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

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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" using natural language processing methods and campaign statements from school board candidates in California. I use a regression discontinuity design to test how district outcomes evolve in the years following the narrow victory of a hawk over a non-hawk. The election of a budget hawk leads to large and prolonged cuts in district spending. Using test score data, I find suggestive evidence that students in these districts exhibit lower rates of test-based proficiency in subsequent years. Heterogeneity analyses show evidence that districts that exhibit higher reductions in spending experience larger test score declines.

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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 almost 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 student achievement. For identification, I leverage a particular source of dramatic changes in district management: 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 approach to district financial matters. I use this data in a regression discontinuity design, identifying the causal impact of an additional budget hawk on the school board on the financial and academic trajectory of the district.

My setting is school boards in California. These boards have a high degree of autonomy in allocating funding; boards are responsible for setting and overseeing district budgets, negotiating with teachers unions, and making a broad set of decisions regarding district operations. In this setting, I test whether an additional board member who runs on promises of "financial responsi-

[1] "K-12 School Spending Up 4.7% in 2019 From Previous Year," May 18, 2021, United States Census Bureau; Bureau of Economic Analysis, "Table 3.1. Government Current Receipts and Expenditures," August 26, 2021.

[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).

bility" affects district financial and academic outcomes.

I find that school board composition has large effects on district spending. In the 4 years following the election of an additional budget hawk to the district school board, per-pupil spending falls by 10 percent; equal to roughly $1,300 per pupil in 2021 dollars. These reductions are concentrated in capital spending and persist for 3 to 4 years before returning to their pre-election levels. Lower spending is not offset by changes in collected revenue; districts run short-term surpluses that lead to lower levels of outstanding debt.

Using test score data, I assess whether the election of a budget hawk is associated with changes in student achievement. Contrary to the stated goals of budget hawk candidates—many of whom aim to reduce spending without sacrificing achievement—I find suggestive evidence that budget hawk victories are associated with lower test scores in math in post-election years. While test score estimates are somewhat imprecise, the magnitudes are generally consistent with the broader literature on spending shocks and student achievement (Jackson and Mackevicius (2021)).

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 a 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 0.0352" standard deviations. 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 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. My work demonstrates that school boards, in the normal course of budgeting, can and do impose voluntary cuts to school budgets that affect district finances as much as external policy shocks that have been studied much more extensively.

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 geographic subsection 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.

Most school boards agree on a district budget, which projects anticipated revenues and expenses, during the summer. Over the course of the following academic year, the board can make adjustments to spending in response to unanticipated changes in district needs or projections.[3] During this time, boards must file two interim reports to the California Department of Education, which detail the district's financial health, including "a certification of whether or not the [district] is able to meet its financial obligations."[4] Boards typically discuss these reports, including the degree to which their actual spending during the school year aligns with their planned expenses as of the preceding summer, during periodic board meetings throughout the year.[5]

While boards have substantial autonomy in how they allocate funds (even after a budget has been agreed upon), 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

[3]For example, the Santa Ana Unified School District 2013 Annual Financial Report states, "Over the course of the year, the District revises its budget as it attempts to deal with unexpected changes in revenues and expenditures." "Annual Financial Report," Santa Ana Unified School District, June 30, 2013. Many district annual reports include similar language.

[4]"Interim Status," California Department of Education.

[5]For example, the Santa Ana Unified Board meeting agenda from May 28, 2013 included a presentation detailing their Third Interim Budget. The presentation details mid-year savings measures implemented in anticipation of future reductions in revenue. "Board Meeting Agenda," Santa Ana Unified School District Board of Education, May 28, 2013, pp. 31–52.

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 funds[6] and how to spend 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.[7]) 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.[8]

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 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 district outcomes in subsequent years. To do so, I combine detailed text data on candidate priorities with annual data on district finances and academic performance. For identification, I use a regression discontinuity design that compares outcomes in districts where a budget hawk narrowly won to outcomes in districts where a budget hawk narrowly lost. In the subsections below, I describe my candidate priorities data, identification strategy, and data on district financial and academic outcomes.

[6] 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.

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

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

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.[9] 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. Table 1 summarizes characteristics of these elections. The first column displays characteristics for all elections in my data. In total, my data includes over 13,000 candidate-election observations, over 4,000 candidate profiles, and over 3,000 elections. On average, elections featured roughly 4 candidates competing for 2 seats on the school board. Not all candidates complete SmartVoter profiles; overall, roughly one-third of candidates in my data have SmartVoter profiles. Figure 1 shows the geographic distribution of candidate profiles in my data.

In SmartVoter profiles, the text of each candidate's "top priorities" consists of three bullets, summarizing their priorities as a candidate. The language used in these "top priorities" illustrates the issues most frequently discussed during school board elections. Figure 2 shows the 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: roughly 7 percent of all bullets in my data use the word "fiscal" and the phrase "fiscal respons[ibility]" is among the most common two-word phrases in my data. Finance-oriented candidates often discuss a "balanced

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

budget," and "fiscal account[ability]" and "fiscal manag[ement]."

Figure 2 distinguishes between two sets of terms that candidates often use when discussing district finances, which I refer to as "hawk" and "non-hawk" terms. I define hawk terms as terms that refer to management and allocation of the existing district budget. Alternatively, non-hawk terms are focused on raising additional funds, often for capital projects. As I discuss below, the distinction between candidates who discuss better management of district finances and candidates who advocate for increased revenues proves to be empirically meaningful.

I identify budget hawk school board 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 focused on close management of district finances.

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 researcher to provide a small number of keywords prior to model fitting to guide the topic model.

In my application, I specify two sets of terms associated with financially-oriented candidates: hawk and non-hawk terms. 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 select terms from the top 250 unigrams and bigrams; selected terms are 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 5 no-keyword topics.

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 2 displays the set of bullets with the highest values of p_{bcm} for both the hawk-finance topic as well as the non-hawk finance topic. In Table 2, bullets identified as hawkish discuss “fiscal responsibility" and directing tax dollars to “the classroom." Non-hawk finance bullets often emphasize “adequate" or “equal" funding for the district.

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 $p_{c,Hawk} > 0.5$. In practice, the distribution of $p_{c,Hawk}$ has most mass near 0 and 1, so most comparisons between hawks and non-hawks are not affected by the location of this cutoff. Appendix Figure E1 shows the empirical distribution of $p_{c,Hawk}$; very few candidates have values near 0.5.

While my budget hawk measure $p_{c,Hawk}$ is based on a relatively small set of information from each candidate—three short bullets regarding their priorities—it has strong correlation with other indicators of candidate ideologies and backgrounds. To illustrate, I run candidate-level regressions with $p_{c,Hawk}$ as the outcome and other candidate characteristics as predictors. All candidate characteristics are from other fields in Smartvoter data (specifically, the “bio" and “endorsements" fields). (Candidates who do not submit information to SmartVoter are excluded from regressions.)

Table 3 displays my results. Column 1 indicates that candidates who report receiving the Republican endorsement are 11 percentage points more likely to be budget hawks. Oppositely, Column 2 shows that candidates who report receiving the Democrat endorsement are 7 percent less likely to be budget hawks. Some candidates report occupational history in the SmartVoter "Biographical Highlights" section. Columns 3 and 4 of Table 3 show that candidates who report having been a teacher are less likely to be budget hawks, while candidates with a background in business are more likely to be a budget hawk. Incumbent candidates are also more likely to be budget hawks.

I test whether the share of budget hawks varies over time. To do so, I estimate regressions with fixed-effects for election years, grouping sets of two years together (2004-05, 2005-06, and so forth). I omit the first two election years in my data, 2000 and 2001, so estimates reflect differences between the indicated years and 2000-01. Column 6 displays results. Elections in 2008, 2010, and 2012 featured the highest share of budget hawks, which may reflect the effect of the Great Recession on school budgets or the (direct or indirect) effect of the Tea Party movement on political speech.

Finally, Column 7 shows that the relationships described above are robust to the inclusion of all variables simultaneously.

2.2 Regression Discontinuity Design

To estimate the causal effect of a budget hawk election to the district school board, I use a regression discontinuity design that compares outcomes in districts where a budget hawk narrowly won an election to districts where a budget hawk narrowly lost. Doing so requires specifying, for each election, a running variable that represents the budget hawk's margin of victory. I denote this value for election e as v_e. I calculate this v_e following the procedure outlined by Macartney and Singleton (2018).

Specifically, I identify the identity (hawk or non-hawk) and vote share of the least popular election winner and the most popular opposite-identity loser in each contest. I define the running variable as the margin of victory (or loss) for the budget hawk. I restrict the set of elections to include only elections in which pre- and post-election financial data and pre and post-election test score data is available. This procedure produces a set of 535 elections from 248 unique school districts.

Naturally, this procedure excludes many elections in my data. Column 3 of Table 1 shows how average election characteristics for my regression discontinuity sample differ from all elections and elections with at least one SmartVoter profile, shown in Columns 1 and 2, respectively. Compared to elections with profiles, elections in my regression discontinuity sample have more seats and more candidates per seat. On average, elections in my regression discontinuity sample involve 4.9 candidates, 3.6 of whom provided SmartVoter with priorities. The left panel of Figure 1 shows the geographic distribution of candidate profiles in my data, which mirrors the geographic distribution of candidates overall.

I index elections by e, districts by j, calendar years by t, and years since each election by τ. With v_e as constructed above, I estimate regression discontinuity models. For outcomes, I gather data from 5 years before each election year and 8 years years after. (For elections that occur later in calendar time, I don't observe the full 8 year post-election period.) School board members serve 4 year terms, so my post-election data covers two full school board terms, if elected candidates run and win following their term on the school board.

I focus on effects in years relative to the election, indexed by τ. For an election e in district j in year t, I estimate the effect of budget hawk victory on outcome y in τ years after the election: $y_{j,e,t+\tau}$. I define $\Delta y_{j,e,t+\tau}$ as the change in outcome y between the year prior to election $(t-1)$ and τ years since election $(t+\tau)$. Formally, $\Delta y_{j,e,t+\tau} = y_{j,e,t+\tau} - y_{j,e,t-1}$.

My identifying equation is

$$\Delta y_{j,e,t+\tau} = \gamma_\tau \mathbb{1}[v_e \geq 0] + f^\tau(v_e) + \varepsilon_{j,e,t+\tau}. \tag{1}$$

$\mathbb{1}[v_e \geq 0]$ is an indicator variable equal to 1 when $v_e \geq 0$ and 0 otherwise. f^τ is a potentially nonlinear function of v_e that I approximate through local linear regression with a triangular kernel.

γ_τ is the effect of budget hawk victory τ years after election. I estimate Equation 1 using the Calonico et al. (2014) robust regression discontinuity estimator, with MSE-optimal bandwidth. (This bandwidth is typically between 5 and 10 percentage points. In Appendix C, I show the sensitivity of my main results to bandwidth selection.) Throughout, I report point estimates alongside Calonico et al. (2014) robust confidence intervals. In addition to τ-specific estimates, which I present visually in the form of event studies, I estimate Equation 1 using two pooled sets of years: 1 to 4 years post-election and 5 to 8 years post-election. In these regressions, I additionally include year fixed-effects as controls. Throughout, I cluster standard errors at the district level to account for repeated district-year observations in my data.

For school-by-grade level test score data, changes since pre-election are not always available, as schools and grades may enter the data after an election occurs (for example, when a new school opens or a school expands its grade span). As such, in analyses of test scores, my outcomes are in levels rather than changes, and I include pre-election mean test scores at the district or school-level as covariates. I include year fixed-effects and school demographic characteristics in all analyses of test score 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: $v_e = 0$. 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. In Appendix C, which I describe in more detail below, I provide evidence that this is the case; in the years before an election, districts on either side of the threshold have similar characteristics, both in levels and trends.

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 2021 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 100 pupils.

Table 4 provides summary statistics for district financial and staffing data. The first column of Table 4 displays means, medians, and standard deviations for all district-year combinations in California between 1997 and 2018. (I restrict this district-years in this sample to those with at least 100 students enrolled.) In the second column, I restrict my data to districts in my regression discontinuity sample in the year prior to the election.

The districts in my regression discontinuity sample are, on average, much larger than the average district in California. Still, spending levels and shares are reasonably similar across the two samples. The median district in my regression discontinuity sample spent roughly $13,000 per pupil in the year prior to the election. On average, half of district spending spending went towards instruction, 28% went towards support services, and 11% went towards capital spending. The average district had 9.1 employees per 100 pupils, roughly half of whom were teachers.

2.4 School Test Score Data

I collect test score data at the school-by-grade-by-subject level from the California Department of Education ("CDE"). CDE data reports the percent of students who fall into four proficiency groups representing students who "exceed," "meet," "nearly meet," or "do not meet" the performance standard. In my main results, I report results with respect to these raw percentages. In supplementary results, I report results using transformed versions of these variables that are meant to approximate student-level standard deviations. Specifically, for the share of students scoring at least at "meet" or "exceed," I divide each school-by-grade percentage by $\sqrt{\bar{p}(1-\bar{p})}$, where $\bar{p}$ is equal to the subject-grade-specific state-level average. $\sqrt{\bar{p}(1-\bar{p})}$ is the standard deviation of a binary variable with mean $\bar{p}$, so these estimates approximate student-level standard deviations and are more well-suited for comparisons with the broader literature.[10] I set the means of these measures to zero, and refer to these variables as "standardized scores" throughout this paper.

