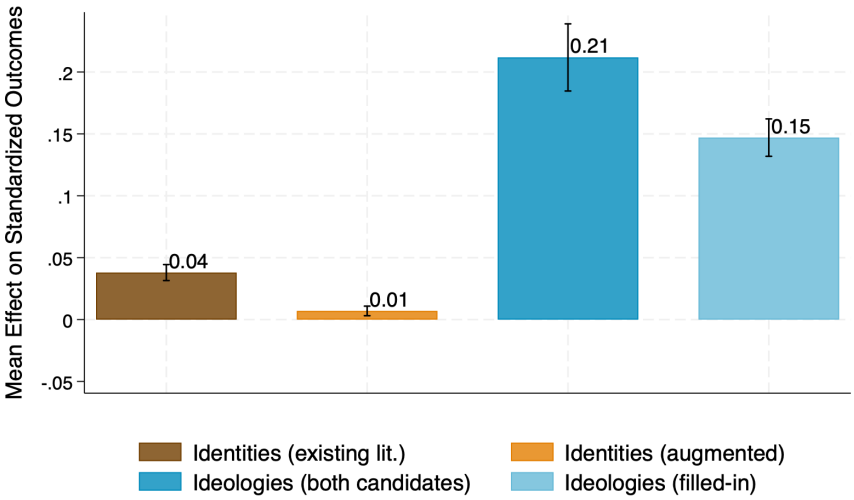
Notes: Panels (a) and (b) report the effect of electing a Hispanic candidate on district per-pupil spending. Panels (c) and (d) report the effect on passed bond amounts per pupil. In panels (a) and (c), we pool observations across years 0 to 4 since the election. Confidence intervals for the RD point estimates are taken from Calonico et al. (2020). We obtain 95% confidence bands for the local linear fits in the shaded region from separate linear regressions on the RD sample (Stata's lfitci), so the bands for the fitted lines and the intervals for the discontinuity need not coincide exactly at the threshold. Panels (b) and (d) report RD estimates in which the same model is estimated separately for each year relative to the election. "Lose" points are mean below-threshold values. "Win" points are equal to the below-threshold mean plus the RD estimate and include a 95% confidence interval. All estimates are obtained using equation (1), where D_i and m_i indicate the election of a Hispanic candidate and the candidate's vote margin, respectively. The dependent variables are measured in thousands of 2019 U.S. dollars per pupil and expressed as the change relative to the year before the election. All models include year fixed effects. We use a triangular kernel with MSE-optimal bandwidths from Calonico et al. (2020). We use standard errors clustered at the election level in the pooled panels and robust standard errors in the dynamic panels.

Figure A7: Results Under Alternative Approaches to Missing Platform Data

(a) Aggregate Effects on the Ideological Composition of School Boards



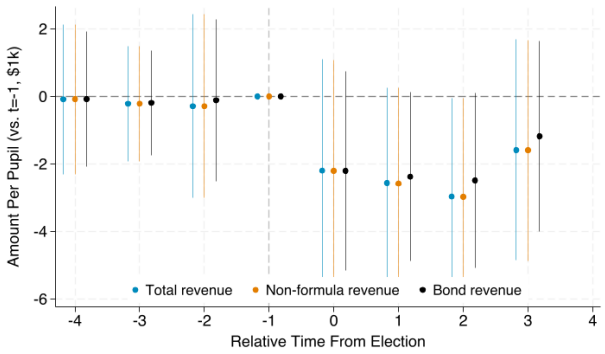
(b) Aggregate Effects on School Districts' Outcomes



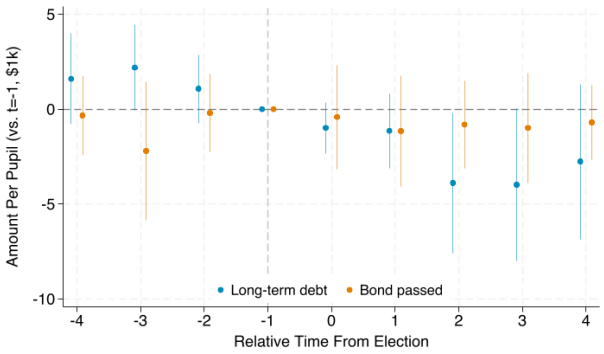
Notes: This figure reports aggregate effects under two alternative approaches to accounting for missing platform data when estimating ideology effects. Panel (a) replicates panel (a) of Figure 2. Panel (b) replicates panel (a) of Figure 4. In both panels, the left two bars are identical to those in the original figures. The third bar reports the aggregate effects of the *Ideologies* category when the sample is restricted to elections where platform data are observed for both marginal candidates. The fourth bar reports the aggregate effects of the *Ideologies* category when we impute a candidate's priorities using platform information from the candidate's other campaigns within a four-year window. Estimates are obtained by estimating equation (2) on a stacked dataset where each stack is constructed by considering a focal characteristic and its corresponding dependent variable, and where D_i and m_i indicate the election of a candidate with the relevant characteristic and their vote margin, respectively. We include six characteristic–outcome pairs for the *Identities (existing literature)* category, 23 for the *Identities (augmented)* category, and 14 for the *Ideologies* category (see Table B6 and B5 for the full list of characteristics-outcome pairs). In panel (a), the dependent variable is the change in share relative to the year before the election and we pool observations across years 0 to 3 since the election, corresponding to the elected candidate's four-year term. In panel (b), the dependent variable in each stack is standardized to have variance equal to one in each year and expressed as the change relative to the year before the election; we pool observations over the five years following an election. For ease of interpretation, we multiply each outcome by the hypothesized direction of the effects (for ideology-outcome pairs, expected signs are taken from Table B5; for identity-outcome pairs, expected signs are taken from Table B6). All models control for year and stack fixed effects. We use a triangular kernel with MSE-optimal bandwidths from Calonico et al. (2020). Standard errors are clustered at the election level.

Figure A8: Fiscally Conservative Candidates and District Finances

(a) Revenue in Facility-related Funds (CDE)



(b) Debt (CCD) and Bond Passage (Biasi et al. (2025))



Notes: The figure reports RD estimates in which the effect of electing a fiscally conservative candidate on fiscal outcomes is estimated separately for each year relative to the election. The dependent variables are total revenue, non-formula revenue, and bond revenue in facility-related funds from CDE in panel (a); and the amount of long-term debt by the end of fiscal year from CCD and bond passage from Biasi et al. (2025) in panel (b). All variables are measured per pupil and in thousands of 2019 U.S. dollars. All estimates are obtained using equation (1), where D_i and m_i indicate the election of a fiscally conservative candidate and the candidate's vote margin, respectively. The dependent variable in all panels is expressed as the change relative to the year before the election. We use a triangular kernel with MSE-optimal bandwidths from Calonico et al. (2020). We use standard errors clustered at the election level. The year-4 estimates in panel (a) are omitted due to a small number of observations. The estimates and their 95% confidence intervals are: total revenue = -1.629 [-5.492, 2.235], non-formula revenue = -1.630 [-5.493, 2.234], and bond revenue = -1.245 [-4.972, 2.482].

B Additional Tables

Table B1: Candidates, Elections, and Districts With and Without Platforms

	Without Platform (1)	With Platform (2)
Panel A: Candidate Characteristics		
Female	**0.42**	**0.47**
Hispanic	**0.26**	**0.16**
Teacher	**0.12**	**0.26**
Democrat	**0.45**	**0.56**
Panel B: Election Characteristics		
N Candidates/election	**4.81**	**4.69**
N Positions/election	**2.27**	**2.24**
Vote Margin (ΔVote Share, Least Popular Winner vs. Most Popular Loser)	7.95	8.06
Panel C: District Characteristics		
Enrollment	**10232**	**17645**
Share White	**0.34**	**0.37**
Share Hispanic	**0.49**	**0.39**
Share Black	**0.05**	**0.05**
Share Asian	**0.07**	**0.14**
Spending Per Pupil ($1k)	**9.11**	**9.89**
Total N Candidates	13959	8467

Notes: The table reports summary statistics for candidate, election, and district characteristics without a platform (column 1) and with a platform (column 2). For election- and district-level characteristics, an election or district is assigned to column 2 if at least one candidate has a platform. Panel A summarizes candidates' demographic characteristics; Panel B summarizes characteristics of the elections in which the candidates ran; and Panel C summarizes characteristics of the districts in which those elections took place. Values in bold indicate that the difference between candidates with and without a stated platform is statistically significant at the 5% level.

Table B2: Inter-Rater Reliability Across Human Graders and LLMs - Candidate Platforms

Priority	RA 1 - GPT	RA 2 - GPT	RA 1 - RA 2	RA 1 - Claude	RA 2 - Claude	GPT - Claude
Equity	0.77	0.62	0.60	0.74	0.67	0.74
Fiscal Conservatism	0.92	0.79	0.83	0.90	0.80	0.94
Agenda Bias	0.62	0.58	0.61	0.69	0.56	0.65
Community Engagement	0.87	0.66	0.65	0.84	0.66	0.89
Career and Technical Education	0.88	0.79	0.82	0.84	0.80	0.91
Dropout	0.69	0.43	0.53	0.39	0.24	0.56
Enrollment	0.85	0.75	0.82	0.72	0.66	0.88
Facility Improvement	0.63	0.40	0.45	0.69	0.46	0.81
Safety	0.96	0.81	0.81	0.92	0.85	0.92
Superintendent Accountability	0.37	0.37	1.00	0.31	0.31	0.64
Teacher Support	0.59	0.45	0.47	0.46	0.35	0.54
Test Scores	0.71	0.65	0.67	0.57	0.46	0.66
Overall	0.83	0.73	0.77	0.76	0.69	0.80

Notes: Pearson correlations between ratings assigned by two research assistants and two large language models (LLMs) for a random sample of 200 school board candidate platforms drawn from our full sample. GPT refers to OpenAI's GPT-4.0 model. Claude refers to Anthropic's Claude 3.5 Sonnet model.

