Election Timing and the Composition of the Electorate*

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Abstract

There is considerable debate about how election timing shapes who votes, election outcomes, and, ultimately, public policy. We examine these matters by combining information on more than 10,000 school tax referenda with detailed micro-targeting data on voters participating in each election. The analysis confirms that timing influences voter composition in terms of partisanship, ideology, and the numerical strength of powerful interest groups. But, in contrast to prominent theories of election timing, these effects are modest in terms of their likely impact on election outcomes. Instead, timing has the most significant impact on voter age, with the elderly being the most over-represented group in low-turnout special elections. The electoral (and policy) implications of this effect vary between states, and we offer one explanation for this variation.

Keywords: election timing, school bonds, school referenda, direct democracy, turnout

Word Count: 9,304

Participation in American subnational elections follows a predictable pattern (e.g., Hajnal and Lewis 2003). Turnout is highest when state and local governments hold their elections on the same day as high-profile federal contests — particularly in November of presidential election years. It declines significantly during midterm and off-year November elections, and it is even lower during irregularly scheduled special elections. Indeed, there is something approaching a consensus about the relationship between election timing and how many voters cast ballots in state and local elections.

There remains considerable debate, however, about how timing shapes *who* votes and the consequences for public policy. One school of thought argues that low-turnout elections privilege "high demanders" made up of narrow, well-organized groups whose pecuniary interests are at stake (e.g., Anzia 2013, Berry 2009, Dunne, Reed and Wilbanks 1997, Pecquet, Coats and Yen 1996). Because members of such interest groups are expected to vote regardless of when an election is held, lower overall turnout amplifies their influence and increases the chance that they and their allies will cast the pivotal vote.

A second stream of research argues instead that off-cycle elections tend to discourage participation among the young, the economically disadvantaged, and minorities (e.g., Bridges 1997).[1] Since these demographic characteristics are also correlated with liberal policy preferences and support for Democratic candidates (Citrin, Schickler and Sides 2003), this perspective suggests that low turnout elections should stack the deck in favor of more conservative policies. Consistent with this view, Bechtel, Hangartner and Schmid (2016) find that the introduction of compulsory voting in parts of Switzerland significantly increased voter support for leftist policies, suggesting that right-leaning voters tend to rep-

[1]For cross-national evidence on the compositional consequences of low turnout, see also Dassonneville, Hooghe and Miller (2017), Hoffmann, León and Lombardi (2017) and Singh (2015).

resent a larger share of the electorate when overall turnout is low.

These two perspectives do not necessarily lead to conflicting expectations about electoral outcomes, since there is no reason to think conservative voters will always oppose the policy priorities favored by well-organized interest groups. But they lead to divergent predictions when electoral competition focuses on the size of government. Since worker compensation accounts for an overwhelming majority of local government operational spending, public employees have a clear interest in protecting and expanding government budgets (Anzia and Moe 2015). By contrast, conservative voters are generally thought to have a distaste for government spending, a pattern that holds at the local level as well (Einstein and Kogan 2016).

We draw on two original datasets to adjudicate between these theories. The first covers more than 10,000 school-related tax and bond referenda considered by voters in California, Ohio, Texas, and Wisconsin over the past 15 years or so. The expansive temporal coverage of the data allows us to examine how election outcomes differ *within* the same school district, depending on when its measures appear on the ballot. The second draws from the Catalist national voter file and includes a variety of commercial and proprietary microtargeting data about voters who participated in these elections, allowing us to characterize how the composition of the electorate varies depending on the timing of the election. By providing information on the demographic and occupational background of voters who turn out in each election, the Catalist data allow us to directly assess the hypothesized relationships between when an election is held and who casts their ballots. Although much of the existing academic research (and conventional wisdom among practitioners) focuses specifically on how election timing shapes the composition of the electorate, this key mechanism has not been examined using a credible empirical strategy.[2]

[2] One exception is Moe (2006), which we discuss at greater length below.

The analysis confirms that election timing has significant consequences for aggregate voter turnout. It also reveals differences in the partisanship, ideology, demographics, and occupational background of voters across election dates. In particular, consistent with Moe (2006), we find that teachers and other public school employees are more likely to participate in off-cycle elections compared to other voters. But even in low-turnout special elections, education employees rarely account for more than 10 percent of the overall electorate. The results do indicate, however, that there is a large compositional change with respect to voter age. The share of elderly voters (who are least likely to have school-aged children) is approximately 20-40 percent greater in special elections than in presidential elections, accounting for about half of the total electorate when turnout is low.

The large fluctuation in the share of elderly voters corresponds to observed variation in the passage rate of school district referenda. In three of the four states we examine, the probability that a school district tax or bond measure passes is lowest during off-cycle special elections. The exception is Texas, where school property taxes for those over 65 are capped. The results suggest that, because state law makes additional school tax and bonds nominally free to seniors, school-related measures may be significantly more likely to pass on days when older voters represent a larger share of the electorate. Thus, to some extent, our results echo the conclusions that Hamilton and Cohen (1974) drew more than four decades ago: "There is no turnout-outcome law; the relationship tends to be positive in some communities and negative in others."

These findings provide strong reason to reconsider the claim that interest groups — in our context, teachers unions — dominate off-cycle special elections and that this explains other important policy outcomes, such as the generosity of teacher compensation (Anzia 2011; 2012). Although we do find that teachers and other school employees represent a larger share of the electorate in off-cycle elections, the difference is far smaller than the typical margin of victory in these contests. We conclude with several potential explanations that may help reconcile our findings with the extant literature.

The Logic(s) of Election Timing

Although election timing surely matters in a variety of policy domains, local school tax referenda provide a particularly informative context in which to situate our empirical inquiry because competing theories yield unambiguous and conflicting predictions. Outside of the scholarly literature, there are also several inconsistent conventional wisdoms popular among local districts and the political consultants who advise them.

Instructional spending represents by far the largest category of expenditures in local school budgets, and the compensation of classroom teachers accounts for most of these costs. Indeed, when combined with the salaries and benefits of school and district administrators, support staff (including counselors and aides), and workers involved in maintaining physical facilities and providing transportation to students, employee compensation accounts for more than 80 percent of total education expenditures by school districts (National Center for Education Statistics 2016). Since education employees are one of the best organized interest groups in local school elections (Anzia 2011), they should be particularly motivated to participate in school tax referenda, knowing that their future compensation hangs in the balance.

The connection between capital investments (which are funded by bond referenda) and employee interests is less clear, but there is evidence that education employees take a keen interest in school facilities (for an overview, see Gunter 2016). The quality of school buildings has a direct impact on working conditions and has been shown to affect student achievement (Cellini, Ferreira and Rothstein 2010, Hong and Zimmer 2016), an important consideration in an era when schools and teachers face sanctions for low achievement.[3] Thus, electoral dynamics that increase the influence of school employees should improve the prospects for passing both school-related tax and bond referenda.

While school-related tax measures are generally less partisan and ideological than other issues, there is also a widespread belief — supported by our data — that these proposals fare better among Democratic voters. Indeed, we find a significant positive cross-sectional relationship between the percent of Democratic voters in a school district (proxied by President Obama's two-party vote share in 2008) and the probability of referendum passage (p=0.001).[4] Increasing Obama's vote share from one standard deviation below the mean to one standard deviation above the mean is associated with a nearly 3 percentage point increase in the probability of passage.

Thus, the overall impact of election timing on referendum passage should depend on how it influences the turnout of school district employees on one hand and left-leaning voters on the other. In this section, we briefly summarize the arguments often made about how timing affects both of these compositional outcomes.

Case for Off-Cycle Elections

The claim that low-turnout elections privilege narrow interest groups generally, and public employees in particular, has ample precedent in the research on American elections. Writing in the early 1970s, Hamilton and Cohen (1974) noted, "The hypothesis of a negative association between turnout and [school referenda] success has acquired such wide currency in political science literature that it has acquired almost the status of a law ..." (p. 75).

[3]Indeed, the American Federation of Teachers has long made improving school facilities a primary objective and called for a "Marshall Plan" in the late 1980s to invest in educational capital infrastructure (American Federation of Teachers 2006).

[4]These results come from a model regressing a dummy variable indicating whether a measure passed on Obama vote share along with state and year fixed-effects, with standard errors clustered at the district level.

As they explain:

> The reasoning is straightforward from the incontrovertible premise that low turnout is the norm. Since two-thirds or more of the populace “stay home,” it is a plausible inference that the participants are those people with an uncommon interest in the schools, which should be parents, teachers, other school employees and their relatives, and perhaps some suppliers. The habitual voters also include that of unswerving “support for our schools.” Thus the defacto school electorate has two elements, a hard core of loyalists and a fluctuating proportion of other “disinterested” persons. To the extent that this analysis is factually correct, it follows that low turnout is propitious for school measures. Then, as the turnout rate rises, the passage rate declines as the composition of the electorate is modified by increments of voters who are not hard-core loyalists. A large turnout of course may mobilize more loyalists, but there is less margin for expansion; there is far more elasticity to the other component (p. 73).

More recently, Anzia (2013) described the intuition that self-interested groups who participate regardless of timing — in this case, teachers and other school employees — make up a larger share of the electorate when overall turnout declines as the “individual effect.” In addition, however, there may be a complementary “group effect” driven by the mobilization of other like-minded voters. As she notes, “The organized groups that have a stake in an election do not just passively sit around and hope their members and supporters will turn out to vote. Rather, they take an *active* role in mobilizing supporters and persuading likely voters to vote for their preferred candidates. Interest group leaders remind their members to vote, and they encourage them to contact their friends, neighbors, and co-workers.” (p. 21). Since interest groups have only fixed human and financial resources to mobilize other supporters, these resources go a longer way when the overall number of voters is lower. Together, the "individual" and "group" effects imply that interest groups should exercise greater influence during low-turnout off-cycle elections, which in our setting implies a higher probability of passage for school-related referenda.

Case for High-Turnout Elections

One potential countervailing effect comes from other compositional changes in the electorate that may occur as turnout declines. If turnout drops more precipitously among left-leaning voters, leaving only hard-core conservatives in low-turnout elections, these compositional dynamics may offset some or all of the advantage described above. Indeed, Hamilton and Cohen (1974) stress that declining turnout involves two separate dynamics: "(1) a large reservoir of potential pro-voters among large segments of the population which have a tradition of non-voting, who are only intermittent voters, principally women, [blacks], and apartment dwellers; and (2) a solid core of opponents who are regular voters" (p. 75).

Consistent with the results reported by Bechtel, Hangartner and Schmid (2016) in the Swiss context, there is evidence that higher-turnout elections tend to produce a more Democratic-leaning electorate in the U.S. For example, Hansford and Gomez (2010) use election day rainfall as an instrument for turnout in presidential elections and find that Democratic presidential candidates win more votes when turnout is higher on average, although they also show that this effect is conditioned in important ways by local partisan balance and the party of the incumbent candidate. If Democratic voters are more likely to support higher taxes to fund local schools, a common belief among district administrators, there may be important electoral benefits from fielding these measures when turnout is highest.

Summary

In short, the consequences of election timing are far from obvious. As turnout changes, so do the characteristics of voters, and these changes occur across a number of dimensions simultaneously. Some of these shifts should increase support for larger government, while others may decrease it, leaving the net effect a largely open empirical question. In the next section, we describe why our two new datasets are well positioned to help adjudicate between these competing predictions.

Data

Our empirical analysis utilizes two original data sources. The first includes all tax and bond referenda fielded by local school districts in California, Ohio, Texas, and Wisconsin roughly during the period 2000-2015. The precise years vary slightly between states,[5] and the final dataset used in the analysis, summarized in Table 1, includes more than 10,000 measures.[6] For each referendum, we observe the date it appeared on the ballot and the outcome of the vote (passage vs. defeat). We also observe the exact number of votes cast for and against each measure for all referenda in California, Ohio, and Wisconsin, although the votes are missing for roughly half of the Texas measures.[7]

We relied on a number of different sources to assemble these data. For California, we used records maintained by the California Elections Data Archive at Sacramento State University's Institute of Social Research. In Ohio, we obtained the vote breakdowns for

[5]California: 2000-2014; Ohio: 2000-2013; Texas: 2000-2015; Wisconsin: 2000-2016.

[6]Looking at the full universe of districts operating in each state as of 2010, our sample covers 65 percent of districts in California, 90 percent in Ohio, 87 percent in Texas, and 78 percent in Wisconsin. Additional descriptive statistics are provided in Supplemental Appendix A.

[7]We focus on these states because they contain an unusually large number of school districts and frequently hold local tax and bond elections, providing the maximum statistical power and within-district variation in timing. In addition, the states provide important variation in terms of region, culture, party competitiveness, and public-sector union influence (e.g., Hertel-Fernandez 2017).

