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Election Timing, Electorate Composition, and Policy Outcomes: Evidence from School Districts

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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 overrepresented 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.*

Replication Materials: The data, code, and any additional materials required to replicate all analyses in this article are available on the *American Journal of Political Science* Dataverse within the Harvard Dataverse Network, at: https://doi.org/10.7910/DVN/8R3LTP.

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 near-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 have reason 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 (e.g., Uhlaner 1989).

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

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[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).

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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 represent 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 data sets 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 since 2000. 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 micro-targeting 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 academic research (and conventional wisdom among practitioners) focuses specifically on how election timing shapes who votes, we provide the first systematic examination of voter composition using a convincing empirical strategy.

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% of the overall electorate. Timing's largest effect is on the size of the elderly voting bloc, which consists of individuals who are least likely to have school-age children. The share of the elderly is approximately 20–40% 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 these measures are most likely to pass during special elections. Texas also happens to be the only one of the four states where school property taxes are capped for those over 65. Thus, it appears that tax and bond measures are more likely to pass in off-cycle elections because Texas makes taxes and bonds nominally free to seniors.

These findings provide strong reason to reconsider the claim that interest groups—in our context, teachers unions—represent the pivotal voting bloc in 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, employee salaries and benefits make up more than 80% 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. 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 ($p = .001$).[2] 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.

Why Off-Cycle Elections Could Increase Odds of Passage

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, 75) 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." As they explain:

> 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.... Thus the de-facto 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. (1974, 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" (2013, 21). Since interest groups have only fixed human and financial resources to mobilize other supporters, these resources could—all else equal[3]—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.

Why Off-Cycle Elections Could Decrease Odds of Passage

One potential countervailing effect comes from other compositional changes in the electorate that may occur

[2]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.

[3]All else might not be equal if, for example, off-cycle mobilization is more costly or voters who turn out in low-salience elections are more difficult to persuade because they have entrenched preferences.

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, 75) 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."

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 United States. 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, whereas others may decrease it, leaving the net effect a largely open empirical question. In the next section, we describe why our two new data sets 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.[4] The precise years vary slightly between states,[5] and the final data set 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.

Table 1 Overview of Referenda Data

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%

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 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.

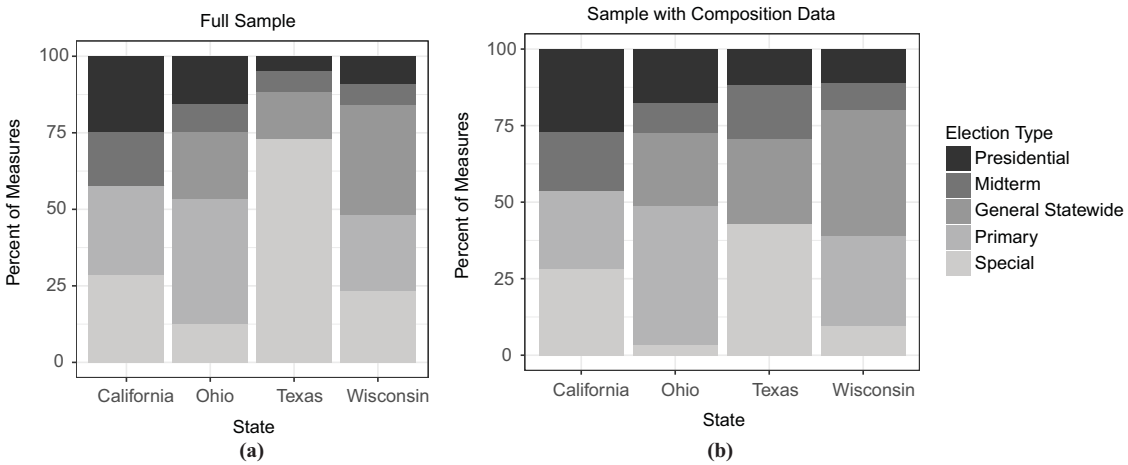
In Ohio, districts may place school-related referenda on the ballot in February and August special elections, November general elections, or the May primary. In presidential election years, the primary takes place in March, and no February special elections are held. Each school district can return to 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, state law

[4]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.