Table B3: Priorities Identified Via Latent Dirichlet Allocation Algorithm

Top Words (1)	Topic (2)	Mapped Priorities (3)
community, class, health, safe, board, learning, increase, support, mental, environment	Supportive learning environment and academic rigor	Equity, Test Scores
safety, success, needs, ensure, increase, improve, academic, retention, safe, meet	Safety, student needs, and success outcomes	Safety, Dropout
communication, fiscal, teachers, sure, children, special, plan, accountability, transparency, community	Parent-teacher communication and fiscal transparency	Community Engagement, Fiscal Conservatism
fiscal, parents, responsibility, fiscal responsibility, community, education, making, decision, teacher, decision making	Financial responsibility and parental involvement	Fiscal Conservatism, Community Engagement
financial, college, career, budget, funding, programs, funds, resources, college career, continue	College and career readiness	CTE, Test Scores
teachers, staff, support, high, administrators, quality, retain, teachers staff, community, best	Teacher retention and professional development	Teacher Support
community, teachers, parents, educational, build, administration, stakeholders, needs, ensure, programs	Community collaboration and school success	Community Engagement
budget, programs, achievement, technology, gap, access, social, achievement gap, education, balanced	Equity in programs, tech access, and achievement gaps	Equity, Facility Improvement
education, academic, quality, achievement, improve, excellence, learning, quality education, continue, focus	Raising academic standards and student opportunity	Test Scores, Equity
accountability, best, test, scores, ensure, test scores, board, educational, music, high	Testing, evaluation, and accountability systems	Test Scores

Notes: Set of priorities identified via a Latent Dirichlet Allocation (LDA) topic modeling of school board candidate platforms. Column (1) shows the highest-probability words within each topic, as produced by the LDA model. Column (2) shows a list of researcher-labeled topics based on the content of the top associated terms. Column (3) contains subsets of the 12 priorities used in our analysis that the authors map to the topics identified from the LDA model. Before implementing the LDA model, we converted all text to lower case, removed standard stop-words, and performed lemmatization. The model uses 10 topics with a symmetric Dirichlet prior (alpha=auto) and 1,000 training iterations.

Table B4: Priorities Identified by a Large Language Model

Top Words (1)	Topic (2)	Mapped Priorities (3)
academic achievement, test scores, standards, rigor, excellence, core subjects, learning outcomes, proficiency, performance	Academic Achievement & Standards	Test Scores
teacher pay, attract, retain, quality teaching, professional development, staffing, support teachers, qualified, educator pipeline	Teacher Quality & Pay	Teacher Support
class size, instructional materials, resources, reduce ratio, classroom support, supplies, staffing levels	Class Size & Instructional Resources	Teacher Support, Facility Improvement
fiscal responsibility, transparency, budget, oversight, bond spending, financial planning, efficient use	Fiscal Accountability & Efficiency	Fiscal Conservatism
mental health, social emotional, well-being, counselors, SEL, whole child, behavior, trauma-informed	Social-Emotional & Mental Health Supports	Safety
college readiness, career pathways, vocational, CTE, dual enrollment, workforce skills, future preparation	College & Career Readiness	CTE, Dropout
early childhood, kindergarten readiness, reading by 3rd grade, literacy, foundational skills, preschool, early education	Early Childhood & Foundational Learning	N/A
safe schools, discipline, security, bullying, campus safety, violence prevention, emergency response	School Safety & Discipline	Safety
parental involvement, community input, communication, stakeholder voice, family engagement, board access, outreach	Parent & Community Engagement	Community Engagement
equity, underserved, marginalized, close gaps, inclusive, justice, opportunity for all, underrepresented	Equity in Access & Outcomes	Equity

Notes: Set of priorities identified by a Large Language Model (LLM, ChatGPT 4o, used on June 16, 2025) given the following prompt: *"I want you to imagine a mindset of an established economist who has expertise in economics of education. Based on the sample priorities I uploaded and frequent topics studied in prior economics of education literature, pick 10 topics you think important to study. After picking these 10 priorities, we will investigate the effect of electing a member with each priority on relevant educational outcomes. So the choice doesn't necessarily have to based on only the frequency of words, as you would do in LDA. Give me 10 topics, and associated keywords."* Columns (1) and (2) show the associated keywords within each topic, and list of topics as produced by the LLM. Column (3) contains subsets of the 12 priorities used in our analysis that authors find being mapped to topics identified by the LLM.

Table B5: Mapping Between Priorities and Outcomes

Priority	Outcomes (Expected Direction)
Equity	Low-Income & Hispanic test scores (↑)
Fiscal conservatism	Spending per pupil (↓); Bond amount passed per pupil (↓)
Agenda bias	N/A
Community Engagement	N/A
CTE	Revenues for CTE programs per pupil (↑)
Dropout	log(Dropout count) (↓)
Enrollment	log(Enrollment) (↑)
Facility Improvement	Capital spending per pupil (↑)
Safety	Share students suspended (↑)
Superintendent Accountability and Hiring	Superintendent turnover (↑)
Teacher support	Teacher exit rates (↓) Salaries of novice & experienced teachers (↑)
Test Scores	Achievement (↑)

Notes: Full list of priorities and associated outcomes, with the expected direction of effects. These characteristic-outcome pairs define the *Ideologies* category used in Figure 4, Figure A7, Table B14, and Table G2.

Table B6: Mapping Between Identities and Outcomes

Identity	Study	Priorities	Outcomes (Expected Direction)
Panel A: Prior Literature			
Hispanic	Kogan et al. (2021), Fischer (2023)	Equity, Fiscal Conservatism	Hispanic test scores (↑), Spending per pupil (↑), Bond passage per pupil (↑)
Teacher	Shi and Singleton (2023)	Teacher support	Teacher exit rates (↓), Salaries of novice (↑) & experienced teachers (↑)
Panel B: Augmented Based on Difference in Viewpoints			
Hispanic	-	CTE	Revenues for CTE programs per pupil (↑)
Hispanic	-	Dropout	log(Dropout count) (↓)
Hispanic	-	Safety	Share students suspended (↑)
Hispanic	-	Test Scores	Test scores (↑)
Female	-	Equity	Low-Income & Hispanic test scores (↑)
Female	-	Dropout	log(Dropout count) (↓)
Female	-	Safety	Share students suspended (↑)
Female	-	Test Scores	Test scores (↑)
Teacher	-	Fiscal Conservatism	Spending per pupil (↑); Bond passage per pupil (↑)
Teacher	-	CTE	Revenues for CTE programs per pupil (↑)
Teacher	-	Dropout	log(Dropout count) (↓)
Teacher	-	Facility Improvement	Capital spending per pupil (↓)
Democrat	-	Equity	Low-Income & Hispanic test scores (↑)
Democrat	-	Fiscal Conservatism	Spending per pupil (↑); Bond passage per pupil (↑)
Democrat	-	Dropout	log(Dropout count) (↓)
Democrat	-	Facility Improvement	Capital spending per pupil (↓)
Democrat	-	Teacher support	Teacher exit rates (↓); Salaries of novice & experienced teachers (↑)

Notes: Panel A reports the identity-outcome pairs tested in prior studies. These pairs define the *Identities (existing literature)* category used in Figure 4, Figure A7, Table B14, and Table G2. Panel B presents additional identity-outcome pairs motivated by differences in stated priorities across identity groups; a pair is added when the raw difference in candidate viewpoints across identity groups in Table 3 is statistically significant at the 5 percent level. These pairs define the *Identities (augmented)* category used in Figure 4, Figure A7, Table B14, and Table G2. Outcomes associated with each priority are mapped according to Table B5. The expected direction is based on the sign of the raw difference in Table 3.

Table B7: Examples of Meeting Minutes and Motion Topics

Example of Meeting Motions	Equity	Fiscal Conservatism	Agenda Bias	Community Engagement	CTE	Dropout	Enrollment	Facility Improv.	Safety	Sup Accountability/Hiring	Teacher Support	Test Scores
Barbara Patterson, Deputy Superintendent, Business & Operations, requested approval of the Retiree Benefits Actuarial Study. Superintendent Stock stated that Rocklin Unified School District is one of the few public entities that has nearly funded this liability and commended the Board for their foresight and fiscal discipline to address this issue. (2017, Rocklin Unified, Passed Unanimously)		✓									✓	
Approve tentative settlement agreement of Special Education matter - Case No. 2016070742, Case No. 2016070703, Case No. 2016070134, Case No. 2016070060, and Case No. 2016070266. (2016, Antioch Unified, Passed by 3 vs. 2)	✓	✓										
...in Closed Session, the Board discussed the expulsion of student No. 06-2019/2020. The student shall be expelled from the El Rancho Unified School District and shall have successful participation in an alternative education program... (2018, El Rancho, Passed Unanimously)									✓			

Notes: Illustrative excerpts from school board meeting minutes and the associated topic indicators produced by the coding procedure described in Section 3.4. Each row reports a motion excerpt, and checkmarks indicate the topics assigned to that motion. A motion can be assigned to more than one topic.

Table B8: Inter-Rater Reliability Across Human Graders and LLMs - Meeting Minutes

	RA1 - RA2	RA1 - RA3	RA2 - RA3	RA1 - LLM	RA2 - LLM	RA3 - LLM
Panel A: General Information						
Date	0.93	0.97	1.00	0.99	0.97	0.99
Cancelled	0.97	1.00	1.00	1.00	0.99	1.00
Regular	0.93	0.93	0.93	0.97	0.97	0.93
Runtime	0.95	1.00	0.95	0.99	0.99	0.97
Present	0.90	0.93	0.90	0.94	0.96	0.96
Absent	0.95	0.97	0.95	0.96	0.97	0.97
Comments	0.72	0.70	0.93	0.90	0.73	0.67
Document Quality	0.95	0.85	0.80	0.99	0.99	0.80
Dissent	0.97	0.60	0.55	1.00	0.97	0.57
Panel B: Motions						
Total Number Voted on	0.68	0.68	0.60	0.69	0.66	0.54
Description	0.95	0.93	0.88	0.99	0.90	0.97
Member Proposed	1.00	0.93	1.00	1.00	1.00	0.96
Member Seconded	1.00	0.97	1.00	0.99	1.00	0.97
Passed	1.00	0.95	0.97	1.00	0.99	0.97
Unanimous	0.93	0.95	0.95	1.00	0.96	0.96
Votes	0.93	0.95	0.95	1.00	0.94	0.94
Keywords	0.50	0.95	0.70	0.89	0.64	0.96
Panel C: Members						
Member	0.82	0.88	0.75	0.94	0.90	0.86
Present	1.00	1.00	1.00	1.00	1.00	1.00
Role	0.88	1.00	0.97	1.00	0.91	1.00
Keywords	0.60	0.88	0.62	0.86	0.54	0.94

Notes: Pearson correlations between ratings assigned by three research assistants and a large language model (LLM, Anthropic's Claude 3.5 Sonnet model) for a random sample of 200 school board meeting minutes from our full sample.