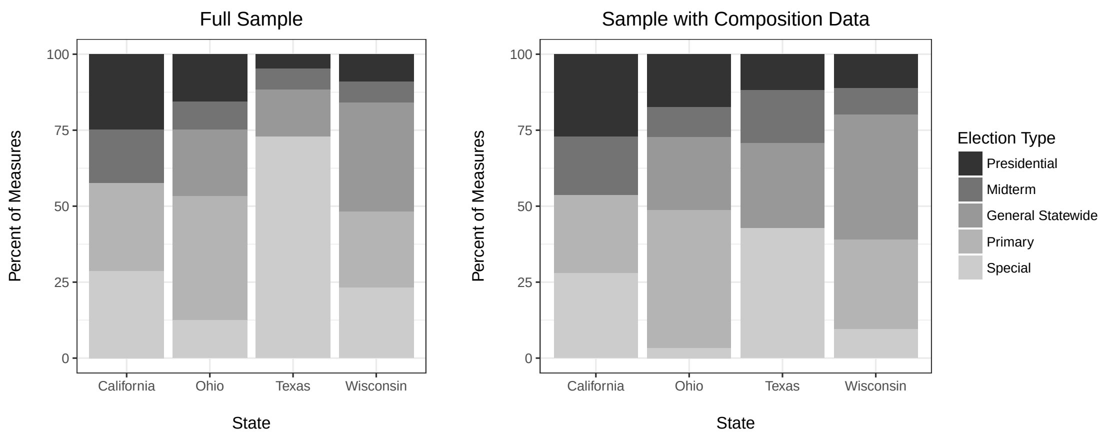
State	No. of Districts	No. of Measures	Voter Composition Data Coverage
California	623	1,423	89.2%
Ohio	571	4,412	84.3%
Texas	896	2,891	38.4%
Wisconsin	333	1,627	75.5%

Table 1: Overview of Referenda Data

levies fielded from 2008 to 2013 from the Ohio School Boards Association. For earlier years, we located the election results in archived paper records maintained by the Ohio Secretary of State. We obtained a listing of Texas school bonds from the Texas Bond Review Board and combined it with information on school tax ratification elections that `TexasISD.com` generously shared with us. Jared Knowles at the Wisconsin Department of Public Instruction provided the Wisconsin school referenda results.

In Ohio, districts may place school-related referenda on the ballot in February and August special elections, November general elections, or the May primary. In presidential election years, the primary takes place in March, and no February special elections are held. Each school district can return to the ballot up to three times each year. Prior to 2006, Texas had few restrictions on when districts could place measures on the ballot, producing wide variation in election dates. Starting that year, however, changes in state law limited referenda to either May or November elections, and almost all of the November elections we observe took place after this reform. California and Wisconsin similarly have few restrictions on the timing of school referenda, although there are significant cost savings for local districts if they place measures on the ballot concurrently with other elections.

After considering a variety of coding approaches, we ultimately classified the election dates into five distinct categories. The first two are November even-year elections, corresponding to presidential and midterm years, respectively. The third category, which we call "general statewide" elections, refers to other regularly scheduled statewide elections that include either statewide ballot measures or feature candidates from multiple jurisdictions.[8] The final two categories are primary and special elections. California does not hold statewide elections aside from those that occur in November of even years, so our dataset does not include any ballot measures in the "general statewide" category for that state. In addition, we observe no ballot measures on primary election dates in Texas.



(a) (b)

Figure 1: Distribution of Election Dates.

Figure 1 reports the distribution of election dates by state. The left panel includes all observations — which we use in our analysis of turnout and election outcomes — whereas the right panel includes only observations for which we have data on voter composition. The figure documents significant within-state heterogeneity in terms of election timing. Some of the variation in timing within states is likely driven by uncertainty and conflicting conventional wisdoms about the optimal time to put a tax increase on the ballot.[9] But districts also have imperfect control over the planning and preparation efforts that precede

[8]This category includes annual Wisconsin spring judicial elections, the Texas November odd-year state constitutional elections, and Ohio's November odd-year local elections.

[9]There may also be idiosyncratic factors unique to each district that we cannot observe but which may be known to local leaders.

a referendum campaign and, in the case of Ohio, are further constrained by the expiration of previously approved levies.

Our second dataset includes detailed demographic information about voters who turn out in each district, culled from the Catalist voter file. In the Catalist records, a variety of Census, commercial, and proprietary individual-level data are appended to each state's and county's official voter file. The Catalist data include two sets of variables that are of particular interest for our analysis. The first is voter partisanship and ideology, defined as the predicted probability of identifying as a Democrat or a liberal.[10] Second, the Catalist voter file identifies public school employees using official state licensure records. In most states, public school employees must obtain licenses from various state regulatory bodies, and Catalist obtained these lists from each state's licensing board and/or education agency to identify public school employees, listed separately as teachers, school support staff, and school administrators. Although the precise algorithm used to merge these administrative data to the voter file is proprietary, the procedure is described in detail in Ansolabehere and Hersh (2012), who carried out an independent verification of the matching procedure and ultimately concluded that they had great confidence in the method. As these authors also note, Catalist came in second in an international name-matching challenge organized by a third party, beating out prominent technology companies such as IBM (Ansolabehere and Hersh 2012, pp. 443-444).

The Catalist variables capture only education employees themselves — excluding their spouses and other potential allies.[11] But we believe these measures are likely to represent

[10]Supplemental Appendix C provides additional information about the models Catalist used to make these predictions and how we aggregate the individual-level predicted probabilities into a single measure of district-level partisanship and ideology in each election.

[11]Ideally, we would also have information on the remaining categories of school employees whose occupations do not require special licensure, such as custodians, and membership in a building trades union or employment in a construction-related field, since these groups also have pecuniary interests in the passage of school bonds. Unfortunately, these variables are not available in the Catalist data. According to the 2012 Census of Governments, instructional employees — almost all of whom are credentialed — account

the very upper bound for the size of the interest group voting bloc in each election for two reasons. First, our data identifies education employees based on their district of residence, not employment. As Moe (2006) shows, only about half of school employees in his sample of districts live in the same district as the one that employs them, so our measure may be inflated for this reason. Second, the Catalist variables for educational employees are time-invariant (as are all of the other compositional variables we use in the analysis). Voters are flagged as education employees if they are currently *or were at any time previously* licensed to work in education. Thus, this coding captures many retired educators and, since our data span over a decade, they include both voters who had not yet begun work in the education sector at the time of the election as well as those who had already left the profession, either through retirement or a career change. Thus, to the extent that there is measurement error on our compositional measures of school employees, we expect this error to overstate the number of school district employees in the elections we examine.

As the last column Table 1 shows, we do not have compositional measures for all of the referenda in our sample. We sought to achieve complete coverage in California and Ohio, but the final sample includes some missing observations for several reasons. First, a handful of election dates were not available in the Catalist data. Second, we could match the Catalist variables to our referenda data only on the basis of school district names and no other geographic information, so we had to exclude districts within the same state that had the same names.[12] Additionally, due to the large number of possible election dates in both Wisconsin and Texas, we limited our collection to a subset of dates that maximized the coverage of the referenda in our data.

The final dataset we have assembled provides more fine-grained details about the voters

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for between 70 percent and 80 percent of all full-time K-12 education employees in the states in our sample. Since many non-instructional occupations (e.g., counselors, principals) also require state licensure, our coding should capture the vast majority of public school employees.

[12]For example, Ohio has three different "Perry Local" school districts in various counties.

participating in local elections than has previously been available to any researcher and, thus, provides an important empirical contribution. These data are a major asset for our analysis, but they also have some limitations that are important to highlight. As we note above, the voter composition measures are cross-sectional, based on information in the voter file at the time we downloaded it.[13] Thus, if voters moved since the time of each election, we observe only their most current address and match them to their present district of residence. Such changes are problematic only if there are significant differences between the types of voters who live in each school district over time. In most cases, previous voters are likely to replaced by demographically similar individuals, posing few problems. In some cases, however, the demographics of the voter base might evolve quickly within districts, creating significant measurement error in our compositional outcome measures.

A second limitation is that many of the demographic and political characteristics we examine are estimated by Catalist using its proprietary models.[14] Although the firm's statistical sophistication is widely recognized and applauded, the Catalist models are essentially black boxes. Our analysis assumes that the measurement error in the predicted partisanship, ideology, race, income, and other demographic characteristics of voters is uncorrelated with election timing, the key independent variable of interest in our analysis. This assumption, too, is ultimately untestable. However, we present several validation exercises in Supplemental Appendix D, which collectively show that the data are of sufficient accuracy for our analysis.

[13]Sometime between 2015 and early 2017, depending on the state.

[14]We emphasize that this is not true for the educational employment variables, which are taken from official government records and thus not model-based.

Empirical Strategy

We estimate the impact of election timing on overall turnout, voter composition, and the probability that a tax or bond referendum passes. Our empirical strategy entails comparing these outcomes across five types of election timing — presidential, midterm, general statewide, primary, and special (as per Figure 1 above) — within school districts. Specifically, we estimate some variant of the following Ordinary Least Squares model:

$$y_{id} = \alpha_d + \text{Timing}_{id}\tau + \text{Controls}_{id}\beta + \epsilon_{id}$$

Subscript i indexes individual ballot measures, while d indexes the school district associated with each measure. Variable y_{id} is the outcome of interest, α_d are district fixed effects, and $Timing$ is a row of dummy variables indicating the election timing, with presidential elections serving as the baseline. The fixed-effect specifications exploit only within-district variation in referenda timing, accounting for any time-invariant differences between districts.[15] This is essential for capturing potentially unobservable political differences between districts that may cause some districts to consistently prefer on-cycle or off-cycle elections (e.g., Meredith 2009). We report heteroskedasticity robust standard errors that are clustered at the district level.

Turnout is defined as the sum of "yes" and "no" votes — so our measure of turnout is net of potential ballot roll-off — and our denominator is the voting-age population in each district as measured in the 2010 Census.[16] Note that the Catalist records indicate only whether a voter cast a ballot in each election and do not reveal whether the individual

[15]We observe within-district variation in timing for 46 percent of the districts in our California sample, 93 percent in Ohio; 47 percent in Texas; and 74 percent in Wisconsin.

[16]We exclude a small number of observations where turnout exceeds 100 percent.

marked a vote in any given race, so the compositional measures do not account for potential roll-off.[17]

In addition to the district fixed-effects and indicators corresponding to election timing, all of our models include several controls that may predict both the date on which the proposal appears on the ballot and our outcomes of interest. First, we differentiate school bond proposals, which finance capital projects, from tax increases meant to pay for operational expenditures.[18] We also control for the threshold necessary for passage, which is a simple majority in Ohio, Texas, and Wisconsin, but varies across measures in California. Finally, we include an indicator for whether a similar type of measure passed or failed in the same district within the previous 11 months, to account for the possibility that proposals later in the year follow earlier failed referenda as districts return to the ballot in hopes of overcoming the initial voter resistance.[19]

Despite these controls, we acknowledge that our analysis of election outcomes — whether or not a proposal passes and "yes" vote share — is descriptive rather than causal. The key identifying assumption in this part of the analysis is that, conditional on our covariates, there are no average differences between the attributes of referenda proposed or campaign efforts made by the same school districts on different election dates. This assumption seems implausible. Districts are likely to be strategic in choosing the date they go to the ballot — although, as we note above, they may make mistakes based on faulty conventional wisdom and they may be constrained in their choices. For example, districts that anticipate low achievement to be publicized in the fall may try to beat the bad news by scheduling a

[17] The attributes of the overall electorate are arguably the quantities of greatest interest to those making strategic timing decisions, since it is difficult to anticipate which specific voters will fail to fill out the full ballot. We discuss roll-off in Supplemental Appendix E and show why it is unlikely to affect our main compositional results.

[18] For Texas, our classification codes tax ratification elections as taxes. In Wisconsin, we code both recurring and non-recurring tax cap increases as tax measures.

[19] We also report results for models including year fixed-effects in the appendix. The results are substantively similar.

special election in the spring or summer before. Because it would be impossible for us to fully observe all of these factors or convincingly model the districts' timing decisions, we believe caution is warranted when interpreting our analysis of election outcomes. We view the election outcome models as suggestive, not definitive, and emphasize that our analysis of voter composition is our most novel and substantively important contribution.

Results

We estimate all models separately by state. For clarity and presentation purposes, however, we combine these results into single tables. In the results tables that follow, each row corresponds to a district fixed-effects model, run separately for each state and outcome variable of interest. Presidential elections serve as the baseline category, and we include a column that reports the district-level average of each outcome for presidential elections in each state to aid in evaluating the substantive magnitude of the effects. Note that the significance flags mean that the election timing indicators for non-presidential elections are significantly different from this baseline category — presidential elections — but may not necessarily be significantly different from each other. We emphasize this point when it is important for substantive interpretation.

Turnout

We begin by examining turnout in the school tax and bond elections. The results, presented in Table 2, capture conventional wisdom. We find that turnout is highest in presidential elections, falls somewhat during midterm elections, declines further in off-cycle general elections, and is particularly low during both primary and special elections. Indeed, voter turnout during special election is under 30 percent across all four states and below 20 percent of the voting-age population in both California and Texas.

Outcome Variable	State	Presidential	Midterm	General Statewide	Primary	Special
Turnout	California	41.00%	-11.79%***	NA	-18.83%***	-23.07%***
			(0.80)		(0.79)	(1.07)
	Ohio	62.44%	-15.14%***	-22.66%***	-32.91%***	-36.22%***
			(0.53)	(0.42)	(0.46)	(0.54)
	Texas	33.51%	-12.62%***	-22.70%***	NA	-24.39%***
			(2.70)	(2.16)		(2.13)
	Wisconsin	63.80%	-9.15%***	-30.12%***	-29.86%***	-34.86%***
			(1.67)	(1.49)	(1.65)	(1.68)

Robust standard errors in parentheses, clustered by district
*** $p<0.001$, ** $p<0.01$, * $p<0.05$

Note: Each row presents results from individual regression models that are estimated separately by state. The models include district fixed effects and control for measure type, passage threshold, and whether an earlier proposal passed or failed within the previous calendar year. Presidential elections serve as the omitted baseline category. The district-level average turnout for presidential elections is calculated as a percent of voting-age population in 2010.

