Budget Hawks on the Board: School Boards, Education Finance, and Education Production*

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Abstract

Funding for education in America is spread across multiple levels of government, but financial decision-making is handled by locally elected school boards. During elections, many candidates for board seats run on promises of reforming district finances. I identify such "budget hawks" by applying natural language processing methods to campaign statements. Using data from California, I leverage narrowly decided elections involving these budget hawks, combined with pre-election spending patterns, as a window into the education production function. In districts with high levels of pre-election spending, the election of a budget hawk leads to cuts to capital spending, large budget surpluses, and lower outstanding debt. However, I find no evidence of negative effects across numerous measures of student achievement. In districts with low levels of pre-election spending, budget hawks impose cuts to staffing but keep overall spending levels unchanged. Students in these districts exhibit lower rates of test-based proficiency in subsequent years. Taken together, these results suggest education production functions may exhibit large returns to instructional spending and staffing relative to capital spending. However, results with respect to house prices suggest that local homeowners may value capital spending, even in the absence of measurable effects on student achievement.

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Spending on K-12 education exceeds 700 billion dollars and comprises over 10 percent of total government spending in the US.[1] While real education spending per pupil has doubled since 1980, student performance on national and international tests has been relatively flat (Hanushek (2021)).[2] Increases in spending over time were driven largely by expansions in financial support from states (Snyder (1993)), but management of these funds was (and continues to be) handled entirely by local school boards—typically five or seven lay members of the community elected to administer local education. Altogether, school boards allocate hundreds of billions of dollars towards educational expenses each year, and survey evidence suggests that board members view budget setting as one of their top priorities.

In elections for school board seats, issues of financial (mis)management often take center stage. Many candidates emphasize their plans to "cut waste," "balance the budget," or "spend tax dollars wisely." The promise of these candidates, who I refer to as "budget hawks," is that school boards are misallocating funds: spending too much on resources that contribute little to education production—vanity capital projects or bloated administrator salaries, for example—and too little on more productive educational resources.

This paper evaluates whether budget hawks live up to this promise. In particular, I estimate the effect that school board ideological composition has on local education spending, and how these changes in spending affect student achievement. For identification, I leverage a particular source of dramatic changes in district financial choices: the narrow electoral victory of a budget hawk to the district school board. I use novel text data on candidates' self-reported priorities and natural language processing methods to quantify each candidate's financial focus. I combine this data with a dynamic regression discontinuity design, identifying the causal impact of the election of a budget hawk on the financial and academic trajectory of the district.

My setting is school boards in California. Observationally, the financial decisions that these boards make vary substantially across school districts. Figure 1 displays how financial allocations vary across districts in California over the 2000 to 2016 period. The horizontal axis orders schools based on their average total expenditure per pupil. The vertical axis indicates the share of this spending allocated to two major spending categories: instruction (which consist primarily of salaries and benefits of instructional staff) and capital expenses (which consist primarily of construction costs and equipment

[1] https://www.census.gov/library/stories/2021/05/united-states-spending-on-public-schools-in-2019-highest-since-2008.html, Table 3.1 https://apps.bea.gov/national/pdf/SNTables.pdf

[2] Note that this claim is distinct from the claim that the causal effect of school spending on achievement is positive or negative, as discussed in Jackson and Mackevicius (2021).

purchases).

Figure 1 illustrates two points. First, the allocation of the average dollar varies substantially between high- and low-spending districts. In high-spending districts, 50 cents of each dollar is spent on instruction, versus 60 cents in lower-spending districts. The opposite is true of capital spending; high-spending districts spend much more of their budgets on capital expenditures. Second, allocations vary *within* levels of spending. For example, Barstow Unified School District and Western Placer Unified School District both spend between $10,000 and $11,000 per pupil, have roughly equal student enrollments, and both serve all grade levels K-12. However, Barstow Unified allocates 55 percent of its spending towards instruction, versus 48 percent in Western Placer Unified. Differences in capital spending are even more stark: 5 percent versus 17 percent, respectively. These two districts are highlighted in Figure 1, as are the two largest districts in California: Los Angeles Unified and San Diego Unified.

Motivated by the patterns in Figure 1, I separate my analysis between high-spending districts—districts that typically spend a larger share of their budgets on capital expenditures—and low-spending districts—districts that typically spend a larger share of their budgets on operational expenditures. Once elected, budget hawks face very different constraints in high- versus low-spending districts. These constraints prove to be quite important empirically.

In high-spending districts, capital expenditures comprise a relatively larger share of spending, and budget hawks have levers to fulfill promises to "direct [...] tax dollars to the classroom" by cutting capital spending and concentrating spending on instruction and other operational expenditures. In these districts, I find that the election of a budget hawk leads to large and persistent cuts to total spending, and long-term shifts away from capital expenditures and towards instructional spending. Measures of financial health improve; districts run budget surpluses, and these surpluses generate reductions in outstanding debt. However, in contrast to dramatic effects on spending levels, I find little evidence that these changes affect measures of student achievement; effects on test scores are generally positive and insignificant.

In low-spending districts, where most spending is directed towards operations, attempts to reform district finances necessarily entail changes to operations—laying off staff or cutting salaries and benefits. In these districts, I find that the election of a budget hawk causes no significant changes in spending levels, but leads to short-term cuts to staffing, particularly for non-teaching staff. These districts experience reductions in test-based measures of proficiency. Effect sizes on test scores are roughly 0.05 standard deviations, approximately equal to the average effect of a $1,000 per pupil spending shock from Jackson and Mackevicius (2021).

I consult data from Zillow to test whether these changes in district operations and achievement are reflected in house prices. While effects are imprecise, my estimates rule out large increases in house prices in both high- and low-spending districts.

Altogether, these results suggests potentially large decreasing returns to scale in school finance: large cuts to large budgets seem to have little measurable effect on test scores, while small cuts to small budgets have large impacts.[3] More specifically, my results suggest potentially high returns to school staffing, a result consistent with Brunner et al. (2020), who find that school finance reforms generated the largest gains in student achievement in districts with strong teachers unions.

However, my results with respect to house prices suggest that local homeowners may value capital spending, even in the absence of measurable effects on student achievement, a result consistent with Cellini et al. (2010); so-called "wasteful spending" may reflect local preferences for school facilities that have little measurable effect on educational outcomes. Alternatively, it is possible that homeowners cannot distinguish between productive and non-productive school spending, and use changes in spending as a proxy for school quality. In this vein, Abdulkadiroğlu et al. (2020) find that parents in New York City appear to rank schools based on peer quality rather than causal effects, suggesting that parents may rely on student composition as a proxy for effectiveness.

This work builds on three distinct strains of literature. First, my work relates to a long literature on school spending and student outcomes dating back to Coleman (1968), which first raised the question as to whether school spending affects student outcomes. Across the US, district levels of school spending are highly correlated with other indicators of socioeconomic status, which poses an challenge for researchers. Recent empirical work has focused on identifying idiosyncratic shocks to district funding, for example due to the timing of school finance reforms (as in Jackson et al. (2015)) or the narrow passage of tax levies (as in Abott et al. (2020)). Jackson and Mackevicius (2021) provide a review of this literature; their meta-analysis suggests that "a $1000 increase in per-pupil public school spending (for four years) increases test scores by 0.044." These estimates provide a useful benchmark for the test score effects in this paper.

By their nature, these studies focus entirely on spending shocks that arise outside of normal budgeting processes. However, these shocks may be different in nature than the financial decisions school

[3] Jackson and Mackevicius (2021) find little evidence of decreasing returns to scale with respect to spending shocks. However, the spending cuts in my setting may be qualitatively different from positive spending shocks they study.

boards make annually. For example, to the degree that there is wasteful spending in school budgets—on vanity capital projects or unnecessary administrative staff, for example—the presence (or absence) of such resources may not be affected by narrowly-passed tax levies or increased state support generated by school finance reforms.

Second, my work relates to the literature on the effect of school board composition on educational inputs and student outcomes. While older contributions to this literature are primarily descriptive (Fraga et al. (1986), Meier and England (1984), Grissom (2010)), more recent contributions focus on quasi-experimental shocks to school board composition. These papers include Macartney and Singleton (2018), Shi and Singleton (2018), Fischer (2020), and Kogan et al. (2020). Broadly, this work suggests that small changes in school board membership can have large and persistent effects on school inputs. My paper employs many of the tools used commonly in this literature, but moves beyond coarse measures of candidate identities (e.g. Democrat party affiliation, experience as a teacher, and Hispanic/racial identity, as in the papers cited above), which serve as proxies for ideology or preferences in prior work. Instead, I use each candidate's self-reported priorities to quantify aspects of their ideology, with a particular focus on financial matters.

In this respect, my work relates to a long literature related to extracting political positions from text, which dates back to Laver et al. (2003). Over the past decades, text data has become more accessible and computing power has increased substantially, and this literature has expanded as a result; Grimmer and Stewart (2013) and Gentzkow et al. (2019) provide reviews of recent methodological and empirical contributions.

This paper proceeds as follows. Section 1 provides an overview of my setting: school board elections and school finance in California. Section 2 describes my data and methodology. Section 3 presents my results, and Section 4 concludes.

1 Setting

1.1 School Boards in California

School boards in California consist of three, five, or seven members who have a broad range of responsibilities with respect to the administration of education within the district. These responsibilities typically include hiring (and firing) the district superintendent, overseeing budgets, negotiating with teachers unions, implementing federal and state laws or court orders, and giving out contracts for jobs, supplies, and services (Hochschild (2005)). Survey evidence from California finds that school board members rank "allocating the district budget correctly" among their top priorities (Grissom (2010)).

Districts in California typically hold school board elections every two years, with a subset of the board's seats contested. Elections are either "at-large," where all candidates represent the entire district, or "by-ward," where each school board seat corresponds to a local section of the district. Elected school board members serve four year terms.

1.2 Education Finance in California

In California, school district revenues come from a combination of federal, state, and local sources. School boards have freedom to allocate most of these funds as they wish—as of 2004, 65 percent of district revenues were unrestricted (Loeb et al. (2006)). More recently, the adoption of the Local Control Funding Formula ("LCFF") in 2013 gave districts more freedom to allocate state-mandated funds as they wish.

While boards have substantial autonomy in how they allocate funds, boards have strict limitations on their ability to raise new funds. Proposition 13, an amendment to the California state Constitution enacted in 1978, places strict limits on local property taxes. As a result school boards in California have limited power to levy additional taxes to increase their budget, with two exceptions.

First, boards can propose issuing general obligation bonds for capital expenditures under Proposition 46. After a board proposes an issuance, bonds are approved by a local referendum, and, if approved, the board has decision-making power with respect to when to issue[4] and when and how to spend the funds thereafter. (Spending of general obligation bonds is subject to review by a citizens' oversight committee and annual, independent audits, to ensure that boards spend bond funds on approved capital expenses, rather than operational expenditures.[5]) Funds from local bond referenda constitute a large share of overall capital spending in California schools. Between 1987 and 2006, roughly 60 percent of school districts voted on at least one referendum (Cellini et al. (2010)), and Brunner and Rueben (2001) find that these funds contributed 32 percent of total school facility spending in California.[6]

Second, boards can impose local parcel taxes: property taxes levied on a per-unit-of-property-basis. However, parcel taxes are relatively uncommon and contribute little to overall differences in district resources. Loeb et al. (2006) and Bruno (2018) find that, on average, parcel taxes constitute 0.3

[4] Roughly 33 percent of voter-approved bond funds are unissued. "K-14 Voter Approved General Obligation Bonds: Authorized, But Unissued – 2021 Update," California Debt and Investment Advisory Commission, February 2021

[5] "The XYZs of California School District Debt Financing," Orrick, Herrington & Sutcliffe LLP, 2005.

[6] Other large sources of funding include state aid (30 percent) and developer fees (11 percent).

and 0.5 percent of total district revenues in 2004 and 2016, respectively.

2 Data and Methodology

My methodology aims to identify the causal effect of an additional budget hawk on a district's school board on subsequent district outcomess. To do so, I combine detailed text data on candidate priorities with annual data on district finances and achievement and use a dynamic regression discontinuity design for identification. In the subsections below, I describe my candidate priorities data, identification strategy, and data on district financial and academic outcomes.

2.1 Identifying Budget Hawks from Candidate Statements

I collect data on school board elections and candidates from SmartVoter, an election information website run by the League of Women Voters of California, accessible at smartvoter.org.[7] Since 1996, SmartVoter has collected self-reported information from candidates in local elections. SmartVoter publishes this information online on candidate-specific websites that typically include three sets of information: "Biographical Information," "Top Priorities if Elected," and "Key Endorsements." Appendix Figure B1 provides an example of one such page, corresponding to a candidate for school board in Pleasanton Unified School District in November 2003. After the election is decided, SmartVoter publishes results.

From SmartVoter, I collect a large set of candidate and election information from school board elections between 2001 and 2015. In total, my data includes over 13,000 candidate-year observations, over 8,500 candidate profiles, over 3,000 elections, and 600 unique districts.[8]

The text of each candidate's "top priorities" consists of three bullets, summarizing their priorities as a candidate. The text these bullets illustrates the issues most frequently discussed in school board races. Figure 2 shows the 100 most common unigrams (single words, such as "fiscal" or "teachers") and bigrams (two-word phrases, such as "fiscal responsibility" or "qualified teachers") in this data, as well as the set of terms that I identify as finance-related. Top unigrams often involve stakeholders (teachers, communities, and parents) or other general terms (improve, programs, academic). The most common bigrams are more specific, and reflect familiar issues in education policy: fiscal responsibility, class sizes, test scores, and the achievement gap. Many candidates focus on district finances: 7.8 percent of all bullets in my data use the word "fiscal," and the phrase "fiscal respons[ibility]" is the most common

[7] More recently, SmartVoter has rebranded their website under the name Voter's Edge, accessible at votersedge.org.