I restrict my analysis to grades in which tests in math and ELA were widely administered: grades 3 through 7. I link CDE data with school-level demographics—specifically, the share of students eligible for free lunch and the share eligible for reduced lunch—and racial composition at the school-by-grade level.

Table 5 summarizes this data. Tests in my data are equally split between math and ELA subjects, and school-by-grade averages are based on samples of approximately 100 students. Students in my data fall roughly equally into four performance categories, and standardized measures in-

[10] Jackson and Mackevicius (2021) use a similar transformation in their meta-analysis of the effects of school spending shocks on student outcomes.

dicate that average scores are roughly equal to state-level means.

Because this data is at the school-by-grade level, larger districts are represented more frequently than smaller ones. I account for this issue in regression discontinuity analyses by weighting each observation by the total number of test takers (at the school-by-grade level) as a share of total district test takers.[11]

3 Results

In the section below, I first provide evidence on the validity of my regression discontinuity design. I then provide two sets of results, which detail how budget hawks affect school inputs and student achievement, followed by a section on heterogeneity across districts. 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: 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 budget hawk candidates are as good as random, we should expect that the density function of the running variable is continuous at $v_e = 0$. Figure 3 displays the density of my running variable, showing that the data does not reject this prediction. Figure 3 shows 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.

Second, I test for differences in levels and trends of main outcomes prior to the election. If close elections involving budget hawk candidates are as good as random, pre-election characteristics (or trends of characteristics) should not systematically vary across 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 ($y_{j,e,t-1}$) or (b) the change in the outcome

[11] More formally, let n_{sdgye} denote the number of students tested in school s in district d in grade g in year t relative to election e. I weight each observation by $w_{sdgye} = \frac{n_{sdgye}}{N_{dye}}$, where N_{dye} is equal to the total number of students tested in district d in year y relative to election e.

between years $t-1$ and $t-2$ (denoted as $\Delta y_{j,e,t-2}$). Similar to my main estimates, I use a triangular kernel and the MSE-optimal bandwidth.

Appendix Table C1 shows the results of these tests with respect to district inputs data. Appendix Table C1 indicates that pre-election financial characteristics and trends do not systematically differ between districts where a budget hawk narrowly won and districts where a budget hawk narrowly lost.

Still, I note that, while not statistically significant, the magnitude of the point estimate on differences in the log of total spending per pupil is reasonably high: 17 log points. In Appendix C, I show that this difference is entirely attributable to a small number of large outliers: districts whose pre-election spending was greater than \$20,000 per pupil. Removing these districts reduces my sample size only slightly (from 535 elections to 487 elections), but reduces the pre-election difference in spending levels from 17 log points to 4 log points. I show in Appendix C that excluding these outlier districts has little effect on my main results with respect to district finances or test scores.

Similarly, Appendix Table C2 tests for balance in my test score data, which show little evidence of differences in levels or trends when crossing the regression discontinuity threshold $v_e = 0$.

Finally, I show evidence of treatment. In Figure 4 I plot the budget hawk margin of victory against the (a) the number of budget hawks running in the corresponding district-election year and (b) the number of budget hawks elected in the corresponding district-election year. The left panel of Figure 4 shows that elections just above the threshold $v_e = 0$ show no differences with respect to the total number of budget hawks running in the district-election year combination. However, elections in which v_e is just above 0 elect, on average, 1 more budget hawk than districts in which v_e is just below 0. Below, I assess how these changes to district school board composition affect district outcomes.

3.2 Effect of Elections on School Spending and School Staffing

Figure 5 provides preliminary visual evidence for my regression discontinuity estimates with respect to district finances. To construct Figure 5, I calculate changes between year $\tau = -1$ and years $\tau = 1$ to $\tau = 4$ for each election in my sample. I then construct means for each 0.5 percentage point bin: these are shown as points in Figure 5. Regression lines in Figure 5 are local linear regressions with a triangular weight.

The plots in Figure 5 suggest that the election of a budget hawk has large effects on district finances. In particular, narrow budget hawk victories appear to be associated with large reductions in spending levels, as indicated by the discontinuous drop when crossing $v_e = 0$ from left to right. The other panels of Figure 5 show effects on spending shares. While smaller in magnitude than the estimated effects on the log of spending per pupil, these plots suggest that the election of a budget hawk is associated with a reduction in the share of spending on capital, and an increase in the instruction share of spending.

Figure 6 displays the dynamic effects of budget hawk victory on the same outcomes in the form of event study plots. Each point in Figure 6 is a separate regression discontinuity estimate, in which the outcome is the change since the pre-election year value. The results in Figure 6 indicate that spending falls sharply in the years immediately following a budget hawk victory; in the first full year of a budget hawk's term (indicated by year 1 on in Figure 6), point estimates suggest a spending reduction of 10 log points, roughly 10 percent of total spending. Over time, this effect attenuates and loses precision, but point estimates remain negative even 8 years post-election.

The other panels of Figure 6 illustrate how spending shares evolve in the years following a budget hawk's election. In the years immediately following a budget hawk's election, capital spending falls precipitously; the share of district budgets allocated to capital spending falls by roughly 10 percentage points. The timing of these changes—which are largely realized in the year of the election—suggests that discretionary reductions or delays in capital spending, rather than changes in bond issuance or budgeting decisions, account for a large share the board-induced changes in spending patterns. As shares of capital spending decrease, shares of instruction and support services spending increase.

However, these changes in spending composition do not last indefinitely; by 3 years after the election, spending shares return roughly back to their previous levels, indicating that spending shares have approximately returned to their prior levels.

Table 6 displays tabular estimates of effects across all school inputs for two sets of pooled years: 1 to 4 years post-election and 5 to 8 years post-election. Consistent with the visual evidence in Figures 5 and 6, Table 6 suggests that the election of a budget hawk to a district school board leads to large short-term cuts to spending. In the 4 years following a budget hawk's victory, spending falls by 10 log points. Cuts are concentrated in capital spending, whose share of the total budget falls by 7 percentage points. Spending shares in other categories increase accordingly.

Over the long-term effects on financial metrics are less clear. Point estimates for total spending in years 5 to 8 following the election are negative but smaller in magnitude—3 log points—and imprecise. Effects on spending shares in these periods are all small. While effects in later years are imprecise, the point estimates are consistent with budget hawks inducing large short-term cuts to capital spending and smaller long-term cuts to spending across all spending types.

Table 6 also shows effects on three measures of district financial health: surplus (annual revenues less annual expenses) per pupil, long-term debt issued per pupil, and long-term debt outstanding per pupil. Consistent with the changes described above, budget hawks lead districts to run budget surpluses, which lead to lower levels of outstanding debt. (Estimates with respect to outstanding debt levels are imprecise but economically significant.) Staffing levels don't appear to change in response to the election of a budget hawk.

Altogether, my results with respect to district finances suggest that school board members have substantial discretion in determining district spending levels and composition, and that hawk-induced changes in district spending are concentrated in capital spending, consistent with many candidates' plans to keep budget cuts "away from the classroom."

The magnitude of these changes is large. I estimate that in the years 1 to 4 following an election a budget hawk victory decreases per pupil district spending by 10 log points. This is roughly equivalent to $1,300 in per pupil spending in the median district ($13,188 from Table 4 times 0.10). For comparison, Abott et al. (2020) find that, in sample from districts in seven states, the typical increase in operational spending following the passage of a tax levy was roughly $600 in 2012 dollars ($729 in 2021 dollars). Using data from California, Cellini et al. (2010) find that the passage of bond issues leads to a roughly $1,000 increase in capital spending in 2000 dollars ($1,602 in 2021 dollars). My results suggest that the budgetary impact of the election of a budget hawk is of a similar magnitude as these widely-studied tax and bond elections. Next, I test whether budget hawk victories are associated with changes in student achievement.

3.3 Effect of Elections on Student Achievement

I first present preliminary visual evidence on the effect of budget hawk victories on test scores in math and ELA. Figure 7 plots test score residuals, separately for the four test score proficiency categories, against budget hawk margins of victory. To construct Figure 7, I first construct residual test scores in years 5 to 8 after election by regressing proficiency rates against the corresponding pre-election rates at the district and school level. I additionally include controls for demographics and year fixed-effects. I then construct means for each 0.5 percentage point bin: these are shown as points in Figure 7.[12]. Regression lines in Figure 7 are local linear regressions.

The patterns in Figure 7 show little systematic evidence of differences in scores for tests in ELA. Across all four categories, values on both sides of the threshold are reasonably similar. For math scores, there is some evidence that budget hawk victories are associated with a smaller share of students scoring in the “exceed" category, and a larger share scoring in the intermediate categories: “met" and “nearly met."

Figure 8 displays the corresponding yearly estimates for all four performance categories, which are consistent with the patterns in Figure 7. Each point in Figure 8 is a separate regression discontinuity estimate, and all regressions control for pre-election performance at the district and school level. Changes in ELA performance is largely unchanged following the election of a budget hawk. Estimates with respect to math scores suggest that the share of students exceeding expectations in math falls by 2 to 4 percentage points in the years after the election of a budget hawk. Increases appear in the two categories below, which correspond to students meeting or nearly meeting expectations. Table 7 shows point estimates for pooled year groups, with and without controls for school-specific performance in the year prior to the election; these estimates show similar patterns.

In gauging the magnitude of effects on test scores, meta-analytical estimates from Jackson and Mackevicius (2021) provide a useful point of reference. Pooling the results of 31 “credibly causal" studies, the authors estimate that “on average, a $1000 increase in per-pupil public school spending (for four years) increases test scores by 0.0352." For better comparison, I test effects on my “standardized" test score measures, which are meant to approximate student standard deviations,

[12] I weight each mean by the school's share of district totals, as described above.

in Appendix Table E1. In years 5 to 8 following the election of a budget hawk, point estimates for these measures suggest test score reductions of 0.05 to 0.10 standard deviations in math. While the context of my study differs substantially from the studies analyzed by Jackson and Mackevicius (2021), it is notable that my estimates fall reasonably close to Jackson and Mackevicius (2021)'s 90% confidence interval of [-0.007, 0.077].

3.4 Heterogeneity

Next, I assess heterogeneity along two dimensions that likely mediate the effects that budget hawks have on district operations: district size (as measured by total enrollment) and (pre-election) district spending levels (as measured by total spending per-pupil). For both variables, I separate my elections sample into elections with above-median values in the year preceding election and elections with values at or below the median.

This produces four samples: high- and low-enrollment samples, and high- and low-spending samples. To explore potentially heterogeneous responses, I estimate effects on finances and test scores within each of these samples separately. My results are shown in Table 8. Panel A displays effects on school inputs in years 1 to 4 post-election. Panel B displays effects on test scores in years 5 to 8 post-election.

Column 1 of Table 8 shows effects on large districts. In these districts, budget hawk victories appear to have little effect on school inputs: estimates for spending levels and shares are small and statistically insignificant. Alternatively, the election of a budget hawk appears to have large effects on financial outcomes in small districts: spending falls by 32 log points, and the capital share of spending declines by 12 percentage points.

Panel B displays similar patterns with respect to test scores. Large districts show little evidence of changes in test scores: point estimates for all four categorical performance measures are never above 2.5 percentage points. In small districts, the share of students who exceed expectations in math falls by 7 percentage points. Shares in the lowest two categories, for students who did not meet or nearly met expectations, both increase by approximately 3 percentage points. (Across all samples, effects on ELA are small.)

The third and fourth columns of Table 8 display results separately for districts with high and low pre-election spending. Panel A demonstrates that effects on district finances are concentrated among districts with high pre-election spending. Among these districts, total spending falls by 23 log points, and cuts are concentrated in capital spending. Among districts with relatively lower pre-election spending, I find little evidence of changes in spending levels or composition.

In Panel B, I again test for changes in test score performance. Here, there appears to be little difference in the responses of high- versus low-spending districts. This finding lends itself to two interpretations, which unfortunately I cannot distinguish between in my data. First, it is possible that, in low-spending districts (and perhaps in all districts), budget hawks make changes to other school inputs that negatively affect student achievement. Changes in teacher composition or curriculum, for example, are not reflected in aggregate district-level data. Alternatively, this finding could reflect decreasing returns to school spending, where large cuts to large budgets have similarly-sized effects as small (and statistically imprecise) cuts to small budgets.

3.5 Robustness

I present a number of additional sets of results in appendices that demonstrate the robustness of my main estimates.

First, in Appendix Figures C1 and C2, I show the sensitivity of my main results to bandwidth selection. Specifically, for my main outcomes, I reestimate the effect of a budget hawk victory, setting manual bandwidths ranging from 3 to 12 percentage points. These figures display how my estimates and confidence intervals vary when moving from a narrow to wide bandwidth. Generally, results attenuate slightly when moving to wider bandwidths, but broadly remain economically and statistically significant for a wide range of bandwidths.