Table 2: Effect of Timing on Turnout

Who Votes?

We examine how election timing affects the types of voters who participate in each election in a series of three tables. Each table speaks directly to a distinct hypothesis we derive from the literature. Table 3 examines how timing affects the partisanship and ideology of voters; Table 4 looks at the compositional effects for school district employees; and Table 5 covers other demographic characteristics of voters, including their race, income, probability of having children, and age. Although not the primary focus of the political science literature, we discuss below how these latter characteristics may have profound consequences for election outcomes.

Across the various outcomes, we generally find consistent trends across all four states (with some exceptions in Texas). Generally speaking, the results confirm that presidential elections, which produce the highest overall turnout, also result in the most politically left-leaning (Table 3) and demographically diverse electorate (Table 5). Across most of the outcomes we examine, the biggest differences we observe are between presidential elections on one hand and all other election types (including even-year midterm elections) on the other. For many variables of interest, we actually find few sizeable differences between general statewide elections (excluding presidential and midterm federal elections) and low-turnout primary and special elections. Thus, less habitual voters appear to participate during presidential election years, but sit out local democracy most other times, and these voters tend to be younger, less white, poorer, and more liberal than the voters who participate more regularly.

In addition, Table 4 confirms that school employees make up a larger share of the electorate in low-turnout elections. We should stress, however, that the differences across election dates seem quite small in absolute terms. In California, for example, the teacher share of the electorate increases from about 1.75 percent in high-turnout presidential elections to about 2 percent in special elections. In Ohio and Texas, the increase is somewhat larger, but across all four states, teachers and other school employees represent a very small segment of the electorate regardless of election timing. Because education employees represent such a small slice of the overall population, even big differences in turnout produce fairly modest compositional effects.

Of course, the measurement error in the Catalist data may dampen the magnitude of the differences we observe between election types. However, our substantive interest is not on the election timing coefficients themselves but what they imply about the total size of the employee voting bloc in each election. As we note above, measurement errors likely lead us to *consistently overestimate* the total size of the school employee voting bloc regardless of election timing, making our estimates plausible upper bounds for each election category.

These results appear to be in some tension with the arguments made in the literature (e.g., Anzia 2013, Moe 2006), which emphasizes the influential electoral role that education employees play in low turnout elections. Yet our estimates are quite similar in magnitude. Consider Moe (2006), who examines a subset of school board elections in Southern California and finds that school employees participate at much higher rates than other voters when they live in the school district that employs them. Although he focuses on differences in turnout rates — not differences in the absolute size of the voting blocs represented by school employees vs. other voters, which are only partly a function of turnout — Moe offers an example to illustrate that the differences in turnout could prove pivotal. Specifically, Moe discusses a school board election in the Charter Oak school district in 1997, which would be coded as a special election using our typology. He notes that the election was decided by just 89 votes, whereas school district employees accounted for 108 total votes — enough to have decided the election. Although Moe specifically picked this example to illustrate his point — and was careful not to claim the example was representative —

the numbers imply that district employees made up only 6.2 percent of the voters in this election. Similarly, our estimates based on all school district referenda in California during the entire time period we examine, indicate that education employees make up about 3 percent of the electorate during special elections.[20]

The most substantial differences we observe between elections across all four states is in the percent of voters who live in households with children[21] and who are elderly, reported in the bottom two panels of Table 5.[22] In presidential elections, seniors make up roughly 35 percent[23] of the electorate, but their share increases by between 8 and 16 percentage points during special elections.[24]

[20]This is calculated by adding the special election effects to the baseline composition in presidential elections, then summing across teachers, school support staff, and administrators. Note that our figure does not include employees who do not require state licensure, whereas these categories of workers are included in Moe's calculation.

[21]Note that for younger voters, this may include underage siblings.

[22]As we show in Supplemental Appendix J, these two quantities are strongly correlated with each other.

[23]In Supplemental Appendix H, we discuss why this number is higher than reported in national exit polls.

[24]The voter age is calculated based on the date of birth in the voter file, so is not a model estimate. However, note that the age is calculated at the time we examined the voter file, so some voters we classify as seniors may have been as young as 50 at the time of the election. To ensure that differential change over time is not driving these results, we have also estimated them after including year fixed-effects.

Voter Composition DV	State	Presidential	Midterm	General Statewide	Primary	Special
Democrats	California	57.26%	-1.76%***	NA	-2.28%***	-1.92%***
			(0.30)		(0.31)	(0.48)
	Ohio	44.23%	-2.23%***	-2.23%***	-2.93%***	-4.31%***
			(0.14)	(0.12)	(0.16)	(1.08)
	Texas	42.67%	+0.52%	-7.55%**	NA	-0.84%
			(2.81)	(2.92)		(3.21)
	Wisconsin	50.73%	-3.14%**	-2.26%**	-3.08%**	-3.65%*
			(1.05)	(0.79)	(1.05)	(1.65)
Liberal	California	48.00%	-1.08%***	NA	-1.16%***	-1.09%***
			(0.18)		(0.18)	(0.29)
	Ohio	40.53%	-1.85%***	-1.83%***	-2.35%***	-1.92%***
			(0.07)	(0.06)	(0.09)	(0.46)
	Texas	35.37%	+0.52%	-3.23%*	NA	+1.18%
			(1.45)	(1.56)		(1.71)
	Wisconsin	40.51%	-1.78%***	-1.89%***	-2.19%***	-2.72%***
			(0.48)	(0.39)	(0.48)	(0.81)

Robust standard errors in parentheses, clustered by district
*** $p<0.001$, ** $p<0.01$, * $p<0.05$

Note: Each row presents results from individual regression models that are estimated separately by state. The models include district fixed effects and control for measure type, passage threshold, and whether an earlier proposal passed or failed within the previous calendar year. Presidential elections serve as the omitted baseline category.

Table 3: Effect of Timing on Political Composition of Voters

Voter Composition DV	State	Presidential	Midterm	General Statewide	Primary	Special
Teacher	California	1.77%	+0.14%***	NA	+0.17%***	+0.25%***
			(0.03)		(0.03)	(0.07)
	Ohio	2.29%	+0.38%***	+0.65%***	+1.18%***	+1.52%**
			(0.03)	(0.02)	(0.03)	(0.56)
	Texas	2.74%	-0.17%	+0.88%	NA	+1.28%
			(1.54)	(1.57)		(1.54)
	Wisconsin	4.24%	+0.28%	+1.44%***	+0.92%*	+0.59%
			(0.43)	(0.32)	(0.40)	(0.55)
School Support Staff	California	0.03%	+0.01%***	NA	+0.00%**	+0.00%
			(0.00)		(0.00)	(0.00)
	Ohio	2.43%	+0.39%***	+0.72%***	+1.31%***	+1.64%**
			(0.03)	(0.02)	(0.04)	(0.55)
	Texas	3.26%	+0.14%	-0.55%	NA	-0.27%
			(0.63)	(0.95)		(0.84)
	Wisconsin	4.61%	+0.65%	+1.92%***	+1.40%**	+0.93%
			(0.58)	(0.37)	(0.47)	(0.60)
School Administrator	California	0.19%	+0.01%	NA	+0.03%**	+0.06%***
			(0.01)		(0.01)	(0.01)
	Ohio	0.73%	+0.10%***	+0.22%***	+0.44%***	+0.56%
			(0.01)	(0.01)	(0.01)	(0.42)
	Texas	0.80%	+0.08%	-0.08%	NA	-0.12%
			(0.18)	(0.21)		(0.26)
	Wisconsin	0.83%	+0.27%	+0.82%***	+0.44%**	+0.57%***
			(0.20)	(0.12)	(0.16)	(0.16)

Robust standard errors in parentheses, clustered by district
*** $p<0.001$, ** $p<0.01$, * $p<0.05$

Note: Each row presents results from individual regression models that are estimated separately by state. The models include district fixed effects and control for measure type, passage threshold, and whether an earlier proposal passed or failed within the previous calendar year. Presidential elections serve as the omitted baseline category.

Table 4: Effect of Timing on Education Employees as Share of Voters

Voter Composition DV	State	Presidential	Midterm	General Statewide	Primary	Special
White	California	67.02%	+3.11%***	NA	+6.94%***	+5.54%***
			(0.41)		(0.58)	(0.69)
	Ohio	95.16%	+0.82%***	+1.32%***	+1.59%***	+2.46%***
			(0.13)	(0.15)	(0.18)	(0.42)
	Texas	77.47%	-1.66%	+2.15%	NA	+2.42%
			(3.03)	(3.34)		(3.16)
	Wisconsin	98.59%	+0.30%	+0.47%*	+0.57%	+0.73%
			(0.20)	(0.21)	(0.31)	(0.47)
Family Income <$40K	California	27.11%	-1.80%***	NA	-2.38%***	-3.77%***
			(0.47)		(0.48)	(0.51)
	Ohio	34.51%	-1.38%***	-1.56%***	-2.46%***	-3.13%*
			(0.13)	(0.14)	(0.16)	(1.44)
	Texas	44.43%	+0.31%	+0.15%	NA	+0.70%
			(3.67)	(3.94)		(3.90)
	Wisconsin	26.45%	-2.06%	-1.68%	-2.63%*	+1.65%
			(1.49)	(1.12)	(1.23)	(3.17)
Family Income >$100K	California	33.27%	+2.19%**	NA	+3.97%***	+6.62%***
			(0.74)		(0.66)	(0.97)
	Ohio	22.60%	+1.13%***	+1.03%***	+1.71%***	+3.10%
			(0.10)	(0.09)	(0.11)	(1.70)
	Texas	17.08%	-0.13%	-1.86%	NA	+1.81%
			(2.78)	(3.08)		(3.27)
	Wisconsin	27.31%	-0.57%	-0.32%	+0.66%	+1.95%
			(1.35)	(0.95)	(1.10)	(2.88)
Child in Household	California	30.97%	-4.48%***	NA	-8.28%***	-8.84%***
			(0.50)		(0.53)	(0.67)
	Ohio	36.70%	-2.73%***	-4.43%***	-4.59%***	-2.33%
			(0.13)	(0.13)	(0.15)	(1.86)
	Texas	26.93%	-4.10%	-5.99%*	NA	-8.77%**
			(2.65)	(2.91)		(3.09)
	Wisconsin	31.66%	-0.77%	-3.99%***	-3.13%**	-0.84%
			(1.25)	(0.81)	(0.99)	(2.73)
65 and Older	California	36.81%	+8.70%***	NA	+14.51%***	+14.84%***
			(0.86)		(0.77)	(1.01)
	Ohio	35.07%	+6.07%***	+9.73%***	+11.66%***	+8.07%***
			(0.20)	(0.22)	(0.23)	(1.58)
	Texas	38.69%	+6.16%	+17.00%***	NA	+16.32%***
			(3.49)	(3.83)		(3.63)
	Wisconsin	36.09%	+1.59%	+10.01%***	+7.24%***	+8.02%*
			(1.83)	(1.55)	(1.71)	(3.66)

Robust standard errors in parentheses, clustered by district
*** $p<0.001$, ** $p<0.01$, * $p<0.05$

Note: Each row presents results from individual regression models that are estimated separately by state. The models include district fixed effects and control for measure type, passage threshold, and whether an earlier proposal passed or failed within the previous calendar year. Presidential elections serve as the omitted baseline category.

Table 5: Effect of Timing on Demographic Composition of Voters

Even if seniors are the dominant political force in low-turnout elections across jurisdictions, however, the political consequences vary significantly between states. In particular, laws differ across state lines in the type of special property tax breaks that senior citizens receive. There are significant differences in such tax provisions across states (which we describe in Supplemental Appendix I), but Texas is particularly unusual in its generous treatment of seniors. When homeowners turn 65, their property taxes are permanently frozen and cannot increase even when local tax rates go up or their homes appreciate in value.[25] Indeed, taxes can increase only when homeowners make substantial additions to their property. As a result, school bonds and taxes are essentially free to senior citizens. Since school construction also significantly increases surrounding property values (Cellini, Ferreira and Rothstein 2010) and can trigger desirable restrictions on local zoning and land-use (e.g., limiting sex offenders from moving in nearby or the opening of bars), seniors in Texas have very strong incentives to vote in favor of these measures. They effectively enjoy many of the benefits without shouldering any of the costs. Indeed, Reback (2015) provides evidence that such age-targeted tax breaks can affect support for school taxation among older voters. The other states, by contrast, offer much smaller (if any) discounts for seniors. We examine the consequences of this variation in the next section.

Before moving ahead, we pause to note two findings that may initially seem puzzling. First, the relationship between timing and voter partisanship and ideology that we document in the other three states does not appear to hold consistently in Texas. Although we find the familiar rightward shift when we compare presidential and off-year statewide elections, the effects are not present for midterm or special elections. These peculiar results are due to low statistical power in Texas when we include district fixed effects, and we present an alternative specification that addresses this issue in Supplemental Appendix

[25] This is only true for school district property taxes, not taxes that go to fund other jurisdictions.

G. The additional analysis shows that partisan and ideological dynamics in Texas largely mirror that of the other three states.