[8] A subset of these districts–470 total—have at least one candidate profile, and a smaller subset—310 districts—are ultimately included in my regression discontinuity sample.

two-word phrase in my data. Finance-related priorities often discuss a "balanced budget," and "fiscal account[ability]" and "fiscal manag[ement]."

I identify finance-oriented candidates based on their stated priorities using a Keyword-Assisted Topic Model ("KeyATM") algorithm. Introduced by Eshima et al. (2020), KeyATM is a topic modeling algorithm designed to extract topics from documents. In my application, I use KeyATM to identify candidates whose priorities suggest they are financially-oriented—candidates who I refer to as budget hawks.

As with Latent Dirichlet Allocation ("LDA"), the workhorse topic model introduced by Blei et al. (2003), KeyATM is an unsupervised machine learning algorithm that takes a set of documents as inputs. Topic models represent each document as a probability distribution over topics, and represent each topic as a probability distribution over terms. In an application related to newspaper articles, Blei et al. (2003) find that words such as "new" or "film" are likely to appear in articles from the "arts" topic, and words such as "million" or "tax" are likely to appear in articles related to budgets. The probabalistic structure of topic model algorithms allows for documents to concern more than one topic; for example, a newspaper article may be 50 percent "arts" topic and 50 percent "budgets" topic.

KeyATM is largely similar to LDA, but differs in one important respect: KeyATM allows the researcher to label topics by specifying keywords prior to model fitting. As Eshima et al. (2020) note, LDA models "often fail to measure specific concepts of substantive interest by inadvertently creating multiple topics with similar content and combining distinct themes into a single topic." KeyATM overcomes this issue by allowing the research to provide a small number of keywords prior to model fitting to guide the topic model.

In my application, I specify a set of terms associated with financially-oriented candidates. I fit a KeyATM model on each bullet appearing in each candidate's "priorities." As inputs, I include all common unigrams and bigrams appearing in text. I stem each word, so words with common stems, such as "financial" and "finance," are treated identically as "financ-." Appendix B describes my text processing steps in greater detail.

For simplicity and transparency, I define finance-related terms as the subset of the top 100 unigrams and bigrams that are clearly finance-related, highlighted in Figure 2. The KeyATM algorithm also requires the researcher to specify the total number of no-keyword topics: topics whose content is not specified by researcher-provided keywords. My main estimates use five no-keyword topics. I show in Appendix D that my main estimates are robust to different choices.

The KeyATM model produces, for each bullet, a probability p_{bcm} representing the likelihood that bullet b from candidate c concerns topic m. For illustration, Table 1 lists the bullets nearest to each tenth percentile (i.e. from $p_{bcm} = 0$, $p_{bcm} = 0.1$, $p_{bcm} = 0.2$, and so forth) of p_{bcm}. As shown in Table 1, finance-related bullets discuss "cut[ting] waste," "us[ing] construction dollars wisely" and "keeping the district's financial house in order." Non-finance-related bullets are varied, but generally discuss issues that are distinctly separate from finance: class size, teacher quality, and parental involvement, for example.

I aggregate bullet-level probabilities to the candidate-level using probability rules. Specifically, the probability that candidate c discusses topic m is given by the equation below.

$$\underbrace{p_{cm}}_{\text{prob. cand. } c \text{ discusses topic } m} = 1 - \underbrace{\prod_b (1 - p_{bcm})}_{\text{prob. cand. } c \textit{ doesn't} \text{ discuss topic } m}$$

Intuitively, candidates with high p_{cm} values are candidates who discuss topic m with high probability in at least one of their bullets.

I define budget hawks as candidates for whom $p_{cm} > 0.5$. In practice, the distribution of p_{cm} has most mass near 0 and 1, so the particular threshold makes little difference. Not all candidates in my data submit priorities to SmartVoter. I treat these candidates as non-budget hawks, effectively setting $p_{cm} = 0$ for these candidates.

Budget hawks run for school board seats more frequently in some districts than others. Table 2 explores how financial, staffing, and academic characteristics of districts covary with the number of budget hawks who run in district school board elections. For each district-year combination in SmartVoter data, I count the number of budget hawk candidates running. I link these candidate counts to district characteristics in the year prior to the election. I then run regressions in which the outcome is the number of budget hawk candidates running in district j in year t and the independent variables are district characteristics in year $t - 1$. I control for year fixed-effects and cluster standard errors at the district level.

Columns 1 through 4 of Table 2 demonstrate that budget hawks are more likely to run in districts with high levels of spending, high local revenues, lower staffing ratios, and higher levels of proficiency in math. Column 5 of Table 2 indicates that these finance, staffing, and achievement characteristics have independent predictive power even in a "horse race" regression including all four characteristics. Table 2 highlights the selection issues at play when identifying the effects of school school board composition on district outcomes. Comparisons between districts with and without school board members with a financial focus may reflect differences in the candidate pools of these districts, rather than the causal effects of these candidates on district outcomes. I use a dynamic regression discontinuity design to identify these causal effects.

2.2 Dynamic Regression Discontinuity

I estimate the effects of the election of a budget hawk to a district's school board using a dynamic regression discontinuity design as in Cellini et al. (2010). Similar to Cellini et al. (2010), my setting is dynamic in nature—districts are observed on a yearly basis—and treatment can occur multiple times in the same district—a district may elect a budget hawk multiple times over my analysis period. However, my setting differs from the setting in Cellini et al. (2010) in two important respects.

First, among "at-large" districts, multiple candidate typically run for multiple seats during each election. For example, in the November 8, 2005 election in Acton-Agua Dulce Unified School District, 10 candidates ran for three seats on the school board. Voters could vote for as many as three candidates.[9] This election format is differs from the design in canonical election regression discontinuity designs (e.g. Lee (2008)), which features two candidates (or parties) and one seat. For these elections, I follow Macartney and Singleton (2018) in constructing my running variable. I identify the identity (hawk or non-hawk) of the least popular election winner and the most popular opposite-orientation loser in each contest. For this pair, I define the running variable as the margin of victory (or loss) for the budget hawk.

Second, among districts that elect school board members "by-ward," I often observe multiple electoral outcomes per district per year. For example, in the November 7, 2006 election in Capistrano Unified School District, three wards held separate elections for three seats on the school board.[10]. Including all of these races separately in my analysis will assign more weight to those districts that hold by-ward races. I mitigate this concern by selecting, for each district-election year pair, the closest election between a budget hawk and a non-budget hawk.

I index districts by j, calendar years by t, and years since each election by τ. I denote the running variable for district j in election year t as v_{jt}. With v_{jt} as constructed above, I estimate dynamic regression discontinuity models. For each district-year pair, I gather data from three years before the election

[9] The corresponding SmartVoter webpage for this election can be found at smartvoter.org/2005/11/08/ca/la/race/037/.

[10] The corresponding SmartVoter webpage for these elections can be found at smartvoter.org/2006/11/07/ca/or/school.html

year and seven years years after. (For elections that occur later in calendar time, I don't observe the full seven year post-election period.)

I estimate the effect of a budget hawk victory τ years since election on district-level outcomes $y_{j,t+\tau}$. My estimating equation takes the form below.

$$y_{j,t+\tau} = b_{jt}\theta_\tau + P(v_{jt}, \gamma_\tau) + \alpha_\tau + \kappa_t + \lambda_{jt} + e_{jt\tau} \tag{1}$$

b_{jt} is a binary variable equal to 1 for districts-election year pairs in which a budget hawk won. θ_τ is my coefficient of interest, reflecting the effect of a budget hawk victory τ years since the election. I constrain θ_τ to be equal to zero for the year prior to the election $\tau = -1$, meaning θ_τ should be interpreted as the effect of b_{jt} relative to $\tau = -1$. α_τ, κ_t, and λ_{jt} represent fixed effects for years since election, calendar years, and district-election year pairs. $P(v_{jt}, \gamma_\tau)$ is a τ-specific function of the running variable v_{jt}. I cluster standard errors at the district level to account for repeated district-year observations in my data.

My design rests on the identifying assumption that potential outcomes—each district's expected value of y with and without treatment—are continuous at the treatment cutoff: $v_{jt} = 0$. Intuitively, among sufficiently close elections, election results should be unrelated to observed and unobserved district characteristics. While this assumption is not directly testable, my design allows me to test whether districts on either side of the threshold are similar with respect to pre-election district characteristics. Later, I provide evidence that this is the case; in the years before an election, districts on either side of the threshold have similar characteristics.

In their setting—school bond elections—Cellini et al. (2010) distinguish between intention-to-treat ("ITT") effects and treatment-on-the-treated ("TOT") effects, which arise due to relationships between election outcomes and the likelihood of future elections occurring. I focus on ITT effects identified in Equation 1, noting that outcomes in years long after the election reflect both the effect of a school board member's initial four year term as well as any subsequent terms thereafter.

Motivated by the patterns in Figure 1, I separate my results between districts with high and low pre-election levels of spending. To do so, I calculate the average total per-pupil spending in the three years prior to the election, y^{pre}_{jt}. Mathematically,

$$y^{pre}_{jt} = \frac{y_{j,t-3} + y_{j,t-2} + y_{j,t-1}}{3} \tag{2}$$

where y_{jt} represents total (inflation-adjusted) per-pupil spending in district j in year t. I define high-spending districts-election year pairs as those for which y_{jt}^{pre} is above the median value in my regression discontinuity data.

My main results estimate Equation 1 via local linear regression (*i.e.* I set $P(v_{jt}, \gamma_\tau)$ to be linear with different slopes above and below the threshold $v_{jt} = 0$) separately for high- and low-spending districts. I use a triangular kernel and use a bandwidth of 7.6 percentage points, the optimal bandwidth based on Calonico et al. (2020)[11].

Equation 1 is dynamic in nature, making inference about average effects difficult. For simplicity, I report both the τ-specific θ_τ terms as well as the results of a t-test of the null hypothesis below, which tests whether the average effect between years l and m is equal to zero.

$$H_0: \quad \left[\frac{1}{m-l+1}\sum_{\tau=l}^{\tau=m}\theta_\tau\right] = 0 \tag{3}$$

I test the null hypothesis above for the (l, m) combinations $(0, 3)$ and $(4, 7)$, which correspond to the years of the first term and potential second term of each elected school board member. I additionally test the full series, (l, m) equal to $(0, 7)$, which tests whether average effects in the eight post-election years are non-zero.

2.3 School Districts Data

I compile district-by-year data from a number of public sources. Below, I provide a brief overview, and Appendix A summarizes these data sources and the processing steps in more detail.

I measure school inputs—school spending and school staffing—using data reported to the National Center for Education Statistics ("NCES"). In particular, school finance data comes from the School District Finance Survey (Form F33) survey. This data captures spending, revenues, and debt per pupil. I convert all dollar-denominated figures to 2019 dollars based on Consumer Price Index. School staffing data comes from the Local Education Agency Universe Survey. I calculate staffing ratios as the number of staff per 1,000 pupils.

District-level student achievement data comes from the California Department of Education. Over this period, California had two separate testing regimes: Standardized Testing and Reporting ("STAR"),

[11]There is not an accepted method for optimal bandwidth selection in dynamic regression discontinuity settings. I select my bandwidth by running the procedure described in Calonico et al. (2020) using the difference in log total spending per pupil between years $\tau = -1$ and $\tau = 3$. I have two analysis samples, representing high- and low-spending districts, so I estimate the optimal bandwidth separately for both samples, and estimate my main results using the average optimal bandwidth across these two samples. Later, I demonstrate that my results are not sensitive to bandwidth choice.

for which I have data from 2000 to 2013, and California Assessment of Student Performance and Progress ("CAASPP"), for which I have data from 2015 to 2017. I combine results from both regimes and calculate, for each district in each year, the share of students who score at proficiency in math and English language arts ("ELA").[12] I additionally calculate the share of ninth graders who graduate on time, as well as the share of ninth graders who graduate on time and completed all courses required for entry into the University of California and/or California State University. The California Department of Education additionally reports the share of twelfth graders who take the SAT.

I construct district-level house prices using data from Zillow's house price index data. Zillow constructs this index by estimating the sale price of each house in their national database, which they refer to as a "Zestimate." Zillow aggregates these estimates to the zip code level by taking the value-weighted average Zestimate in the area, excluding houses that "undergo significant physical changes."[13] I calculate mean house prices by school district by weighting zip code-level estimates based on a combination of land area and zip code population in 2010, detailed in Appendix A.

Finally, I restrict my sample to districts that have nonzero enrollment, expenditures, and staffing data over the range 1998 to 2017, and have test score data for each year between 2000 to 2017, inclusive.[14]

Table 3 shows summary statistics for districts in my data as of 2000, prior to any election in my data. The first column of Table 3 shows characteristics of all districts in California. The second column of Table 3 displays characteristics of districts with any SmartVoter profiles between 2001 and 2015 and the third column displays characteristics of the subset of districts in my regression discontinuity sample: districts that have at least one close election involving a budget hawk.

The schools in my regression discontinuity sample, shown in the third column, served over four million students and spent over 50 billion dollars on education in 2000. On a per-pupil basis, these districts tend to spend less and employ fewer staff than the typical district in California. However, these districts exhibit slightly higher levels of academic achievement.