Second, as described above, in Appendix C I provide estimates of my main financial and test score results after excluding large outliers. These results are similar in direction and magnitude as the results presented in the body of this paper.

Finally, my setting and research design allows for an additional test that budget hawks have a distinct effect on district finances and test scores. In Appendix D, I repeat my main event study analyses using my non-hawk finance topic in place of my budget hawk topic. Thus, the estimates shown in Appendix D reflect the causal effect of electing a school board candidate who campaigns on promises to increase the district budget. I show in Appendix D that the election of these candidates is associated with a small and statistically insignificant increase in school spending, and no effect on subsequent achievement. This effect is not surprising, given the asymmetry of school board budgeting capacity in my setting: school boards in California can reduce district spending much more easily than they can increase spending.

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 results presented in this paper suggest that local officials can and do exercise a high degree of control over district spending. The electoral victory of a budget hawk is associated with a large reduction in district spending. The magnitude of this reduction is as large as many policy-induced changes in spending that have been studied much more extensively. Results with respect to student achievement do not support the notion that the election of a budget hawk generates the Pareto improvements described above; while imprecise, the evidence presented here suggests that budget hawk victories lower student achievement in subsequent years.

A few caveats are in order, which offer motivation for future research. First, my measures of student achievement—rates of test-based proficiency—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 the election of a budget hawk leads to changes 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 other 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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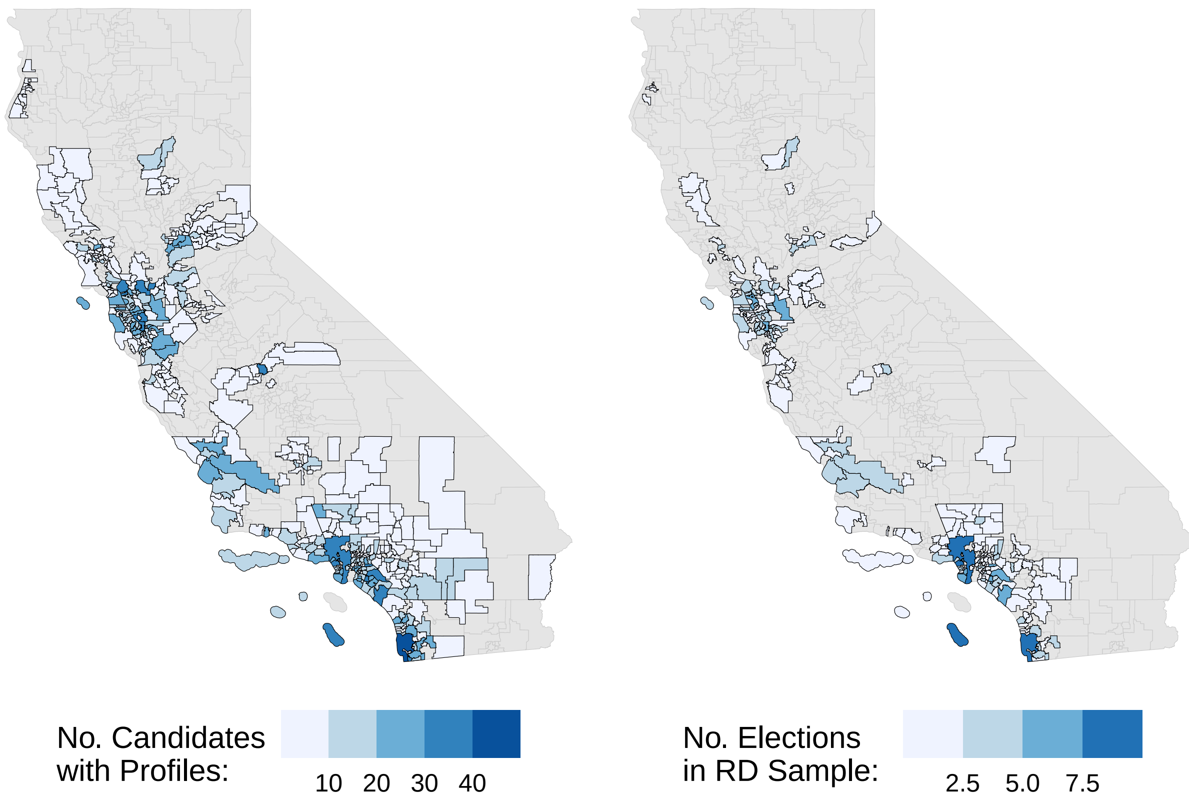
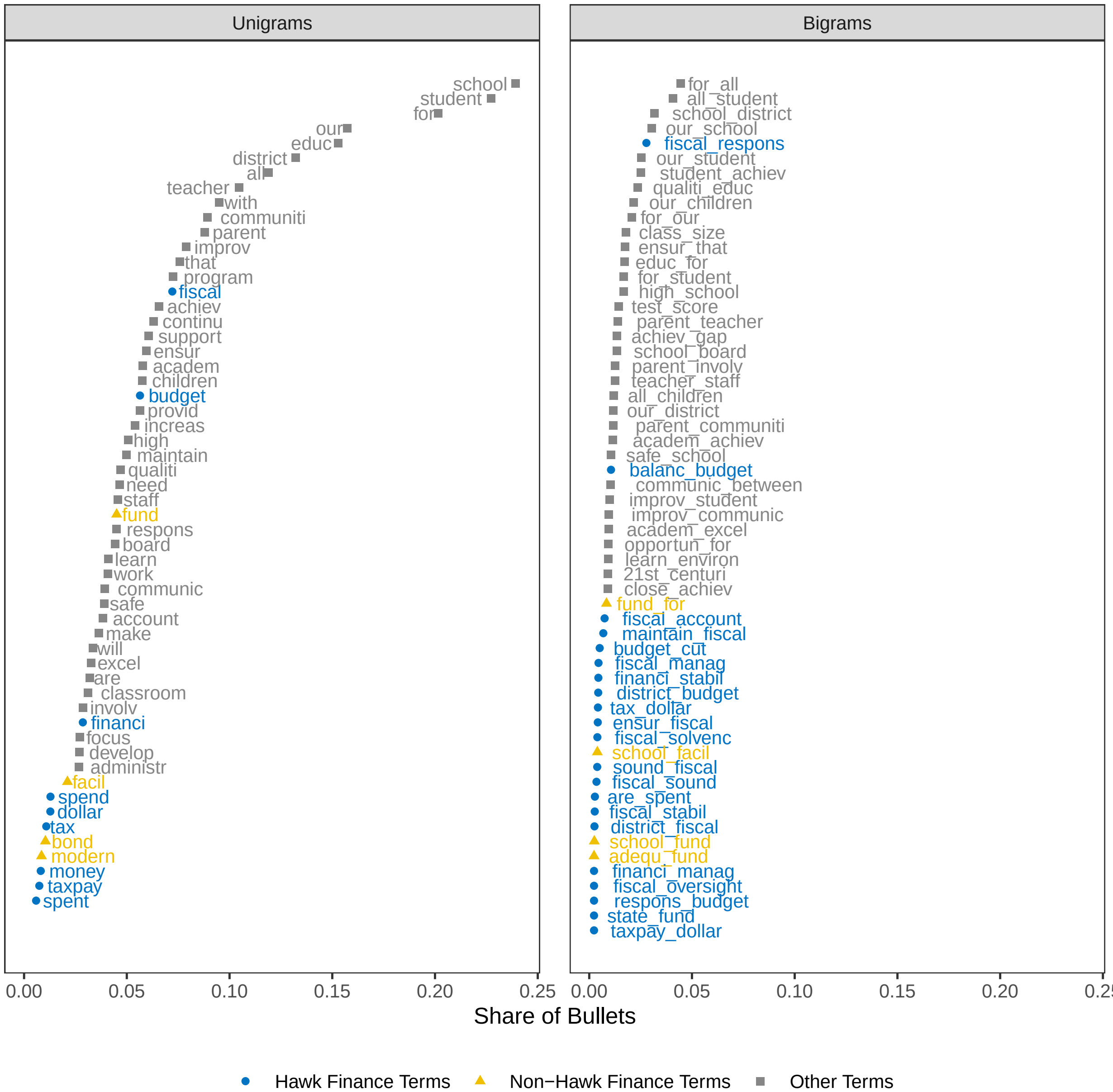
Figure 1: Geographic Distribution of Elections Data



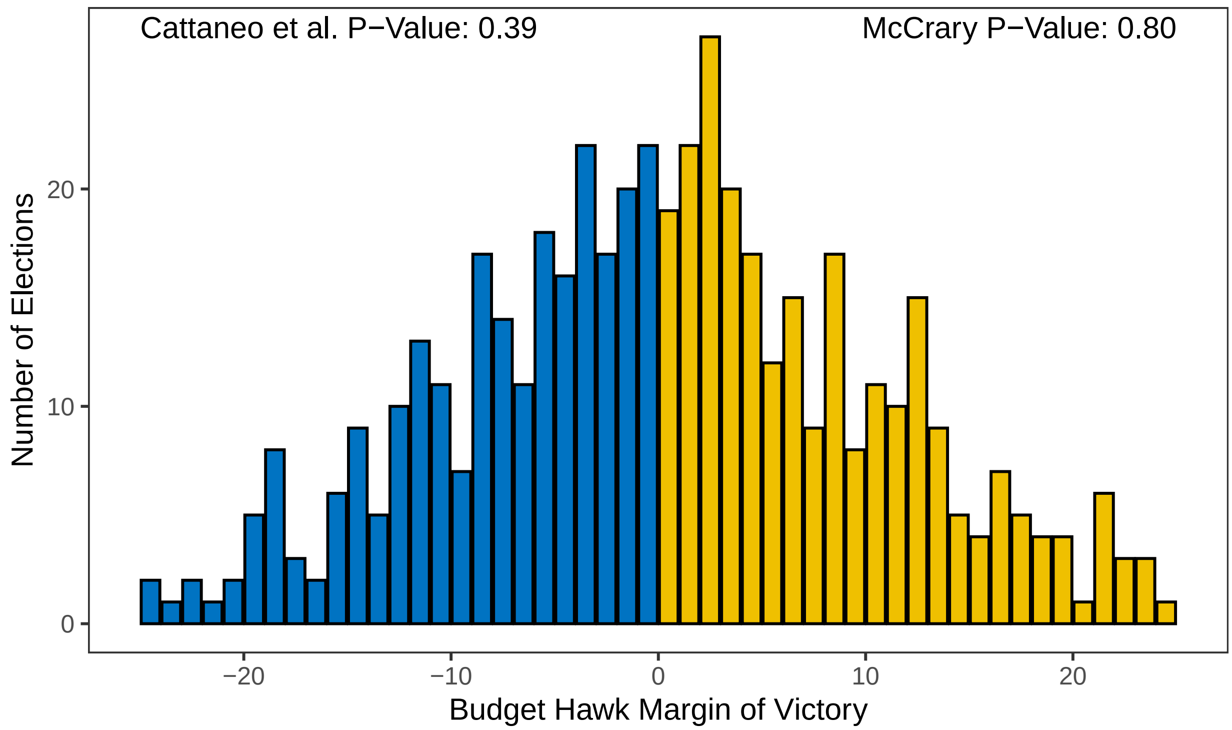
Notes: Figure displays the location of districts in elections data.

Figure 2: Most Common Unigrams and Bigrams in School Board Candidate Priorities



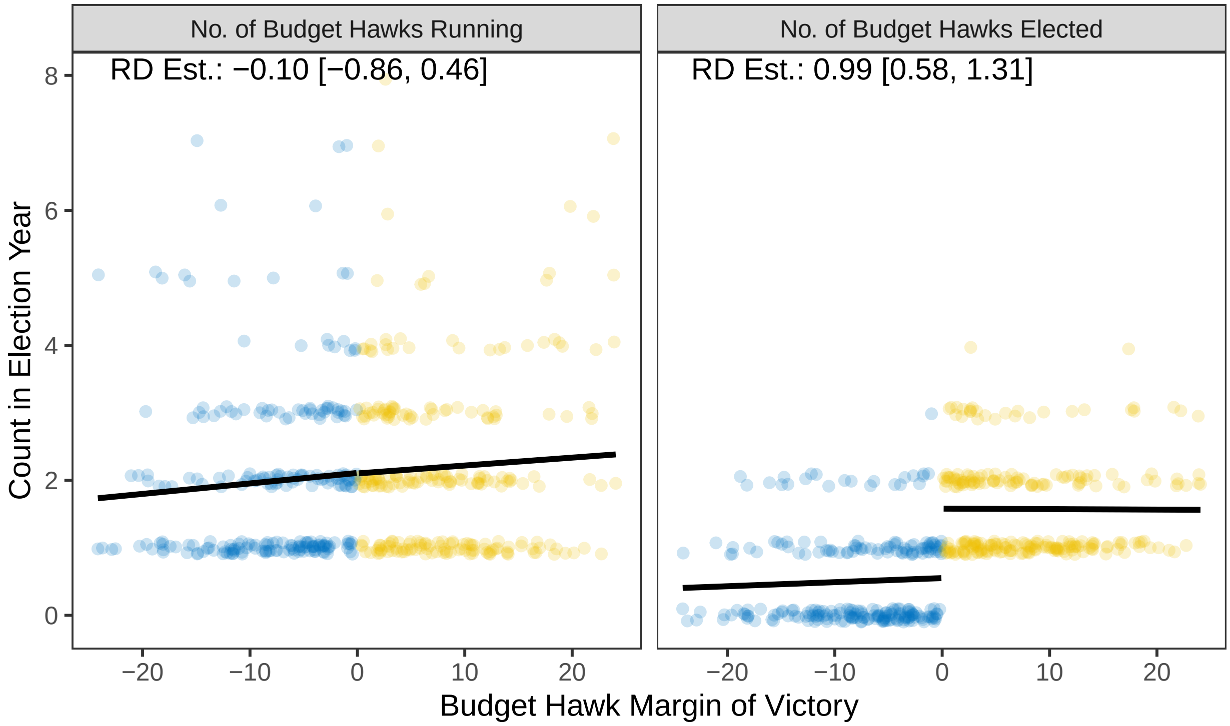
Notes: Figure displays the most common unigrams and bigrams in school board candidates' priorities. Horizontal axis measures the share of total bullets in which each unigram or bigram appears. Hawk and non-hawk finance-related terms are shown as blue circles or yellow triangles, respectively. Other terms are shown as grey squares.