Second, the baseline compositional numbers for the share of voters who are teachers, school support staff, and administrators during presidential elections vary significantly between states. For example, teachers account for less than 2 percent of voters in these high-profile elections in California but more than 4 percent in Wisconsin. One explanation is that the baseline differences may reflect variation in union strength or intensity of labor electioneering activities. For the school support staff variable (but not the teacher variable), it may also capture differences in the occupations covered by state licensing requirements across states. Based on conversations with the Catalist staff, however, we believe the primary explanation is much simpler and reflects a combination of the firm's coding rules and the availability of historical licensure records. Recall that voters are coded as educators if they were ever licensed to work in this profession, even if they subsequently let the license lapse. Thus, the more years of licensure data that Catalist was able to obtain, the more voters who have since left the profession — due to either retirement or career changes[26] — are likely to be tagged as "false positives" in the firm's voter file. The historical availability of licensure data varies by state, and seems to be particularly good in Wisconsin. One clear indicator of this is that more than 40 percent of the teachers in the Wisconsin voter file are over the age of 60, compared to just a quarter in California. As a result, our measure of interest group electoral participation is likely to be particularly inflated in Wisconsin, and we probably substantially overstate the actual political influence of education employees in the electorate in this state. As we note above, our measures in California come very close to the calculation using contemporaneous employment records reported by Moe (2006).

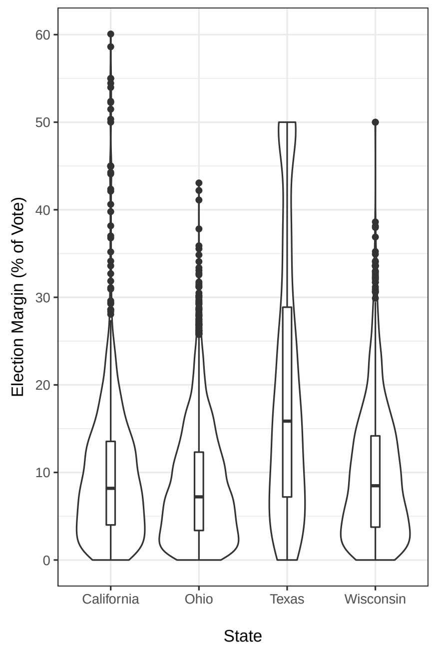
[26]Federal data suggests that a fifth of all newly minted teachers leave the profession within five years (Gray and Taie 2015), although other estimates suggest the figure could be as high as 50 percent.

Consequences for Public Policy

Overall, we find that election timing does produce statistically significant effects for voter composition. In substantive terms, however, the magnitude of the effects for the variables emphasized in the literature — voter partisanship or ideology and government employment — is modest. Only in Ohio does the share of education employees in the electorate increase by more than 3 percentage points between presidential and special elections. The increase is much smaller in other states, and in Ohio the total (and, as we noted, probably inflated) size of this voting bloc is just one in ten voters even during low-turnout special elections. Our results for partisanship and ideology are of similar magnitude. Theoretically, these two effects should partially offset one another, although the net impact on election outcomes depends on the degree to which each group represents a coherent voting bloc — a quantity we cannot measure precisely using our aggregate data.

The practical consequences of these effects ultimately depend on the competitiveness of the elections. Figure 2 summarizes the distributions for the margins of victory (and defeat) of tax and bond referenda across the four states in our sample. In none of the states is the average electoral margin smaller than 8 percent, and it is considerably larger in Texas.[27] Thus, we should not expect these timing effects to produce meaningful electoral consequences across the board. Moreover, it seems unlikely that education employees represent the pivotal voting bloc in typical contests. In a few particularly competitive elections, however, these effects could influence the outcomes.

[27] In all four states, three-fourths or more of the elections were decided by more than 3 percentage points.



Note: The "yes" vs. "no" vote breakdowns are not available for more than half of the Texas referenda prior to 2012, so these results may not generalize to the full sample of ballot measures.

Figure 2: Distribution of Win/Loss Margins by State.

We do, however, find very large effects for voter age, and these may be consistently consequential. As we demonstrate in Supplemental Appendix K, the compositional effects for voter age are likely to be far more consequential for election outcomes under plausible conditions than the change in the share of education employees. In most states, theory leads us to expect a negative association between the political influence of the elderly and support for school expenditures. First, seniors rarely have school-aged children in their household, so they receive limited utility from higher operational or capital spending by local districts. Second, many are also on a fixed income and are thus particularly sensitive to tax increases. Together, these two factors lead seniors to be resistant to school taxes — a logic known as the "gray peril" in the literature on education economics (e.g., Rauh 2017). However, this cost-benefit calculation is flipped in Texas. There, new school taxes cost seniors nothing but potentially bring important benefits — both through the well-established capitalization of student achievement into home values and through beneficial land-use restrictions that new schools bring, in a state famous for otherwise loose planning laws.

Our analysis of election outcomes, although primarily descriptive, does allow us one opportunity to examine which of these effects dominates. We focus our discussion on the results for referenda passage, presented in Table 6, but report analogous estimates for the percent of votes cast in favor of each measure in Supplemental Appendix L. For both dependent variables, we find that lower turnout elections are consistently associated with less voter support and lower probability of passage in California, Ohio, and Wisconsin. In Texas, by contrast, the effect has the opposite sign. With the exception of Texas, these results are inconsistent with the claim that education employees dominate low-turnout elections, although they are consistent with a more conservative electorate being detrimental to school funding. After accounting for the different treatment of seniors under state tax laws, however, the results are consistent with seniors being pivotal in low-turnout elections in all four states.

Outcome Variable	State	Presidential	Midterm	General Statewide	Primary	Special
Passage Probability	California	0.87	-0.15***	NA	-0.15***	-0.16**
			(0.04)		(0.04)	(0.05)
	Ohio	0.62	-0.01	-0.02	+0.03	-0.10***
			(0.03)	(0.02)	(0.02)	(0.03)
	Texas	0.70	+0.11	+0.05	NA	+0.15*
			(0.07)	(0.07)		(0.06)
	Wisconsin	0.78	-0.07	-0.23***	-0.08	-0.18**
			(0.08)	(0.06)	(0.06)	(0.06)

Robust standard errors in parentheses, clustered by district
*** $p<0.001$, ** $p<0.01$, * $p<0.05$

Note: Each row presents results from individual regression models that are estimated separately by state. The models include district fixed effects and control for measure type, passage threshold, and whether an earlier proposal passed or failed within the previous calendar year. Bond measures in presidential elections in districts where a measure has not failed recently serve as the omitted baseline category. The threshold for passage is 50% in Ohio, Texas, and Wisconsin. It is 55% for most school bonds in California and $\frac{2}{3}$ for tax measures.

Table 6: Effect of Timing on Passage Probability

Discussion and Conclusion

In summary, our results provide strong evidence that election timing matters — but the biggest consequences are not the ones emphasized by existing theory. While turnout appears to affect the composition of the electorate similarly across states on many dimensions, some of these effects offset one another and their political import is variable and dependent on the local political and legal context.[28] Our findings also identify another possible mechanism through which decisions made by one level of government can ultimately influence voter behavior in another (see, e.g., Berry 2008, Kogan, Lavertu and Peskowitz 2016*b*). The consequences of Texas's generous property tax provisions for the voting behavior of seniors citizens are likely well understood by both state and local officials there. However, these implications may be less obvious to researchers studying voting behavior in local elections, and analyses that pool observations across states without accounting for the important ways in which state laws interact with local electoral dynamics may lead to incorrect inferences about local democracy. We conclude by briefly summarizing the other noteworthy implications of our results — for broader theories of politics, for the empirical examination of local elections more specifically, and for education governance.

First, low-turnout elections do not appear to materially increase the share of "high demanders" in the electorate. Although education employees make up a somewhat larger share of the electorate in low-turnout elections, the difference is small and they still account for a small slice of voters. This raises new questions about what alternative mechanisms might explain the robust finding that school employee compensation is higher in districts that hold off-cycle elections compared to those that elect school boards on-cycle (Anzia 2011; 2012, Berry and Gersen 2011). We offer two possible explanations — both emphasiz-

[28]We show in Supplemental Appendix M that these results also extend to candidate elections, using variation in school board election timing in California.

ing the increase in the political influence of older voters, not interest groups, produced by low-turnout elections. Since seniors are unlikely to have children in schools, it is possible that they don't monitor local districts as attentively, making it easier for administrators and policymakers to be captured by employee interest groups. Alternatively, in the absence of first-hand information about local schools, the elderly may be particularly influenced by endorsements of school board candidates from local teacher groups. We believe both sets of hypotheses deserve closer study in future work, and are best examined using micro-level data.

Our results may also help reconcile conflicting findings in the research on student achievement and retrospective voting in local elections. Consider two recent examples, Kogan, Lavertu and Peskowitz (2016*a*) and Holbein (2016). Both examine the "Adequate Yearly Progress" achievement designation created by No Child Left Behind Act, and ask how it affected incumbent re-election rates (in the former study) and voter turnout (in the latter). Yet they come to different conclusions about whether the AYP designation affected school board elections. These studies differ in important ways — including the unit of analysis and outcomes of interest — that may help explain the divergent findings about the political salience of the AYP designations. However, election timing may also contribute: Kogan, Lavertu and Peskowitz (2016*a*) examine odd-year school board elections in Ohio, while Holbein (2016) focuses on even-year November elections in North Carolina.[29] Given the large compositional differences we find among the electorates between these election times, there is little reason to expect that voters in these two sets of elections to be similarly responsive to school performance information.[30] Instead, we believe the key is in the share of voters who are seniors. These individuals may be particularly unresponsive to school

[29] A small handful of districts in North Carolina have a statutory exemption to run their elections during odd-numbered years.

[30] Payson (2017) finds this type of heterogeneity in California, showing that academic designations affect school board elections during presidential elections but not during other times.

performance information, both because they are unlikely to have school-aged children and thus may put little weight on performance, and because these voters may be less aware of the performance information in the first place.[31]

Our findings also have broader implications for the dynamics of local elections. In particular, our results are clearly inconsistent with full-information models of agenda setting (e.g., Romer and Rosenthal 1978), which would predict that school district officials should anticipate how timing affects voter composition and adjust their proposals accordingly. If this actually occurred, we would not find any differences in observed passage rates across election dates — a prediction clearly at odds with the empirical reality in each of the four states we examine. Relaxing these assumptions by introducing uncertainty (e.g., Romer and Rosenthal 1979) leads to a variety of novel empirical predictions rarely examined in existing research, some of which are not only of theoretical interest but also have considerable welfare implications (see, e.g., Kogan, Lavertu and Peskowitz 2017).

Finally, we believe our results offer important lessons for educational governance by documenting a substantial demographic gap between the students being taught in local schools and the voters who exercise political control over school districts. This gap is easiest to see in the percent of voters with children in their household and is particularly large during low-turnout elections. However, the deficit may also exist even in high-profile elections and on other demographic dimensions, including race and ethnicity and socioeconomic status. Existing research in both public administration and education policy find that having a "representative bureaucracy" — government administrators who are demographically similar to the clients they serve — can have important benefits for service delivery. To the extent that a representation gap at the bureaucratic level reflects a divide between who local schools serve and who votes in local elections, timing may be both an

[31] Under the No Child Left Behind Act, school districts were required to send notification letters of their AYP status to *parents*, who make up a particularly small share of voters in low-turnout elections.

important cause of concern and also an area of potentially beneficial reform, particularly in jurisdictions that utilize off-cycle elections.

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SUPPLEMENTAL APPENDIX (ONLINE ONLY)

A Descriptive Statistics

	All Districts	Referenda Sample	Catalist Subsample
California			
Mean Student Enrollment	6352	8853	9154
Urban	14.8%	19.9%	20.4%
Rural	41.5%	25.4%	24.3%
Mean Student-Teacher Ratio	19.5	20.9	20.9
Special Ed	9.9%	10.2%	10.4%
English Learners	23.6%	24.8%	25%
Ohio			
Mean Student Enrollment	2719	2812	2842
Urban	3.4%	3.5%	3.8%
Rural	47%	45.5%	43.9%
Mean Student-Teacher Ratio	16.4	16.3	16.3
Special Ed	14.9%	14.8%	14.9%
English Learners	1.3%	1.2%	1.3%
Texas			
Mean Student Enrollment	4582	5118	6715
Urban	6.4%	7.2%	9.1%
Rural	66.3%	63.6%	57.6%
Mean Student-Teacher Ratio	12.2	12.4	12.7
Special Ed	8.4%	10.1%	10.1%
English Learners	7.4%	7.5%	7.7%
Wisconsin			
Mean Student Enrollment	2021	1978	2209
Urban	3.5%	3.9%	4.7%
Rural	59.3%	56.5%	57%
Mean Student-Teacher Ratio	13.8	13.9	13.9
Special Ed	14.6%	14.2%	13.9%
English Learners	2.8%	2.7%	2.2%

Source: 2009-2010 Common Core of Data LEA Universe Survey

Table A.1: Descriptive Statistics of Analytic Samples

B Special Elections by Month

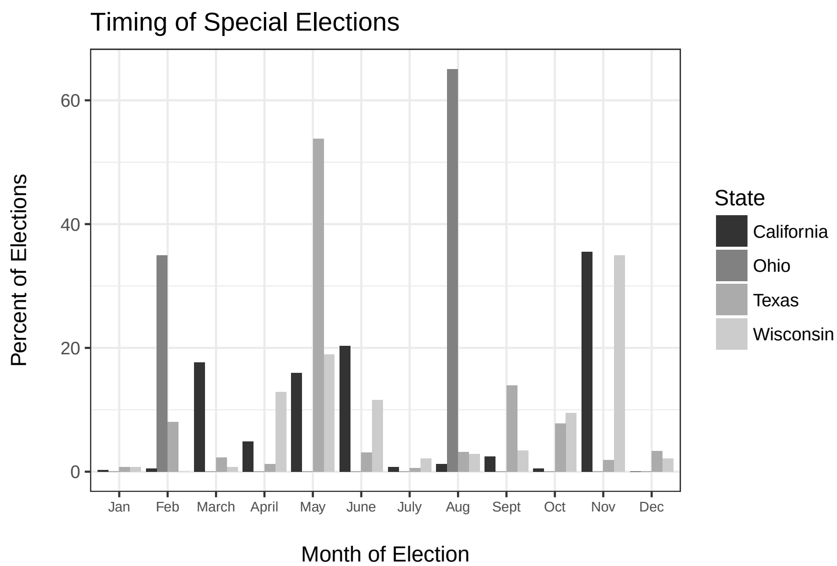


Figure A.1: Special Elections by Month.