3 Results

In the section below, I first provide evidence on the validity of my regression discontinuity design. I then provide three sets of results, which detail how budget hawks affect school inputs, student

[12] I restrict my analysis to grades in which these tests were widely administered: grades 3 through 8 and 11.

[13] Zillow provides more detail on their calculations here: https://www.zillow.com/research/zhvi-methodology-2019-deep-26226/.

[14] Some districts have small gaps in some data series. I describe my process for interpolating these gaps in Appendix A.

achievement, and house prices. I conclude with a brief discussion about robustness.

3.1 Validity of the Regression Discontinuity Design

Prior to discussing my main results, I present brief evidence on the validity of my identifying assumption: that among sufficiently close elections, election results should be unrelated to observed and unobserved district characteristics.

First, my running variable exhibits little evidence of manipulation. If close elections involving financially-oriented candidates are as good as random, we should expect that the density function of the running variable is continuous. Figure 3 displays the density of my running variable, showing that the data does not reject this prediction. The top panel of Figure 3 shows, for all elections, the number of elections in 1 percentage point bins across the distribution of my running variable. I also display the p-values associated with two common tests for manipulation: Cattaneo et al. (2020) and McCrary (2008). Both p-values are well above typical thresholds for statistical significance. The bottom panels of Figure 3 repeats this analysis separately for high- and low-spending districts, with similar results.

Second, I test for differences in levels and trends of main outcomes prior to the election. If close elections involving financially-oriented candidates are as good as random, pre-election characteristics (or trends of characteristics) should not systematically vary along the threshold for victory. To test this prediction, I perform local linear regressions, setting the outcome as either (a) the level of the outcome in the year immediately before the election ($y_{jt,-1}$) or (b) the difference between the outcome years $\tau = -1$ and $\tau = -2$ ($y_{jt,-1} - y_{jt,-2}$). Similar to my main estimates, I use a triangular kernel, a bandwidth of 7.6 percentage points, and include year fixed-effects as controls.

Table 4 shows the results of these tests, for all districts and separately for high- and low-spending districts. Table 4 indicates that, across 14 outcomes and 3 samples, a small number of outcomes exhibit differences in levels that are significant at the 5 percent level. However, differences in levels, which form the basis for my main estimates, are generally insignificant and small.

3.2 Effect of Elections on School Spending and School Staffing

Figure 4 summarizes the results with respect to log per-pupil expenditures. Panel A of Figure 4 displays a pooled regression discontinuity plot, where the horizontal axis represents the budget hawk margin of victory v_{jt} and the vertical axis displays the change in log total per-pupil spending between the year prior to election and the eight years thereafter. Panel B of Figure 4 displays the corresponding coefficients from equation 1.

In high-spending districts, the election of a budget hawk leads to a large and immediate reduction in total spending, on the magnitude of roughly 10 percent. Perhaps more surprisingly, these cuts are persistent, and remain intact seven years after the initial election. Oppositely, expenditures in low-spending districts do not appear to change; effects on spending levels are generally positive and insignificant.

Figure 5 displays results with respect to instructional spending shares: the share of total spending allocated to instruction. In high-spending districts, budget hawks increase the share of total spending on instruction by roughly 5 percentage points. Cuts to expenditures in these districts were coming from capital expenses, not instruction. Oppositely, in low-spending districts, instruction spending shares fall by roughly 2 percentage points, though these estimates are imprecise. Appendix Table C1 tabulates these results.

While budgets in low-spending districts exhibit much less financial sensitivity to the election of a budget hawk, data on staffing reveals changes that these candidates make. Appendix Table C2 presents estimates of effects on staffing ratios over time, represented in terms of staff per 1,000 pupils. High-spending districts show no evidence of changes in staffing ratios. Low-spending districts exhibit much larger effects. In these districts, election of a budget hawk is associated with a large, immediate reduction in staff. The magnitude of this drop is large: 5 staff per 1,000 pupils, or roughly 6 percent of the mean in Table 3. Separating results between teaching and non-teaching staff shows that the reduction is driven entirely by non-teaching staff. This is unsurprising, given that teaching staff are covered by unions, and union contracts offer stronger protections against layoffs. In subsequent years, coefficient estimates attenuate but remain negative and economically significant.

Table 5 summarizes effects across all school inputs, showing results of a t-test on average effects across years from Equation 3. In addition, Table 5 shows effects on two measures of district financial health: surplus (annual revenues less annual expenses) per pupil and long-term debt outstanding per pupil. Consistent with the changes described above, high-spending districts run budget surpluses, which lead to reductions in outstanding debt. (Estimates with respect to outstanding debt levels are imprecise but economically significant.) Low-spending districts exhibit no changes in these measures.

Altogether, these results reflect the constraints that school board members may face in pursuit of reforming school finances in high- and low-spending districts. High-spending districts have larger capital spending budgets, providing budget hawks a viable line item to cut from district budgets. Budget hawks in low-spending districts have no such option, limiting their capacity to pursue cost-savings without reducing operational resources. In these districts, non-teaching staff (e.g. support staff, counselors, librarians, etc.) are one such resource. These differential responses provide a useful window into the district-level determinants of student achievement; I analyze these effects below.

3.3 Effect of Elections on Student Achievement

I first analyze overall effects on test scores in math and ELA. Figure 6 displays the dynamic effects of a budget hawk victory on proficiency rates. In high-spending districts, effects on proficiency rates are positive and insignificant for both subjects. While these estimates are imprecise, they do rule out large decreases in test scores. Low-spending districts exhibit the opposite; scores in math and ELA are steady until three years post-election, then fall by 2 to 3 percentage points.

Table 6 provides the corresponding estimates, showing results of a t-test on average effects across years according to Equation 3. In addition, Table 6 shows test proficiency estimates separated by grade span (elementary, middle, and high school grades) as well as effects on other high school outcomes. Results with respect to grade spans reveal that the decline in proficiency is concentrated in earlier grades. These results point to limited effects in later grades, though I note that negative effects on high school outcomes—graduation rates and SAT-taking rates—are large in magnitude but imprecise.

In gauging the magnitude of effects on test scores, meta-analytical estimates from Jackson and Mackevicius (2021) provide a useful point of reference. My estimated district-level effect on test scores in ELA is 2.8 percentage points. I divide this estimate by $\sqrt{p \times (1-p)}$ to convert district-level proficiency rates to student-level standard deviations, where p equals the total proficiency rate. Setting p equal to the mean ELA proficiency rate in the year prior to the elections in my data (52.6 percent) yields a student-level estimate of 0.056 standard deviations. For comparison, Jackson and Mackevicius (2021) combine estimates from 24 "credibly causal" studies, finding that a \$1,000 increase in school spending is associated with a 0.044 standard deviation increase in test scores. My estimate falls comfortably within Jackson and Mackevicius (2021)'s 90% confidence interval of [-0.006, 0.093].

3.4 Effect of Elections on House Prices

A long literature uses house prices to infer how parents value school quality (e.g. Black (1999), Cellini et al. (2010)). In my setting, this exercise is particularly useful, given the numerous changes that school board members bring about in school districts. I analyze effects on house price indices from Zillow to assess how local homeowners value these changes in aggregate.

Table 7 shows results with respect to house prices. The results are imprecise and fail to reach statistical significance in any post-election year, but serve as a useful guide in bounding average effect sizes. Notably, in high-spending districts, where overall spending falls, spending shifts in favor of instruction, and test scores exhibit no significant difference, house prices fall by roughly 5 percent. Confidence intervals rule out increases of more than 2 percent, suggesting that homeowners generally do not place large, positive value on potential tax savings associated with lower capital spending (and presumably lower property taxes in the future). This result is consistent with Cellini et al. (2010), who find that narrowly passed bond measures increase house prices, but do not generate detectable effects on student achievement. These results suggest that either local homeowners value some types of school spending, absent detectable effects on student achievement or that homeowners cannot distinguish between productive and non-productive school spending, and use changes in spending as a proxy for school quality.

In low-spending districts, where budget hawks cause little change in district spending but reduce student achievement, house price effects are slightly negative and imprecise. These estimates don't rule out reductions on the same magnitude as Black (1999) or Bayer et al. (2007), who find that a 5 percent increase in school performance is associated with 2.5 and 1 percent increase in house prices, respectively. As such, these effects tell us little about aggregate effects on house prices in these districts.

3.5 Robustness

I present three sets of results in Appendix D that demonstrate the robustness of my main estimates.

In Appendix Figure D1, I show the sensitivity of my main results to bandwidth selection. Specifically, for my main outcomes, I estimate the effect of a budget hawk victory for bandwidths ranging from half of the optimal bandwidth to 1.5 times the optimal bandwidth. I then calculate average effects based on Equation 3. Appendix Figure D1 displays how my estimates and confidence intervals vary when moving from a narrow to wide bandwidth. Generally, results with respect to district finances attenuate slightly when moving to a wider bandwidth, but remain economically and statistically significant in high-spending districts. Results with staffing and test scores are less precise when narrow bandwidths are used, but become more precise with wider bandwidths.

I additionally perform placebo tests, evaluating my results at different thresholds of my running variable vjt. Appendix Figure D2 shows the results of this exercise. Shifting the thresholds by 1 or 2 percentage points in either direction produces estimates that are lower in magnitude generally statistically insignificant, suggesting that the clearest discontinuities are at the correct thresholds for electoral victory, $v_{jt} = 0$.

Finally, I demonstrate that choices in my topic modeling approach have little impact on my estimates. Appendix Figure D3 shows how estimates change when I vary the number of topics used in the KeyATM topic model. To produce this estimate, I fit separate KeyATM models, varying the number of no-keyword topics. My main estimates use 5 no-keyword topics. The estimates in Appendix Figure D3 show results when 3, 5, 10, or 15 no-keyword topics are used. After fitting each KeyATM model, I replicate all subsequent analyses from scratch. This includes identifying budget hawks, constructing a regression discontinuity sample, separating high- and low-spending districts, estimating the optimal bandwidth, and estimating treatment effects. Effect sizes in Appendix Figure D3 are reasonably consistent, suggesting that my estimates are not sensitive to topic modelling choices.

4 Conclusion

Funding for education in America is large and spread across multiple levels of government, but financial decision-making is distinctively local. In practice, approaches to education spending vary meaningfully across school districts. To the degree that some districts are allocating resources inefficiently, local financial reforms may be offer a pareto improvement by shifting resources from low- to high-productivity educational inputs. This paper tests whether local officials can successfully enact such reforms.

The estimates in this paper show a mixed story. First, I demonstrate that some capital spending appears to have little measurable impact on student achievement. In high-spending districts, the election of a budget hawk to the district school board leads to capital spending cuts, but no observable changes in test-based proficiency rates. Still, effects on house prices caution that cuts to spending may not be welfare improving, if local homeowners value these amenities, or if such spending affects valuable but unobservable or immeasurable facets of district value-added.

Oppositely, in low-spending districts, attempts at reforms appear to take a different form. In these districts, reformers appear to cut staffing ratios, and academic achievement suffers: subsequent proficiency rates fall. These results suggest potentially large returns to staffing in education production.

A few caveats are in order, which offer motivation for future research. First, my measures of student achievement—rates of test-based proficiency and graduation—are limited in scope, and I can't rule out any effects beyond the measures I offer here. For example, it could be the case that cuts to capital spending in high-spending districts lead to reductions in college attendance, future wages, or other outcomes that I do not measure. As longitudinal education-workforce data becomes more widespread, researchers may be able to document such effects.

Second, my estimates reflect intention-to-treat effects of school board members on district-level outcomes, and should not be interpreted strictly as the effect of school spending on outcomes. Existing research demonstrates that school boards affect many school inputs, so it is possible that the reductions in proficiency I find are driven by board-induced changes that I cannot measure. For example, school boards may change curriculum or teacher composition, inputs that are beyond the scope of my analysis. To the degree that these non-financial, non-staffing inputs matter, researchers may find it useful to quantify them, as in Blazar et al. (2020).

Finally, the estimates in this paper are based on a subset of large districts in California, which overall constitute a small share of total student enrollments in the US. Future research may leverage large-scale, multi-state data on school board elections (as Abott et al. (2020) do for tax elections) to expand the scope of this growing literature beyond state-specific studies. Given that differences in school administration vary much more across districts than within districts (Hochschild (2005)), this approach may be useful in documenting how differences in local governance affect differences in educational productivity.