Figure 3: Density of Running Variable



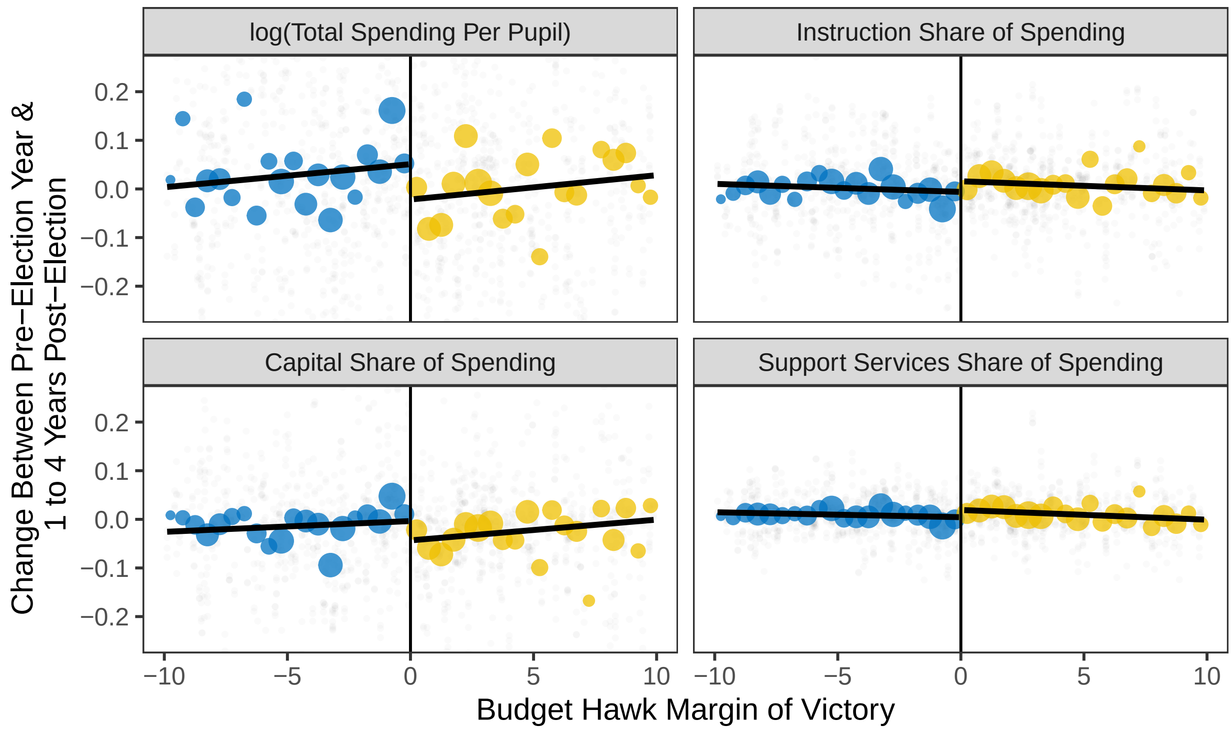
Notes: Figure displays the density of my elections as a function of running variable, the budget hawk margin of victory: v_e. P-values in the top left and right correspond to density tests proposed by Cattaneo et al. (2020) and McCrary (2008), respectively.

Figure 4: Evidence of Treatment



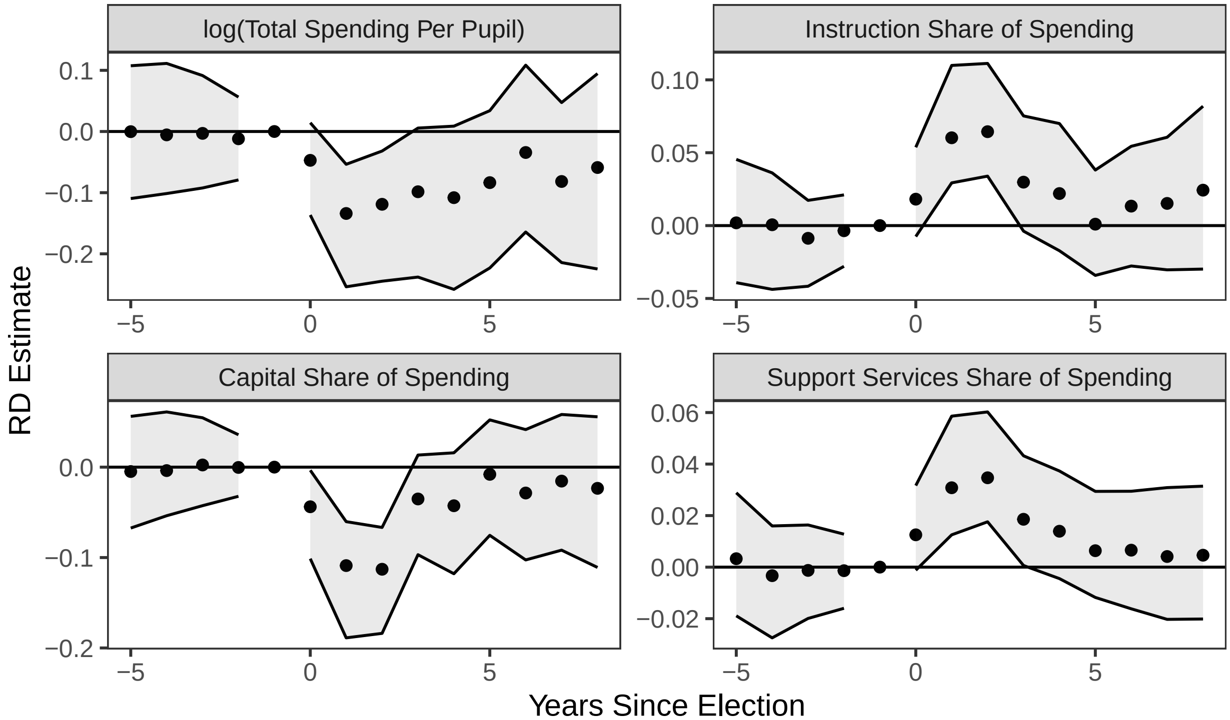
Notes: Figure displays the number of budget hawks running (in the right panel) and the number of budget hawks elected (in the left panel) as a function of the budget hawk margin of victory v_e. Outcomes are totals at the district-by-election year.

Figure 5: Effect of Budget Hawk Victory on School Inputs



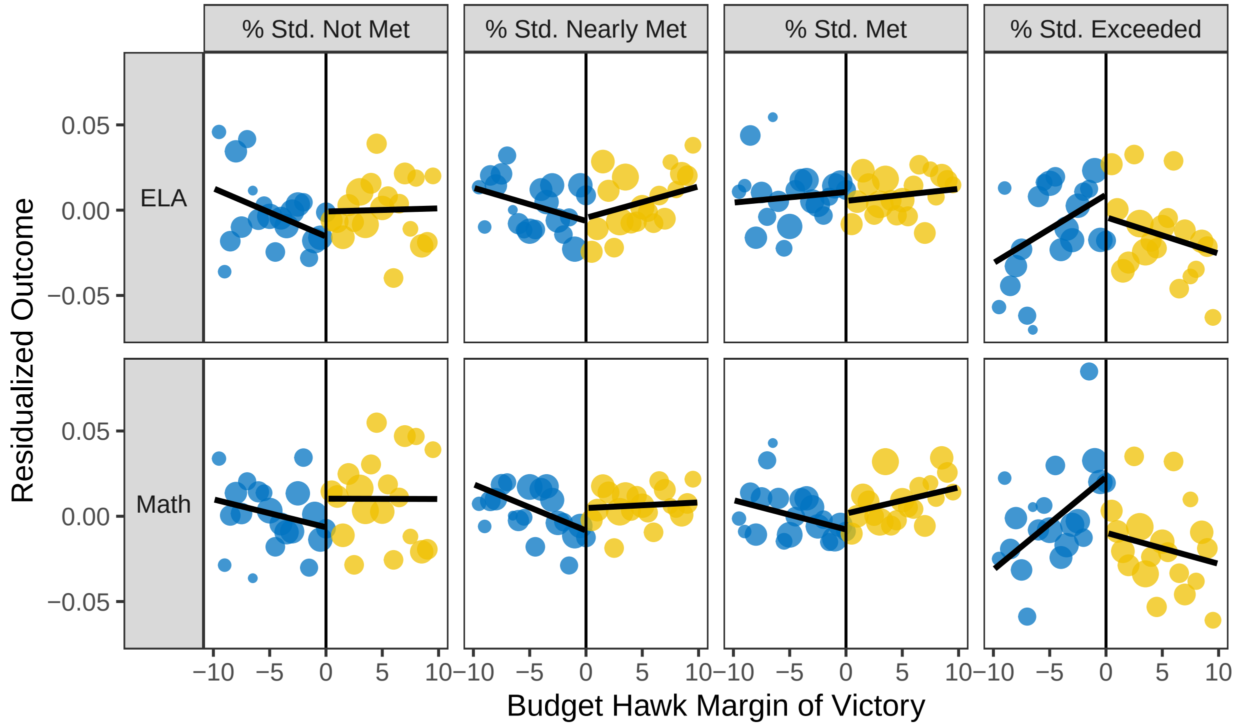
Notes: Figure displays average changes in financial outcomes between the pre-election year and 1 to 4 years post-election as a function of the budget hawk margin of victory v_e. Binned points are means for each 0.5 percentage point. The size of each binned points is proportionate to the number of election-year observations. All dollar-denominated values are in 2021 dollars.

Figure 6: Dynamic Effect of Budget Hawk Victory on School Inputs



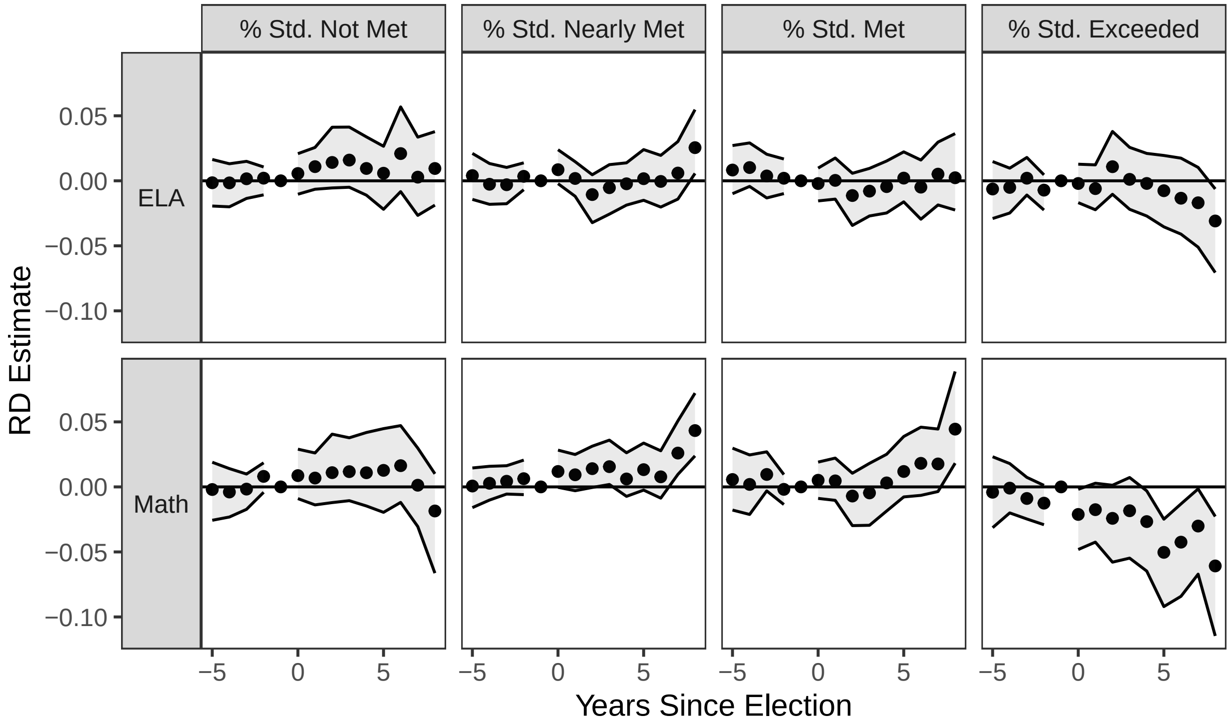
Notes: Figure displays the dynamic effect of budget hawk victory on financial outcomes. Each point is a separate regression discontinuity estimate, in which outcomes are changes since pre-election year. Confidence bands are Calonico et al. (2014) robust 95% confidence intervals. All dollar-denominated values are in 2021 dollars.