C Modeling Voter Partisanship and Ideology

The measure of partisanship we use is based on the proprietary Catalist Partisanship Model, which provides every voter in the firm's database with a score indicating his or her probability of identifying as a Democrat rather than a Republican. These predicted probabilities come from a two-layer model that uses machine-learning algorithms trained on a large national sample — five million people from 31 states — of registered voters, using their declared partisanship in the voter file, as well as self-reported partisanship from public opinion polls. As inputs, the model relies on more than 150 separate variables, including gender, race, ethnicity, income, housing and family structure, past electoral returns, occupation, religious adherence, and economic conditions. One advantage of these predicted probabilities is that they are comparable across states.

The Catalist Ideology Model is constructed similarly, using thermometer ratings from questions that appeared in national polls fielded by the AFL-CIO polling consortium as the basis for the training set. The issues included same-sex marriage, immigration, attitudes toward the NRA and Tea Party, as well as other standard policy questions. Answers to these questions were aggregated into a single index of "progressivism" that provided the dependent variable for the model. Both models were revalidated against new polling data in 2015, several years after their initial development.

Using these predicted probabilities, we created district-level cross-tabs for each election date. A hypothetical example for one district is presented in Table A.2. Each cross-tab contains the the number of voters (N) in each 5 percentage point probability bin, which is listed in the first column in the table. The hypothetical district depicted in the table contains a total of 2,000 voters, who are uniformly distributed across all probability bins. As a first step, we took the midpoint of each probability range, presented in the column labeled p. To calculate the expected number of Democrats in each district, we then multiplied each cell count in the N column by the midpoint of the probability range ($p * N$) and took the sum. In this case, our measure would indicate that 1,000 of the 2,000 voters are predicted to be Democrats, for an expected share of 50 percent.

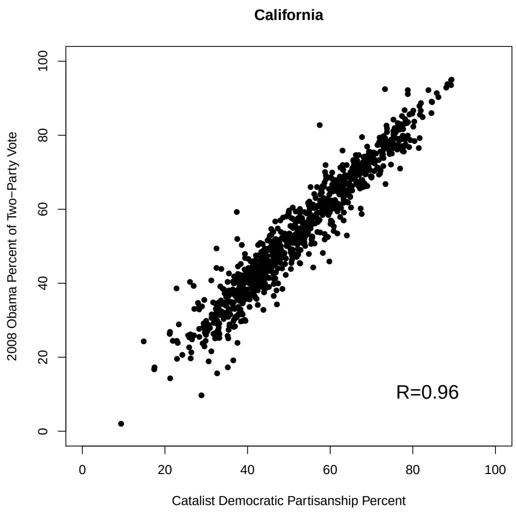
Pr(Democrat)	N	Probability Range Midpoint (p)	Democrats ($p * N$)
0-0.05	100	2.50	2.5
0.05-0.10	100	0.075	7.5
0.10-0.15	100	0.125	12.5
0.15-0.20	100	0.175	17.5
0.20-0.25	100	0.225	22.5
0.25-0.30	100	0.275	27.5
0.30-0.35	100	0.325	32.5
0.35-0.40	100	0.375	37.5
0.40-0.45	100	0.425	42.5
0.45-0.50	100	0.475	47.5
0.50-0.55	100	0.525	52.5
0.55-0.60	100	0.575	57.5
0.60-0.65	100	0.625	62.5
0.65-0.70	100	0.675	67.5
0.70-0.75	100	0.725	72.5
0.75-0.80	100	0.775	77.5
0.80-0.85	100	0.825	82.5
0.85-0.90	100	0.875	87.5
0.90-0.95	100	0.925	92.5
0.95-1	100	0.975	97.5
Total	**2000**		**1000**

Table A.2: Catalist Partisanship Calculation Example.

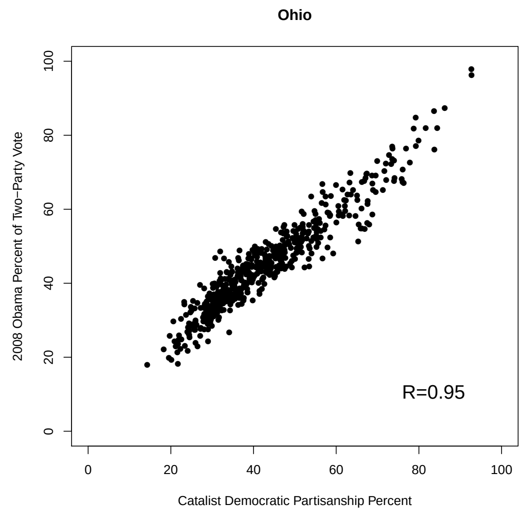
D Validation of Catalist Data

One way to assess whether changes in the voter file and the quality of the Catalist models pose a problem for our analysis is to compare the firm's predicted partisanship of voters based on their most current addresses against the official 2008 presidential election results.[1] Because none of the states report presidential results at the school district level, following Einstein and Kogan (2016), we constructed our own district-level measures by aggregating up official precinct-level results. The results of our validation exercise are presented in Figure A.2, which plots the predicted share of Democrats in each district (from the Catalist data) against the actual share of the two-party vote won by Obama in 2008 in each school district. Overall, there is a clear and strong relationship between the two measures, with a correlation of 0.8 or above across all four states. The close fit between Catalist's modeled partisanship of voters and the actual election results in 2008 suggests that the data are of sufficiently good quality for our analysis.

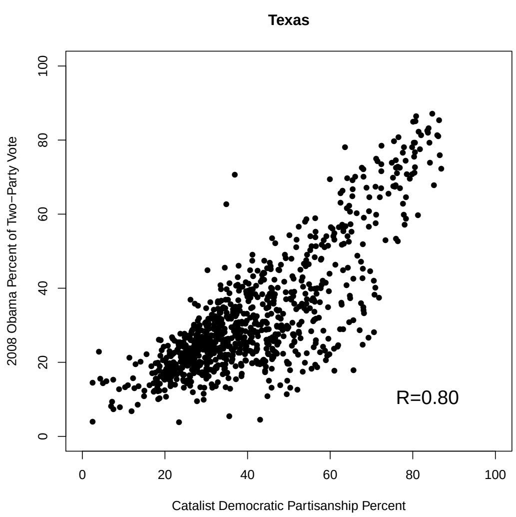
[1] Note that for this, and the validation exercises that follow, we limit the Catalist sample only to voters who are flagged as having voted in the respective election.



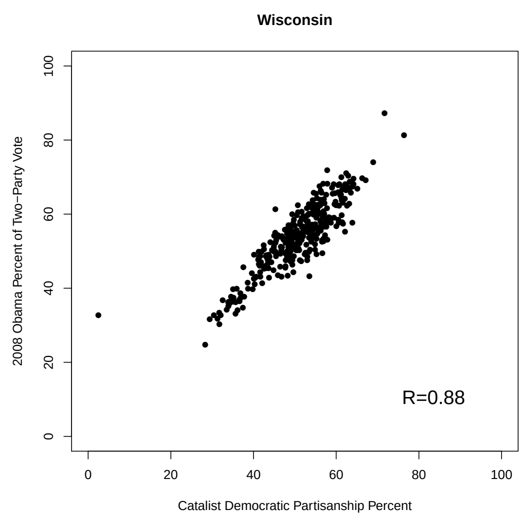
(a) California



(b) Ohio



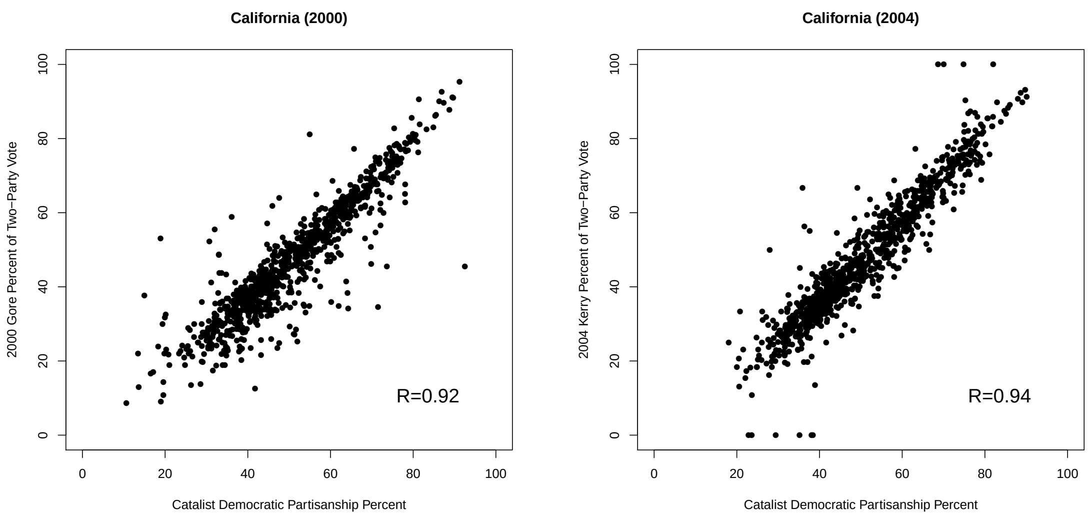
(c) Texas



(d) Wisconsin

Figure A.2: Validating Catalist Partisanship Predictions Against 2008 Election Results in School Districts.

Because precinct-level results are available in California for earlier elections, we were also able to carry out a similar validation exercise using data from the 2004 and 2000 presidential elections in that state. These results are presented in Figure A.3. Even in these earlier elections, the correlation between the Catalist-modeled partisanship and actual election results are striking, exceeding 0.9 in both elections. These results further bolster our confidence in using the contemporary district-level measures we built from the Catalist voter file over the full time period covered by the referenda dataset.



(a) 2000 Election

(b) 2004 Election

Figure A.3: Validating Catalist Partisanship Predictions Against 2000 and 2004 Election Results in California School Districts.

E Ballot Roll-Off

One limitation of our Catalist measures is that they capture the demographic characteristics of all voters who cast a ballot in a given election, including voters who may have skipped a particular contest or measure on the ballot. As we note in the body of the manuscript, school district officials who make strategic decisions about when to place their referenda on the ballot are likely most interested in — and responsive to — the overall electorate because it is difficult to predict which specific voters will fail to vote on their particular question. Nevertheless, it is important to consider the consequences of roll-off for the broader conclusions we can draw from our analyses. Summarizing other research, Cronin (1989) estimates roll-off of about 10 percent during presidential election years but considerably smaller during lower-turnout elections. These figures, as we show below, are somewhat larger than the rates we calculate for our referenda sample. We also explain why roll-off is unlikely to affect our substantive conclusions about how election timing affects the composition of the electorate.

The primary challenge to measuring roll-off among our school district tax and bond referenda proposals is that we cannot observe official turnout figures or votes cast for top-of-the-ticket races — the suitable denominators for calculating ballot roll-off — aggregated at the school district level.[2] We can, however, approximate roll-off by comparing the total number of ballots cast for and against local referenda that were held concurrently with statewide elections. For these measures, we can compare the total number of votes cast on the school referenda questions (as a percent of each district's 2010 voting-age population) with the official statewide turnout (as a percent of the statewide 2010 voting-age population). This comparison is not perfect because the districts scheduling their

[2] The 2008 precinct-level presidential election results we do have access to unfortunately omit votes cast for minor-party candidates in several of the states.

referenda during statewide elections may not be representative of all school districts in the state; nevertheless, this is the best metric we have.

Table A.3 compares the statewide turnout during the November elections in 2008, 2010, and 2012[3], to the calculated turnout in our data for measures considered by voters on these same dates.[4] During the 2008 and 2012 presidential elections, the number of votes we observe for the school referenda are about 3 to 5 percentage points lower than the statewide turnout in California, Ohio, and Wisconsin, similar to the roll-off Cronin (1989) estimated. Roll-off in Texas during these elections is closer to 10 percentage points. Consistent with his findings, we also calculate negligible roll-off during the 2010 midterm election. Thus, the roll-off we observe is of similar magnitude to what one would expect from the existing literature and occurs primarily during high-turnout presidential elections.