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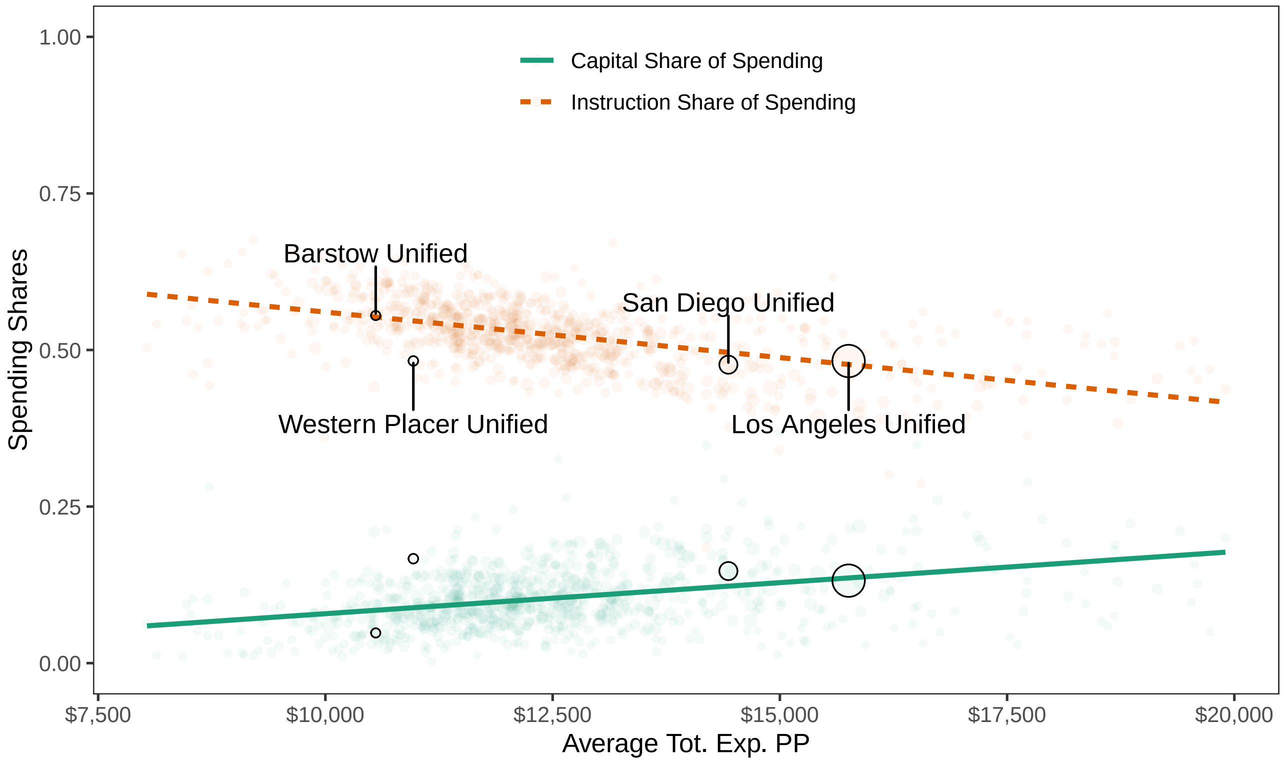
Figure 1: Per-Pupil Spending and Spending Composition



Notes: Figure displays the relationship between total per-pupil spending and spending shares for districts in California. Sample includes all districts in California for which fall enrollment was higher than 100 for more than 10 years between 2000 and 2017. The size of each point reflects average district enrollment over this period. For these districts, I calculate average inflation-adjusted district spending overall, and for instruction and capital specifically. Instruction spending includes payments from all funds for salaries, employee benefits, supplies, materials, and contractual services for elementary/secondary instruction and excludes capital outlay, debt service, and interfund transfers for elementary/secondary instruction. Capital expenditures include direct expenditure for construction of buildings, roads, and other improvements, and for purchases of equipment, land, and existing structures. I exclude districts with average total per-pupil spending below $8,000 or above $20,000.

Figure 2: Most Common Unigrams and Bigrams

Panel A: Top 100 Unigrams

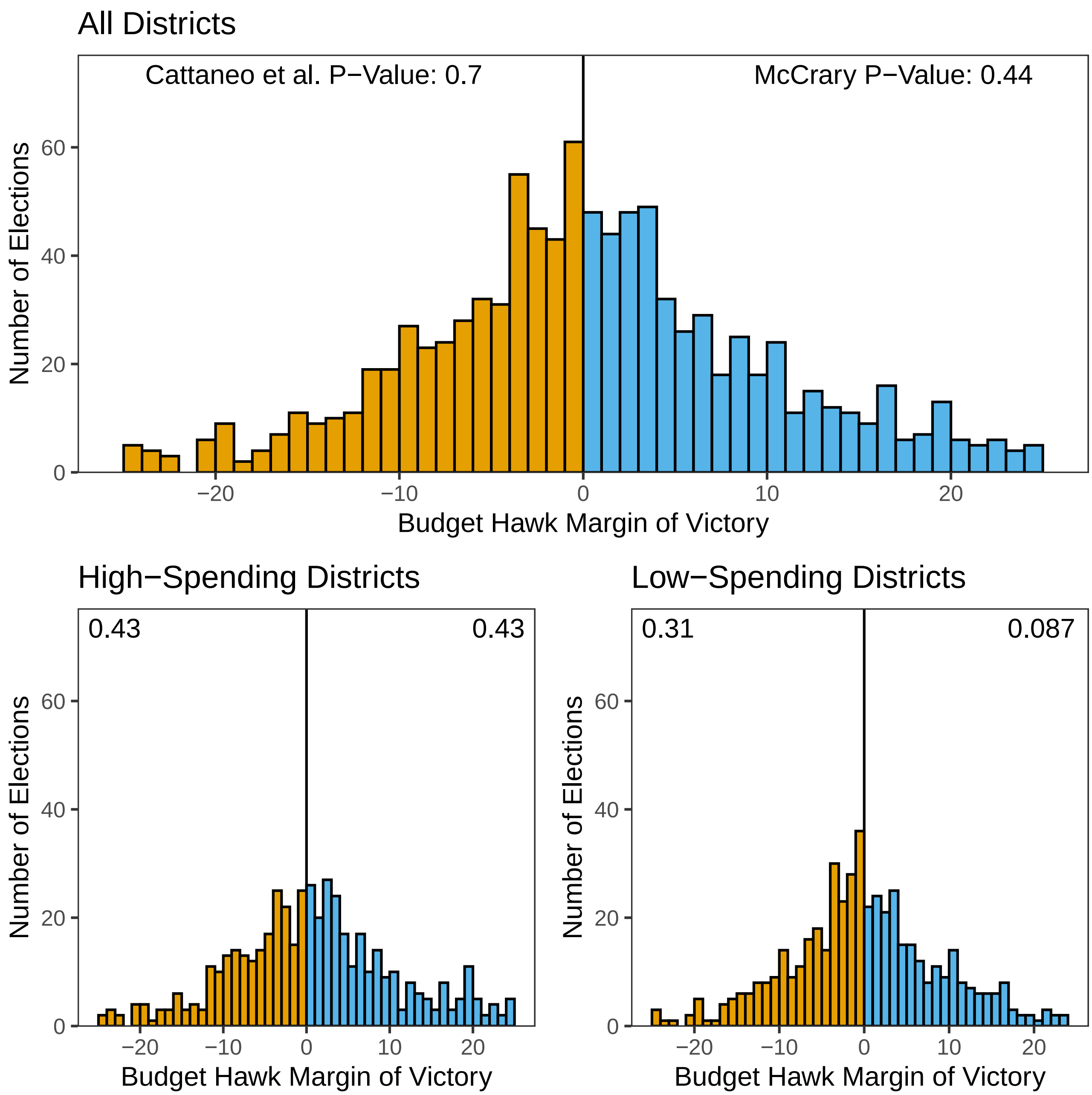
district	qualiti	new	transpar	gap
teacher	need	resourc	opportun	meet
communiti	fund (4.3%)	environ	balanc	teach
parent	board	standard	size	better
improv	learn	safeti	test	career
fiscal (7.8%)	communic	build	creat	well
program	safe	promot	level	help
achiev	account	keep	child	close
continu	work	technolog	strong	local
academ	make	everi	score	collabor
ensur	excel	plan	must	employe
support	classroom	manag	public	rais
budget (5.7%)	financi (3.0%)	facil	implement	bring
provid	focus	state	open	spend (1.3%)
high	involv	curriculum	partnership	dollar (1.3%)
increas	best	decis	use	enhanc
maintain	class	retain	effect	first
children	develop	prepar	prioriti	sound
respons	success	art	reduc	cut
staff	administr	colleg	perform	polici

Panel B: Top 100 Bigrams

fiscal_respons (3.0%)	high_qualiti	budget_cut (0.5%)	fiscal_solvenc (0.4%)	improv_test
class_size	maintain_fiscal (0.7%)	provid_safe	best_possibl	continu_provid
test_score	continu_improv	long_term	high_standard	fiscal_stabil (0.3%)
parent_teacher	qualiti_teacher	meet_need	local_control	size_reduct
parent_communiti	teacher_parent	maintain_high	art_music	staff_communiti
teacher_staff	attract_retain	open_communic	safe_environ	ensur_district
achiev_gap	support_teacher	strateg_plan	manag_district	throughout_district
academ_achiev	everi_child	provid_best	provid_qualiti	communic_board
parent_involv	communiti_involv	recruit_retain	teacher_communiti	profession_develop
balanc_budget (1.2%)	increas_parent	district_budget (0.5%)	communiti_member	safe_secur
21st_centuri	improv_achiev	ensur_fiscal (0.5%)	unifi_district	district_communiti
academ_excel	qualifi_teacher	high_academ	well_round	parent_staff
improv_communic	academ_standard	financi_stabil (0.4%)	best_teacher	build_new
learn_environ	common_core	sound_fiscal (0.4%)	maintain_balanc	maintain_safe
close_achiev	board_member	top_prioriti	safe_learn	retain_high
decis_make	communic_parent	graduat_rate	staff_parent	within_district
teacher_administr	reduc_class	district_wide	music_art	district_fiscal (0.3%)
make_sure	fiscal_manag (0.5%)	high_qualifi	prepar_colleg	retain_best
fiscal_account (0.8%)	colleg_career	increas_achiev	support_staff	academ_success
improv_academ	academ_perform	tax_dollar (0.4%)	fiscal_sound (0.3%)	declin_enrol

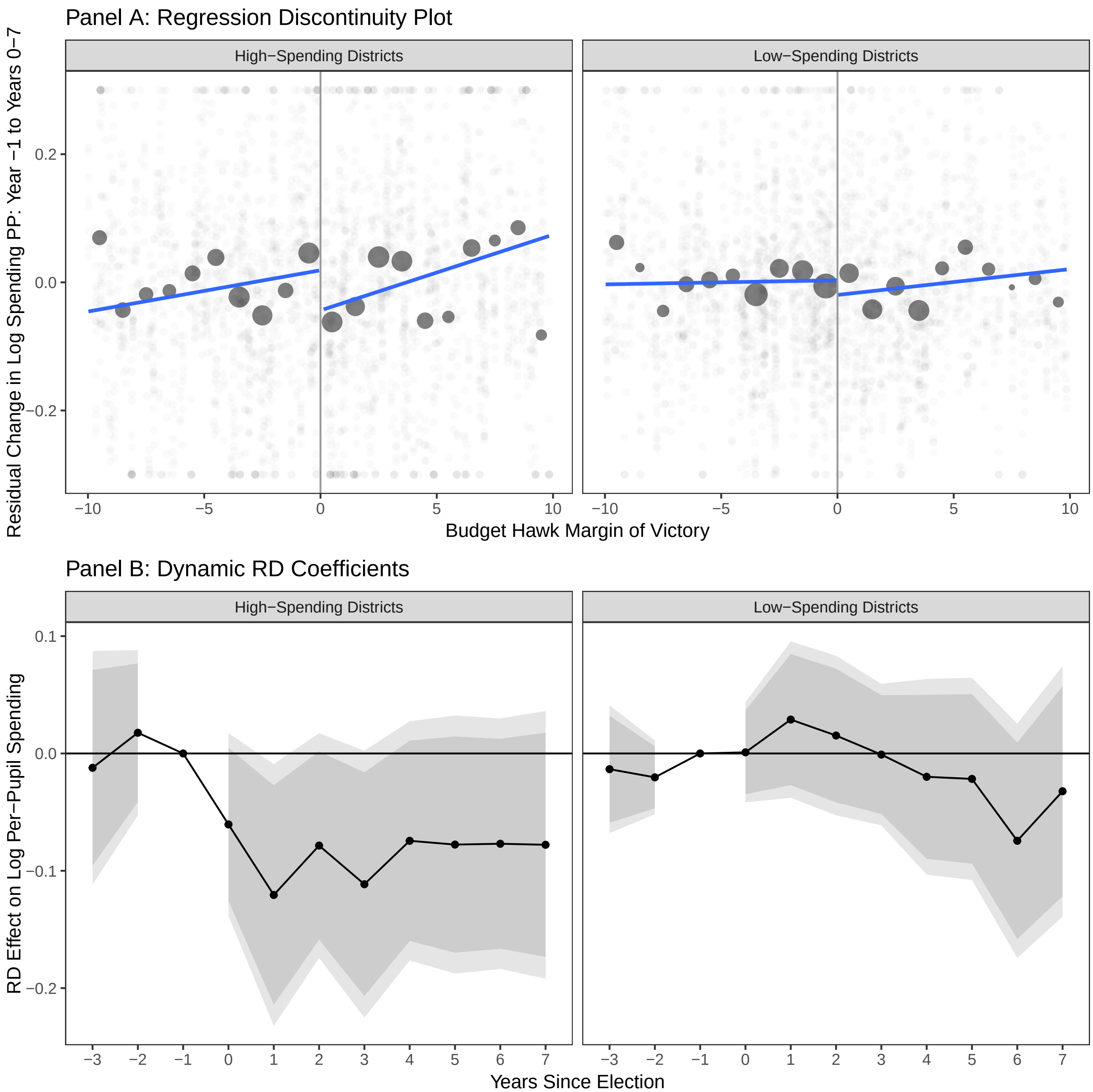
Notes: Figure displays the 100 most common unigrams and bigrams in school board candidates' priorities. Words are ordered by most frequent (in top left) to least frequent (in bottom right). Finance-related terms are shown in black, and their frequency (i.e. the share of bullets in which they appear) is shown in parentheses.