Figure 7: Effect of Budget Hawk Victory on Student Achievement



Notes: Figure displays average residualized test score performance 5 to 8 years post-election as a function of the budget hawk margin of victory v_e. Controls include pre-election performance at the district-by-grade-by-subject and school-by-grade-by-subject level, as well as school demographics and year fixed-effects. Binned points are means for each 0.5 percentage point. The size of each binned points is proportionate to the number of election-year observations.

Figure 8: Dynamic Effect of Budget Hawk Victory on Student Achievement



Notes: Figure displays the dynamic effect of budget hawk victory on test scores. Each point is a separate regression discontinuity estimate, in which outcomes are levels. All estimates include year fixed-effects, demographic controls, controls for district-grade-subject performance in the pre-election year, and school-grade-subject performance in the pre-election year. Confidence bands are Calonico et al. (2014) robust 95% confidence intervals.

Table 1: Election Summary Statistics

Outcome	All Elections	Elections with Profiles	Elections in RD Sample
Election Year	2007.60	2007.52	2007.63
	(4.44)	(4.34)	(4.17)
Number of Seats Available	2.07	2.16	2.29
	(0.87)	(0.86)	(0.83)
Number of Seats Available = 1	0.32	0.27	0.21
	(0.47)	(0.45)	(0.41)
Number of Candidates	4.07	4.33	4.93
	(1.94)	(2.00)	(2.27)
Number of Candidates with Priorities	1.39	2.39	3.62
	(1.64)	(1.51)	(1.56)
Number of Candidates per Seat	2.06	2.11	2.26
	(0.75)	(0.77)	(0.88)
Margin of Victory (pp)	10.62	9.05	7.02
	(14.50)	(11.82)	(7.97)
N Elecs.	3421	1987	535

Notes: Table displays means and standard deviations (in parentheses) of election-level variables. Election-level margin of victory refers to the difference in vote share between the winner with the least votes and the loser with the most votes.

Table 2: Representative Candidate Priorities

Bullet Text	P(Hawk)	P(Finance, Non-Hawk)
Panel A: Top Hawk Finance Bullets		
Continue to ensure that our district is operating under a fiscally responsible budget while maintaining quality education.	0.9975	0.0005
Supporting fiscal responsibility to ensure our tax dollars are spent in the classroom, not on bureaucracy.	0.9969	0.0007
To make sure that any budget cuts are made as far away from children as possible and to preserve quality programs despite budget cuts.	0.9969	0.0007
To ensure that the District operates under a fiscally responsible budget while sustaining a quality educational system	0.9967	0.0007
Use every education dollar efficiently, keeping budget cuts as far away from the classroom as possible.	0.9966	0.0008
Panel B: Top Non-Hawk Finance Bullets		
Continue to seek equal funding for our District and to implement the State's new Local Control Funding Formula LCFF	0.0006	0.9972
Lobby at the State level for adequate funding for education. Advocate for policies that will advance public education.	0.0006	0.997
Continue to seek equal funding for our district and to implement the new Local Control Funding Formula	0.0006	0.9969
Advocate for adequate local, state and federal funding for Contra Costa County Office of Education programs and services	0.0007	0.9968
Assist in planning for the continued growth of our schools which will almost certainly include construction of a new school site.	0.0007	0.9966

Notes: Table displays the candidate-written bullets with the highest values of $p_{bc,Hawk}$, the probability that the bullet concerns the hawk-related finance topic and $p_{bc,Non-Hawk}$, the probability that the bullet concerns the non-hawk-related finance topic. These probabilities are produced by the KeyATM model described in text.

Table 3: Correlates of Finance Hawk Preferences

	Dep. Var: P(Hawk)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Repub.-Endorsed	0.113***						0.107***
	(0.035)						(0.033)
Dem.-Endorsed		−0.074***					−0.083***
		(0.022)					(0.022)
Teacher			−0.036**				−0.027*
			(0.017)				(0.016)
Business Background				0.047***			0.039***
				(0.015)			(0.015)
Incumbent					0.048***		0.056***
					(0.018)		(0.018)
Elec. Year 2002						0.136***	0.134***
						(0.028)	(0.028)
Elec. Year 2004						0.148***	0.146***
						(0.027)	(0.027)
Elec. Year 2006						0.087***	0.081***
						(0.026)	(0.025)
Elec. Year 2008						0.211***	0.212***
						(0.029)	(0.028)
Elec. Year 2010						0.272***	0.276***
						(0.025)	(0.025)
Elec. Year 2012						0.188***	0.193***
						(0.028)	(0.028)
Elec. Year 2014						0.143***	0.149***
						(0.031)	(0.031)
Constant	0.462***	0.472***	0.475***	0.450***	0.457***	0.309***	0.293***
	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)	(0.020)	(0.022)
Observations	4,694	4,694	4,694	4,694	4,694	4,694	4,694
R^2	0.003	0.002	0.001	0.003	0.002	0.029	0.040
Adjusted R^2	0.002	0.002	0.001	0.002	0.002	0.027	0.037

Notes: Table displays candidate-level regression results that predict $p_{c,Hawk}$, the probability that candidate c is a budget hawk, as a function of candidate and election characteristics. Republican and Democrat-endorsed candidates are identified as candidates whose SmartVoter endorsements include the words "republican" or "democrat," respectively. Teacher candidates are identified as candidates whose SmartVoter biographies include the word "teacher." Candidates with a business background are identified as candidates whose SmartVoter biographies include any of the words "business," "executive," "mba," "ceo," or "cfo." Incumbent candidates are identified as candidates whose SmartVoter biographies include the word "incumbent." Election year fixed effects identify the listed year as well as the following year. Years 2000 and 2001 are the reference years.

Table 4: District Summary Statistics

Variable	All Dists. (1997-2018)	Dists. in RD Sample (Pre-Election Year)
District Characteristics		
Fall Enrollment	6393 [1942]	27861 [9867]
	(24152)	(87886)
Year	2007.9 [2008.0]	2006.6 [2007.0]
	(6.6)	(4.2)
Spending Levels		
Total Spending Per Pupil	18546 [13051]	17119 [13188]
	(27680)	(20347)
Capital Spending Per Pupil	1640 [784]	1773 [1229]
	(2943)	(1981)
Instruction Spending Per Pupil	8292 [6891]	7607 [6905]
	(8131)	(3727)
Support Services Spending Per Pupil	5738 [3821]	4948 [3589]
	(9803)	(7021)
Spending Shares		
Instruction Share of Spending	0.52 [0.54]	0.51 [0.53]
	(0.11)	(0.09)
Capital Share of Spending	0.10 [0.06]	0.11 [0.08]
	(0.14)	(0.10)
Support Services Share of Spending	0.31 [0.30]	0.28 [0.28]
	(0.10)	(0.05)
Financial Metrics		
Surplus Per Pupil	311 [240]	-305 [-210]
	(4898)	(2292)
Long-Term Debt Outs. Per Pupil	6078 [2870]	9040 [7123]
	(9537)	(8492)
Long-Term Debt Issued Per Pupil	833 [0]	1183 [0]
	(3148)	(2702)
Staffing		
Total Staff Per 100 Pupils	10.9 [8.9]	9.1 [8.3]
	(9.6)	(4.8)
Teaching Staff Per 100 Pupils	5.0 [4.7]	4.7 [4.6]
	(1.6)	(0.8)
Unique Districts	1946	248

Notes: Table displays means, medians, and standard deviations of district characteristics. The first column shows characteristics of all district-year combinations in California with at least 100 enrolled students. The second column displays characteristics of districts in my regression discontinuity sample in the year prior to the election. Means and medians (in brackets) are displayed above standard deviations (in parentheses). All dollar-denominated values are in 2021 dollars.

Table 5: Test Score Summary Statistics

Variable	All Tests (1999-2017)	Tests in RD Sample (Pre-Election Year)
Grade	4.58	4.48
	(1.28)	(1.25)
Subject: Math	0.51	0.51
	(0.50)	(0.50)
Standardized Score: Proficiency	0.01	0.03
	(0.44)	(0.46)
Standardized Score: Exceeded Standard	0.00	0.03
	(0.40)	(0.43)
% Std. Not Met	0.260	0.220
	(0.180)	(0.170)
% Std. Nearly Met	0.270	0.260
	(0.100)	(0.110)
% Std. Met	0.260	0.270
	(0.100)	(0.090)
% Std. Exceeded	0.210	0.250
	(0.180)	(0.200)
Students Tested	105.7	117.6
	(89.9)	(110.5)
% of Students Tested	98.57	98.58
	(1.58)	(1.60)
N	597273	63150
Unique Schools	8459	4304
Unique Districts	951	248

Notes: Table displays means and standard deviations (in parentheses) of school-by-grade-by-subject test score data. The first column summarizes all available test score data. The second column displays test score data for districts in my regression discontinuity sample in the year prior to the election.

Table 6: Effects of Budget Hawk Victory on School Inputs

Outcome	Years 1 to 4	Years 5 to 8
	Spending Levels and Shares	
log(Total Spending Per Pupil)	-0.10 [-0.21, -0.03]	-0.03 [-0.13, 0.05]
Support Services Share of Spending	0.024 [0.009, 0.046]	0.004 [-0.015, 0.025]
Capital Share of Spending	-0.071 [-0.131, -0.030]	-0.016 [-0.083, 0.043]
Instruction Share of Spending	0.041 [0.014, 0.082]	0.008 [-0.029, 0.049]
Other Share of Spending	0.008 [-0.004, 0.021]	0.002 [-0.015, 0.019]
	Financial Metrics	
Surplus Per Pupil	1541 [439, 3177]	653 [-535, 2009]
Long-Term Debt Issued Per Pupil	-89 [-2203, 1781]	-388 [-1548, 1102]
Long-Term Debt Outs. Per Pupil	-2030 [-6197, 851]	-910 [-4855, 2133]
	Staffing	
log(Total Staff Per 100 Pupils)	-0.02 [-0.08, 0.04]	-0.03 [-0.10, 0.05]
log(Teaching Staff Per 100 Pupils)	0.01 [-0.03, 0.05]	0.01 [-0.04, 0.06]
N	2116	1734
N Elecs.	535	476

Notes: Table summarizes the effects of budget hawk victory on district outcomes for separate sets of years relative to the election. Outcomes are changes since pre-election year. Calonico et al. (2014) robust 95% confidence intervals are shown in brackets. All regressions include year fixed-effects. All dollar-denominated values are in 2021 dollars.

Table 7: Effects of Budget Hawk Victory on Test Scores

Outcome	Math (1)	Math (2)	ELA (3)	ELA (4)
Years 1 to 4				
% Std. Exceeded	-0.012 [-0.044, 0.011]	-0.019 [-0.047, 0.003]	0.004 [-0.018, 0.028]	0.002 [-0.016, 0.024]
% Std. Met	-0.004 [-0.023, 0.010]	-0.001 [-0.020, 0.014]	-0.007 [-0.025, 0.006]	-0.008 [-0.025, 0.007]
% Std. Nearly Met	0.006 [-0.005, 0.019]	0.010 [-0.001, 0.025]	-0.006 [-0.023, 0.007]	-0.005 [-0.021, 0.008]
% Std. Not Met	0.010 [-0.008, 0.036]	0.009 [-0.007, 0.030]	0.008 [-0.007, 0.029]	0.011 [-0.003, 0.029]
N	132511	85969	128190	79875
N Elecs.	535	535	535	535
Years 5 to 8				
% Std. Exceeded	-0.033 [-0.074, -0.003]	-0.041 [-0.080, -0.013]	-0.007 [-0.041, 0.023]	-0.010 [-0.038, 0.016]
% Std. Met	0.008 [-0.011, 0.027]	0.015 [-0.003, 0.040]	0.001 [-0.018, 0.018]	0.000 [-0.019, 0.018]
% Std. Nearly Met	0.014 [0.000, 0.034]	0.017 [0.002, 0.037]	0.001 [-0.016, 0.020]	0.001 [-0.017, 0.020]
% Std. Not Met	0.010 [-0.016, 0.042]	0.014 [-0.014, 0.040]	0.003 [-0.022, 0.028]	0.009 [-0.017, 0.032]
N	97761	62464	94095	57904
N Elecs.	458	454	459	455
Year Fixed-Effects	Y	Y	Y	Y
Demographic Controls	Y	Y	Y	Y
Pre-Elec. Dist. Perf.	Y	Y	Y	Y
Pre-Elec. Sch Perf.	N	Y	N	Y

Notes: Table summarizes the effects of budget hawk victory on test scores for separate sets of years relative to the election. Outcomes are levels. All regressions include year fixed-effects, demographic controls, and controls for district-grade-subject performance in the pre-election year. Columns 2 and 4 include controls for school-grade-subject performance in the pre-election year. Calonico et al. (2014) robust 95% confidence intervals are shown in brackets.