[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% of districts in California, 90% in Ohio, 87% in Texas, and 78% in Wisconsin. Additional descriptive statistics are provided in Appendix A in the supporting information (SI).

FIGURE 1 Distribution of Election Dates



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 either include statewide ballot measures or feature candidates from multiple jurisdictions.[7] The final two categories are primary and special elections. California does not hold statewide elections aside from those occurring in November of even years, so our data set 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.

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.[8] But districts also have imperfect control over the planning and preparation efforts that precede a referendum campaign and, in the case of Ohio, are further constrained by the expiration of previously approved levies.

Our second data set 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 includes both voter partisanship and ideology, defined as the predicted probability of identifying as a Democrat or a liberal.[9] 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 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

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

[8]There may also be idiosyncratic factors unique to each district that we cannot observe but which may be known to local leaders (e.g., Meredith 2009).

[9]Appendix C in the SI 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.

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, 443–44).

The Catalist variables capture only education employees themselves—excluding their spouses and other potential allies.[10] But we believe these measures are likely to represent the very upper bound for the size of the interest group voting bloc in each election for two reasons. First, our data identify education employees based on their district of residence, not employment. As Moe (2006) shows, only about half of the school employees in his sample of districts live in the same district as the one that employs them, so our measure likely includes school employees who do not have a personal stake in the outcome of elections in the district where they are allowed to vote. 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. 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 in 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.[11] 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 data set we assembled provides more fine-grained details about the voters participating in local elections than have 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.[12] 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.[13] In some cases, 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 Appendix D in the SI, which collectively show that the data are of sufficient accuracy for our analysis.

[10] 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 for between 70% and 80% of all full-time K–12 education employees in the states in our sample. Since many noninstructional occupations (e.g., counselors, principals) also require state licensure, our coding should capture the vast majority of public school employees. Nevertheless, since we do not account for other sympathetic voters (including those mobilized by the unions), our estimates may not fully capture the "group effect."

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

[12] This occurred sometime between 2015 and early 2017, depending on the state.

[13] Fortunately, Catalist does not "purge" its data set to remove voters who die or fall off the voter rolls.

[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, and 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 the timing of referenda, 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 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 are due to districts' returning to the ballot in an effort to overcome an initial failure.[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 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.

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

[16] We exclude a small number of observations where turnout exceeds 100%, although including them does not affect the results.

[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 Appendix E in the SI 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 nonrecurring tax cap increases as tax measures.

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

TABLE 2 Effect of Timing on Turnout

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)

Note: Robust standard errors are in parentheses, clustered by district. 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.
***$p < .001$, **$p < .01$, *$p < .05$.

Note that the significance flags mean that the election timing indicators for nonpresidential 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 elections is under 30% across all four states and below 20% of the voting-age population in both California and Texas.

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 they are not the primary focus of the political science literature, we discuss below how these latter characteristics may have important 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 sizable 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% in high-turnout presidential elections to about 2% 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

Table 3 Effect of Timing on Political Composition of Voters

Voter Composition DV	State	Presidential	Midterm	General Statewide	Primary	Special
Democrat	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)

Note: Robust standard errors are in parentheses, clustered by district. 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.
***$p < .001$, **$p < .01$, *$p < .05$.

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 emphasize 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 versus 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% 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% of the electorate during special elections.[20]

The most substantial difference 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%[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 and 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 Appendix J in the SI, these two quantities are strongly correlated with each other.

[23]In Appendix H in the SI, 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

Table 4 Effect of Timing on Education Employees as Share 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)

Note: Robust standard errors are in parentheses, clustered by district. 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.
***$p < .001$, **$p < .01$, *$p < .05$.

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,[25] 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.[26] 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

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 models that include year fixed effects.

[25]We describe these differences in Appendix I in the SI.

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

Table 5 Effect of Timing on Demographic Composition 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)

Note: Robust standard errors are in parentheses, clustered by district. 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.
***$p < .001$, **$p < .01$, *$p < .05$.