Table B9: Elected Candidates' Identity and School Districts' Financial and Student Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Spending Per Pupil		Revenue Per Pupil	log(Enrollment)	log(Dropout)	Test Scores			% Suspended
	Total	Capital	CTE			Low-Income	Hispanic	All	
Panel A: Hispanic									
Estimate	0.124	-0.125	-0.444	0.034	0.175	0.009	0.021	0.018	-0.005
	[-0.042, 0.235]	[-0.228, -0.068]	[-0.977, -0.065]	[0.029, 0.043]	[-0.040, 0.321]	[-0.009, 0.035]	[0.000, 0.046]	[0.000, 0.041]	[-0.019, 0.007]
Bandwidth	5.547	3.605	8.627	3.419	9.925	9.946	8.530	8.828	12.625
N	1717	1348	2135	1673	773	885	766	833	601
Mean Dep. Var.	9.422	0.992	7.545	8.728	4.382	-0.538	-0.517	-0.378	0.034
Panel B: Female									
Estimate	-0.206	-0.018	-0.312	0.010	0.253	0.029	-0.012	-0.005	-0.005
	[-0.344, -0.120]	[-0.092, 0.033]	[-0.520, -0.200]	[0.007, 0.012]	[0.132, 0.430]	[0.017, 0.048]	[-0.033, 0.003]	[-0.020, 0.006]	[-0.016, 0.008]
Bandwidth	3.307	4.638	3.378	4.294	7.457	7.149	7.292	10.558	9.058
N	2399	3005	2436	3705	1202	1259	1166	1581	880
Mean Dep. Var.	9.475	1.114	7.620	8.505	3.866	-0.490	-0.445	-0.165	0.032
Panel C: Teacher									
Estimate	0.268	0.116	-0.385	0.014	-0.047	-0.010	0.003	-0.003	0.027
	[0.186, 0.335]	[0.064, 0.171]	[-0.626, -0.238]	[0.011, 0.018]	[-0.158, 0.063]	[-0.026, 0.006]	[-0.016, 0.016]	[-0.012, 0.009]	[0.016, 0.043]
Bandwidth	5.512	7.577	5.096	1.849	8.787	10.300	6.349	12.520	5.831
N	2481	2830	2403	1497	1080	1078	798	1261	548
Mean Dep. Var.	9.563	1.009	7.462	8.892	4.227	-0.469	-0.432	-0.152	0.034
Panel D: Democrat									
Estimate	0.009	0.099	-0.160	0.012	0.008	-0.004	-0.012	-0.001	0.005
	[-0.104, 0.187]	[0.035, 0.201]	[-0.416, 0.198]	[0.008, 0.018]	[-0.163, 0.200]	[-0.021, 0.018]	[-0.034, 0.007]	[-0.020, 0.017]	[-0.008, 0.021]
Bandwidth	4.780	5.990	4.882	1.814	9.820	7.623	8.761	14.911	8.912
N	1387	1651	1404	914	778	669	649	996	476
Mean Dep. Var.	9.517	0.858	7.487	8.363	3.878	-0.495	-0.456	-0.189	0.033

Notes: Estimates of the parameters β in equation (1), where D_i and m_i indicate the election of a candidate with the identity characteristic indicated in the panel label. The dependent variable is total spending per pupil (column 1), spending per pupil on capital projects (column 2), revenues per pupil for CTE programs (column 3), log enrollment (column 4), log dropout (column 5), test scores for low-income students (column 6), test scores for Hispanic students (column 7), test scores for all students (column 8), and the share of suspended students (column 9). The dependent variables in columns 1-2 are reported in thousands of 2019 U.S. dollars. The dependent variable in column 3 is reported in 2019 U.S. dollars. All variables are measured as changes relative to the year before the election. We pool observations across the five years following each election. All models control for year fixed effects. We use a triangular kernel with MSE-optimal bandwidths from Calonico et al. (2020), and we cluster standard errors at the election level. 95% confidence intervals are shown in brackets.

Table B10: Elected Candidates' Identity and School Districts' Staffing Outcomes

	(1)	(2)	(3)	(4)
		Teachers		Superintendent
	Exit Rate	BA60 Salary	Entry Salary	Turnover
Panel A: Hispanic				
Estimate	0.037	0.039	0.014	0.018
	[-0.012, 0.106]	[0.019, 0.064]	[-0.004, 0.044]	[-0.004, 0.041]
Bandwidth	11.040	7.204	5.378	10.968
N	417	1266	1497	2209
Mean Dep. Var.	0.106	10.990	10.586	0.198
Panel B: Female				
Estimate	0.033	0.013	-0.049	0.000
	[0.009, 0.061]	[0.005, 0.025]	[-0.059, -0.046]	[-0.016, 0.018]
Bandwidth	12.752	5.563	2.680	9.486
N	781	2126	1573	3583
Mean Dep. Var.	0.105	11.000	10.568	0.196
Panel C: Teacher				
Estimate	-0.016	-0.013	0.013	-0.010
	[-0.024, -0.008]	[-0.018, -0.009]	[0.003, 0.024]	[-0.029, 0.010]
Bandwidth	9.070	3.079	8.008	10.599
N	505	1190	2544	2886
Mean Dep. Var.	0.094	10.999	10.593	0.190
Panel D: Democrat				
Estimate	-0.007	-0.017	0.004	-0.041
	[-0.037, 0.020]	[-0.026, -0.002]	[-0.010, 0.024]	[-0.064, -0.013]
Bandwidth	13.938	7.722	7.271	8.147
N	436	1205	1566	1822
Mean Dep. Var.	0.104	11.021	10.589	0.210

Notes: Estimates of the parameters β in equation (1), where D_i and m_i indicate the election of a candidate with the identity characteristic indicated in the panel label. The dependent variable is the share of teachers who leave the district (column 1), the log of the scheduled salary for a teacher with a BA and 60 semester units (column 2), the log of the scheduled teacher entry salary (column 3), and an indicator for superintendent turnover (column 4). *BA60 Salary* and *Entry Salary* are from J90 data and are calculated by fixing the composition of teachers across steps to the year before the election. Thus, coefficients capture the change in the wage schedule net of changes in teacher composition across steps. Both salary variables are expressed in 2019 dollars. All variables are measured as changes relative to the year before the election. We pool observations across the five years following each election. All models control for year fixed effects. We use a triangular kernel with MSE-optimal bandwidths from Calonico et al. (2020), and we cluster standard errors at the election level. 95% confidence intervals are shown in brackets.

Table B11: Effects of Equity Candidates on Intermediate Outcomes

	(1)	(2)	(3)	(4)
Panel A: School-level Staffing by Low-Income Share				
Outcome:	Number of Certified Staff			
Subgroup	High Low-Income Share	Low Low-Income Share		
Estimate	2.293	1.843		
CI	[0.796, 3.790]	[-5.098, 8.784]		
Bandwidth	6.817	15.765		
N	12,288	13,289		
Mean Dep. Var.	35.897	40.915		
Panel B: School-level Staffing by Hispanic Share				
Outcome:	Number of Certified Staff		Number of Classified Staff	
Subgroup	High Hispanic Share	Low Hispanic Share	High Hispanic Share	Low Hispanic Share
Estimate	2.996	1.414	5.126	1.738
CI	[1.504, 4.488]	[-5.788, 8.617]	[2.530, 7.722]	[-0.921, 4.397]
Bandwidth	6.174	17.693	8.301	11.522
N	8,595	18,369	12,375	15,516
Mean Dep. Var.	36.981	39.057	19.900	20.636
Panel C: District-wide Revenue for Specific Programs Per Pupil				
Outcome:	GATE	Free Lunch	Basic Ed.	CTE
Estimate	-0.392	3.292	23.682	-0.110
CI	[-0.515, -0.268]	[0.128, 6.457]	[15.235, 32.128]	[-0.353, 0.133]
Bandwidth	1.795	4.036	3.528	4.459
N	590	1,149	1,023	1,185
Mean Dep. Var.	2.583	249.120	103.695	5.908

Notes: Estimates of the parameter β in equation (1), where D_i and m_i indicate the election of an equity-focused candidate. Panels A and B report effects on school-level staffing totals, split by whether a school's low-income share is above or below the districtwide low-income share, or whether its Hispanic share is above or below the districtwide Hispanic share. Certified staff refer to teachers and other certified staff at the school. Classified staff refer to non-certified staff, including paraprofessional, clerical, and other classified staff. Revenue dependent variables are restricted to fiscal years through 2017, consistent with the coverage of the underlying finance data. In Panel C, the dependent variables are district-level revenues per pupil for Gifted and Talented Education (GATE), lunch/nutrition programs, state compensatory basic-education programs, and Career and Technical Education (CTE) programs, all measured in 2019 U.S. dollars. We pool observations over the five years following an election. Dependent variables are expressed as the change relative to the year before the election. All models control for year fixed effects. We use a triangular kernel with MSE-optimal bandwidths from Calonico et al. (2020), and we cluster standard errors at the election level. 95% confidence intervals are shown in brackets.