Table 8: Heterogeneous Effects of Budget Hawk Victory on School Inputs and Test Scores

	Districts with:			
Outcome	High Enrollment	Low Enrollment	High Spending	Low Spending
Panel A: Effect on School Inputs, Years 1 to 4				
log(Total Spending Per Pupil)	-0.03 [-0.09, 0.04]	-0.32 [-0.59, -0.15]	-0.23 [-0.42, -0.13]	-0.03 [-0.11, 0.07]
Support Services Share of Spending	0.007 [-0.010, 0.025]	0.034 [0.007, 0.069]	0.039 [0.016, 0.074]	0.013 [-0.007, 0.037]
Capital Share of Spending	-0.033 [-0.095, 0.015]	-0.116 [-0.228, -0.037]	-0.147 [-0.245, -0.090]	-0.003 [-0.048, 0.045]
Instruction Share of Spending	0.013 [-0.018, 0.051]	0.072 [0.020, 0.147]	0.094 [0.061, 0.155]	-0.013 [-0.049, 0.015]
N	1056	1060	1053	1063
N Elecs.	267	268	267	268
Panel B: Effect on Test Scores, Years 5 to 8				
ELA				
% Std. Exceeded	-0.004 [-0.029, 0.025]	-0.010 [-0.064, 0.035]	-0.002 [-0.036, 0.030]	-0.022 [-0.063, 0.021]
% Std. Met	0.008 [-0.011, 0.029]	-0.009 [-0.047, 0.027]	0.005 [-0.024, 0.034]	-0.015 [-0.050, 0.013]
% Std. Nearly Met	0.002 [-0.019, 0.021]	-0.003 [-0.038, 0.032]	0.004 [-0.016, 0.029]	0.002 [-0.028, 0.032]
% Std. Not Met	-0.011 [-0.038, 0.011]	0.022 [-0.016, 0.062]	0.001 [-0.030, 0.033]	0.018 [-0.016, 0.047]
N	50167	7737	39829	18075
N Elecs	228	227	229	226
Math				
% Std. Exceeded	-0.022 [-0.058, 0.016]	-0.069 [-0.133, -0.023]	-0.044 [-0.090, -0.013]	-0.041 [-0.079, 0.000]
% Std. Met	0.025 [0.006, 0.051]	0.002 [-0.027, 0.032]	0.022 [-0.002, 0.051]	0.003 [-0.025, 0.035]
% Std. Nearly Met	0.007 [-0.012, 0.023]	0.033 [0.011, 0.066]	0.022 [0.009, 0.044]	0.013 [-0.008, 0.037]
% Std. Not Met	-0.011 [-0.049, 0.017]	0.036 [0.001, 0.075]	0.009 [-0.026, 0.041]	0.025 [-0.006, 0.057]
N	53951	8513	42516	19948
N Elecs	225	229	227	227

Notes: Table summarizes the effects of budget hawk victory on district outcomes and test scores for separate sets of years relative to the election. Calonico et al. (2014) robust 95% confidence intervals are shown in brackets. Outcomes in Panel A are changes since pre-election year. All regressions in Panel A include year fixed-effects. Outcomes in Panel B are levels. All regressions in Panel B include year fixed-effects, demographic controls, controls for district-grade-subject performance in the pre-election year, and school-grade-subject performance in the pre-election year. All dollar-denominated values are in 2021 dollars.

Appendix A Data Sources

This appendix details district and school-level 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 100 pupils.

A.3 State Standardized Testing Data

School-by-grade-by-subject level student achievement data comes from the California Department of Education. Data for years 2001 to 2012 come from the STAR testing regime research files. Data for years 2014 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. I standardize these shares into four discrete categories by combining STAR's two lowest categories, "below basic," and "far below basic," into one category.

I restrict my analysis to grades in which tests in Math and ELA were widely administered: grades 3 through 7. I drop any school-by-grade-by-subject observation in which less than 95% of eligible students participated in the test.

I link this data with NCES-reported school-level demographics—specifically, the share of students eligible for free lunch and the share eligible for reduced lunch—and racial composition at the school-by-grade level.

After merging with school demographics, I complete the panel, which extends from 2001 to 2012 and 2014 to 2017, as follows. First, I create two copies of 2001 data and relabel them as 1999 and 2000. This ensures that all elections have pre-election performance data. Second, I create a copy of 2012 data and label it as 2013, ensuring that elections in 2014 have pre-election performance data.

A.4 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 2021 dollars.

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 the words "and" and "the." Additionally, 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 14,157 rows and 11,958 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 250 most common unigrams and bigrams.

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.[13] Using the inputs described above, I fit a KeyATM model using the document-term matrix and the set of hawk and non-hawk finance-related terms described above, setting the number of no-keyword topics to 5.

[13] 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.

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/>

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Notes: Figure displays an example SmartVoter candidate profile, available here: Smart Voter: Stephen Pulido, November 4, 2003 Election.

Appendix C Regression Discontinuity Robustness

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

C.1 Balance Tests

Tables C1 and C2 display balance tests with respect to district inputs and test scores, respectively.

C.2 Regression Discontinuity Bandwidth

In Figures C1 and C2, 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 3 to 12 percentage points. These figures displays how my estimates and confidence intervals vary when moving from a narrow to wide bandwidth. Generally, results attenuate slightly when moving to a wider bandwidths, but broadly remain economically and statistically significant for a wide range of bandwidths.

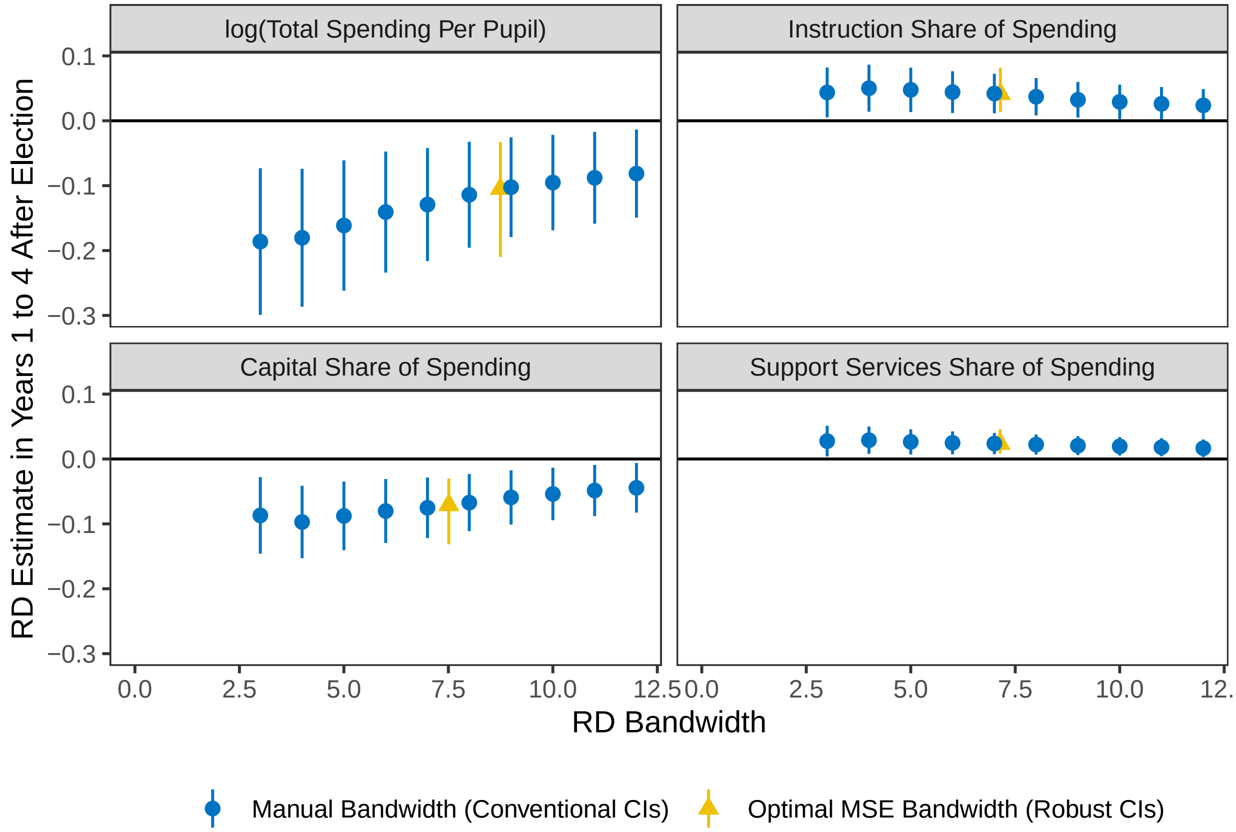
C.3 Excluding Pre-Election Spending Outliers

As noted in the body of the paper, Table C1 suggests that a large but statistically insignificant difference in pre-election spending between districts in which a budget hawk narrowly won versus districts in which a budget hawk narrowly lost. While not statistically significant, this difference is large enough—roughly 17 log points—to justify further attention.

This difference is driven by a small number of elections in which pre-election spending was extremely high. To show this, I repeat my main analyses after excluding districts for which total spending per pupil was greater than $20,0000. This exclusion affects a very small number of elections (50 out of 539 elections total), but results in a much more precisely estimated zero in tests for pre-election balance.

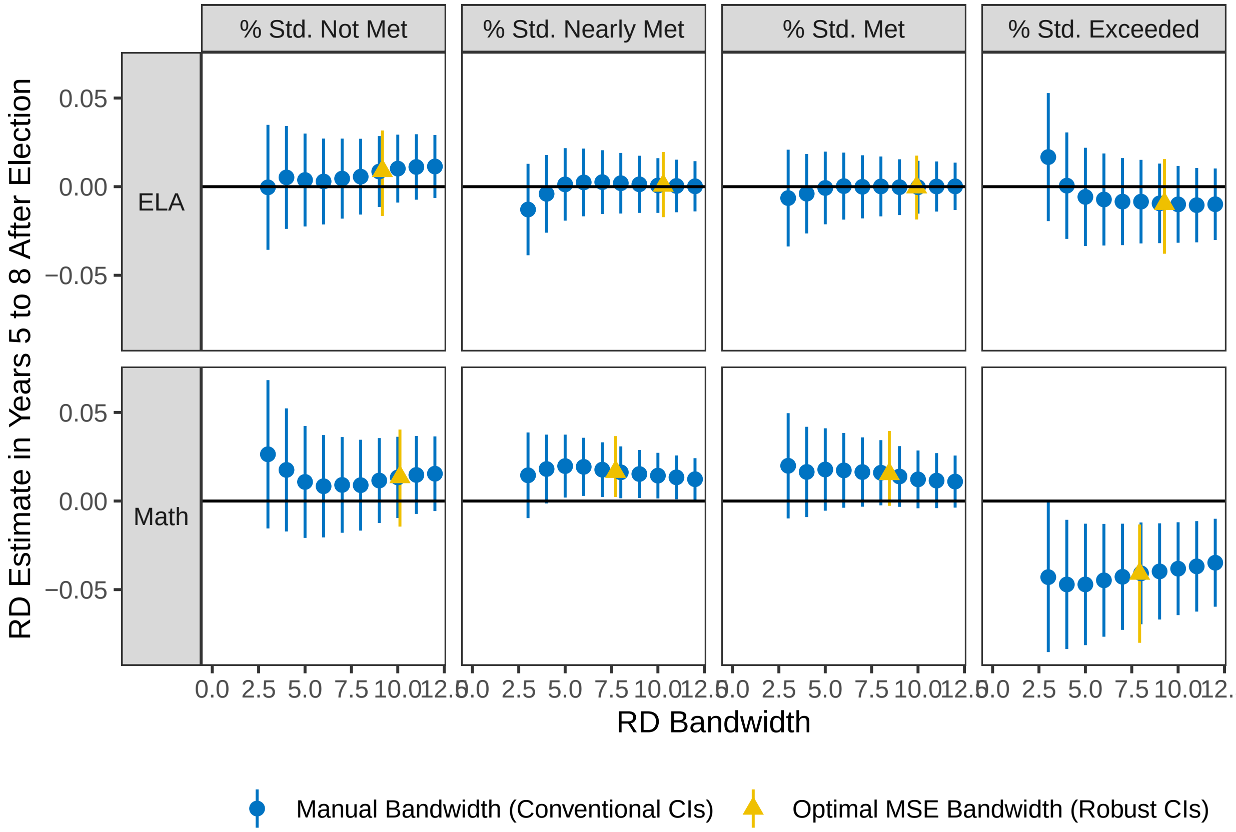
Table C3 recreates Table C1 after the exclusion of pre-election spending outliers. Figure C3 recreates Figure 6 after the same exclusion. Finally, Figure C4 recreates Figure 8.