		2008	2010	2012
California	Statewide Turnout	49.15	36.84	47.22
	Average Referenda Turnout	43.27	35.78	42.81
Ohio	Statewide Turnout	65.57	44.93	63.97
	Average Referenda Turnout	64.49	45.50	58.65
Texas	Statewide Turnout	44.19	27.24	43.73
	Average Referenda Turnout	33.32	26.13	33.67
Wisconsin	Statewide Turnout	68.93	50.26	70.86
	Average Referenda Turnout	63.84	NA	67.41

Table A.3: Comparing Turnout in November Elections

Note: *Statewide Turnout* refers to overall turnout at the state level, based on official records. *Average Referenda Turnout* refers to votes cast for school referenda among the subset of districts that had measures appearing on the ballot at the same time as the statewide election. Both are measured as a percent of 2010 voting-age population.

We now consider whether roll-off on the order of 5 to 10 percentage points during

[3]We focus on these three statewide elections because they are most proximate to the 2010 census, which we use as the denominator for calculating turnout.

[4]Our sample does not include any referenda in Wisconsin in November 2010.

presidential elections is likely to affect our substantive conclusions. Recall that we find that election timing (1) has a statistically significant but substantively modest impact on the ideology and partisanship of the electorate; (2) an even smaller effect on the share of school employees; and (3) a substantively large impact on the age of voters. Overall, we do not expect roll-off to affect these three conclusions. We now consider each compositional outcome in turn:

Partisanship/Ideology: Roll-off will meaningfully impact our conclusions about the ideological leanings of the electorate only to the extent that it is correlated with voter partisanship and ideology. In our review of the existing research, we found no empirical work that speaks directly to this issue, so we instead carry out a sensitivity analysis that considers the "worst-case" scenarios in which roll-off is perfectly correlated with voter partisanship and ideology. First, suppose that roll-off during presidential elections occurs only among Democratic or liberal voters. This would mean that presidential elections have the most Republican or conservative referenda electorate, but the compositional differences between election types would be of roughly similar magnitude as we find in our analysis (but with the opposite sign). Second, suppose that roll-off during presidential elections occurs only among Republican or conservative voters. In this unlikely scenario,[5] the electorate during presidential election years would be more left-leaning than we calculate, and the right-ward shift during lower-turnout elections is likely to be 5 to 10 percentage points larger than what we report. Although roll-off may be somewhat correlated with voter partisanship and ideology, the most likely scenario is that there is some roll-off among both left- and right-leaning voters, so the net effect on our estimates is likely to be fairly small.

Education Employees: Given the considerable personal interests that school em-

[5] We think this scenario is unlikely because the individual-level variables, such as income and race, found by prior research to be negatively associated with roll-off are likely to be positively associated with Republican partisan identification and conservative ideology.

ployees have at stake in the referenda we examine, it is likely that roll-off is concentrated among the non-educator portion of the electorate. If we assume roll-off of 10 percentage points (near the top of our observed range) exclusively among non-educators, this would increase educators' share of the electorate during presidential elections. However, non-educators would still account for between 80 and 90 percent or more of all voters during these high-turnout elections. Moreover, roll-off would not significantly affect our estimates for the lower-turnout elections, when it is likely negligible. Indeed, to the extent that roll-off during high-turnout elections is concentrated among non-educator voters, this is likely to further reduce (or eliminate) the small observed increase in the share of education employees between presidential and lower-turnout election types.

Voter Age: We conclude by considering how roll-off might affect our results with respect to voter age. First, suppose that roll-off during presidential elections occurs only among senior voters. Since this would further reduce the share of seniors during presidential elections, it would lead us to only understate the increase in senior share for other, lower-turnout election types. Second, suppose that roll-off during presidential elections occurs only among younger voters. This would lead us to overstate the extent to which lower-turnout special elections increase the share of seniors in the electorate. In each of the four states, however, the amount of roll-off we estimate during presidential elections is still half to a third smaller than the increase in senior voting share we observe between presidential and low-turnout special elections. In other words, even if roll-off during high-turnout elections is concentrated among younger voters, we would still see a significant increase in senior political influence during low-turnout special elections.

F Robustness Check: Results with Year Fixed Effects

Outcome Variable	State	Presidential	Midterm	General Statewide	Primary	Special
Turnout	California	41.00%	-8.94%***	NA	-17.42%***	-21.12%***
			(1.12)		(0.88)	(1.55)
	Ohio	62.44%	-9.89%***	-13.70%***	-26.75%***	-30.29%***
			(0.60)	(0.52)	(0.45)	(0.58)
	Texas	33.51%	-13.81%***	-22.37%***	NA	-23.59%***
			(2.95)	(2.43)		(2.24)
	Wisconsin	63.80%	-9.70%***	-30.14%***	-32.72%***	-34.95%***
			(1.93)	(1.56)	(1.46)	(1.76)

Robust standard errors in parentheses, clustered by district
*** $p<0.001$, ** $p<0.01$, * $p<0.05$

Note: Each row presents results from individual regression models that are estimated separately by state. The models include district fixed effects and control for measure type, passage threshold, and whether an earlier proposal passed or failed within the previous calendar year. Presidential elections serve as the omitted baseline category. The district-level average turnout for presidential elections is calculated as a percent of voting-age population in 2010.

Table A.4: Effect of Timing on Turnout (with Year FEs)

Voter Composition DV	State	Presidential	Midterm	General Statewide	Primary	Special
Democrats	California	57.26%	-0.31%	NA	-1.61%**	-2.31%
			(0.79)		(0.60)	(1.60)
	Ohio	44.23%	-0.09%	-1.14%***	-1.85%***	-4.06%***
			(0.27)	(0.30)	(0.24)	(1.20)
	Texas	42.67%	+16.78%*	+4.81%	NA	+14.15%*
			(7.71)	(7.28)		(6.57)
	Wisconsin	50.73%	-5.64%***	-2.48%**	-3.33%***	-4.40%*
			(1.60)	(0.92)	(0.98)	(2.00)
Liberal	California	48.00%	-0.17%	NA	-0.84%**	-1.61%
			(0.40)		(0.30)	(0.83)
	Ohio	40.53%	+0.03%	-0.76%***	-1.38%***	-1.43%**
			(0.12)	(0.13)	(0.11)	(0.49)
	Texas	35.37%	+10.78%*	+4.88%	NA	+10.69%**
			(4.19)	(4.03)		(3.70)
	Wisconsin	40.51%	-3.04%***	-1.82%***	-2.50%***	-3.15%***
			(0.78)	(0.47)	(0.48)	(0.93)

Robust standard errors in parentheses, clustered by district
*** $p<0.001$, ** $p<0.01$, * $p<0.05$

Note: Each row presents results from individual regression models that are estimated separately by state. The models include district fixed effects and control for measure type, passage threshold, and whether an earlier proposal passed or failed within the previous calendar year. Presidential elections serve as the omitted baseline category.

Table A.5: Effect of Timing on Political Composition of Voters (with Year FEs)

Voter Composition DV	State	Presidential	Midterm	General Statewide	Primary	Special
Teacher	California	1.77%	-0.53%	NA	-0.29%	+1.09%
			(0.53)		(0.38)	(1.08)
	Ohio	2.29%	+0.08%	-0.02%	+0.73%***	+1.12%
			(0.08)	(0.12)	(0.08)	(0.61)
	Texas	2.74%	-3.57%	-3.32%	NA	-0.50%
			(2.82)	(2.35)		(1.91)
	Wisconsin	4.24%	-1.33%	+0.16%	+0.61%	-0.91%
			(0.77)	(0.45)	(0.42)	(0.88)
School Support Staff	California	0.03%	+0.00%	NA	+0.00%	-0.01%
			(0.00)		(0.00)	(0.01)
	Ohio	2.43%	+0.08%	-0.05%	+0.80%***	+1.16%
			(0.08)	(0.12)	(0.08)	(0.60)
	Texas	3.26%	-0.14%	+0.64%	NA	+1.60%
			(3.30)	(2.75)		(2.75)
	Wisconsin	4.61%	-1.35%	+0.51%	+1.12%*	-0.57%
			(0.89)	(0.49)	(0.50)	(0.92)
School Administrator	California	0.19%	-0.02%	NA	+0.01%	+0.04%
			(0.01)		(0.01)	(0.02)
	Ohio	0.73%	+0.02%	+0.00%	+0.29%***	+0.41%
			(0.07)	(0.10)	(0.07)	(0.51)
	Texas	0.80%	-0.43%	-0.62%	NA	-0.55%*
			(0.40)	(0.38)		(0.26)
	Wisconsin	0.83%	-0.22%	+0.38%*	+0.31%	-0.08%
			(0.33)	(0.15)	(0.19)	(0.35)

Robust standard errors in parentheses, clustered by district
*** $p<0.001$, ** $p<0.01$, * $p<0.05$

Note: Each row presents results from individual regression models that are estimated separately by state. The models include district fixed effects and control for measure type, passage threshold, and whether an earlier proposal passed or failed within the previous calendar year. Presidential elections serve as the omitted baseline category.

Table A.6: Effect of Timing on Education Employees as Share of Voters (with Year FEs)

Voter Composition DV	State	Presidential	Midterm	General Statewide	Primary	Special
White	California	67.02%	+2.39%**	NA	+6.42%***	+3.51%**
			(0.79)		(0.71)	(1.27)
	Ohio	95.16%	+0.39%**	+0.80%***	+1.22%***	+1.94%***
			(0.15)	(0.14)	(0.16)	(0.43)
	Texas	77.47%	+2.73%	+12.49%	NA	+5.88%
			(8.44)	(8.16)		(6.77)
	Wisconsin	98.59%	+0.18%	+0.15%	+0.53%	+0.64%
			(0.27)	(0.19)	(0.35)	(0.56)
Family Income <$40K	California	27.11%	-1.76%**	NA	-1.21%*	-4.98%***
			(0.63)		(0.52)	(0.92)
	Ohio	34.51%	-1.08%***	-1.09%**	-1.85%***	-3.62%*
			(0.25)	(0.35)	(0.24)	(1.54)
	Texas	44.43%	-3.21%	-15.87%	NA	-7.56%
			(7.68)	(8.86)		(7.36)
	Wisconsin	26.45%	+1.59%	-0.50%	-1.77%	+3.66%
			(2.43)	(1.33)	(1.41)	(4.05)
Family Income >$100K	California	33.27%	-1.06%	NA	+0.18%	+6.34%**
			(1.02)		(0.81)	(2.36)
	Ohio	22.60%	+1.25%***	+0.91%*	+1.52%***	+3.71%
			(0.33)	(0.43)	(0.28)	(1.96)
	Texas	17.08%	-14.12%	-2.63%	NA	-4.37%
			(7.63)	(7.84)		(6.13)
	Wisconsin	27.31%	-0.88%	-0.68%	+0.61%	+0.45%
			(2.43)	(1.30)	(1.32)	(3.56)
Child in Household	California	30.97%	-2.24%***	NA	-6.14%***	-6.99%***
			(0.58)		(0.48)	(1.39)
	Ohio	36.70%	-0.88%**	-3.46%***	-4.04%***	+0.24%
			(0.34)	(0.41)	(0.29)	(2.11)
	Texas	26.93%	-8.34%	-16.62%*	NA	-13.71%*
			(6.35)	(7.13)		(5.52)
	Wisconsin	31.66%	+0.69%	-3.79%***	-3.50%***	-0.81%
			(1.68)	(1.11)	(0.99)	(3.24)
65 and Older	California	36.81%	+3.60%***	NA	+11.21%***	+10.15%***
			(1.01)		(0.77)	(1.96)
	Ohio	35.07%	+1.35%***	+4.59%***	+8.45%***	+2.04%
			(0.31)	(0.35)	(0.25)	(1.67)
	Texas	38.69%	+21.17%	+28.71%**	NA	+19.72%*
			(10.85)	(10.46)		(9.40)
	Wisconsin	36.09%	+3.07%	+10.55%***	+8.89%***	+10.72%*
			(2.54)	(1.94)	(1.51)	(4.39)

Robust standard errors in parentheses, clustered by district
*** p<0.001, ** p<0.01, * p<0.05

Note: Each row presents results from individual regression models that are estimated separately by state. The models include district fixed effects and control for measure type, passage threshold, and whether an earlier proposal passed or failed within the previous calendar year. Presidential elections serve as the omitted baseline category.

Table A.7: Effect of Timing on Demographic Composition of Voters (with Year FEs)

Outcome Variable	State	Presidential	Midterm	General Statewide	Primary	Special
Passage Probability	California	0.87	-0.13*	NA	-0.14**	-0.11
			(0.06)		(0.05)	(0.08)
	Ohio	0.62	-0.08	-0.06	-0.01	-0.13***
			(0.04)	(0.03)	(0.03)	(0.03)
	Texas	0.70	+0.09	-0.05	NA	+0.10
			(0.08)	(0.08)		(0.07)
	Wisconsin	0.78	-0.06	-0.22**	-0.10	-0.15*
			(0.09)	(0.07)	(0.06)	(0.07)

Robust standard errors in parentheses, clustered by district
*** p<0.001, ** p<0.01, * p<0.05

Note: Each row presents results from individual regression models that are estimated separately by state. The models include district fixed effects and control for measure type, passage threshold, and whether an earlier proposal passed or failed within the previous calendar year. Bond measures in presidential elections in districts where a measure has not failed recently serve as the omitted baseline category. The threshold for passage is 50% in Ohio, Texas, and Wisconsin. It is 55% for most school bonds in California and $\frac{2}{3}$ for tax measures.