Table B12: Effects of Fiscally Conservative Candidates on Intermediate Outcomes

	(1)	(2)	(3)	(4)
Panel A: Consistent Evidence of Reduced Capital Spending Across CCD and CDE				
Source:	CCD		CDE	
Outcome:	Capital Outlay	Construction	Capital Outlay	Facility Spending
Estimate	-1.227	-1.205	-1.424	-1.421
CI	[-1.315, -1.140]	[-1.288, -1.122]	[-1.598, -1.250]	[-1.595, -1.247]
Bandwidth	2.126	2.131	2.093	2.105
N	924	924	871	876
Mean Dep. Var.	1.087	0.997	1.367	1.361
Panel B: Evidence on Limited Authority over Formula-driven Revenue (CDE)				
Outcome:	Formula-driven Revenue Per Pupil			
	Facility-related Funds	All-funds		
Estimate	0.002	0.150		
CI	[0.002, 0.002]	[0.052, 0.247]		
Bandwidth	2.192	7.458		
N	894	2,087		
Mean Dep. Var.	0.001	9.315		

Notes: Estimates of the parameter β in equation (1), where D_i and m_i indicate the election of a fiscally conservative candidate. The dependent variables are total capital spending and construction spending from CCD, and total capital and facility spending in facility-related funds from CDE in Panel A; and formula-driven revenue in facility-related funds and across all funds in Panel B. All dependent variables are per-pupil and measured in thousands of 2019 U.S. dollars. We pool observations over the five years following an election. Dependent variables are expressed as the change relative to the year before the election. All models control for year fixed effects. We use a triangular kernel with MSE-optimal bandwidths from Calonico et al. (2020), and we cluster standard errors at the election level. 95% confidence intervals are shown in brackets.

Table B13: Robustness Checks – Case Studies

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		RD Tuning Parameters					
	Main	Quadratic polynomial	Uniform kernel	N Neighbors 4	N Neighbors 5	Dep. var. in levels	With post-2019 spending
Panel A: Effect of Equity-Focused on Low-Income Test Scores							
Estimate	0.088	0.090	0.069	0.060	0.054	0.086	
	[0.062, 0.134]	[0.062, 0.130]	[0.046, 0.108]	[0.025, 0.117]	[0.015, 0.115]	[0.061, 0.132]	
Bandwidth	3.686	8.687	3.923	5.927	6.486	3.858	
N	400	626	411	536	551	407	
Mean Dep. Var.	-0.438	-0.435	-0.434	-0.419	-0.425	-0.436	
Panel B: Effect of Fiscally Conservative on Spending Per Pupil							
Estimate	-1.212	-1.194	-1.411	-1.159	-0.799	-1.069	-1.057
	[-1.455, -1.086]	[-1.424, -1.045]	[-1.652, -1.282]	[-1.436, -1.021]	[-1.365, -0.423]	[-1.310, -0.944]	[-1.367, -0.874]
Bandwidth	2.420	5.787	1.817	2.750	5.776	2.618	2.683
N	1018	1965	816	1119	1965	1071	1288
Mean Dep. Var.	9.622	9.434	9.477	9.593	9.434	9.672	10.829
Panel C: Effect of Hispanic on Hispanic Test Scores							
Estimate	0.021	0.024	0.030	0.020	0.020	0.016	
	[0.000, 0.046]	[-0.002, 0.047]	[0.006, 0.054]	[-0.011, 0.060]	[-0.012, 0.061]	[-0.006, 0.042]	
Bandwidth	8.530	14.214	6.317	9.173	9.251	8.900	
N	766	960	681	794	806	777	
Mean Dep. Var.	-0.517	-0.507	-0.518	-0.521	-0.513	-0.519	

Notes: Estimates of the parameters β in equation (1), where D_i and m_i indicate the election of a candidate with the focal characteristic and the dependent variable indicated in the panel label. Column 1 presents the main specification, with the dependent variable measured as changes relative to the year before the election and a triangular kernel with MSE-optimal bandwidths from Calonico et al. (2020). Columns 2–5 vary the default RD tuning parameters from Calonico et al. (2020): column 2 controls for a local quadratic polynomial rather than a linear function of the running variable; column 3 uses a uniform kernel; and columns 4–5 increase the number of neighbors used to calculate clustered standard errors from 3 to 4 and 5, respectively. Column 6 reports results when dependent variable is in levels rather than changes from the year before the election, including the level in the year before the election as a control. Column 7 extends the sample to include fiscal years after 2019, when the CCD starts to report funding information on charter schools separately from that of their main districts in California (section 2.2.3). We cluster standard errors at the election level. 95% confidence intervals are shown in brackets.

Table B14: Robustness Checks – Aggregated Effects

		RD Tuning Parameters				
	Main	Quadratic polynomial	Uniform kernel	N Neighbors 4	N Neighbors 5	Dep. var. in levels
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Effect of Identities (Existing Literature)						
Estimate	0.038	0.056	0.059	0.078	0.083	0.089
CI	[0.032, 0.044]	[0.040, 0.072]	[0.051, 0.066]	[0.069, 0.086]	[0.069, 0.097]	[0.041, 0.138]
Bandwidth	2.614	5.500	7.460	2.661	6.870	7.749
N	5258	8250	9896	5329	9595	10058
Panel B: Effect of Identities (Other)						
Estimate	0.007	0.007	-0.004	0.009	0.007	0.008
CI	[0.003, 0.011]	[0.003, 0.011]	[-0.010, 0.002]	[0.004, 0.013]	[0.003, 0.012]	[0.001, 0.015]
Bandwidth	8.222	6.708	4.891	3.420	6.872	8.758
N	37207	31392	28337	22605	34353	38230
Panel C: Effect of Ideologies						
Estimate	0.212	0.108	0.224	0.219	0.212	0.214
CI	[0.185, 0.239]	[0.088, 0.127]	[0.196, 0.252]	[0.187, 0.251]	[0.184, 0.239]	[0.179, 0.248]
Bandwidth	2.431	2.294	4.988	1.810	2.356	2.448
N	6948	6052	11694	5476	6769	6956

Notes: Mean RD estimates on district outcomes by characteristic group, obtained by estimating equation (2) on a stacked dataset where each stack is constructed by considering a focal characteristic and its corresponding outcome, and where D_i and m_i indicate the election of a candidate with the relevant characteristic and their vote margin, respectively. We include six characteristic–outcome pairs for the *Identities (existing literature)* category, 23 for the *Identities (augmented)* category, and 14 for the *Ideologies* category (see Table B6 and B5 for the full list of characteristics-outcome pairs). The dependent variable in each stack is standardized to have variance equal to one; we pool observations over the five years following an election. For ease of interpretation, we multiply each outcome by the hypothesized direction of the effects (for ideology-outcome pairs, expected signs are taken from Table B5; for identity-outcome pairs, expected signs are taken from Table B6). All models control for year and stack fixed effects. Column 1 presents the main specification, with the dependent variable measured as changes relative to the year before the election and a triangular kernel with MSE-optimal bandwidths from Calonico et al. (2020). Columns 2–5 vary the default RD tuning parameters from Calonico et al. (2020): column 2 controls for a local quadratic polynomial rather than a linear function of the running variable; column 3 uses a uniform kernel; and columns 4–5 increase the number of neighbors used to calculate clustered standard errors from 3 to 4 and 5, respectively. Column 6 reports results when dependent variable is in levels rather than changes from the year before the election, including the level in the year before the election as a control. We cluster standard errors at the election level. 95% confidence intervals are shown in brackets.

Table B15: Minutes Analysis: Member Votes

	Equity		Fiscal		Hispanic	
	Focal	All	Focal	All	Equity	Fiscal
Panel A: Yes votes, own						
Estimate	0.136	-5.824	-13.785	-75.616	1.774	-1.170
	[-1.644, 1.916]	[-24.493, 12.844]	[-18.525, -9.045]	[-98.143, -53.089]	[-0.534, 4.081]	[-4.813, 2.472]
Mean Dep. Var.	11.212	188.779	33.997	187.662	13.317	34.103
N	782	816	528	511	666	796
Panel B: No votes, own						
Estimate	0.245	0.615	-0.027	-0.818	0.468	0.219
	[0.058, 0.431]	[-0.785, 2.015]	[-0.294, 0.239]	[-2.165, 0.529]	[0.285, 0.652]	[-0.149, 0.588]
Mean Dep. Var.	0.469	4.824	0.727	4.696	0.283	0.769
N	751	962	968	940	722	672
Panel C: Yes votes, others						
Estimate	-2.036	17.806	-63.729	-241.360	15.472	0.597
	[-12.524, 8.453]	[-60.802, 96.413]	[-90.462, -36.996]	[-353.914, -128.807]	[4.607, 26.336]	[-21.753, 22.948]
Mean Dep. Var.	63.452	998.923	180.326	1042.316	71.519	185.082
N	790	1,031	620	741	692	683
Panel D: No votes, others						
Estimate	1.179	8.907	0.383	-2.564	1.285	1.573
	[0.358, 2.000]	[3.624, 14.189]	[-1.151, 1.917]	[-8.376, 3.249]	[0.306, 2.263]	[0.327, 2.819]
Mean Dep. Var.	2.342	24.071	4.101	24.297	2.822	4.960
N	870	798	792	986	635	700

Notes: RD estimates of the effects of electing an equity candidate (columns 1 and 2), a fiscally conservative candidate (columns 3 and 4), or a Hispanic candidate (columns 5 and 6) on votes cast by the elected member and by others. In Panel A, the dependent variable is the number of yes votes cast by the focal member; in Panel B, it is the number of no votes cast by the focal member; in Panel C, it is the number of yes votes cast by other members; and in Panel D, it is the number of no votes cast by other members. The sample is motions on each candidate's focal issue (e.g., equity for equity candidates; columns 1 and 3), on all issues (columns 2 and 4), on equity motions (column 5), and on budget motions (column 6). We pool observations across years 0 to 3 since the election, corresponding to the elected candidate's four-year term. Dependent variables are expressed as the change relative to the year before the election. All models control for year fixed effects. We use a triangular kernel with MSE-optimal bandwidths from Calonico et al. (2020), and we cluster standard errors at the election level. 95% confidence intervals are shown in brackets.