Figure C1: Bandwidth Tests: School Inputs



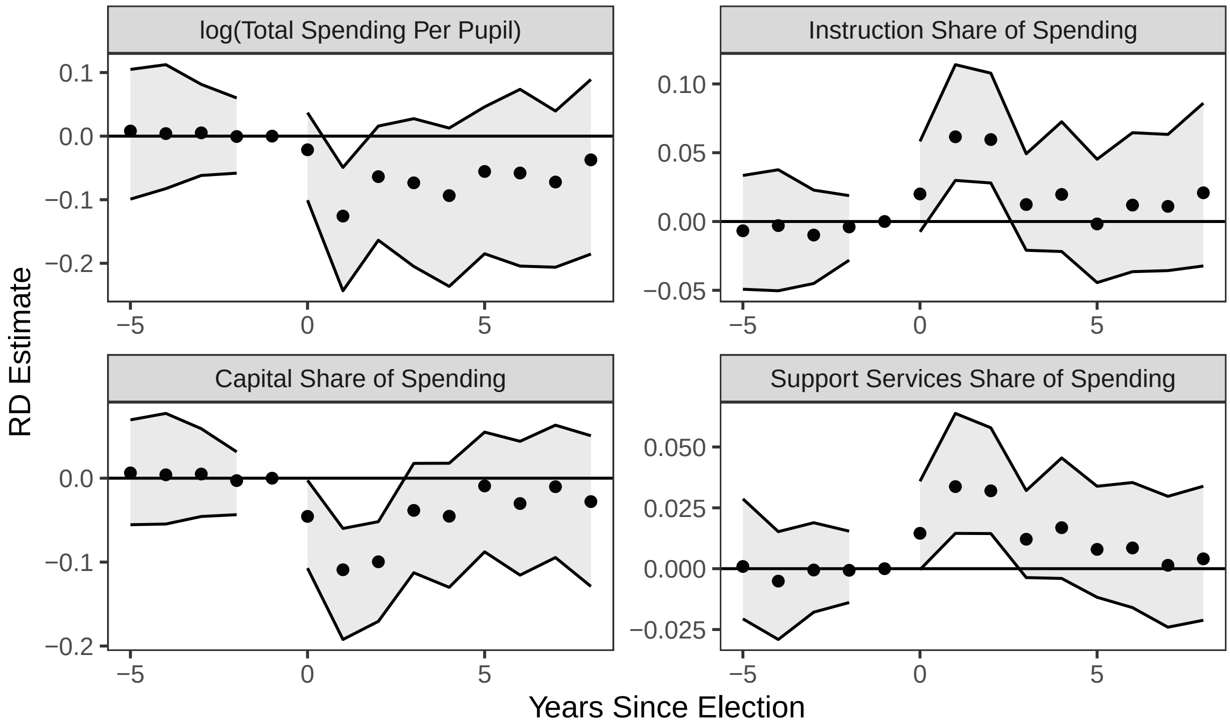
Notes: Figure displays the estimated effect of budget hawk victory on district outcomes in years 1 to 4 after election. Calonico et al. (2014) conventional 95% confidence intervals are shown around blue circles. Main estimates, which use Calonico et al. (2014) MSE optimal bandwidth and Calonico et al. (2014) robust 95% confidence intervals are shown around yellow triangles. Outcomes are changes since pre-election year. All regressions include year fixed-effects. All dollar-denominated values are in 2021 dollars.

Figure C2: Bandwidth Tests: Tests Scores



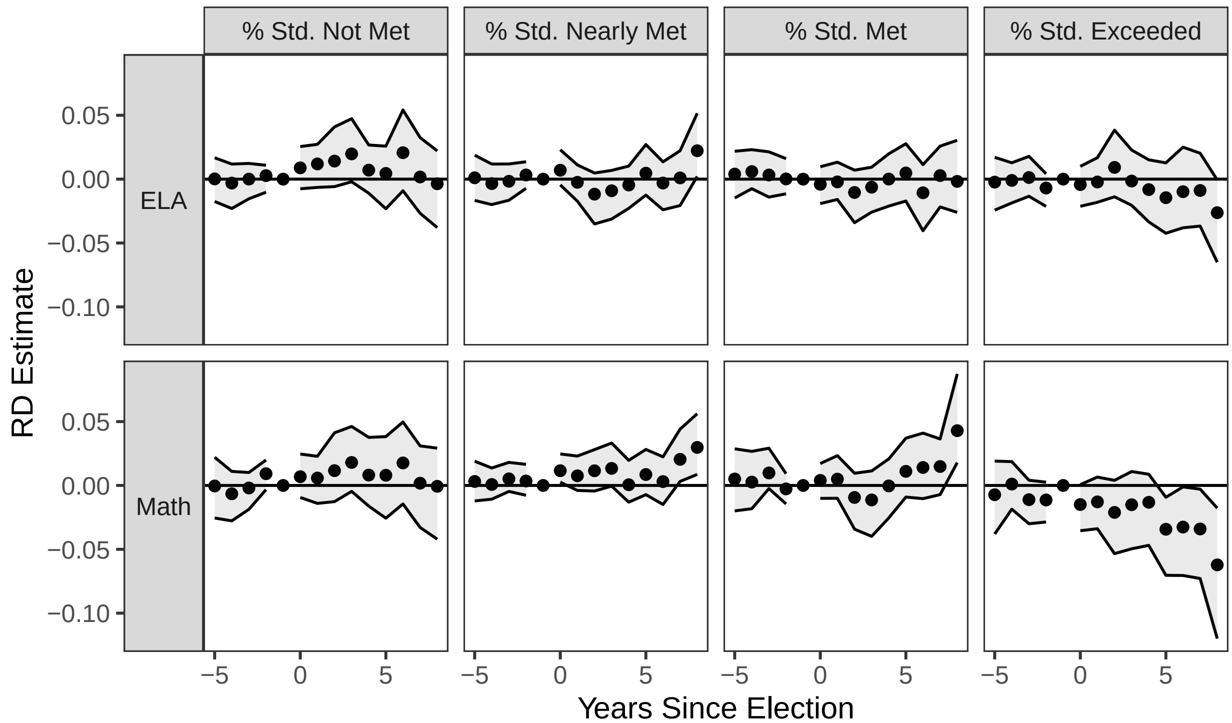
Notes: Figure displays the estimated effect of budget hawk victory on test scores in years 5 to 8 after election. Calonico et al. (2014) conventional 95% confidence intervals are shown around blue circles. Main estimates, which use Calonico et al. (2014) MSE optimal bandwidth and Calonico et al. (2014) robust 95% confidence intervals are shown around yellow triangles. Outcomes are levels. All regressions include year fixed-effects, demographic controls, controls for district-grade-subject performance in the pre-election year, and controls for school-grade-subject performance in the pre-election year.

Figure C3: Dynamic Effect of Budget Hawk Victory on School Inputs (Excluding Pre-Election Outliers)



Notes: Figure displays the dynamic effect of budget hawk victory on financial outcomes. Each point is a separate regression discontinuity estimate. Outcomes are changes since pre-election year. Confidence bands are Calonico et al. (2014) robust 95% confidence intervals. Sample excludes elections with pre-election per-pupil spending above $20,000. All dollar-denominated values are in 2021 dollars.

Figure C4: Dynamic Effect of Budget Hawk Victory on Test Scores (Excluding Pre-Election Outliers)



Notes: Figure displays the dynamic effect of budget hawk victory on test scores. Each point is a separate regression discontinuity estimate. Controls include pre-election performance at the district-by-grade-by-subject and school-by-grade level, as well as school demographics and year fixed-effects. Outcomes are levels. Confidence bands are Calonico et al. (2014) robust 95% confidence intervals. Sample excludes elections with pre-election per-pupil spending above $20,000.

Table C1: Balance Tests

Outcome	Estimate	Robust CI
Panel A: Levels in Pre-Election Year		
log(Total Spending Per Pupil)	0.170	[-0.02, 0.40]
Support Services Share of Spending	-0.003	[-0.029, 0.021]
Capital Share of Spending	0.039	[-0.004, 0.096]
Instruction Share of Spending	-0.041	[-0.093, 0.003]
Other Share of Spending	0.012	[-0.029, 0.050]
Surplus Per Pupil	-908.500	[-1930, -149]
Long-Term Debt Issued Per Pupil	-384.205	[-2593, 1353]
Long-Term Debt Outs. Per Pupil	-580.007	[-4494, 4184]
log(Total Staff Per 100 Pupils)	0.109	[-0.06, 0.29]
log(Teaching Staff Per 100 Pupils)	0.008	[-0.08, 0.09]
N Elecs.	535	
Panel B: Pre-Election Trends		
log(Total Spending Per Pupil)	-0.012	[-0.08, 0.06]
Support Services Share of Spending	-0.001	[-0.016, 0.013]
Capital Share of Spending	-0.0003	[-0.032, 0.036]
Instruction Share of Spending	-0.004	[-0.028, 0.021]
Other Share of Spending	0.007	[-0.002, 0.018]
Surplus Per Pupil	103.280	[-804, 1056]
Long-Term Debt Issued Per Pupil	614.995	[-1352, 2373]
Long-Term Debt Outs. Per Pupil	717.657	[-702, 2449]
log(Total Staff Per 100 Pupils)	-0.034	[-0.09, 0.02]
log(Teaching Staff Per 100 Pupils)	-0.040	[-0.09, 0.00]
N Elecs.	534	

Notes: Table displays results of regression discontinuity tests for differences in levels or trends in the years prior to election. In Panel A, outcomes are levels in pre-election year. In Panel B, outcomes are $\Delta y_{j,e,t-2}$, changes between pre-election year and the year prior. Calonico et al. (2014) robust 95% confidence intervals are shown in brackets. All dollar-denominated values are in 2021 dollars.

Table C2: Balance Tests: Test Score Data

Outcome	Estimate	Robust CI
Panel A: Levels in Pre-Election Year		
ELA		
Standardized Score: Proficiency	0.004	[-0.07, 0.08]
Standardized Score: Exceeded Standard	-0.026	[-0.12, 0.07]
% Std. Not Met	-0.006	[-0.029, 0.023]
% Std. Nearly Met	0.008	[-0.014, 0.029]
% Std. Met	0.018	[-0.003, 0.043]
% Std. Exceeded	-0.017	[-0.059, 0.024]
Math		
Standardized Score: Proficiency	0.029	[-0.06, 0.13]
Standardized Score: Exceeded Standard	0.015	[-0.09, 0.13]
% Std. Not Met	-0.005	[-0.036, 0.029]
% Std. Nearly Met	-0.009	[-0.033, 0.010]
% Std. Met	0.011	[-0.011, 0.035]
% Std. Exceeded	0.004	[-0.043, 0.056]
Panel B: Pre-Election Trends		
ELA		
Standardized Score: Proficiency	-0.006	[-0.03, 0.03]
Standardized Score: Exceeded Standard	-0.016	[-0.05, 0.02]
% Std. Not Met	0.002	[-0.011, 0.011]
% Std. Nearly Met	0.003	[-0.007, 0.014]
% Std. Met	0.002	[-0.010, 0.017]
% Std. Exceeded	-0.007	[-0.022, 0.005]
Math		
Standardized Score: Proficiency	-0.031	[-0.07, 0.01]
Standardized Score: Exceeded Standard	-0.026	[-0.06, 0.01]
% Std. Not Met	0.008	[-0.004, 0.019]
% Std. Nearly Met	0.006	[-0.006, 0.021]
% Std. Met	-0.002	[-0.014, 0.009]
% Std. Exceeded	-0.013	[-0.029, 0.001]

Notes: Table displays results of regression discontinuity tests for differences in levels or trends in the years prior to election. In Panel A, outcomes are levels in pre-election year, and controls include year fixed-effects and demographic controls. In Panel B, outcomes are levels two years prior to the election, and controls include year fixed-effects, demographic controls, controls for district-grade-subject performance in the pre-election year, and school-grade-subject performance in the pre-election year. Calonico et al. (2014) robust 95% confidence intervals are shown in brackets. All dollar-denominated values are in 2021 dollars.