Table A.8: Effect of Timing on Passage Probability (with Year FEs)

G Robustness Check: Cross-Sectional Results for Partisanship and Ideology

Given the limited number of elections for which we have Catalist data in Texas, lack of statistical power in our district fixed-effects specification may explain why the partisanship and ideology effects we observe in other states do not appear to hold consistently in our Texas sample. To address this issue, we re-estimated the partisanship and ideology models (in all states) using an alternative specification that omits the district fixed-effects. This allows us to use the information from districts where timing does not vary over time and districts in which we observe only a single ballot measure — observations that otherwise fall out of the fixed-effects models. To account for unobserved district heterogeneity that might be correlated with election timing, however, we control for the value of each dependent variable observed in each district in November 2008.[6]

These cross-sectional results, reported in Table A.9, are very similar to the fixed-effects estimates in California, Ohio, and Wisconsin. In addition, the Texas results now look very similar to those in other states — with the electorate during presidential elections being significantly more Democratic and liberal than what we observe during other elections.[7]

[6]This was a particularly high-turnout, statewide election, so it gives us a useful measure of baseline differences in voter composition between districts holding timing constant. This model is analogous to including a "lagged" dependent variable for referenda elections that occur after 2008. For earlier years, the specification thus includes a "lead" of the dependent variable from a future high-turnout election.

[7]The coefficient on ideology for Texas special elections is negative, as is the case in the other three states, but is estimated imprecisely.

Voter Composition DV	State	Presidential	Midterm	General Statewide	Primary	Special
Democrats	California	57.26%	-1.87%***	NA	-2.37%***	-2.73%***
			(0.13)		(0.21)	(0.51)
	Ohio	44.23%	-2.19%***	-2.18%***	-2.91%***	-4.27%***
			(0.11)	(0.12)	(0.16)	(1.01)
	Texas	42.67%	-5.28%***	-7.79%***	NA	-3.64%*
			(0.72)	(1.14)		(1.45)
	Wisconsin	50.73%	-2.69%**	-1.35%	-1.44%	-4.41%*
			(0.83)	(0.90)	(0.99)	(2.05)
Liberal	California	48.00%	-1.23%***	NA	-1.43%***	-1.55%***
			(0.09)		(0.12)	(0.19)
	Ohio	40.53%	-1.83%***	-1.81%***	-2.34%***	-1.94%***
			(0.06)	(0.06)	(0.08)	(0.43)
	Texas	35.37%	-2.72%***	-3.41%***	NA	-0.18%
			(0.34)	(0.54)		(0.77)
	Wisconsin	40.51%	-1.74%***	-1.46%***	-1.46%**	-3.09%**
			(0.38)	(0.44)	(0.46)	(1.08)

Robust standard errors in parentheses, clustered by district
*** p<0.001, ** p<0.01, * p<0.05

Note: Each row presents results from individual regression models that are estimated separately by state. The models control for measure type, passage threshold, whether an earlier proposal passed or failed within the previous calendar year, and for the observed values of the respective compositional DV in the November 2008 presidential election in each district. Presidential elections serve as the omitted baseline category.

Table A.9: Partisanship and Ideology Using Cross-Sectional Variation

H Measuring Senior Share

Our estimates show that seniors made up about 35 percent of the electorate in referenda elections held concurrently with presidential elections for the states in our sample. This figure is about two times larger than the share of seniors reported in national exit polls. This section briefly discusses some of the reasons for the apparent discrepancy.

As we note in the manuscript, voter age is calculated at the time we downloaded the data from the Catalist database — between 2015 and 2017. Since we look at elections as early as 2000, many of the voters we identify as seniors based on their current age were actually somewhat younger at the time of the election. Table A.10 reports the mean share of seniors in our referendum data broken down by presidential election date. The share shrinks predictably over time, suggesting that a substantial part of the disparity between our data and national exit polls is due to this measurement issue. Ideally, we would adjust the Catalist data to calculate each voter's age at the time of the actual election. Unfortunately, we observe voter counts only within broad age bands, not the age of individual voters, so we cannot carry out this kind of adjustment without making strong assumptions about the distribution of voters within each age band used in the Catalist database.

	2000	2004	2008	2012
California	49.66%	42.56%	34.53%	31.7%
Ohio	NA	38.07%	34.32%	31.7%
Texas	NA	43.96%	39.82%	34.3%
Wisconsin	39.8%	39.04%	34.87%	30.51%

Table A.10: Percent Seniors by Presidential Election Year

The second part of the explanation is that our estimates are district-level, rather than population, averages. Using our Catalist age breakdowns for all school districts in November 2012 — not simply those with measures on the ballot in that election — Table A.11

	Unweighted Mean	Population-Weighted Mean
California	35.32%	30.18%
Ohio	31.76%	29.58%
Texas	36.42%	29.76%
Wisconsin	33.31%	29.51%

Table A.11: Percent Seniors in November 2012 by District, Weighted and Unweighted

shows how the compositional measure changes once we account for differences in district size. Weighing in this way reduces the share of seniors by about 5 percentage points. National exit polls in 2012 showed that voters 65 and older made up about 16 percent of the national electorate. Adjusting our estimates to account for (1) measuring senior status based on current age and (2) district population would account for much of the disparity between our estimates and the national benchmark.

I Senior Citizens and State Property Tax Regimes

This section briefly summarizes how each of the four states in our sample treats seniors in the administration and collection of local property taxes. The information in this section is based primarily on *Kiplinger*'s "State-by-State Guide to Taxes on Retirees,"[8] and in the case of California and Texas, our conversations with state experts.

California: California has a tax postponement program, which allows low-income seniors and people with disabilities to defer payment on property taxes. However, this program was suspended for part of the period we examine in our study. This is the only relief that is age-specific. However, the tax regime put in place by Proposition 13 indirectly benefits older residents. Proposition 13 was a tax-limitation amendment added to the state constitution in 1978. The measure caps property taxes at 1 percent of assessed valuations, and further limits these assessed valuations from increasing by more than 2 percent a year regardless of the actual rate of appreciation in the real estate market, unless property changes hands. One consequence of the measure is that long-time property owners — primarily older residents and owners of commercial property — enjoy significant advantages, paying a much lower rate of property taxes when measured against the market value of their home. This provision of Proposition 13 explains why Warren Buffet famously pays less in property taxes on his $4 million mansion in California than he does on his $500,000 home in Nebraska. Because taxes to repay school bonds are collected based on a percentage of the assessed valuation, this also means that seniors indirectly enjoy a break on these taxes compared to neighbors who purchased identical homes more recently, although the amount of the break depends on how long they've owned their home and how quickly home values have increased over time.

Ohio: Homeowners in Ohio pay property taxes on the assessed value of their property,

[8] Available here: `http://www.kiplinger.com/tool/retirement/T055-S001-state-by-state-guide-to-taxes-on-retirees/`

which is set at 35 percent of the current market value (although reappraisals happen only once every six years). Seniors receive a “homestead exemption,” which allows them to deduct $25,0000 from the market value of their home.

Texas: When homeowners turn 65 in Texas, they receive a “homestead exemption” similar to the one in Ohio, although about a third of the size. More importantly, as we discuss in the body of the manuscript, seniors also benefit from a “tax ceiling” that kicks in when they turn 65. Their property taxes are frozen and do not increase — even when the value of their home appreciates or if local tax rates go up. Taxes can rise only when a homeowner makes major improvements to their property, excluding normal maintenance and repairs. Note that the mandatory statewide tax freeze applies only to school-district taxes, although counties, cities, and community college districts can enact similar freezes voluntarily.

Wisconsin: Although Wisconsin has several exemption and deferral programs, these are based on income rather than age. There are no special tax breaks for the elderly.

J Additional Analyses and Comparisons

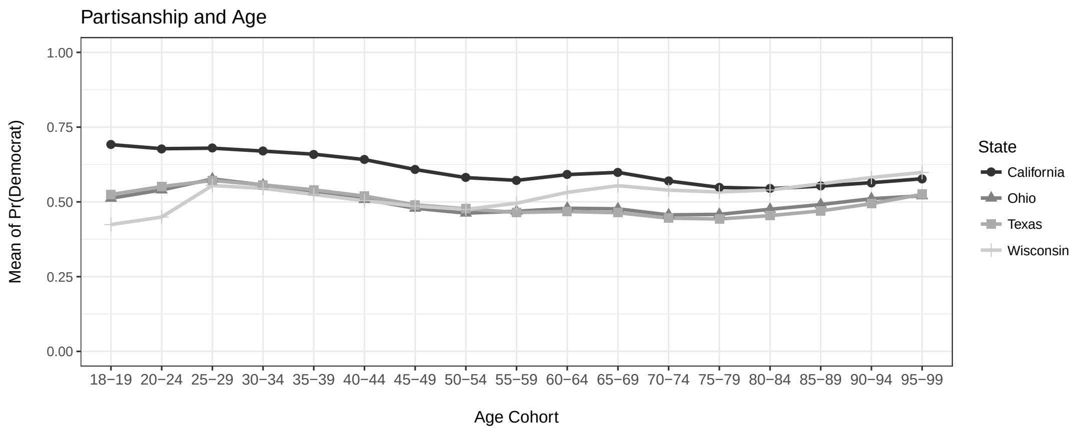


Figure A.4: Catalist-modeled Partisanship Probability by Age Cohort.

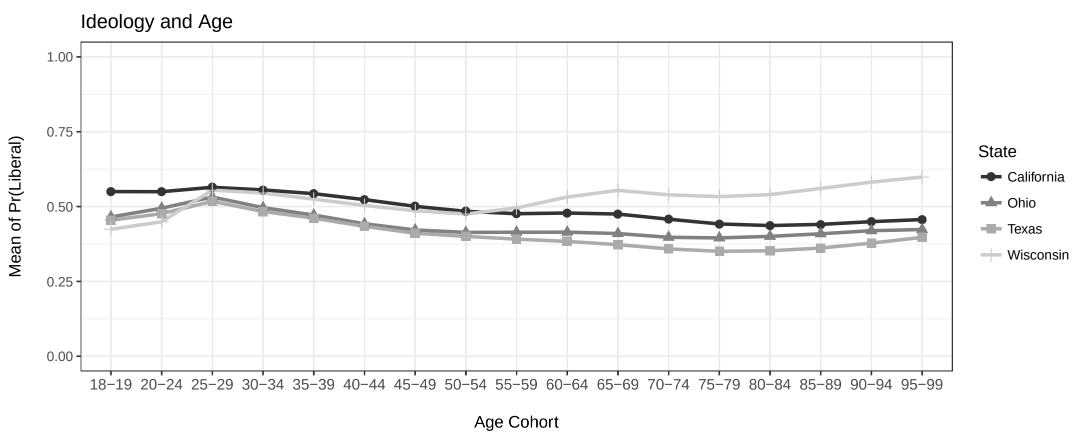


Figure A.5: Catalist-modeled Ideology Probability by Age Cohort.

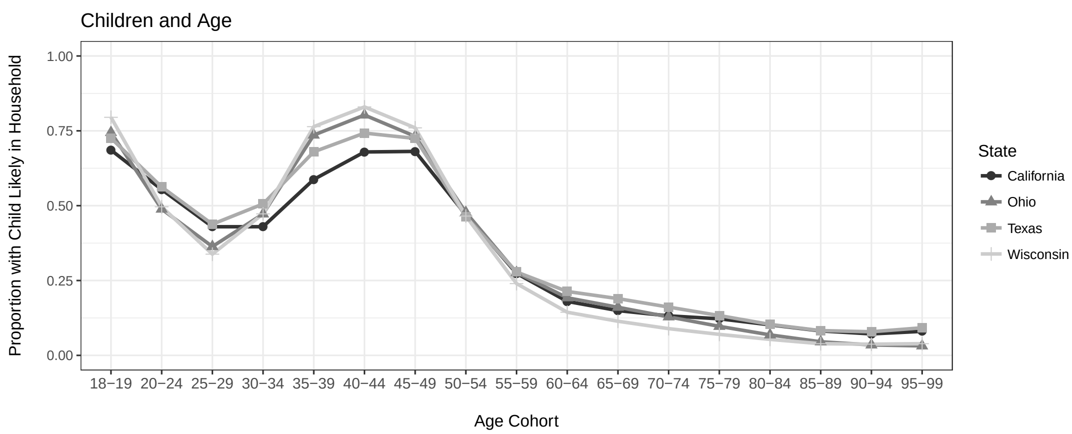


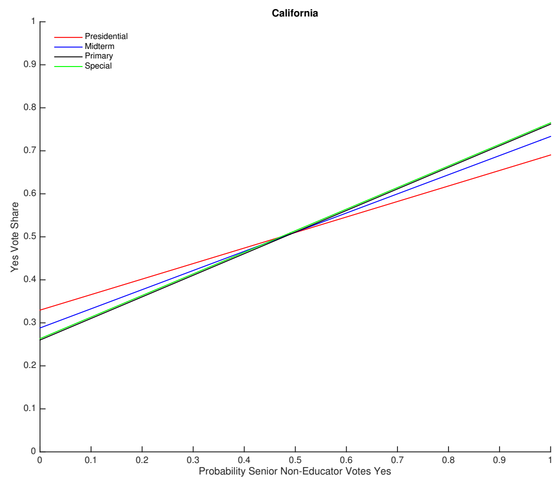
Figure A.6: Proportion of Voters with Children Likely in Household by Age Cohort.

