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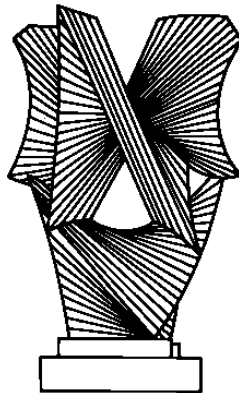
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## VOTERS, NON-VOTERS AND THE IMPLICATIONS OF ELECTION TIMING FOR PUBLIC POLICY

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# Voters, Non-voters, and the Implications of Election Timing for Public Policy

Christopher R. Berry \*

Jacob E. Gersen \*\*

## Abstract

This paper makes use of variation in the timing of local elections to shed light on one of the core questions in democratic politics: what would happen if everyone voted? Does a low voter turnout rate imply that a small subset of special interest voters controls politics and policy? Or, are voters largely representative of non-voters such that neither the outcomes of elections nor resulting public policies would change even if everyone participated? Rather than rely on surveys of nonvoters to extrapolate their hypothetical behavior, we rely on a natural experiment created by a 1980s change in the California Election Code, which allowed school districts to change their elections from off-cycle to on-cycle. Because we are able to observe very large within-district changes in voter turnout resulting from changes in election timing, we are able to isolate the effect of turnout on policy outcomes, including teacher salaries and student achievement tests. Our analysis demonstrates that changes in voter turnout do affect public policy, but modestly.

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## I. Introduction

How would public policy change if everyone voted? Does a low voter turnout rate imply that a small subset of special interest voters controls politics and policy? Or, are voters largely representative of non-voters such that neither the outcomes of elections nor resulting public policies would change even if all eligible voters participated in politics? These longstanding questions are at the core of democratic politics and they continue to beguile modern scholarship (Citrin, Schickler, and Sides 2003; Leighley and Nagler 2009).<sup>1</sup>

The conventional approach to this question relies on survey data to compare the partisanship and policy preferences of voters with those of non-voters, makes extrapolations as to how non-voters would have voted (if they had voted), and asks whether their hypothetical votes would have changed election outcomes. While this approach is both sensible and has been quite fruitful, it also suffers from three notable limitations. First, it assumes that unobservable differences between voters and non-voters—that is, differences in attributes or attitudes not measured in the survey—do not confound the extrapolation from survey responses to vote choice. If a voter and a non-voter differ in some unmeasured way, then it may not be the case a non-voter would make the same vote choice as a voter with the identical observable characteristics. Second, the approach assumes that the politics surrounding the election would not change under the counterfactual of full turnout. But if politicians expected non-voters to turnout, other aspects of the campaign might change accordingly. For instance, if candidates changed their platforms or tactics to appeal to erstwhile non-voters, then the vote choice of *both* groups might change relative to the current state of the world. Finally, and in our view most

importantly, the survey-based approach can say little about how *policy* would change as a result of increased turnout. That is, regardless of whether the identity or party of the winning candidate changes, the ultimate question scholars of politics should care most about is whether implemented public policy would change if turnout increased. This latter question cannot be answered without an additional step of extrapolation beyond survey data.

This paper offers a new approach to these questions, one that we view as complementary to the existing literature. Our research design takes advantage of a 1980s change in the California Election Code that allowed school boards to change their elections from odd years (off-cycle) to even years (on-cycle). This simple change in scheduling, we will show, produced more than a 150 percent increase in voter turnout in school board elections. Because we are able to identify dramatic changes in turnout in similar elections over time that do not stem from differences in the underlying substance of the elections themselves, we are able to avoid some of the pitfalls that have challenged prior studies. We are able to observe elections within the same political jurisdiction under conditions of high and low voter turnout and to identify resulting changes in policy outcomes. We then analyze a conventional measure of interest group influence, teacher salaries, as well as a conventional measure of aggregate performance, student test scores. Our analysis demonstrates that dramatic changes in voter turnout for school board elections produce relatively small, but statistically significant, effects on substantive education policies. We cannot say whether this is because voters in an election with low turnout have similar preferences to voters in the high turnout case, but we can say that the effect of increased turnout on policy is relatively modest. Thus, using a new and different empirical approach that focuses on policy

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<sup>1</sup> Andrew Gelman provides an accessible and informative introduction to these questions:

outcomes directly, our results are consistent with an accumulation of past studies suggesting that substantial increases in voter participation would not substantially alter the outcomes of the democratic process.