C Data Cleaning Procedure

C.1 School Board Elections

- We use data on school board elections in elementary, high, and unified school districts. Starting from 6,312 elections, we drop 393 county- or state-level board elections because they span multiple districts. In the next step, we exclude 88 uncontested elections and 49 elections that lack a declared winner because they resulted in a runoff. This leaves us with 5,782 elections.

C.2 Merging Election Records with L2 Voter Data

- To identify candidates' political orientation, we link election records with L2 Historical Voter records (available starting from the year 2000). These voter records contain individuals' full names, home addresses, ages, and political party affiliations. We employ a fuzzy matching algorithm based on names, constrained within district-year combinations.[A-1] Since California Education Code §35107 requires school board candidates to reside within their district, restricting matches to within-district improves our match accuracy.

- With this matching process, we uniquely identify 71.8% of candidates in our election records within the L2 voter registration database. An additional 3.4% of candidates match multiple L2 entries but with consistent political affiliations, yielding a total of 75.2% usable matches for our analyses of political affiliation.

C.3 District Level Education Outcomes

- For variables related to district finances, we restrict our analysis to the 2017–18 academic year and earlier to minimize inconsistencies arising from changes in how the CCD reports financial data. Starting in 2018–19, the CCD began reporting district-level outcomes for charter schools separately. As a result, the number of school districts in California jumps from 1,165 to 2,198 in the raw CCD data.

  - Prior to 2018–19, only funding for charter schools that was funneled through their geographic school districts is reflected in the districts' finance variables. Direct funding to charter schools' independent accounts in the county treasury is excluded. Due to this accounting practice, financial variables are not comparable across years even if we aggregate financial variables at the geographic school district level post-2018-19. Including post-2018–19 data would lead to a discontinuous increase in reported spending and revenue—on average, a 15% rise in real terms among geographic school districts where at least one charter school district separates—due to the inclusion of direct charter funding only after this reporting change.

- Although we also obtain enrollment data from the CCD, enrollment counts are more comparable across years. Before 2018–19, students enrolled in charter schools were included in the enrollment figures of their geographic districts. After 2018–19, we manually reintegrate charter school enrollment into geographic district totals to maintain comparability.

- For student outcome data (SEDA, CRDC), we use versions reported at the geographic district level, which include charter schools.

[A-1] For elections in odd-numbered years, we use L2 records from the preceding year.

- Teacher outcome data from CDE exclude charter schools. We use these data as reported, since local school districts have no staffing authority over charter schools.
  - See Shi and Singleton (2023) for additional documentation and discussion of these data sources.
- Superintendent data come from Stemper and The Broad Center (2025). This dataset updates Stemper (2022b) through the 2024-25 school year. The approach follows that described in Stemper (2022b).
- Dropout outcomes are constructed from public district-year dropout records, combining EDFacts files with California Department of Education DataQuest dropout files. Dropout specifications are restricted to districts offering grade 12. In the RD sample, 1.1% of observations have zero reported dropouts and are omitted when we take logs.

C.4 CDE Finance Data

We complement financial records from CCD by using CDE finance data. CDE finance data are organized using Standardized Account Code Structure (SACS), a standardized chart of accounts in which each line item is coded by fund–resource–project year–goal–function–object–school. We rely on four dimensions. Fund identifies the accounting entity (e.g., general operations, capital projects, debt service). Object classifies the nature of the entry—expenditures, revenues, and balance-sheet items (e.g., cash and fund balance). Function is mainly relevant for expenditures, classifying the activity supported (e.g., construction vs maintenance). Resource captures the source and restriction status of funds (unrestricted vs restricted; local/state/federal). This coding allows us to identify what the money was spent on and where it came from. To target mechanisms most relevant for the immediate post-election changes in capital spending observed in CCD, we limit the CDE sample to governmental funds that plausibly house facilities activity and debt service:

- Capital projects funds: 21, 25, 30, 35, 40, 49
- Debt service funds: 51, 52, 53, 56
- Deferred maintenance fund: 14
- General fund: 01, only when the item corresponds to maintenance-related resources: 6205 (Deferred Maintenance Apportionment), 8100 (RRRMF), and 8150 (Ongoing & Major Maintenance Account/RMA).

Within the restricted sample, we group line items by object and then aggregate to district-year totals to construct total capital spending, and further spending on construction (brick-and-mortar), and maintenance and operations. Grouping line items by resource helps us track the source of revenue — federal, state, local, or other and whether the revenue is based on formula or not. "Other" resource categories include bond-related financing and interfund transfers, where transfers move money across funds while contributions reallocate across resources within a fund.

Importantly, CDE records follow governmental accounting, meaning that long-term capital assets (e.g., buildings) and long-term debt stocks (e.g., GO bonds payable) are not recorded; instead, capital construction and bond issuance appear as flows in the year they occur, and the primary "stock" measures include only cash balance. For this reason, we use CCD to examine long-run capital and debt stock measures.

D Mapping Political Platform to Priorities: Full Question List

We use twelve questions to identify twelve priorities. We now define each priority and specify the question used to identify it. Each question is designed to have a common opening and closing, main question, and some examples to facilitate the LLM's rating.

Common opening: Here are the top three priorities claimed by a candidate running for a school district board member election. [INSERT QUESTION HERE]

Common closing: Rate the candidate based on the entire statement rather than rating each priority. Provide the rating first, then briefly explain how you decided on the rating. Now, here's the statement you need to rate:

1. *Equity/Equal Distribution of Resources*: Is equal distribution of resources for all students of various socioeconomic statuses one of the main priorities of the candidate? Rate the statement on a scale from 1 to 3 where a rating of 1 means "none of the priorities is relevant", 2 means "at least one of the priorities is somewhat relevant", and 3 means "at least one of the priorities is directly relevant". Rate the candidate based on the entire statement rather than rating each priority. Here are examples of candidates rated as 1 because none of their priorities is directly about equity, inclusion, or diversity: Ex 1) "Raise the district's academic ranking, Restore financial health, Increase the transparency and accountability of the district's decisions." Ex 2) "Increase student achievement through expanded in-class supports and afterschool programs., Eliminate district bureaucracy and implement spending transparency, Streamline parent & teacher resources and communication." Meanwhile, these are examples of candidates rated as 3 because one of the priorities is explicitly about equity, inclusion or diversity. Ex 1) "Priority to special needs students, Strong community involvement, Efficient uses of resources." Ex 2) "School safety, Fiscal Conservatism across the school district, Raising school performance in lower income communities" Ex 3) "Devoting time, and resources to equity begins in our schools. We must have open conversations with district personnel and parents regarding equity for all. Continuing to close the achievement gap for African Americans and low-income children., Establishing policies and protocols to make sure students and staff come back to a safe place during Covid-19 era., Fiscal solvency and transparency to make sure money is spent appropriately."

2. *Fiscal Conservatism*: Is the candidate committed to minimizing school district spending and opposed to increasing the educational budget if it requires raising the tax rate? Rate the candidate's commitment on a scale from 1 to 3, where 1 means 'not committed to minimizing spending and likely supports increasing the budget,' 2 means 'somewhat committed,' and 3 means 'strongly committed.' Rate as 0 if it is hard to decide because it is not the main priority. Rate the candidate based on the entire statement rather than rating each priority. Here is an example of candidates rated as 0. Ex) "Teach to the "whole child," not just "to the test"; Technology and Fine Arts; Restore community confidence in public schools." Meanwhile, this candidate is rated as 1. Ex) "Facilities. We need to build new facilities and renovate existing facilities. Most schools were built in the 1950s and 60s and need modernizing. There are more than 1 billion in identified needs and no money; There is a shortage of Science, Math, and Special Education teachers. We need to raise salaries to be competitive with Silicon Valley and the shortage of supply." Finally, this candidate is rated as 3. Ex) "Will oppose new taxes and bonds; ensure that local students receive the classes they need to graduate from El Camino College; support Wiseburn School District Unification."

3. *Agenda Bias (Curriculum / Free Speech / Ideology)*: Is ensuring that academic instruction is free from what the candidate perceives as external agendas or ideologies one of the main priorities? Rate the statement on a scale from 1 to 3, where 1 means "none of the priorities is relevant," 2 means "at least one of the priorities is somewhat relevant," and 3 means "at least one of the priorities is directly relevant."

4. *Community/Parental Engagement*: Is communicating with parents or the community and encouraging them to get involved with the issues in the school district one of the main priorities of the candidate? Rate the statement on a scale from 1 to 3 where rate 1 means "none of the priorities is relevant", 2 means "at least one of the priorities is somewhat relevant", and 3 means "at least one of the priorities is directly relevant". Rate the candidate based on the entire statement rather than rating each priority. Here are examples of candidates rated as 1 because none of their priorities is directly about the engagement of parents or the community. Ex 1) "Focus on student achievement and every child's growth through core programs, data analysis, differentiation, and targeted instruction., Recruiting and retaining high-quality teachers and staff who can achieve this goal, and supporting their professional development., Provide a safe environment well-equipped for learning." Ex 2) "Equity in opportunity for all students, Timely and safe reopening of schools, More sustainable and available technology." Meanwhile, these are examples of candidates rated as 3 because one of the priorities is explicitly about engagement of parents or community. Ex 1) "Student Achievement - Focusing not only on improving test scores but also ensuring a quality education that is reflective of our diverse community., Community Involvement - Preparing our students for future success through the involvement of parents and the community. Ensuring lines of communication between the district, the school board, and the community are consistently available., Labor Relations - Providing all district staff with fair wages and benefits. Empowering our labor partners with the necessary support to better our students' education." Ex 2) "Improve community engagement and communication, Invest and develop technology infrastructure, Ensure campus safety for all children."