K Consequences of Compositional Changes: Seniors vs. Education Employees

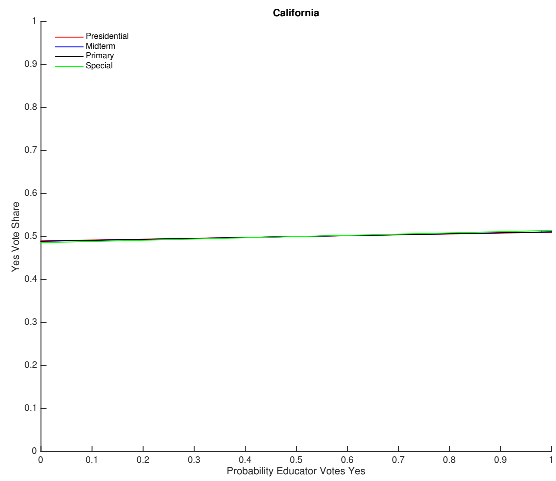
One way to consider the relative consequences of the compositional changes we document on tax referendum support is by calculating hypothetical election outcomes under alternative election times while varying the assumed behavior of each type of voter. This section reports the results of one such calculation to examine the electoral effects of seniors and education employees. In the analysis, we combine teachers, school support staff, and administrators into a single category of education employees, and sum the compositional effects for each subgroup that we report in our analysis.

We assume that education employee and senior citizen status are independent. For example, in California presidential elections 36.81 percent of the electorate are seniors and 1.99 percent are educators. The independence assumption gives us four cells with non-educator non-seniors consisting of 61.93 percent of the electorate, educator non-seniors (1.26 percent), non-educator seniors (36.08 percent), and educator seniors (0.73 percent). We repeat this exercise for each state and election time and then examine the aggregate vote share under alternative assumptions about voting behavior within each cell. In each left panel of Figure A.6, we assume that the probability an education employee (regardless of whether she is a senior or not) votes "yes" on the bond or tax referendum is 1 and that a non-senior non-educator votes "yes" with probability 0.5. We then compute the expected vote share as a function of the probability a senior votes "yes." We repeat this exercise for each election type using the electorate composition from our regression results. In the right panels, we assume that the probability a non-educator votes "yes" (regardless of whether she is a senior) is 0.5. We then compute vote share as a function of the probability an educator votes "yes." As in the left panel, we repeat this exercise for each election type using the electorate composition from our regression results.

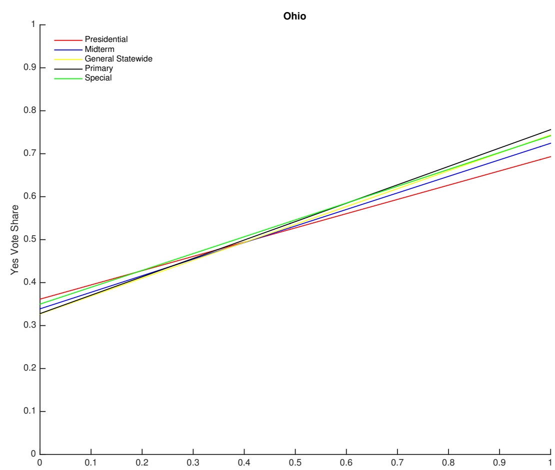
The analysis leads to two conclusions: First, variation in the vote choice of seniors is more consequential for election outcomes than the behavior of education employees. Second, even when education employees vote as a united bloc in favor of bond or tax increases, the magnitude of the election timing effects are quite small when we hold senior voting behavior fixed. Under the implausibly strong assumption that all educators vote "yes" with probability 1, moving from a presidential to special election date increases the "yes" vote percentage by only 0.43 percentage points in California, 1.86 percentage points in Ohio, 0.45 percentage points in Texas, and 1.04 percentage points in Wisconsin.



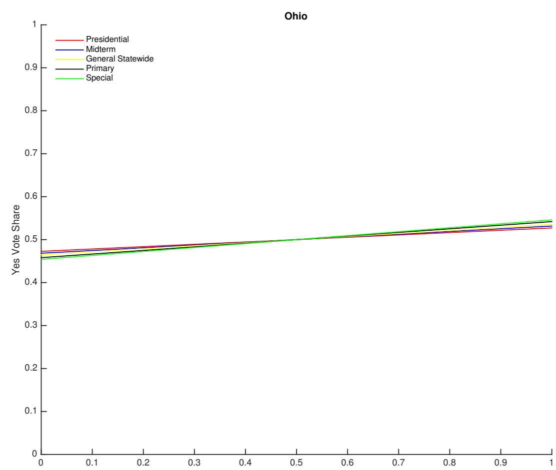
(a) California: Seniors



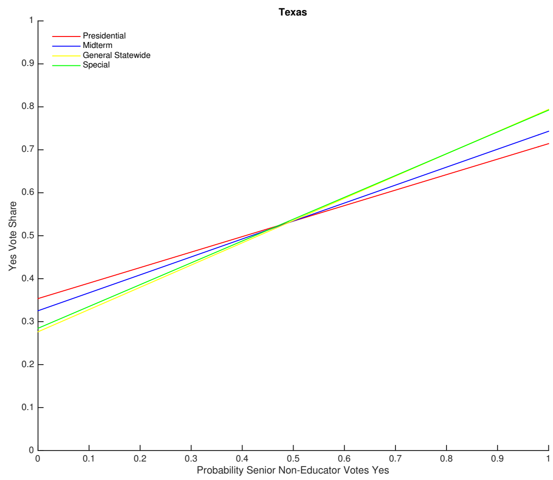
(b) California: Education Employees



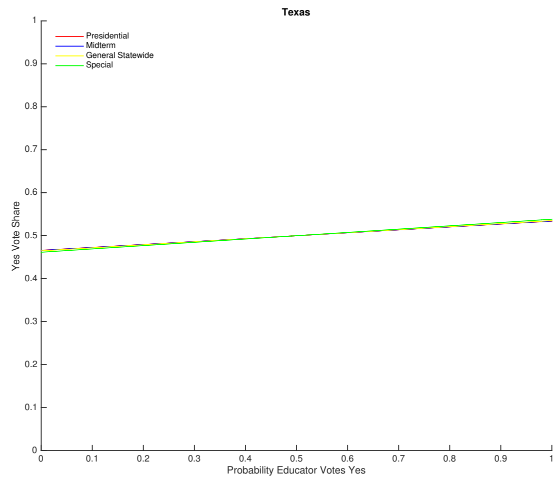
(c) Ohio: Seniors



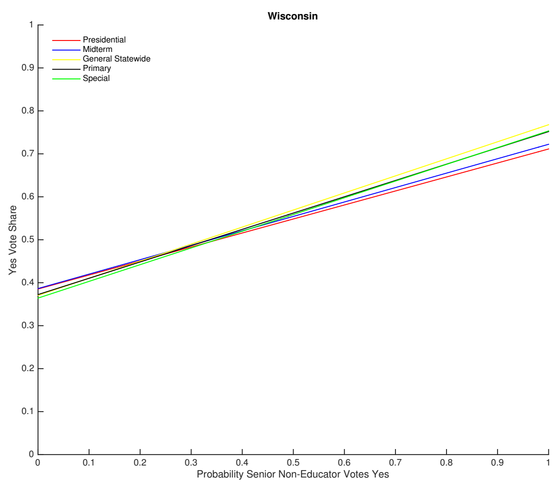
(d) Ohio: Education Employees



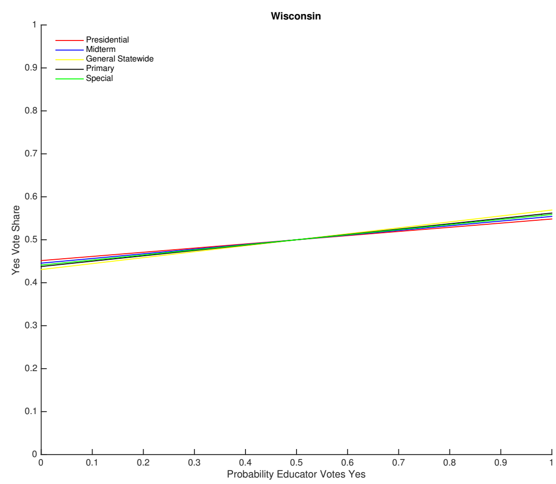
(e) Texas: Seniors



(f) Texas: Education Employees



(g) Wisconsin: Seniors



(h) Wisconsin: Education Employees

Figure A.6: Comparing Predicted “Yes” Vote Share Under Alternative Election Times

L Analysis of "Yes" Vote Share

Outcome Variable	State	Presidential	Midterm	General Statewide	Primary	Special
Percent Voting 'Yes'	California	66.20%	-2.76%***	NA	-3.17%***	-2.50%*
			(0.76)		(0.73)	(0.98)
	Ohio	51.81%	-0.34%	-0.01%	+1.10%**	-3.57%***
			(0.64)	(0.43)	(0.41)	(0.60)
	Texas[1]	54.13%	+14.50%**	+4.96%	NA	+14.88%***
			(4.48)	(3.54)		(3.26)
	Wisconsin	56.09%	-0.81%	-5.05%***	-1.11%	-3.22%*
			(1.60)	(1.23)	(1.32)	(1.59)

Robust standard errors in parentheses, clustered by district
*** $p<0.001$, ** $p<0.01$, * $p<0.05$

Note: Each row presents results from individual regression models that are estimated separately by state. The models include district fixed effects and control for measure type, passage threshold, and whether an earlier proposal passed or failed within the previous calendar year. Bond measures in presidential elections in districts where a measure has not failed recently serve as the omitted baseline category. The threshold for passage is 50% in Ohio, Texas, and Wisconsin. It is 55% for most school bonds in California and $\frac{2}{3}$ for tax measures.

[1] The "yes" vs. "no" vote breakdowns are not available for more than half of the Texas referenda prior to 2012, so these results may not generalize to the full sample of ballot measures.

Table A.12: Effect of Timing on 'Yes' Vote

M Direct Democracy vs. Candidate Elections: Evidence from California

Overall, we find little evidence that school district employees are the pivotal voting bloc in low-turnout special elections. The effects in the compositional analyses are far too small for this to be the case, and the differences in referendum passage rates between election dates are inconsistent with this claim (except in Texas). Our analysis focuses on school referenda both because these are consequential — research suggests that referendum passage instead of defeat has meaningful causal effects on student achievement (Cellini, Ferreira and Rothstein 2010, Kogan, Lavertu and Peskowitz 2017) — and because the within-district variation in timing provides us with a source of credible identification. By contrast, much of the existing literature on election timing and interest group influence focuses on candidate elections (e.g., to school boards). One may worry that our results in the context of direct democracy elections might not generalize to candidate contests, given the different electoral environments and informational costs.

We explore whether our compositional results can be generalized to local school board elections — the focus of Anzia (2011), Moe (2006), and Payson (2017) — by conducting a supplemental analysis in California, where there is significant within-state variation in the timing of these contests. Few districts change the timing of their elections during the period we examine, so we are limited to a cross-sectional analysis. To account for unobserved district heterogeneity that might be correlated with election timing, we control for the value of each dependent variable observed in each district in November 2008 just as we did in the cross-sectional results reported in Supplemental Appendix G.[9]

[9]This was a particularly high-turnout, statewide election, so it gives us a useful measure of baseline differences in voter composition between districts holding timing constant. This model is analogous to including a "lagged" dependent variable for referenda elections that occur after 2008. For earlier years, the specification thus includes a "lead" of the dependent variable from a future high-turnout election.

The results of this analysis are presented in Table A.13, and they are largely consistent with what we report for school referenda. The share of education employees increases by roughly 0.5 percentage points between presidential and special elections. By contrast, the average margin of victory in California school board elections during the period we examine is 9.5 percentage points — almost identical to the average electoral margin for the ballot measures. It thus seems highly implausible that election timing substantially increases the influence of interest groups through the compositional channel emphasized in prior research.

Voter Composition DV	Presidential	Midterm	Primary	Special
Democrats	50.90%	-1.66%***	-2.39%***	-1.47%***
		(0.06)	(0.57)	(0.24)
Liberal	44.11%	-1.06%***	-1.45%**	-0.56%***
		(0.05)	(0.46)	(0.13)
Teacher	1.93%	+0.14%***	+0.22%***	+0.36%***
		(0.01)	(0.05)	(0.04)
School Support Staff	0.03%	+0.00%***	+0.00%	+0.01%**
		(0.00)	(0.00)	(0.00)
School Administrator	0.21%	+0.01%*	+0.03%	+0.11%***
		(0.01)	(0.04)	(0.03)
White	71.50%	+2.19%***	+4.85%***	+5.52%***
		(0.09)	(0.51)	(0.40)
Family Income <$40K	28.12%	-0.31%	-2.88%*	-0.74%
		(0.21)	(1.42)	(0.41)
Family Income >$100K	32.16%	+0.59%***	+3.57%***	+1.38%***
		(0.16)	(0.92)	(0.39)
Child in Household	29.88%	-2.03%***	-5.59%***	-6.00%***
		(0.12)	(0.68)	(0.39)
65 and Older	40.11%	+4.31%***	+12.09%***	+12.94%***
		(0.14)	(1.13)	(0.39)

Robust standard errors in parentheses, clustered by district
*** p<0.001, ** p<0.01, * p<0.05

Note: Each row presents results from individual regression models that control for the observed values of the respective compositional DV in the November 2008 presidential election in each district. Presidential elections serve as the omitted baseline category.

Table A.13: Effect of Timing in California School Board Elections

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