Table C3: Balance Tests (Excluding Pre-Election Outliers)

Outcome	Estimate	Robust CI
Panel A: Levels in Pre-Election Year		
log(Total Spending Per Pupil)	0.044	[-0.03, 0.14]
Support Services Share of Spending	-0.010	[-0.036, 0.013]
Capital Share of Spending	0.048	[0.004, 0.113]
Instruction Share of Spending	-0.011	[-0.049, 0.025]
Other Share of Spending	-0.006	[-0.032, 0.018]
Surplus Per Pupil	-621.032	[-1578, 95]
Long-Term Debt Issued Per Pupil	-407.320	[-2618, 1305]
Long-Term Debt Outs. Per Pupil	-786.801	[-4711, 3640]
log(Total Staff Per 100 Pupils)	0.027	[-0.05, 0.09]
log(Teaching Staff Per 100 Pupils)	-0.014	[-0.08, 0.04]
N Elecs.	487	
Panel B: Pre-Election Trends		
log(Total Spending Per Pupil)	-0.001	[-0.06, 0.06]
Support Services Share of Spending	-0.001	[-0.014, 0.015]
Capital Share of Spending	-0.003	[-0.043, 0.031]
Instruction Share of Spending	-0.004	[-0.028, 0.019]
Other Share of Spending	0.006	[-0.004, 0.017]
Surplus Per Pupil	-182.921	[-955, 615]
Long-Term Debt Issued Per Pupil	357.583	[-1592, 2147]
Long-Term Debt Outs. Per Pupil	638.897	[-657, 2136]
log(Total Staff Per 100 Pupils)	-0.023	[-0.08, 0.03]
log(Teaching Staff Per 100 Pupils)	-0.024	[-0.07, 0.01]
N Elecs.	486	

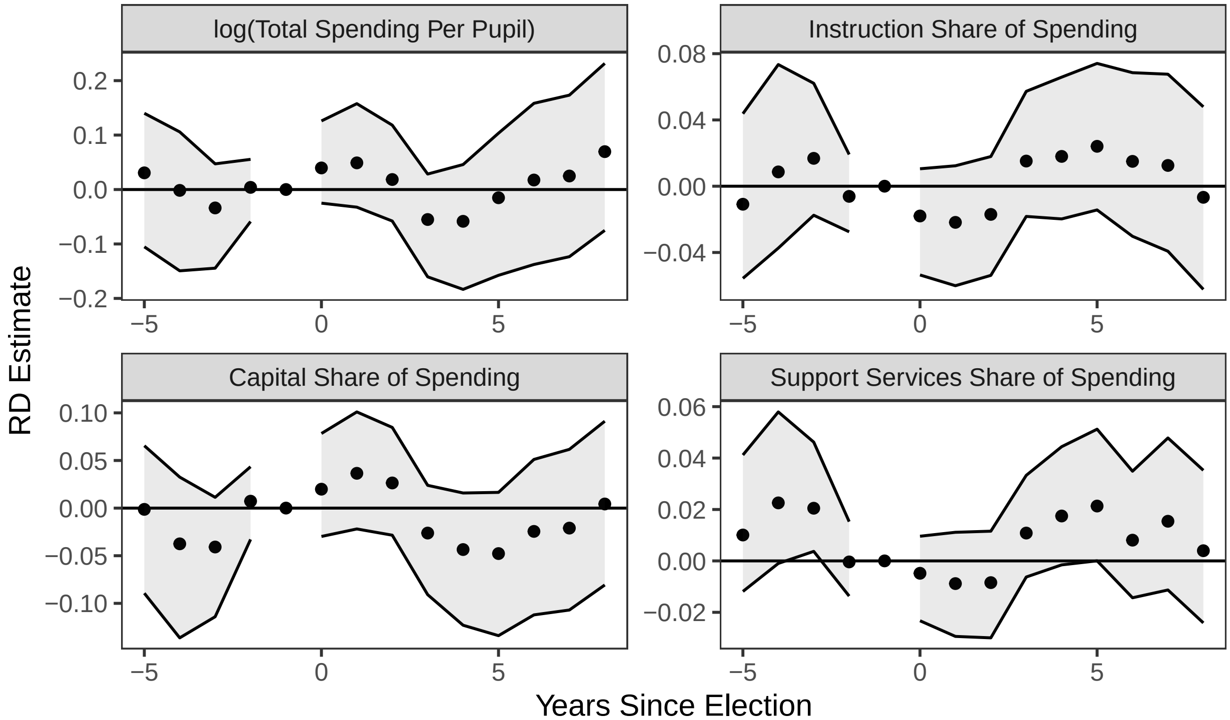
Notes: Table displays results of regression discontinuity tests for differences in levels or trends in the years prior to election. Calonico et al. (2014) robust 95% confidence intervals are shown in brackets. Sample excludes elections with pre-election per-pupil spending above $20,000. All dollar-denominated values are in 2021 dollars.

Appendix D Estimated Effects of Non-Hawk Finance Candidate Victory

As discussed in the body of the paper, my KeyATM model includes a non-hawk finance topic that identifies candidates focused on raising additional funds, often for capital projects. In this appendix, I present a set of results that mirror my main estimates, replacing $p_{c,Hawk}$ with $p_{c,Non-Hawk}$. These estimates identify the effect of the election of a non-Hawk financially-oriented candidate. Table 2 provides examples of candidate statements that align most closely with this topic.

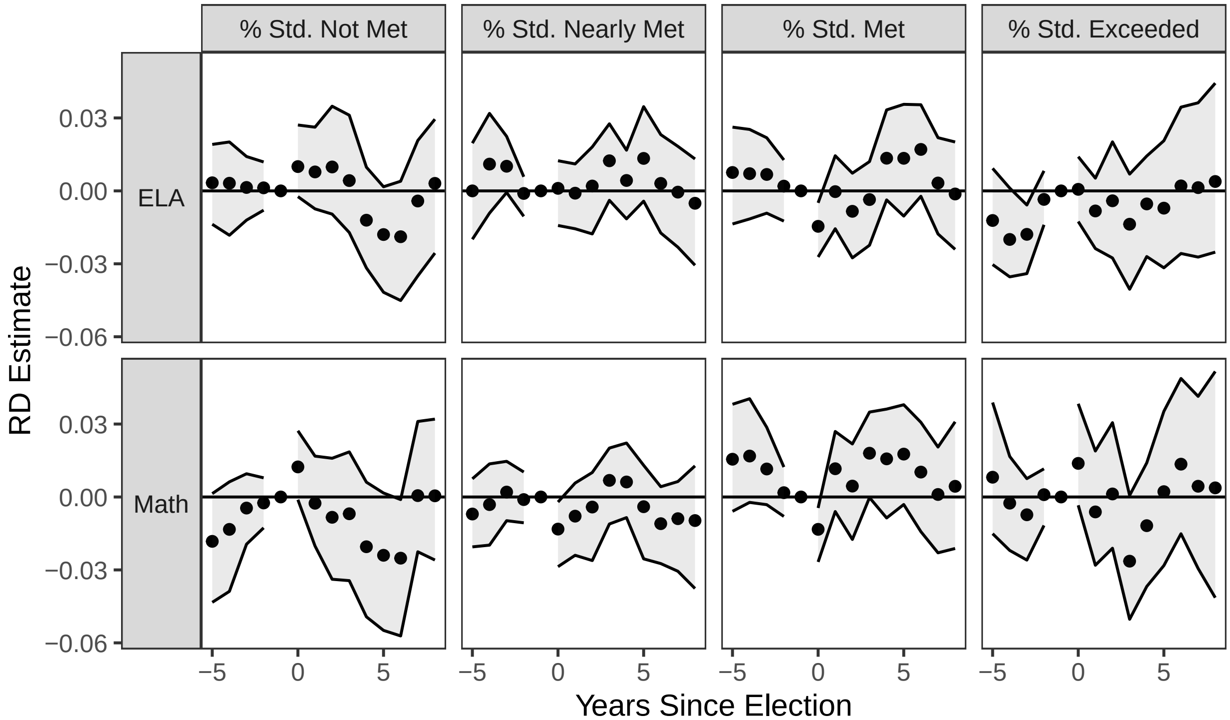
Figure D1 recreates Figure 6, showing the effect of non-hawk finance candidate victory on spending levels and shares. Figure D2 recreates Figure 8, showing the effect of non-hawk finance candidate victory on test scores.

Figure D1: Effect of Non-Hawk Financial Candidate Victory on School Inputs



Notes: Figure displays the dynamic effect of non-hawk finance candidate victory on financial outcomes. Each point is a separate regression discontinuity estimate. Confidence bands are Calonico et al. (2014) robust 95% confidence intervals. All dollar-denominated values are in 2021 dollars.

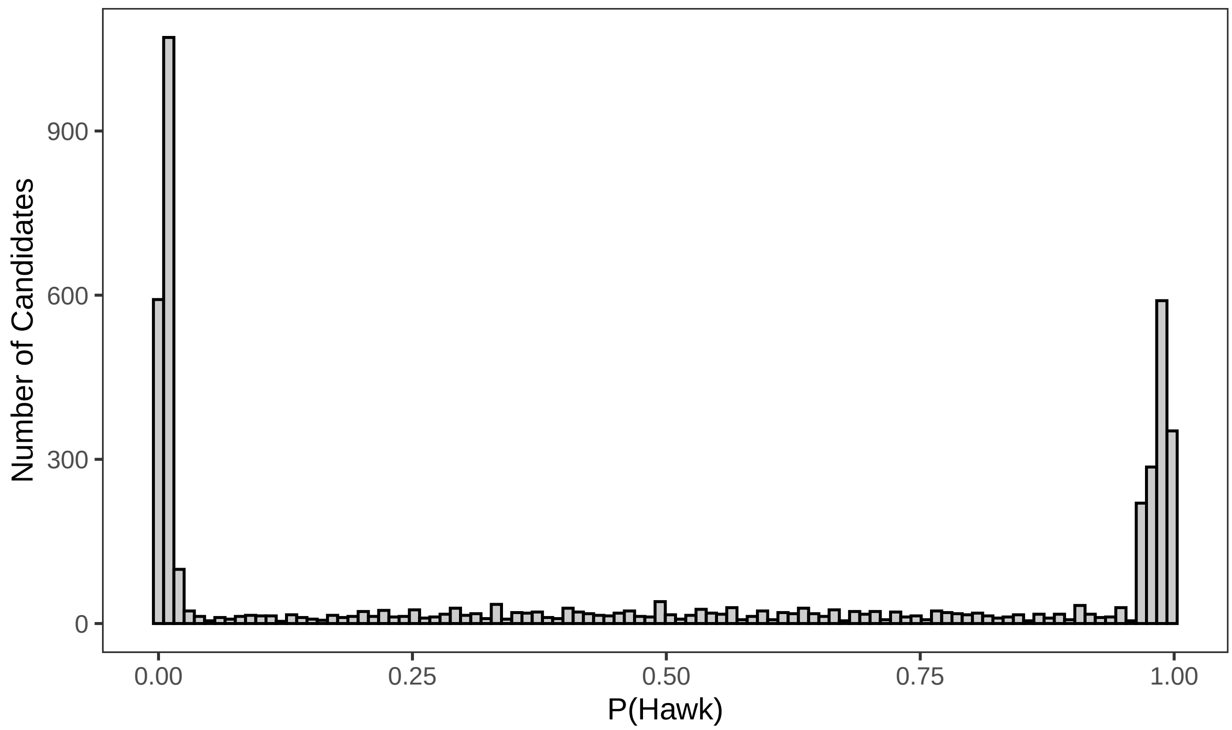
Figure D2: Effect of Non-Hawk Financial Candidate Victory on Test Scores



Notes: Figure displays the dynamic effect of non-hawk finance candidate victory on test scores. Each point is a separate regression discontinuity estimate. All estimates include year fixed-effects, demographic controls, controls for district-grade-subject performance in the pre-election year, and school-grade-subject performance in the pre-election year. Confidence bands are Calonico et al. (2014) robust 95% confidence intervals.

Appendix E Additional Tables and Figures

Figure E1: Distribution of $p_{c,Hawk}$ Among School Board Candidates



Notes: Figure displays the distribution of $p_{c,Hawk}$ among school board candidates. $p_{c,Hawk}$ is an estimate of the probability that candidate c is a budget hawk and is based on campaign statements in SmartVoter data.

Table E1: Effects of Budget Hawk Victory on Test Scores

	Math		ELA	
Outcome	(1)	(2)	(3)	(4)
	Years 1 to 4			
Standardized Score: Proficiency	-0.04 [-0.11, 0.01]	-0.05 [-0.12, 0.00]	0.00 [-0.05, 0.05]	-0.01 [-0.04, 0.03]
Standardized Score: Exceeded Standard	-0.04 [-0.12, 0.02]	-0.05 [-0.13, 0.00]	0.00 [-0.05, 0.06]	0.00 [-0.04, 0.04]
N	132511	85969	128190	79875
N Elecs.	535	535	535	535
	Years 5 to 8			
Standardized Score: Proficiency	-0.06 [-0.15, 0.01]	-0.06 [-0.14, 0.00]	-0.01 [-0.08, 0.05]	-0.03 [-0.08, 0.03]
Standardized Score: Exceeded Standard	-0.07 [-0.17, 0.00]	-0.10 [-0.19, -0.03]	-0.02 [-0.10, 0.04]	-0.04 [-0.10, 0.01]
N	97761	62464	94095	57904
N Elecs.	458	454	459	455
Year Fixed-Effects	Y	Y	Y	Y
Demographic Controls	Y	Y	Y	Y
Pre-Elec. Dist. Perf.	Y	Y	Y	Y
Pre-Elec. Sch Perf.	N	Y	N	Y

Notes: Table summarizes the effects of budget hawk victory on test scores for separate sets of years relative to the election. All regressions include year fixed-effects, demographic controls, and controls for district-grade-subject performance in the pre-election year. Columns 2 and 4 include controls for school-grade-subject performance in the pre-election year. Calonico et al. (2014) robust 95% confidence intervals are shown in brackets.