Figure 3: Density of Running Variable



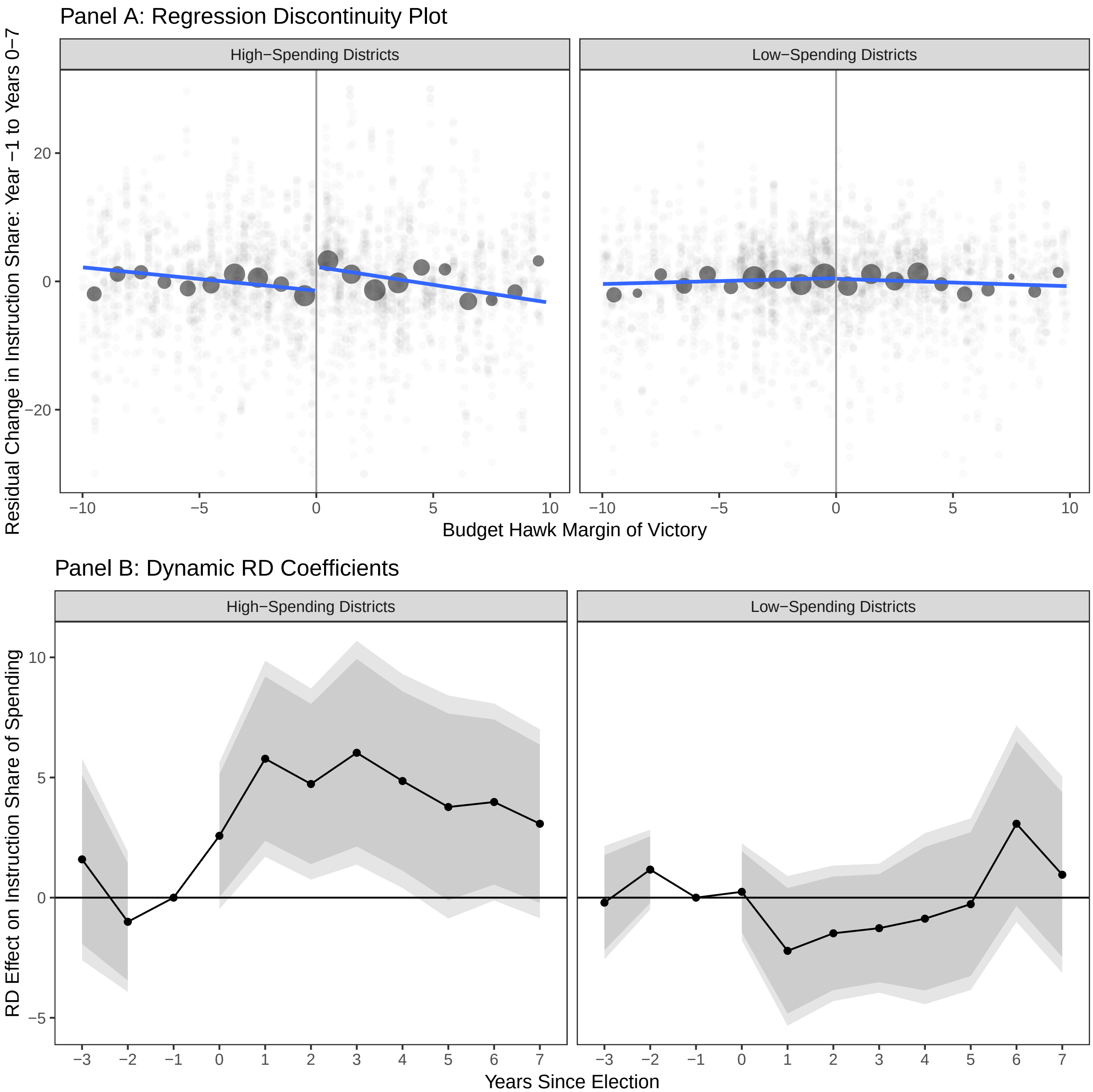
Notes: Figure displays the density of my elections as a function of running variable, the budget hawk margin of victory: v_{jt}. P-values in the top left and right correspond to density tests proposed by Cattaneo et al. (2020) and McCrary (2008), respectively.

Figure 4: Effect of Elections on Log Spending Per Pupil



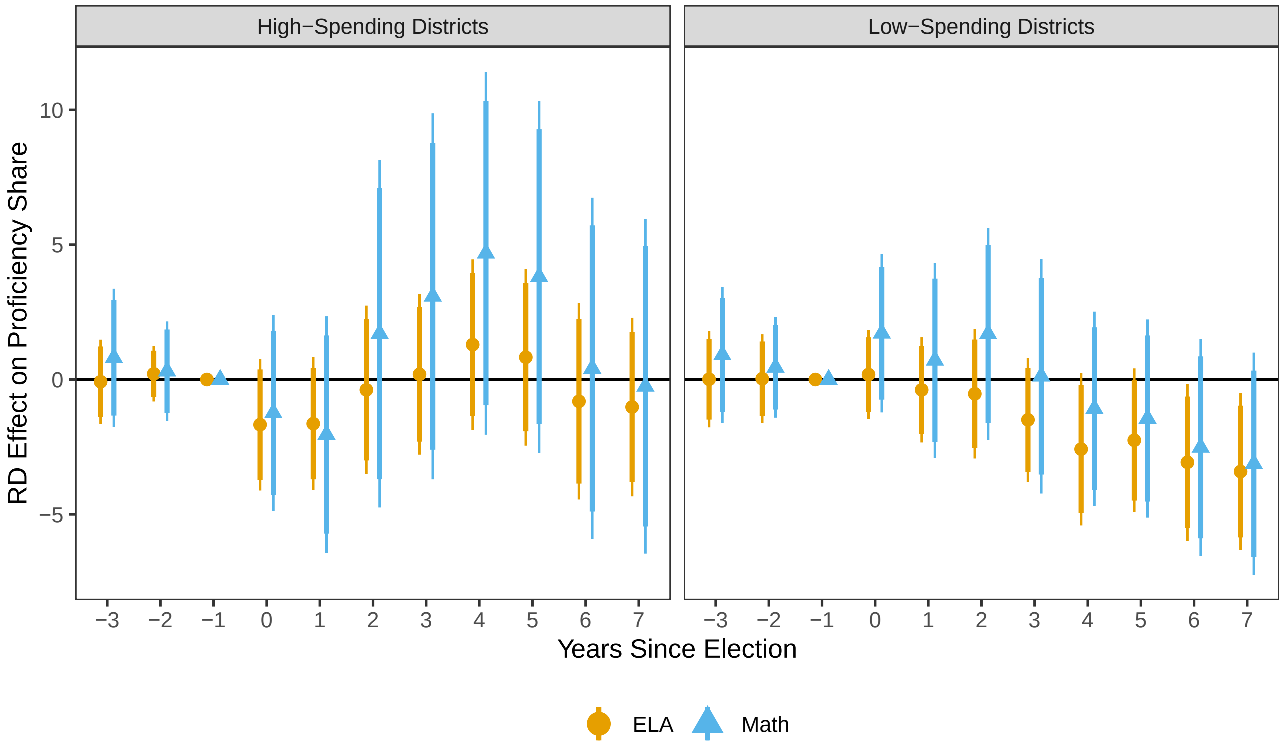
Notes: Figure displays the effect of budget hawks on log per-pupil expenditures (in 2019 dollars). Panel A of Figure displays a pooled regression discontinuity plot, where the horizontal axis represents the budget hawk margin of victory v_{jt} and the vertical axis displays the change in log total per-pupil spending between the year prior to election and the eight years thereafter. Panel B of displays the coefficients from Equation 1, representing the dynamic effect of a budget hawk victory. Confidence bands reflect 90 and 95 percent confidence intervals. Corresponding estimates are found in Appendix Table C1.

Figure 5: Effect of Elections on Spending Composition



Notes: Figure displays the effect of budget hawks on the share of district spending on instruction. Instruction spending includes payments from all funds for salaries, employee benefits, supplies, materials, and contractual services for elementary/secondary instruction and excludes capital outlay, debt service, and interfund transfers for elementary/secondary instruction. All shares and rates are represented in percentage points, ranging from 0 to 100. Panel A of Figure displays a pooled regression discontinuity plot, where the horizontal axis represents the budget hawk margin of victory v_{jt} and the vertical axis displays the change in share of district spending on instruction spending between the year prior to election and the eight years thereafter. Panel B of displays the coefficients from Equation 1, representing the dynamic effect of a budget hawk victory. Confidence bands reflect 90 and 95 percent confidence intervals. Corresponding estimates are found in Appendix Table C2.

Figure 6: Effect of Elections on Test Scores



Notes: Figure displays the effect of budget hawks on the share of students testing at or above proficiency on state standardized tests in math and ELA. All shares and rates are represented in percentage points, ranging from 0 to 100. Figure displays the coefficients from Equation 1, representing the dynamic effect of a budget hawk victory. Confidence bands reflect 90 and 95 percent confidence intervals.

Table 1: Example Priorities and Probability of Finance Topic

Bullet Text	P(Finance)
Fiscally prudent management of budget, finances and resources to cut waste, and ensure every tax dollar is spent in the most cost effective manner.	1.00
Use construction dollars responsibly to modernize existing schools and build capacity to increase school choices for students	0.90
Work with elected officials to ensure smaller schools and districts get their fair share of state and federal educational funding	0.80
First and foremost is keeping the district's financial house in order	0.70
Continue to balance district $320 million budget without staff layoffs and continue funding Athletics, Visual and Performing Arts.	0.60
Supporting prudent fiscal management that ensures additional resources for the classroom that will attract and retain high quality staff	0.50
Providing effective leadership and oversight, responsive to community concerns, throughout our building project.	0.40
Continued use of site-based management to ensure the highest quality child-centered education utilizing the resources of home, school and community.	0.30
Creating a high quality, effective learning environment for all our students and accomplishing this with professionalism and fiscal responsibility	0.20
Promote policies and programs which encourage parent involvement	0.10
EXCELLENCE: maintain small class size, recruit and retain outstanding staff, maintain excellent academic programs, and high test scores	0.0005

Notes: Table displays candidate-written bullets nearest to each tenth percentile of p_{bm}, the probability that the bullet concerns the finance topic. These probabilities are produced by the KeyATM model described in text.

Table 2: Where do Budget Hawks Run in School Board Races?

	Number of Budget Hawks Running				
	(1)	(2)	(3)	(4)	(5)
log(Expend. Per Pupil)	0.09^{***}				0.01
	(0.03)				(0.03)
log(Local Revenue Per Pupil)		0.14^{***}			0.14^{***}
		(0.01)			(0.01)
log(Staff Per 1,000 Pupils)			-0.23^{***}		-0.37^{***}
			(0.04)		(0.04)
Pct. Proficient (Math)				0.004^{***}	0.002^{***}
				(0.001)	(0.0005)
Observations	2,473	2,473	2,473	2,473	2,473

Notes: Table displays how financial, staffing, and academic characteristics of districts covary with the number of budget hawks who run in district school board election. For each district-year combination in SmartVoter data, I count the number of budget hawk candidates running. I link these candidate counts to district characteristics in the year prior to the election. Table displays the result of regressions in which the outcome is the number of budget hawk candidates running in district j in year t and the independent variables are district characteristics in year $t-1$. I control for year fixed-effects and cluster standard errors at the district level. All dollar-denominated values are in 2019 dollars. All shares and rates are represented in percentage points, ranging from 0 to 100. *** p<0.01; ** p<0.05; * p<0.01.

Table 3: District Summary Statistics

Variable	All Dists.	Dists. with Profiles	Dists. in RD Sample
	District Characteristics		
Fall Enrollment	5,841 [1,475]	10,744 [5,305]	13,383 [6,471]
	(24,385)	(35,138)	(42,685)
Percent Underrep. Minority	40 [34]	40 [34]	40 [34]
	(27)	(26)	(26)
	Spending Ratios		
Total Spending Per Pupil	17,743 [11,930]	12,701 [11,657]	12,551 [11,573]
	(24,437)	(3,887)	(3,443)
Instruction Share of Spending	52 [54]	52 [53]	52 [53]
	(10)	(8)	(8)
Capital Share of Spending	12 [10]	15 [13]	16 [13]
	(10)	(11)	(10)
	Staffing		
Total Staff Per 1,000 Pupils	98 [92]	90 [87]	89 [86]
	(24)	(13)	(12)
Teaching Staff Per 1,000 Pupils	55 [51]	50 [49]	49 [48]
	(14)	(6)	(5)
Non-Teaching Staff Per 1,000 Pupils	46 [41]	40 [38]	40 [38]
	(23)	(11)	(10)
	Financial Metrics		
Surplus Per Pupil	748 [314]	117 [124]	90 [177]
	(4,792)	(2,464)	(2,458)
Long-Term Debt Outs. Per Pupil	3,052 [335]	4,112 [2,269]	4,095 [2,236]
	(6,072)	(5,063)	(5,149)
	House Prices		
Mean House Price	406,715 [333,494]	484,330 [386,589]	490,700 [389,547]
	(288,696)	(314,861)	(314,416)
	Academic Performance		
Pct. Proficient: Math	32 [31]	37 [36]	38 [37]
	(18)	(19)	(20)
Pct. Proficient: ELA	33 [32]	38 [38]	38 [38]
	(18)	(18)	(18)
HS Graduation Rate	75 [78]	79 [80]	78 [79]
	(21)	(15)	(15)
UC-Eligible HS Graduation Rate	27 [23]	30 [26]	31 [27]
	(21)	(16)	(17)
SAT-Taking Rate	40 [38]	43 [40]	44 [41]
	(15)	(15)	(15)
Unique Districts	1,060	470	310

Notes: Table displays district characteristics as of 2000 for different subsets of districts. The first column shows characteristics of all districts in California. The second column displays characteristics of districts with any SmartVoter profiles between 2001 and 2015. The third column displays characteristics of the subset of districts in my regression discontinuity sample: districts that have at least 1 close election involving a budget hawk. All dollar-denominated values are in 2019 dollars. All shares and rates are represented in percentage points, ranging from 0 to 100.