5. *Career and Technical Education*: Is vocational or career technical education one of the main priorities of the candidate? Rate the statement on a scale from 1 to 3 where rate 1 means "none of the priorities is relevant", 2 means "at least one of the priorities is somewhat relevant", and 3 means "at least one of the priorities is directly relevant". Rate the candidate based on the entire statement rather than rating each priority. Here are examples of candidates rated as 1 because none of their priorities is directly about vocational or career technical education. Ex 1) "Fiscal Responsibility, Bring back the Open Door Policy, Student, teacher, and community advocacy." Ex 2) "Ensure a physically and emotionally safe learning environment for all students, Encourage and develop family, school, and community partnerships, Identify needs and manage resources effectively and efficiently". Meanwhile, these are examples of candidates rated as 3 because one of the priorities is explicitly about vocational or career technical education. Ex 1) "Increased enrollment through marketing, better communication, and more access to popular programs, Competitive teacher, administrator, and staff value proposition, including better salaries, College AND career readiness through access to rigorous coursework and technical education" Ex 2) "Expanding Career Technical Education, arts, and exploratory electives that meet the A-G requirements, Increase mental health support for students, Increasing SBUSD early literacy rates."

6. *Dropout*: Is reducing dropout rate one of the main priorities of the candidate? Rate the statement on a scale from 1 to 3 where rate 1 means "none of the priorities is relevant", 2 means

"at least one of the priorities is somewhat relevant", and 3 means "at least one of the priorities is directly relevant". Rate the candidate based on the entire statement rather than rating each priority. Here are examples of candidates rated as 1 because none of their priorities is directly about dropout/completion. Ex 1) "Raise the district's academic ranking, Restore financial health, Increase the transparency accountability of the district's decisions." Ex 2) "Equity in opportunity for all students, Timely and safe reopening of schools, More sustainable and available technology." Meanwhile, these are examples of candidates rated as 3 because one of the priorities is explicitly about dropout/completion. Ex 1) "Safe, secure, drug-free schools and reduced dropout rate', 'Wise use of taxpayer dollars', 'Reduction of overcrowding." Ex 2) "'Increase students test scores', 'Decrease student drop out rate', 'Create, Enforce & Implement policies that will bring about a sense of community in our school."

7. *Enrollment*: Is enrollment decline or enrollment growth one of the main priorities of the candidate? Rate the statement on a scale from 1 to 3 where rate 1 means "none of the priorities is relevant", 2 means "at least one of the priorities is somewhat relevant", and 3 means "at least one of the priorities is directly relevant". Rate the candidate based on the entire statement rather than rating each priority. Here are examples of candidates rated as 1 because none of their priorities is directly about enrollment: Ex 1) "Raise the district's academic ranking, Restore financial health, Increase the transparency accountability of the district's decisions." Ex 2) "Increase student achievement through expanded in-class supports and afterschool programs., Eliminate district bureaucracy and implement spending transparency, Streamline parent & teacher resources and communication." Meanwhile, these are examples of candidates rated as 3 because one of the priorities is explicitly about enrollment. Ex 1) "Increased enrollment through marketing, better communication, and more access to popular programs,Competitive teacher, administrator, and staff value proposition, including better salaries,College AND career readiness through access to rigorous coursework and technical education" Ex 2) "Successful construction of facilities to accommodate future enrollment growth - we must live up to past practice with on-time on-budget projects adhering to the Facilities Master Plan as presented to our community, Critical budget decisions as more students enter our schools - given that we are locally funded, increased enrollment means tighter budgets while still continuing to provide a world-class education for all of our students,Support for struggling students and mental health services - academic success for all our students must be the goal; ongoing mental health services play a critical role in this success".

8. *Facilities Improvement*: Is modernizing/upgrading/expanding physical facilities of schools one of the main priorities of the candidate? Rate the statement on a scale from 1 to 3 where rate 1 means "none of the priorities is relevant", 2 means "at least one of the priorities is somewhat relevant", and 3 means "at least one of the priorities is directly relevant". Rate the candidate based on the entire statement rather than rating each priority. Here are examples of candidates rated as 1 because none of their priorities is directly about improving facilities. Ex 1) "Raise the district's academic ranking, Restore financial health, Increase the transparency accountability of the district's decisions." Ex 2) "Equity in opportunity for all students, Timely and safe reopening of schools, More sustainable and available technology." Meanwhile, these are examples of candidates rated as 3 because one of the priorities is explicitly about improving facilities. Ex 1) "'Seeking additional sources of funding', 'Upgrading classroom facilities', 'Recruitment and retention of high quality staff" Ex 2) "Before leaving Grade 3, all children will be successful readers.', 'Ensure safe and clean school facilities that instill pride.', 'Promote a school culture in which learners and teachers can thrive."

9. *Safety*: Is the health and safety of students and staff members one of the main priorities of the

candidate? Rate the statement on a scale from 1 to 3 where rate 1 means "none of the priorities is relevant", 2 means "at least one of the priorities is somewhat relevant", and 3 means "at least one of the priorities is directly relevant". Rate the candidate based on the entire statement rather than rating each priority. Here are examples of candidates rated as 1 because none of their priorities is directly about students'/staff's health and safety. Ex 1) "Fiscal Responsibility, Bring back the Open Door Policy, Student, teacher, and community advocacy." Ex 2) "Increased enrollment through marketing, better communication, and more access to popular programs,Competitive teacher, administrator, and staff value proposition, including better salaries, College AND career readiness through access to rigorous coursework and technical education." Meanwhile, these are examples of candidates rated as 3 because one of the priorities is explicitly about students'/staff's health and safety. Ex 1) "Maintaining a safe environment for students and staff,Having students read by third grade,Continue to upgrade the schools and facilities" Ex 2) "As your school board member, I will promote skills and values that will help students prepare for success in their futures, whether through college or career.,As your school board member, I will promote sound educational policies. This means having safe, healthy classrooms., My goal for the future is to work with students, teachers, staff, and parents to help create a solid educational foundation that offers students choices."

10. *Superintendent Accountability and Hiring*: Is strengthening or addressing the superintendent's role (e.g., hiring, holding accountable, retaining, or collaborating with the superintendent) one of the main priorities of the candidate? Rate the statement on a scale from 1 to 4, where 1 means "none of the priorities are related to the superintendent," 2 means "at least one of the priorities focuses on hiring or holding a superintendent accountable," 3 means "at least one of the priorities focuses on retaining or collaborating with an existing superintendent," and 4 means "at least one of the priorities is somewhat related to the superintendent, but does not clearly fall under category 2 or 3." Rate the candidate based on the entire statement rather than rating each priority separately. Here are examples of candidates rated 1 (no superintendent focus): Ex 1) "Fiscal responsibility, bring back the open-door policy, student-teacher-community advocacy." Here are examples of candidates rated 2 (hiring or accountability focus):Ex 1) "Leadership – recruiting the best superintendent and principals for our schools; improve academic programs; build community trust." Here are examples of candidates rated 3 (retention or collaboration focus): Ex 2) "Continue working with our superintendent to build on recent progress. Here are examples of candidates rated 4 (somewhat related but unclear focus): Ex 1) "Improve district leadership, strengthen communication, and ensure effective decision-making."[A-2]

11. *Teacher Support and Union Relationships*: Considering the candidate's stance on policies endorsed/opposed by teachers' unions, teachers' involvement in educational policy-making, and teachers' collective bargaining rights, how would you rate their overall friendliness towards teachers' unions? Rate the statement on a scale from 1 to 3, where 1 means "not friendly," 2 means "somewhat friendly," and 3 means "very friendly."

12. *Test Scores*: Is raising overall standardized-test performance one of the candidate's main priorities? (Standardized tests include state proficiency exams, district benchmark assessments, SBAC/PARCC, SAT/ACT, etc.). Rate the statement on a scale from 1 to 3, where 1 means "none of the priorities is relevant", 2 means "at least one of the priorities is somewhat relevant" (mentions "academic achievement" or "ranking" but not tests explicitly), and 3 means

[A-2] For this question, we generated a binary variable equal to 1 if the candidate prioritized superintendent accountability or hiring a new superintendent (rating = 2), and 0 otherwise.

"at least one of the priorities is directly relevant" (explicit goal to raise test scores, proficiency %, or named exam results). Rate the candidate based on the entire statement rather than rating each priority separately. Here are examples of candidates rated 1 (no direct reference to standardized tests): Ex 1) "Fiscal responsibility, bring back the open-door policy, student-teacher-community advocacy." Ex 2) "Ensure a physically and emotionally safe learning environment for all students, encourage family-school partnerships, manage resources effectively." Meanwhile, these are examples of candidates rated 3 (explicitly about test performance): Ex 1) "New High School • Better Test Scores • A United PVUSD." Ex 2) "Parents–Teachers–Board of Education all working TOGETHER to continually improve test scores." Important exclusions: Do not upgrade to 3 for language about closing achievement gaps between groups. Graduation rate, college enrollment, CTE pathways, or generic "academic excellence" count only as 2 unless tied to specific standardized tests.

E Constructing Structured Data from School Board Meeting Minutes

To transform unstructured school board meeting minutes into structured data suitable for quantitative analysis, we use large language models (LLMs) to extract consistent information across thousands of heterogeneous PDF files. The processing script is implemented in Python and leverages Anthropic's Claude 3.5 Sonnet model through their official API.

To begin with, each PDF is passed through a preprocessing step that ensures consistent encoding and trims excessively long documents to a maximum of 75 pages. All PDFs are base64-encoded and then submitted in parallel to three LLM prompts, each targeting a distinct information domain:

- General Meeting Information: Extracts the meeting date, whether it was a regular or special meeting, whether it was cancelled, start and end times (to calculate runtime), attendance (present and absent members), and counts of public comments made by parents, teachers, and other community members.
- Motion-Level Data: Identifies each motion proposed during the meeting, including consent agenda items, and records for each: the proposer, the seconder, whether the motion passed, whether the vote was unanimous, individual votes (yes, no, or abstain), a one-line description, and a set of keywords indicating the motion's substantive focus (e.g., equity, budget spending). These keywords are drawn from the same controlled vocabulary used to classify candidate policy platforms in our main analysis, allowing us to link board actions to stated priorities.
- Member-Level Contributions: Extracts data on each board member's role (e.g., President, Clerk, Superintendent), presence or absence, and substantive contributions during the meeting. Based on comments from each member during a meeting (excluding proposing or seconding a motion since that is already considered in the motion-level data construction), we assign a set of keywords corresponding to policy domains such as academic achievement, curriculum bias, or parental engagement. These keywords—again drawn from our main platform classification scheme—capture the thematic content of each member's statements or interventions. This component also includes two metadata scores: a document quality score (0–10) and a dissent score (0–10), reflecting the clarity of the minutes and the level of disagreement among board members, respectively.