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. We present the results of models that employ an alternative specification in Appendix G in the SI. The additional analysis shows that partisan and

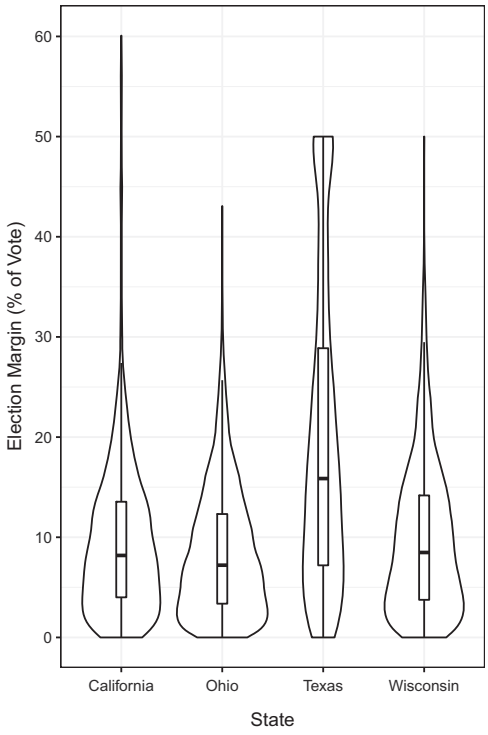
ideological dynamics in Texas largely mirror those of the other three states.

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% of voters in these high-profile elections in California but more than 4% in Wisconsin. One potential explanation is that the baseline differences 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[27]—are likely to be tagged as "false positives" in the firm's voter file. The historical availability of licensure data varies by state, and it seems to be particularly good in Wisconsin. One clear indicator of this is that more than 40% 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).

Consequences for Public Policy

Overall, we find that election timing has a statistically significant effect on voter composition. However, the magnitude of this effect on voter characteristics 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 1 in 10 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.

FIGURE 2 Distribution of Win/Loss Margins by State



Note: The "yes" versus "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.

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%, and it is considerably larger in Texas.[28] 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] Federal data suggest that a fifth of all newly minted teachers leave the profession within 5 years (Gray and Taie 2015), although other estimates suggest the figure could be as high as 50%.

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

Table 6 Effect of Timing on Passage Probability

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)

Note: Robust standard errors are in parentheses, clustered by district. 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 two-thirds for tax measures.
***$p < .001$, **$p < .01$, *$p < .05$.

We do, however, find very large effects for voter age, and these may be consistently consequential.[29] 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-age 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.

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 referendum passage, presented in Table 6.[30] For this dependent variable, 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's 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.

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.[31] 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 2016b). 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

[29] As we demonstrate in Appendix K in the SI, 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.

[30] We report analogous estimates for the percent of votes cast in favor of each measure in Appendix L in the SI.

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

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, with both emphasizing 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 do not monitor local districts as attentively, making it easier for administrators and policy makers to be captured by employee interest groups. Alternatively, in the absence of firsthand 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 microlevel 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 (2016a) and Holbein (2016). Both examine the "Adequate Yearly Progress" (AYP) achievement designation created by the No Child Left Behind Act and ask how it affected incumbent reelection 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 (2016a) examine odd-year school board elections in Ohio, whereas Holbein (2016) focuses on even-year November elections in North Carolina.[32] Given the large compositional differences we find among the electorates between these election times, there is little reason to expect voters in these two sets of elections to be similarly responsive to school performance information.[33] Instead, we believe the key is in the share of voters who are seniors. These individuals may be particularly unresponsive to school performance information, both because they are unlikely to have school-age 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.[34]

Finally, we believe our results may have implications for educational governance by highlighting a potential 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. Existing research in both public administration and education policy finds that having a "representative bureaucracy"—government administrators who are demographically similar to the clients they serve—can have important benefits for service delivery. If underrepresentation of certain voter groups in the electorate affects the demographics of schoolteachers and other staff (among other policies), election timing may be both an important cause of concern and also an area of potentially beneficial reform, particularly in jurisdictions that utilize off-cycle elections.

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

[33] 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.

[34] 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.

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

Appendix A: Descriptive Statistics
Appendix B: Special Elections by Month
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