Table 4: Balance Test

Variable	All Districts Levels	All Districts Changes	High-Spending Districts Levels	High-Spending Districts Changes	Low-Spending Districts Levels	Low-Spending Districts Changes
Spending Ratios						
log(Total Spending Per Pupil)	0.06 (0.04)	-0.00 (0.02)	0.05 (0.04)	-0.02 (0.04)	0.01 (0.03)	0.02 (0.02)
Instruction Share of Spending	-2.14 (1.34)	-0.02 (0.82)	-2.42 (1.88)	1.02 (1.57)	-0.32 (1.31)	-1.12 (0.94)
Capital Share of Spending	2.58 (1.76)	0.66 (1.39)	3.61 (2.59)	-0.16 (2.63)	0.04 (1.72)	1.77 (1.36)
Staffing						
Total Staff Per 1,000 Pupils	2.60 (1.98)	0.35 (1.61)	0.10 (3.27)	-0.38 (2.79)	4.36 (2.67)	1.25 (2.06)
Teaching Staff Per 1,000 Pupils	1.20 (0.90)	0.59 (0.57)	1.32 (1.36)	0.45 (0.84)	0.57 (1.13)	0.66 (0.72)
Non-Teaching Staff Per 1,000 Pupils	1.17 (1.78)	-0.31 (1.53)	-1.78 (2.79)	-0.97 (2.61)	3.79 (2.34)	0.59 (1.95)
Financial Metrics						
Surplus Per Pupil	-721 (292)**	-257 (386)	-1049 (480)**	-271 (713)	-193 (237)	-291 (334)
Long-Term Debt Outs. Per Pupil	2293 (1962)	122 (590)	3183 (3428)	479 (989)	-71 (1295)	-384 (552)
House Prices						
log(Mean House Price)	0.10 (0.10)	-0.03 (0.02)	0.02 (0.15)	0.02 (0.04)	0.14 (0.11)	-0.07 (0.03)**
Academic Performance						
Pct. Proficient: ELA	4.20 (2.95)	-0.16 (0.57)	0.44 (4.56)	-0.23 (0.61)	8.20 (3.91)**	-0.22 (0.97)
Pct. Proficient: Math	1.73 (3.58)	-0.23 (0.87)	0.66 (5.42)	0.29 (1.07)	3.61 (4.63)	-1.04 (1.56)
HS Graduation Rate	1.50 (3.21)	1.50 (2.19)	0.48 (4.39)	5.70 (2.94)*	3.16 (3.92)	-3.30 (3.10)
UC-Eligible HS Graduation Rate	3.28 (4.34)	0.52 (2.45)	0.89 (5.55)	2.81 (3.01)	5.79 (5.89)	-2.17 (3.61)
SAT-Taking Rate	2.05 (3.16)	-1.07 (1.14)	-1.04 (4.29)	-0.98 (1.85)	5.32 (3.91)	-1.19 (1.26)

Notes: Table displays the results of tests for differences in levels and trends of main outcomes prior to the election. Coefficients represent the results of a local linear regression. Columns showing results with respect to levels set the outcome as the level of the outcome in the year immediately before the election ($\tau = -1$). Columns showing results with respect to changes set the outcome as the difference between the outcome years $\tau = -2$ and $\tau = -1$. I use a triangular kernel, a bandwidth of 7.6 percentage points, and include year fixed-effects as controls. All dollar-denominated values are in 2019 dollars. All shares and rates are represented in percentage points, ranging from 0 to 100. *** p<0.01; ** p<0.05; * p<0.01.

Table 5: Effects of Election on School Inputs

Panel A: High-Spending Districts

Dep. Variable	Years Since Election 0-3	4-7	0-7	N	N Elecs.
Spending Ratios					
log(Total Spending Per Pupil)	-0.09 (0.05)**	-0.08 (0.05)	-0.08 (0.04)*	3005	290
Instruction Share of Spending	4.8 (1.8)***	3.9 (2.1)*	4.3 (1.7)**	3005	290
Capital Share of Spending	-6.6 (3.1)**	-3.5 (3.4)	-5.1 (2.8)*	3005	290
Staffing					
Total Staff Per 1,000 Pupils	1.0 (2.0)	-1.0 (2.5)	-0.0 (2.0)	3005	290
Teaching Staff Per 1,000 Pupils	1.4 (1.0)	-1.0 (1.3)	0.2 (1.0)	3005	290
Non-Teaching Staff Per 1,000 Pupils	0.1 (1.8)	0.8 (2.2)	0.4 (1.8)	3005	290
Financial Metrics					
Surplus Per Pupil	1371 (774)*	597 (657)	984 (645)	3005	290
Long-Term Debt Outs. Per Pupil	-907 (916)	-3108 (2069)	-2008 (1366)	3005	290

Panel B: Low-Spending Districts

Dep. Variable	Years Since Election 0-3	4-7	0-7	N	N Elecs.
Spending Ratios					
log(Total Spending Per Pupil)	0.01 (0.03)	-0.04 (0.04)	-0.01 (0.03)	3080	310
Instruction Share of Spending	-1.2 (1.2)	0.7 (1.8)	-0.2 (1.3)	3080	310
Capital Share of Spending	0.7 (1.6)	-2.6 (2.8)	-0.9 (1.9)	3080	310
Staffing					
Total Staff Per 1,000 Pupils	-3.1 (1.8)*	-1.8 (2.3)	-2.4 (1.8)	3080	310
Teaching Staff Per 1,000 Pupils	-0.6 (0.9)	-0.4 (0.9)	-0.5 (0.8)	3080	310
Non-Teaching Staff Per 1,000 Pupils	-2.5 (1.8)	-1.4 (2.0)	-2.0 (1.7)	3080	310
Financial Metrics					
Surplus Per Pupil	-91 (336)	657 (436)	283 (324)	3080	310
Long-Term Debt Outs. Per Pupil	306 (676)	-442 (1329)	-68 (902)	3080	310

Notes: Table summarizes the effects of budget hawk victory on district outcomes for separate sets of years relative to the election. Estimates correspond to average yearly effects from Equation 3. All dollar-denominated values are in 2019 dollars. All shares and rates are represented in percentage points, ranging from 0 to 100. *** p<0.01; ** p<0.05; * p<0.01.

Table 6: Effects of Election on Student Achievement

Dep. Variable	Years Since Election 0-3	4-7	0-7	N	N Elecs.
Panel A: High-Spending Districts					
Test Scores: ELA					
Pct. Proficient: ELA	-0.9 (1.3)	0.1 (1.7)	-0.4 (1.4)	2978	290
Pct. Proficient: Elementary ELA	-0.1 (1.0)	-0.0 (1.7)	-0.0 (1.3)	2490	241
Pct. Proficient: Middle Sch. ELA	0.6 (1.1)	1.0 (1.6)	0.8 (1.3)	2597	251
Pct. Proficient: HS ELA	-0.6 (1.4)	0.1 (2.0)	-0.2 (1.6)	2102	205
Test Scores: Math					
Pct. Proficient: Math	0.4 (2.5)	2.2 (3.2)	1.3 (2.7)	2978	290
Pct. Proficient: Elementary Math	0.7 (1.5)	0.1 (2.5)	0.4 (1.8)	2490	241
Pct. Proficient: Middle Sch. Math	2.2 (1.7)	1.7 (2.6)	2.0 (2.1)	2597	251
Pct. Proficient: HS Math	2.7 (3.3)	1.5 (3.7)	2.1 (3.2)	2026	198
High School Outcomes					
HS Graduation Rate	-0.7 (2.2)	-1.6 (3.0)	-1.1 (2.4)	2131	208
UC-Eligible HS Graduation Rate	-3.6 (3.0)	-4.1 (3.7)	-3.8 (3.2)	2121	207
SAT-Taking Rate	2.1 (2.0)	4.2 (2.3)*	3.2 (1.9)	2136	207
Panel B: Low-Spending Districts					
Dep. Variable	Years Since Election 0-3	4-7	0-7	N	N Elecs.
Test Scores: ELA					
Pct. Proficient: ELA	-0.6 (1.0)	-2.8 (1.3)**	-1.7 (1.0)*	3010	310
Pct. Proficient: Elementary ELA	-0.8 (1.1)	-2.2 (1.5)	-1.5 (1.1)	2837	292
Pct. Proficient: Middle Sch. ELA	-1.0 (1.1)	-2.7 (1.5)*	-1.8 (1.1)	2895	298
Pct. Proficient: HS ELA	0.1 (1.3)	-1.3 (1.4)	-0.6 (1.1)	1729	178
Test Scores: Math					
Pct. Proficient: Math	1.1 (1.9)	-2.0 (1.9)	-0.5 (1.7)	3010	310
Pct. Proficient: Elementary Math	-0.3 (1.8)	-1.2 (1.6)	-0.8 (1.5)	2819	290
Pct. Proficient: Middle Sch. Math	0.4 (1.4)	-3.6 (2.5)	-1.6 (1.8)	2877	296
Pct. Proficient: HS Math	-2.5 (2.2)	-0.0 (3.4)	-1.2 (2.6)	1682	173
High School Outcomes					
HS Graduation Rate	0.8 (2.2)	-2.7 (3.3)	-0.9 (2.4)	1752	183
UC-Eligible HS Graduation Rate	-1.5 (3.5)	-0.1 (4.8)	-0.8 (3.6)	1741	181
SAT-Taking Rate	-1.0 (1.9)	-3.8 (2.9)	-2.4 (2.2)	1766	178

Notes: Table summarizes the effects of budget hawk victory on district outcomes for separate sets of years relative to the election. Estimates correspond to average yearly effects from Equation 3. All dollar-denominated values are in 2019 dollars. All shares and rates are represented in percentage points, ranging from 0 to 100. ***

p<0.01; ** p<0.05; * p<0.01.

Table 7: Regression Discontinuity: Effect of Election on House Prices

	High-Spending Districts	Low-Spending Districts
	log(Mean House Price)	
	(1)	(2)
Above x Year = -3	−0.02	0.01
	(0.02)	(0.02)
Above x Year = -2	−0.0004	0.01
	(0.01)	(0.01)
Above x Year = 0	−0.02	−0.01
	(0.02)	(0.01)
Above x Year = 1	−0.02	−0.02
	(0.02)	(0.01)
Above x Year = 2	−0.02	−0.02
	(0.03)	(0.02)
Above x Year = 3	−0.03	−0.01
	(0.03)	(0.03)
Above x Year = 4	−0.05	−0.01
	(0.04)	(0.03)
Above x Year = 5	−0.05	−0.01
	(0.04)	(0.03)
Above x Year = 6	−0.05	−0.003
	(0.05)	(0.04)
Above x Year = 7	−0.05	−0.03
	(0.05)	(0.03)
P(0 Effect Years 0-3)	0.402	0.366
P(0 Effect Years 4-7)	0.263	0.709
Observations	2,784	2,864

Notes: Table displays the coefficients from Equation 1, representing the dynamic effect of a budget hawk victory on district outcomes. All dollar-denominated values are in 2019 dollars. *** p<0.01; ** p<0.05; * p<0.01.

Appendix A Data Sources

This appendix details on the collection and cleaning of my district-by-year panel data described in Section 2.

A.1 F33 District Finance Data

My school finance data come from the School District Finance Survey (Form F33) survey.

A.2 Local Education Agency Universe Survey

School staffing data comes from the Local Education Agency Universe Survey. I calculate staffing ratios as the number of staff per 1,000 pupils. These ratios are prone to outliers, so I windsorize such that total staff per 1,000 pupils does not exceed 150 and total teaching and non-teaching staff per 1,0000 pupils does not exceed 100.

A.3 State Standardized Testing Data

District-level student achievement data comes from the California Department of Education. Data for years 2000 to 2013 come from the STAR testing regime research files. Data for years 2015 to 2017 come from the CAASPP testing regime research files.

These files report, for each district in each year, the number of students testing across different levels of performance. STAR and CAASP report different discrete performance categories. Specifically, STAR data reports the percent of tested students who are "advanced," "proficient," "basic," "below basic," and "far below basic." CAASP reports the percent of students who "exceed," "meet," "nearly meet," or "do not meet" the performance standard. In my analysis, I report results with respect to the percent of students who are proficient: a category that includes "advanced" and "proficient" students in STAR data and students who "exceed" or "meet" the performance standard in CAASP data.

I restrict my analysis to grades in which tests in Math and ELA were widely administered: grades 3 through 8 and 11. For each district and year, I calculate the total share of students who are proficient, as well as the share of students within the following grade spans: grades 3 - 8 ("elementary"), grades 6 - 8 ("middle"), and grade 11 ("high"). STAR testing data is missing for 2000 math scores, so I use their 2001 values to balance my panel.

A.4 Graduation Rate Data

I collect annual graduation counts from the state of California. To convert graduate counts to graduation rates, I calculate the ratio of total graduates to the total number of 9th graders enrolled in the district 4 years prior (from annual district by grade enrollment data).

Additionally, I collect SAT-taking data and calculate the share of 12th graders who take the SAT in each district in each year.

A.5 Zillow House Price Data

I construct district-level house prices using data from the Zillow Home Value Index. Zillow constructs this index by estimating the sale price of each house in their national database, which they refer to as a "Zestimate." Zillow aggregates these estimates to the zip code level by taking the value-weighted average Zestimate in the area, excluding houses that "undergo significant physical changes."[15] I refer to the average Zestimate in zip code z in year t as h_{zt}. I restrict my sample to December of each year and use the "All Homes," seasonally-adjusted series.