To improve the precision of extraction, all prompts were designed to strictly enforce minified JSON formatting with no free-text explanation. The model was instructed to only extract information explicitly present in the document, to use null for missing values, and to return text in

standardized formats (e.g., 24-hour times, ASCII characters only, and uniform handling of quotation marks and dashes). Each document was processed with temperature set to 0 to encourage deterministic behavior. API responses were cached when possible to reduce costs and latency.

After LLM responses are returned, we run a postprocessing script that parses the raw JSON responses, cleans and standardizes field values, and merges the outputs from the three prompts into a unified record per meeting.

F Tradeoffs and non-focal outcomes

This Appendix examines how our three leading candidate types affect important but non-focal outcomes. The goal is to document the presence or absence of tradeoffs between focal outcomes and these other outcomes. We focus on total spending, district average test scores, and test scores for low-income students and Hispanic students. Table F1 presents estimates of equation (1) for the outcome listed in the rows and the candidate type listed in the columns.

Focus first on equity. We estimate that electing an equity candidate raises average scores for high-income (i.e., non-FRPL) students by 0.022 SDs, and raises overall scores in the district by 0.013 SDs. 95% CIs span zero in both cases, though just barely. These results indicate that equity candidates do not improve low-income students' outcomes by lowering achievement elsewhere in the district. We also see that electing an equity candidate substantially *reduces* spending. As we show below, in contrast to our other results, the magnitude of the negative effect of equity candidates is sensitive to alternate specification choices, so we interpret this finding cautiously. Nevertheless, none of the specifications we estimate suggest that electing equity candidates raises spending, so the *absence* of spending gains does appear to be a robust finding.

Next, turn to fiscal conservatism. Electing a fiscally conservative candidate lowers average district scores by 0.007 SDs (95% CI=[-0.026, 0.008]). Point estimates are smaller for low-income students and somewhat larger for Hispanic students. Scaling by the spending reduction, we find that a $1,000 decline in per-pupil spending lowers scores by about 0.006 SDs, smaller than Jackson and Mackevicius (2024)'s estimate of 0.036 SDs.

Finally, consider Hispanic candidates. Hispanic candidates do not significantly change total spending, although they increase the amount of bonds passed per pupil (Appendix Figure A6). We see no effect of electing a Hispanic candidate on overall scores or low-income scores. We see modest increases in scores for Hispanic students (0.021 SDs, 95% CI=[0.000, 0.046]). This finding echoes Fischer (2023).

Table F2 considers the robustness of the equity and fiscal conservatism spending effects. Columns 1-6 vary the control set and tuning parameters in RD estimation, as in Appendix Table B13. Both results are robust to these changes. Columns 7 and 8 consider two additional checks. Column 7 considers only district-year observations where we also observe test scores—i.e., the data we use to estimate the focal-outcome specifications for equity candidates. Here we see the equity coefficient on spending fall by about half, to -0.768 (95% CI=[-1.406, -0.322]). We can still rule out null effects, but not relatively small effects. In contrast, the fiscal conservative effect becomes more negative (-1.868, 95% CI=[-2.388, -1.581]). Column 8 excludes observations where there are very large changes in per-pupil construction expenditures, defined here as more than $6,000. There are 30 such cases in the equity sample and 54 in the fiscal conservatism sample. Again, the equity effect falls towards zero while the fiscal conservative effect is unchanged. We conclude that evidence for a *reduction* in spending from electing an equity candidate is somewhat uncertain, but that we can rule out that equity candidates *raise* spending.

Table F1: Effects of Candidate Characteristics on Non-Focal Outcomes

	Equity (1)	Fiscal Conservatism (2)	Hispanic (3)
Panel A: Effect on Total Spending per Pupil			
Estimate	-1.210	-1.212	0.124
	[-1.585, -0.997]	[-1.455, -1.086]	[-0.042, 0.235]
N	691	1,018	1,717
Panel B: Effect on Test Scores			
Estimate	0.013	-0.007	0.018
	[-0.005, 0.039]	[-0.026, 0.008]	[-0.000, 0.041]
N	748	1,000	833
Panel C: Effect on Low-Income Test Scores			
Estimate	0.088	-0.002	0.009
	[0.062, 0.134]	[-0.026, 0.017]	[-0.009, 0.035]
N	400	940	889
Panel D: Effect on High-Income Test Scores			
Estimate	0.022	0.035	-0.010
	[-0.006, 0.062]	[0.015, 0.055]	[-0.057, 0.023]
N	642	1,051	697
Panel E: Effect on Hispanic Test Scores			
Estimate	0.016	-0.017	0.021
	[-0.004, 0.043]	[-0.040, -0.000]	[0.000, 0.046]
N	750	827	766
Panel F: Effect on White Test Scores			
Estimate	-0.024	-0.010	0.019
	[-0.057, 0.000]	[-0.042, 0.011]	[-0.007, 0.045]
N	725	830	703

Notes: Estimates of the parameters β in equation (1), where D_i and m_i indicate the election of an equity-focused (column 1), fiscally conservative (column 2), or Hispanic candidate (column 3). The dependent variable is total spending per pupil in thousands of 2019 U.S. dollars (Panel A), test scores (Panel B), test scores for low-income students (Panel C), test scores for high-income students (Panel D), test scores for Hispanic students (Panel E), and test scores for White students (Panel F). All variables are measured as changes relative to the year before the election. We pool observations across the five years following each election. All models control for year fixed effects. We use a triangular kernel with MSE-optimal bandwidths from Calonico et al. (2020), and we cluster standard errors at the election level. 95% confidence intervals are shown in brackets.

Table F2: Robustness: Effects of Board Members' Ideology on Spending Per Pupil

			RD Tuning Parameters				Sample Restriction	
	Main	Dep. var.	Quadratic	Uniform	N Neighbors		w/low-income	Excl. Construction
		in levels	polynomial	kernel	4	5	Test Scores	Outliers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Effect of Equity-Focused								
Estimate	-1.210	-1.227	-1.208	-1.094	-0.656	-1.169	-0.768	-0.511
	[-1.585, -0.997]	[-1.527, -1.014]	[-1.549, -1.024]	[-2.007, -0.587]	[-1.824, 0.226]	[-1.600, -0.939]	[-1.406, -0.322]	[-0.825, -0.327]
Bandwidth	2.137	7.321	1.976	3.756	6.580	2.602	7.843	4.245
N	691	1459	646	1088	1389	815	351	1156
Mean Dep. Var.	10.577	9.871	10.784	10.091	9.889	10.190	11.437	9.723
Panel B: Effect of Fiscal Conservative								
Estimate	-1.212	-1.194	-1.411	-1.159	-0.799	-1.069	-1.868	-0.969
	[-1.455, -1.086]	[-1.424, -1.045]	[-1.652, -1.282]	[-1.436, -1.021]	[-1.365, -0.423]	[-1.310, -0.944]	[-2.388, -1.581]	[-1.181, -0.851]
Bandwidth	2.420	5.787	1.817	2.750	5.776	2.618	3.793	2.963
N	1018	1965	816	1119	1965	1071	415	1177
Mean Dep. Var.	9.622	9.434	9.477	9.593	9.434	9.672	10.557	9.420

Notes: Estimates of the parameters β in equation (1), where D_i and m_i indicate the election of an equity-focused (Panel A) or fiscally conservative candidate (Panel B) on spending per pupil in thousands of 2019 U.S. dollars. Column 1 presents the main specification, with the dependent variable measured as changes relative to the year before the election and a triangular kernel with MSE-optimal bandwidths from Calonico et al. (2020). Column 2 reports results when dependent variable is in levels rather than changes from the year before the election, including the level in the year before the election as a control. Columns 3–6 vary the default RD tuning parameters from Calonico et al. (2020): column 3 controls for a local quadratic polynomial rather than a linear function of the running variable; column 4 uses a uniform kernel; and columns 5–6 increase the number of neighbors used to calculate clustered standard errors from 3 to 4 and 5, respectively. Column 7 restricts the sample to districts included in the low-income test score analysis, and column 8 excludes observations where the change in per-pupil construction spending exceeds $6,000. We cluster standard errors at the election level. 95% confidence intervals are shown in brackets.

G Priorities vs. identities

We extend the univariate RD model in equation (1) to allow for the simultaneous effects of multiple candidate attributes. We exploit close elections where candidates differ along one dimension, but agree on others. Suppose we are interested in attributes j and l. We estimate the following model across the full sample of elections:

$$\begin{aligned} y_{i,k} &= [\beta_0^j + \beta^j D_i^j + \gamma_0^j m_i^j + \gamma^j D_i^j m_i^j] \times S_i^j \mathbf{1}(|m_i^j| \leq BW_j) \\ &+ [\beta_0^l + \beta^l D_i^l + \gamma_0^l m_i^l + \gamma^l D_i^l m_i^l] \times S_i^l \mathbf{1}(|m_i^l| \leq BW_l) \\ &+ \theta_0 \times S_i^l \mathbf{1}(|m_i^l| \leq BW_l) \times S_i^j \mathbf{1}(|m_i^j| \leq BW_j) \\ &+ \theta_{t(i)+k} + \varepsilon_{i,k}, \end{aligned} \tag{G.1}$$

where D_i^n and m_i^n are defined relative to attribute $n \in \{j, l\}$, S_i^n equals one if the winner and the closest losing candidate differ with respect to characteristic n, and BW_n is the chosen bandwidth for characteristic n. Intuitively, this model estimates regression discontinuity specifications for each candidate attribute simultaneously, imposing the assumption that effects are separable across attributes. The parameters of interest are β^j and β^l, capturing the effects of electing a candidate with characteristic j, conditional on l, and vice versa. These parameters are estimated using the subsamples of elections where the winner and the closest losing candidate differ with respect to the characteristic, and where the vote margin falls within the mean-squared error optimal bandwidth of Calonico et al. (2020). The inclusion of the term $S_i^l \mathbf{1}(|m_i^l| \leq BW_l) \times S_i^j \mathbf{1}(|m_i^j| \leq BW_j)$ allows for different intercepts across elections based on whether the vote margin falls within the bandwidth for characteristic j, for characteristic l, for both, or for neither. This framework can be extended to accommodate more than two characteristics.