I aggregate values of h_{zt} to the district level. First, I identify the zip codes associated with each school district in California as of 2013, based on the NCES Geographic Relationship Files. I calculate the fraction of each zip code z's land area that falls in each district z: s_{zd}.

Weighting by s_{zd} would be unreasonable, because large, sparsely-populated zip codes would receive more weight than small, densely-populated zip codes. For this reason, I construct district d's average house price as the average of h_{zt}, weighted by the product of s_{zd} and the population of zip code z, p_z. Mathematically,

$$h_{dt} = \sum_{z \in d} z_{zt} * w_{zd}, \qquad \text{where} \quad w_{zd} = \frac{s_{zd} * p_z}{\sum_{z \in d} s_{zd} * p_z}.$$

These weights calculate district d's population-weighted house price, assuming that each zip code's population is distributed evenly over land.

A.6 Adjusting for Inflation

I adjust all dollar-denominated values (district finance data and house prices) using the Consumer Price Index retroactive series using current methods. I use yearly average CPI values and convert all values to 2019 dollars.

A.7 Sample Restrictions and Interpolation

I restrict my sample to districts that had positive enrollment, positive spending, and positive staffing in 1998 and 2017. I additionally require that districts in my sample have nonmissing state testing

[15] Zillow provides more detail on their calculations here: https://www.zillow.com/research/zhvi-methodology-2019-deep-26226/.

data in 2000 and 2017. These requirements ensure that, for these outcomes, districts in my sample are balanced across years. However, data reporting is inconsistent for a small number of district-years, meaning some values are missing. In these cases, I replace missing data with the the most recent non-missing value.

Appendix B Text Pre-Processing and Topic Modeling

As described in the body of the paper, data on candidate priorities comes from SmartVoter, an election information website run by the League of Women Voters of California. Figure B1 provides an example SmartVoter profile. Each profile in my data contains three bullet points describing the profiled candidate's "Top Priorities if Elected." Below, I detail my steps for processing and analyzing this data.

B.1 Creating a Document-Feature Matrix

I start by removing numbers, punctuation, and other non-word characters from each bullet. Next, I stem each word, so words with common stems, such as "financial" and "finance," are treated identically as "financ-" (Porter (1980)). Once stemmed, I remove three sets of common words. First, I remove stopwords, common words that have little meaning without context. These words include "the," "that," "it," and so on. I use the list of stopwords from Salton (1971). Second, I remove the words "school", "student", and "education," the three most common unigrams in my data that appear so frequently that they convey little information about the statement's content. Finally, I remove any word whose unigram is less than three characters long.

I next create a document-feature matrix, where each row corresponds to a bullet and each column corresponds to a unigram or bigram in my data. Beyond the restrictions described above, I restrict the set of features in this matrix to exclude any term that appears in fewer than five bullets. These restrictions generate a small number of bullets that have zero non-zero features. Typically these statements are either one-word long ("Suspensions") or have substantial misspellings ("I am not a budding politican," "Student egagement"). I drop these bullets from my analysis.

This procedure generates a document-feature matrix with 13,691 rows and 3,301 columns.

B.2 Labeling Finance-Related Terms

As described in text, I label a set of common features as finance-related. To produce the list of eligible terms, I count the frequency of each term and restrict my review to the 100 most common unigrams and bigrams. This lists are displayed in Figure 2.

These lists are short enough to review manually. In labeling finance-related terms, I am strict in excluding terms that may be used in another context. For example, the unigram "balanc-" is most commonly used preceding the term "budget." However, I opt not to label this as a finance-related term because, in some cases, candidates use the term "balanced curriculum." A strict approach to labeling terms limits the scope for false positives—statements that I label as finance-related that are, in fact, not related to district finances.

B.3 Fitting a KeyATM Model

Broadly, topic models typically assume that each document can be described by a distribution of topics and each topic can be described by a distribution of terms. As such, the KeyATM methodology assumes that topics are produced by a particular generative process and uses Markov chain Monte Carlo methods to solve for the parameters that dictate this process. I defer to Eshima et al. (2020) for technical details related to the KeyATM model and sampling algorithm.

In practice, the KeyATM algorithm takes three inputs: a document-term matrix, a set of labeled keywords, and the number of no-keyword topics.[16] Using the inputs described above, I fit a KeyATM model using the document-term matrix and the set of finance-related terms described above, setting the number of no-keyword topics to 5.[17]

[16] Eshima et al. (2020) denote a small number of other inputs which correspond to parameters of prior distributions, but note that "[i]n typical applications, the choice of hyperparameters does not matter so long as the amount of data is sufficiently large." I follow the authors in setting these parameters.

[17] I explore sensitivity to the choice of no-keyword topics in Appendix D.

Figure B1: Example SmartVoter Profile

This is an archive of a past election.
See http://www.smartvoter.org/ca/alm/ for current information.

League of Women Voters of California

Alameda County, CA — November 4, 2003 Election

Stephen Pulido

Candidate for
Governing Board Member; Pleasanton Unified School District

The information on this page and on all pages linked below is provided by the candidate.
The League of Women Voters does not support or oppose any candidate or political party.

Biographical Highlights

- Occupation: Attorney/Parent
- 25 years of experience as Family Law Attorney working with family and children issues
- Lived in Pleasanton for 23 years with wife and two children
- Pleasanton School District Budget Advisory Committee 2002-2003
- Member of the Strategic Planning Team for Pleasanton Unified School District
- Member of Academic Standards Advisory Committee for PUSD, 1998 to present
- Current Member of the new Strategic Planning Team for the Pleasanton Unified School District

More Biographical Information

Top Priorities if Elected

- I will always seek, and place great weight upon, the input and vision of all stake holders in making decisions.
- I will strongly advocate for a wide variety of programs that meet the needs of all students.
- I will work diligently with fellow board members to ensure the fiscal integrity of our school district.

Political Philosophy

Key Endorsements

- Association of Pleasanton Teachers/ Classified Service Employee Association
- Tri-Valley Herald, The Valley Times and The Pleasanton Weekly
- Matt Campbell, Vice Mayor, Pat Kernan, Pleasanton School Board

Full Endorsement List

Position Papers

Letter to the Community

Campaign Contact Information

Website: http://www.stevepulidoforschoolboard.com
E-mail: s.pulido@comcast.net

Feedback to Candidate || All Candidates this Contest
Alameda Home Page || Statewide Links || About Smart Voter

The League of Women Voters does not support or oppose any candidate or political party.

Statements have not been checked for accuracy by the League of Women Voters. Spelling and grammatical errors have not been corrected.

Created from information supplied by the candidate: October 31, 2003 16:12

Smart Voter™ <*http://www.smartvoter.org/*>

Notes: Figure displays an example SmartVoter candidate profile, available at http://www.smartvoter.org/2003/11/04/ca/alm/vote/pulido s/.

Appendix C Event Study Estimates

The tables in this appendix present numerical estimates of event study estimates shown graphically or summarized elsewhere in text. Specifically, Table C1 displays estimated effects on financial ratios (shown graphically in Figures 5 and 4 and summarized in Table 5) and Table C2 displays estimated effects on staffing (summarized in Table 5).

Table C1: Regression Discontinuity: Effect of Election on Financial Ratios

	Pre-Election Spending							
	High	Low	High	Low	High	Low	High	Low
	log(Tot. Exp. PP)		Inst. Share		Cap. Share		Sup. Serv. Share	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Above x Year = -3	−0.01	−0.01	1.59	−0.21	−2.37	−0.22	−0.30	0.16
	(0.05)	(0.03)	(2.13)	(1.20)	(3.49)	(1.71)	(1.13)	(0.66)
Above x Year = -2	0.02	−0.02	−1.01	1.17	0.14	−1.80	−0.63	0.35
	(0.04)	(0.02)	(1.48)	(0.84)	(2.49)	(1.22)	(0.77)	(0.52)
Above x Year = 0	−0.06	0.001	2.57*	0.24	−5.22*	−1.25	1.32	0.14
	(0.04)	(0.02)	(1.55)	(1.02)	(2.77)	(1.61)	(0.99)	(0.55)
Above x Year = 1	−0.12**	0.03	5.78***	−2.21	−8.30**	1.97	2.38**	−0.45
	(0.06)	(0.03)	(2.07)	(1.58)	(3.68)	(2.16)	(1.19)	(0.83)
Above x Year = 2	−0.08	0.02	4.73**	−1.48	−6.53**	0.94	0.92	−0.23
	(0.05)	(0.03)	(2.02)	(1.43)	(3.16)	(2.04)	(1.22)	(0.91)
Above x Year = 3	−0.11*	−0.001	6.03**	−1.27	−6.44*	1.16	0.76	−0.54
	(0.06)	(0.03)	(2.36)	(1.36)	(3.72)	(1.92)	(1.28)	(0.84)
Above x Year = 4	−0.07	−0.02	4.85**	−0.88	−4.30	−0.64	0.54	0.37
	(0.05)	(0.04)	(2.26)	(1.81)	(3.82)	(2.78)	(1.32)	(1.17)
Above x Year = 5	−0.08	−0.02	3.77	−0.27	−3.90	−1.25	1.21	0.56
	(0.06)	(0.04)	(2.35)	(1.81)	(3.88)	(2.82)	(1.31)	(1.25)
Above x Year = 6	−0.08	−0.07	3.98*	3.07	−4.26	−6.10*	1.26	2.25
	(0.05)	(0.05)	(2.08)	(2.07)	(3.37)	(3.16)	(1.24)	(1.42)
Above x Year = 7	−0.08	−0.03	3.08	0.95	−1.71	−2.35	−0.01	0.55
	(0.06)	(0.05)	(1.99)	(2.07)	(3.06)	(3.41)	(1.19)	(1.49)
P(0 Effect Years 0-3)	0.046	0.677	0.009	0.318	0.031	0.659	0.211	0.689
P(0 Effect Years 4-7)	0.137	0.408	0.057	0.692	0.294	0.363	0.526	0.47
Observations	3,005	3,080	3,005	3,080	3,005	3,080	3,005	3,080

Notes: Table displays the coefficients from Equation 1, representing the dynamic effect of a budget hawk victory on district outcomes. All dollar-denominated values are in 2019 dollars. All shares and rates are represented in percentage points, ranging from 0 to 100. *** p<0.01; ** p<0.05; * p<0.01.

Table C2: Regression Discontinuity: Effect of Election on Staff per 1,000 Pupils

	Pre-Election Spending					
	High	Low	High	Low	High	Low
	Total Staff		Teaching Staff		Non-Teaching Staff	
	(1)	(2)	(3)	(4)	(5)	(6)
Above x Year = -3	−0.411	−1.381	0.296	−0.482	−0.131	−0.900
	(2.075)	(2.000)	(0.862)	(0.534)	(1.856)	(1.906)
Above x Year = -2	−0.548	−0.696	0.309	−0.206	−0.688	−0.490
	(2.331)	(1.691)	(0.730)	(0.611)	(2.203)	(1.761)
Above x Year = 0	1.076	−4.838***	1.855	−0.313	−0.438	−4.526**
	(2.853)	(1.771)	(1.527)	(0.650)	(2.473)	(1.859)
Above x Year = 1	1.080	−3.723*	0.899	−0.697	0.449	−3.026
	(1.962)	(2.154)	(0.750)	(0.869)	(1.811)	(2.094)
Above x Year = 2	1.113	−1.757	1.945	−0.704	−0.328	−1.053
	(2.354)	(2.052)	(1.212)	(1.069)	(2.112)	(1.749)
Above x Year = 3	0.583	−1.983	0.771	−0.645	0.721	−1.338
	(2.467)	(2.072)	(1.307)	(1.016)	(2.320)	(1.920)
Above x Year = 4	−0.866	−2.318	0.732	−0.358	−0.170	−1.959
	(2.821)	(2.984)	(1.792)	(0.914)	(2.384)	(2.856)
Above x Year = 5	−1.041	−1.141	−1.047	−0.307	1.406	−0.834
	(2.622)	(2.402)	(1.381)	(0.932)	(2.562)	(2.197)
Above x Year = 6	−2.477	−0.620	−3.500	−0.082	1.025	−0.538
	(3.259)	(2.617)	(2.211)	(1.118)	(2.509)	(2.069)
Above x Year = 7	0.349	−3.080	−0.308	−0.680	0.769	−2.401
	(2.966)	(2.980)	(1.850)	(0.927)	(2.242)	(2.603)
P(0 Effect Years 0-3)	0.634	0.087	0.166	0.504	0.955	0.158
P(0 Effect Years 4-7)	0.691	0.44	0.44	0.687	0.727	0.479
Observations	3,005	3,080	3,005	3,080	3,005	3,080

Notes: Table displays the coefficients from Equation 1, representing the dynamic effect of a budget hawk victory on district outcomes. Staffing ratios reflect staff per 1,000 pupils. *** p<0.01; ** p<0.05; * p<0.01.

Appendix D Robustness

This appendix presents robustness checks for the main effects estimated in the paper's text.

D.1 Regression Discontinuity Bandwidth

Figure D1 shows the sensitivity of my main estimates with respect to the regression discontinuity bandwidth I use. Plotted estimates show the results of the t-test represented by Equation 3.