Table G1 presents estimates of equation (G.1) that consider four dimensions of identity (gender, ethnicity, occupation, and political affiliation) jointly with the priority relevant for our three leading outcomes: school spending and test scores for low-income and Hispanic students. Our estimates confirm that ideology plays a stronger role in determining district outcomes than identity across most domains. For example, even conditioning on the four dimensions of identity, electing a candidate who prioritizes fiscal conservatism reduces spending per pupil by $1,376 (column 1). Similarly, electing a candidate who prioritizes equity increases low-income students' test scores by 0.067 SDs (column 2) and Hispanic students' test scores by 0.038 SDs (column 3).

Meanwhile, the effects of identity are all close to zero and statistically insignificant. Notably, this includes effects of candidate-outcome pairs that were significant in the baseline regression discontinuity specification. For example, in our standard RD specification we found that electing a Hispanic candidate raises school spending by $124 per pupil. In specifications that account for candidates' fiscally conservative views, the effect of electing a Hispanic candidate becomes negative and statistically insignificant.

In Table G2, we expand the analyses in Table G1 and consider all attributes at once. Specifically, we estimate equation (G.1) for different sets of identity and ideology variables and for different outcomes. In each panel, the first row (*Examined in Prior Studies*) refers to specifications that consider the effects of identity traits examined in the literature on their associated outcomes (described in the top panel of Appendix Table B6), accounting for the ideologies associated with those outcomes (Appendix Table B5). For example, we consider the effect of electing a Hispanic candidate on spending, while accounting for fiscal conservatism. In the second row (*Augmented using Ideological Differences*), we identify identity-ideology pairs for which we observe systematic differences across identity groups (described in the bottom panel of Appendix Table B6) and consider the identity-outcome pairs associated with each ideology. For example, we see that Hispanic candi-

dates are more likely to prioritize Career and Technical Education (CTE). We therefore test whether a Hispanic candidate raises more revenue for CTE-related programs. In the third row (*All Identities and Ideologies*) we consider all identity and ideology traits. Panel A shows p-values of chi-squared tests of the joint significance of identity (column 1) and ideology variables (column 2). For reference, Panel B shows the same p-values when we estimate identities and ideologies in separate regressions.

Two main findings emerge from this table. The first is that identity effects tend to be jointly zero: none of the p-values shown in column 1 is below 0.05. This is true whether we test only identity-outcome hypotheses considered in prior studies (first row), the augmented set of hypotheses based on cross-group ideological differences (second row) or all identities considered at once. None of the tests has a p-value below 0.05. The second is that we consistently reject the null that ideology effects are jointly zero: p-values in column 2 are all smaller than 0.02.

Table G1: Identity vs. Ideology: Multivariate Regression-Discontinuity Estimates with Case-study Candidate Characteristics

	(1) Spending per pupil ($1,000)	(2) Low-income Test Scores	(3) Hispanic Test Scores
Fiscal Conservatism	-1.376		
	(0.450)		
Equity		0.0673	0.0382
		(0.029)	(0.025)
Hispanic	-0.0355	0.0248	0.0314
	(0.257)	(0.024)	(0.024)
Female	-0.159	0.0151	-0.00314
	(0.233)	(0.018)	(0.020)
Teacher	0.319	-0.0127	-0.0120
	(0.242)	(0.020)	(0.021)
Democrat	0.0638	-0.0179	-0.00422
	(0.395)	(0.021)	(0.022)
N	6381	2671	2632
R2	0.131	0.128	0.142

Notes: Estimates of the parameters β^j in equation (G.1), where D_i^j and m_i^j indicate the election of a candidate with characteristic j and their vote margin, respectively, and we consider all the characteristics included in each column at once. We pool observations across the five years following each election. The dependent variables are spending per pupil in thousands of 2019 U.S. dollars (column 1), low-income test scores (column 2), and Hispanic test scores (column 3). Dependent variables are expressed as the change relative to the year before the election. All models control for year fixed effects. We use a triangular kernel with MSE-optimal bandwidths from Calonico et al. (2020), and we cluster standard errors at the election level. Standard errors are shown in parentheses.

Table G2: Identity vs. Ideology: Multivariate Regression-Discontinuity Estimates with All Candidate Characteristics

	H_0: Identities jointly zero (p-value) (1)	**H_0: Ideologies jointly zero** (p-value) (2)
Panel A: Identity and Ideology Variables Tested Simultaneously		
Examined in Prior Studies	0.693	0.000
Augmented Using Ideological Differences	0.577	0.005
All Identities and Ideologies	0.459	0.019
Panel B: Identity and Ideology Variables Tested Separately		
Examined in Prior Studies	0.696	0.000
Augmented Using Ideological Differences	0.636	0.002
All Identities and Ideologies	0.293	0.004

Notes: P-values of chi-squared tests of joint significance of identity and ideology characteristics using the multi-outcome regression discontinuity (RD) framework in equation (G.1). Each row in each panel corresponds to different cells of identities, priorities, and outcomes. *Examined in Prior Studies* refers to specifications that consider the effects of identity traits examined in the literature on their associated outcomes (described in the top panel of Appendix Table B6), accounting for the ideologies associated with those outcomes (Appendix Table B5). *Augmented using Ideological Differences* uses identity-ideology pairs for which we observe systematic differences across identity groups (described in the bottom panel of Appendix Table B6) and consider the identity-outcome pairs associated with each ideology. *All Identities and Ideologies* refers to all identity and ideology traits. Panel A shows tests of the joint significance of identity (column 1) and ideology variables (column 2) estimated in the same regression. For reference, Panel B shows the same p-values when we estimate identities and ideologies in separate regressions. All models include year fixed effects. Standard errors are clustered at the election level.

H Randomized Ballot Order

We estimate the following first-stage regression following Shi and Singleton (2023), to assess the feasibility of using randomized ballot order as an instrument:

$$X_i = \alpha Z_i + W_i\gamma + \theta_{d(i)} + \eta_{t(i)} + \epsilon_i, \tag{H.1}$$

where Z_i is an indicator for whether a candidate with the focal characteristic appears first on the ballot in election i. The outcome X_i is either the total number of winners with that characteristic or the share of winners with that characteristic in the election.

The vector W_i includes controls for the number of open seats, the share of candidates with the focal characteristic, their interaction, indicators for cases where the share equals zero or one, and fixed effects for the number of candidates. We also include district fixed effects ($\theta_{d(i)}$) and election year fixed effects ($\eta_{t(i)}$).

We additionally consider a re-centered instrument following Borusyak and Hull (2023), defined as an indicator for whether a candidate with the focal characteristic appears first on the ballot minus

the share of candidates with the focal characteristic. We include the same set of fixed effects and covariates, except for the share of candidates with the focal characteristic.

Table H1 shows that randomized ballot order has limited predictive power for electoral outcomes related to candidates' ideological positions. The corresponding first-stage F-statistics range from about 2 to 4.[A-3] We take this as evidence that the randomized ballot order design cannot be used to test the ideology-related hypotheses that are central to this paper.

Consistent with Shi and Singleton (2023) and Fischer (2023), randomized ballot order does predict electoral outcomes by identity type. A Hispanic candidate being randomly assigned at the top of the ballot increases the total number of Hispanic members by 0.154, and the share of Hispanics among winners by 8.1 percentage points. A teacher being randomly placed at the top of the ballot raises the total number of teacher board members by 0.096.

[A-3]The results presented here include all candidates, assigning zeros to those without valid platforms. Our findings are very similar when the sample is restricted to candidates with valid platforms, with F-statistics for ideological characteristics ranging from 0.3 to 4.

Table H1: First-Stage Estimates from the Randomized Ballot-Order Instrument

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Equity		Fiscally Conservative		Hispanic		Teacher	
	Total	Share	Total	Share	Total	Share	Total	Share
Panel A: Baseline IV								
Equity Top of Ballot	0.049	0.036						
	(0.029)	(0.019)						
Fis. Conservative Top of Ballot			0.047	0.023				
			(0.025)	(0.016)				
Hispanic Top of Ballot					0.154	0.081		
					(0.026)	(0.018)		
Teacher Top of Ballot							0.096	0.052
							(0.021)	(0.014)
N	5611	5611	5611	5611	5593	5593	5611	5611
First-stage F-stat	2.843	3.770	3.465	2.150	36.387	21.556	20.770	13.736
Panel B: Recentered IV								
Equity Top of Ballot-Share	0.048	0.039						
	(0.029)	(0.019)						
Fis. Conservative Top of Ballot-Share			0.046	0.027				
			(0.025)	(0.016)				
Hispanic Top of Ballot-Share					0.154	0.069		
					(0.026)	(0.018)		
Teacher Top of Ballot-Share							0.096	0.049
							(0.021)	(0.015)
N	5611	5611	5611	5611	5593	5593	5611	5611
First-stage F-stat	2.807	4.079	3.407	2.675	36.366	14.704	20.749	10.542

Notes: This table reports first-stage estimates from the randomized ballot order strategy following Shi and Singleton (2023) applied to our case study characteristics. Estimates of the parameters α^j in equation (H.1), with the total number or share of winners with that characteristic as dependent variables. Panel A uses the indicator for appearing at the top of the ballot as the instrumental variable; Panel B re-centers this instrument by subtracting the share of candidates with the focal characteristic from the indicator. All specifications include fixed effects for district, election year, and the number of candidates. Specifications in Panel A further control for the share of candidates with the focal characteristic, the number of open seats, their interaction, and indicators for cases where the share equals zero or one. Specifications in Panel B include the same controls, excluding the share of candidates with the focal characteristic.