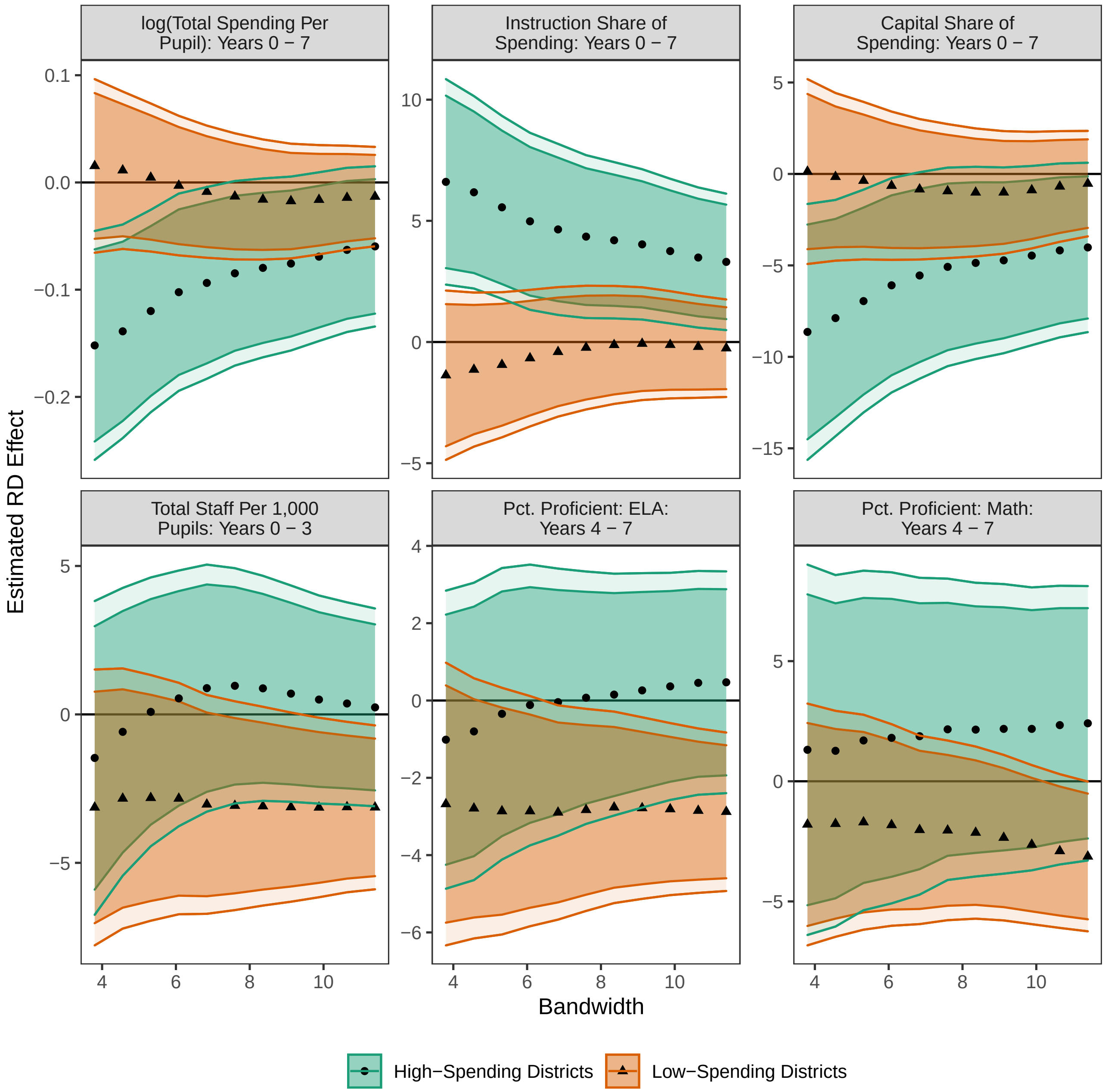
D.2 Placebo Voting Thresholds

Figure D2 shows my dynamic regression discontinuity estimates evaluated at placebo thresholds. Specifically, I estimate Equation 1 after shifting the election threshold (equal to $v_{jt} = 0$) by 1 or 2 percentage points in both directions. Plotted estimates show the results of the t-test represented by Equation 3.

D.3 Number of No-Keyword Topics in KeyATM Model

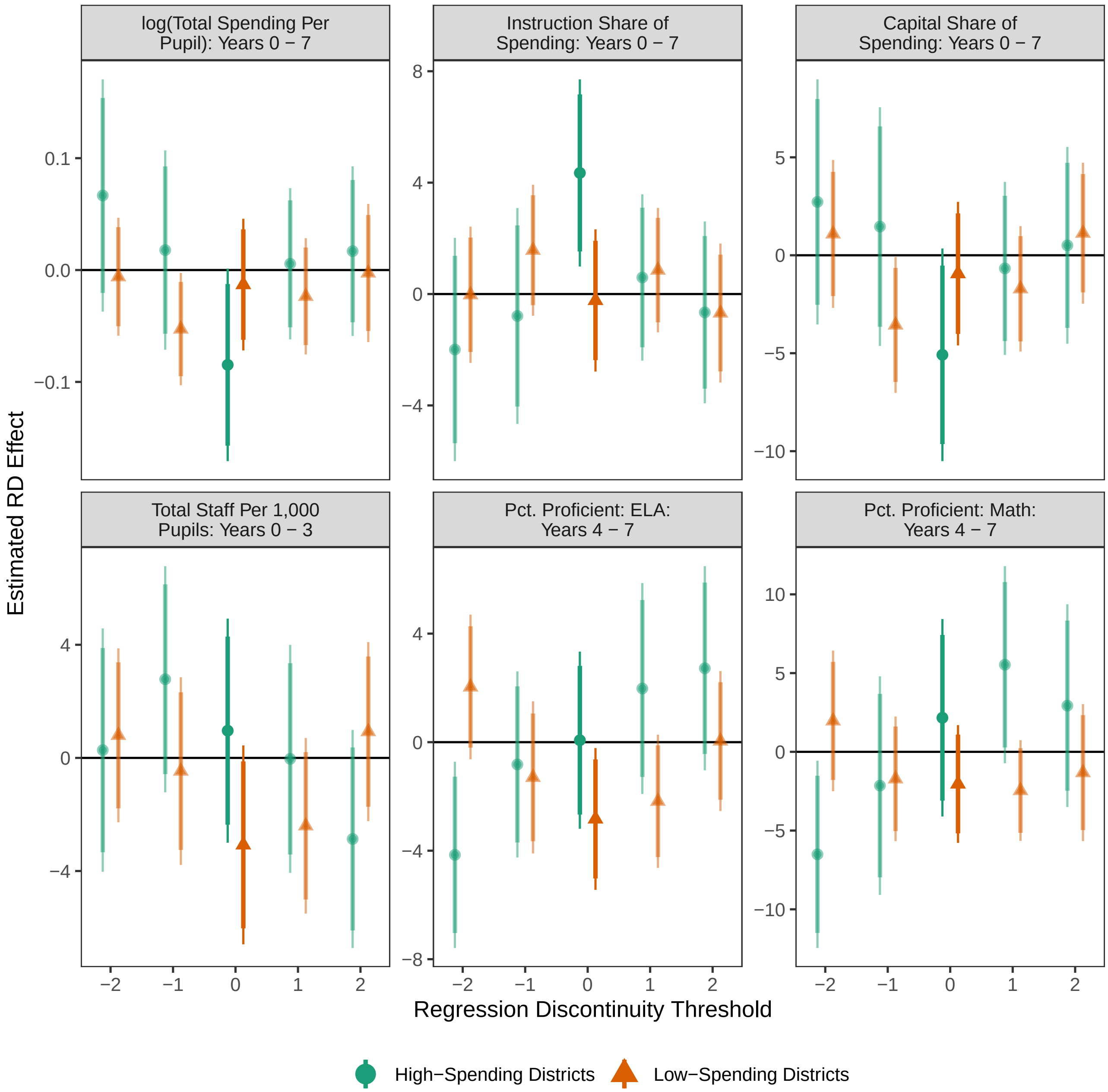
Figure D3 shows shows how estimates change when I vary the number of topics used in the KeyATM topic model. To produce this estimate, I fit separate KeyATM models with different numbers of no-keyword topics, and replicate all subsequent analyses from scratch. This includes identifying budget hawks, constructing a regression discontinuity sample, separating high- and low-spending districts, estimating the optimal bandwidth, and estimating treatment effects.

Figure D1: Sensitivity of Main Effects to Bandwidth Choice



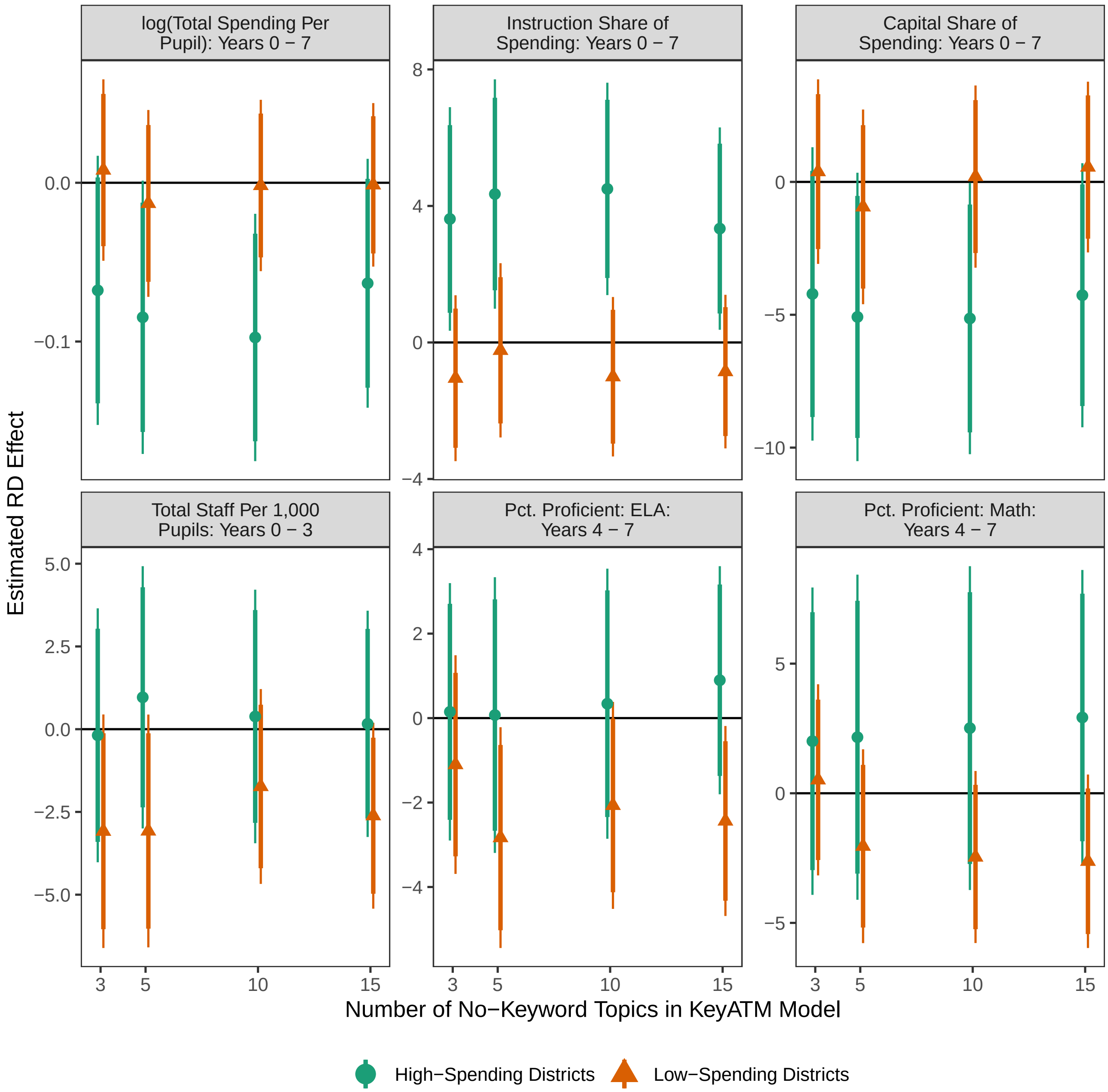
Notes: Figure displays the sensitivity of main estimates to choice of bandwidth. The horizontal axis represents the bandwidth (in percentage points) used to estimate Equation 1, ranging from half of the optimal bandwidth to 1.5 times the optimal bandwidth, as proposed by Calonico et al. (2020). All dollar-denominated values are in 2019 dollars. All shares and rates are represented in percentage points, ranging from 0 to 100. Plotted estimates show the results of the t-test represented by Equation 3. Confidence bands reflect 90 and 95 percent confidence intervals.

Figure D2: Placebo Voting Thresholds



Notes: Figure displays the results of a placebo test, where Equation 1 is estimated at placebo thresholds. The horizontal axis represents the threshold of the running variable, v_{jt} used to estimate Equation 1. All dollar-denominated values are in 2019 dollars. All shares and rates are represented in percentage points, ranging from 0 to 100. Plotted estimates show the results of the t-test represented by Equation 3. Confidence bands reflect 90 and 95 percent confidence intervals.

Figure D3: Sensitivity of Main Effects to Number of Topics in KeyATM Model



Notes: Figure displays the sensitivity of main estimates to the number of no-keyword topics in the KeyATM model. The horizontal axis represents the number of no-keyword topics in the KeyATM model used to identify budget hawks. All dollar-denominated values are in 2019 dollars. All shares and rates are represented in percentage points, ranging from 0 to 100. Plotted estimates show the results of the t-test represented by Equation 3. Confidence bands reflect 90 and 95 percent confidence intervals.