## II. Background

There are three dominant views in political science about the relationship between voters and non-voters. One strand of scholarship dating at least back to Wolfinger and Rosenstone (1980) argues that changes in voter turnout would produce negligible effects on electoral outcomes. As Highton and Wolfinger put it (1999) “voters differ minimally from all citizens” (Bennett and Resnick 1990; Gant and Lyons 1993; Norrander 1989). And because nonvoters are a diverse group rather than one with uniform preferences, the probability that electoral outcomes would shift if nonvoters voted is thought to be relatively small. While low rates of political participation might be troubling for some independent theory of the political good, on this view, even significant increases or decreases in the voter turnout would be unlikely to change the outcomes of elections.

A second prominent view holds that the voting public actually has significantly different preferences from the nonparticipating public and that it matters for public policy. Leighley and Nagler (2009) argue that moderates are under-represented in the voting population (relative to the universe of nonvoters) and conservatives are over-represented, a gap that has increased in the past several decades. Voters, on this view, differ significantly from nonvoters at least raising the specter that elections with higher participation would generate different political outcomes.

Other scholarship attempts to link policy outcomes and rates of voting with cross sectional data: states with higher rates of voting among less affluent demographic groups have policies that are friendlier towards low income populations (Hill and Leighley 1992, 1995).

A third view agrees with the descriptive claim that voters and nonvoters are different, but raise doubts that electoral outcomes would routinely differ even if more nonvoters were to vote, largely because so few elections are competitive enough for the differences to matter (Citrin, Schickler, and Sides 2003). Alternatively, even if the same officials would be elected, it could be that those officials would be more responsive to the views of voters than nonvoters (Griffin and Newman 2005; Bartels 2009; Gilens 2005), implying that policies might differ as a function of turnout even if the winners of any given election would not change.

Much of the related literature focuses on federal elections, but recent work has also targeted local elections, where turnout can be significantly lower, in which changes in turnout may be more likely to affect electoral outcomes (Hajnal, Lewis, and Louch 2002; Bridges 1997). For example, Hajnal and Trounstine (2005) find that in city elections, lower turnout leads to substantial reductions in the representation of Latinos and Asian Americans on city councils and in the mayor's office. Indeed, this recent work echoes much older work on the importance of differential turnout in local government elections as a determinant of policy outcomes. The possibility that single-function elections might be dominated by interests whose preferences deviate from the median voter in the broader electorate was a motivating insight in early work on school district elections (Rubinfeld and Thomas 1980; Rubinfeld 1977), which sought to use survey methods to demonstrate that large changes in turnout shifted the preference of the median voter on school funding questions.

An important empirical challenge for the voters versus nonvoters debate is that one must make counterfactual inferences about how nonvoters would behave if they were to vote or about what policies would have been selected had political participation been different. The most common approach is to estimate how citizens who did not vote would have voted by matching their demographics and political views to voters in the population. Unfortunately, using demographics to predict how nonvoters would have voted is challenging because the two groups differ in a key—arguably, *the* key—respect: their willingness to bear the costs of political participation. This dimension may also be correlated with political views and electoral behavior. Direct surveys of nonvoters make this task somewhat less heroic (e.g. Citrin et al. 2003), but one still needs to posit a model of political participation in the face of a revealed preference for nonparticipation.

If estimating the impact of differential turnout on who wins elections is hard, identifying an effect on real world policy is even more daunting. First, virtually all prior studies rely on cross sectional data, asking whether policy outcomes in a high turnout jurisdiction differ from the policy outcomes in a low turnout jurisdiction. But, of course, policy choices are the result of an enormous number of factors that differ across communities, some of which are observable but many of which are not. Thus, the inference problems that always challenge cross-sectional analysis are particularly relevant in this setting. Second, and related, prior scholarship has focused mainly on elections of general-purpose government officials, such as presidents, congress members, or governors, with responsibility for a wide variety of different policy issues. The marginal impact of turnout on any particular dimension may be small and extremely difficult to isolate in practice. To illustrate, one might study congressional elections during midterm



versus presidential years, thus usefully confining the analysis to the same jurisdiction under differential turnout conditions. However, it is not at all obvious how one would even go about asking whether public policy changed as a result.<sup>2</sup> To even begin to estimate the effect of voter turnout on public policy, one needs a relatively large change in turnout, preferably within the same jurisdiction over time, that is uncorrelated with the substance of the given elections, in an electoral setting with a clearly defined policy domain. This is what our research design seeks to accomplish.

### III. Election Timing, Selective Participation, and Public Policy

Our approach is motivated by a small but growing literature on the timing of elections in local government (see Berry and Gersen 2010). That topic is important unto itself given that most elections in the United States are not federal elections, but state and local government elections. Indeed, there are more than 500,000 elected officials in the United States, and fewer than 600 of them are federal officials (Berry and Gersen 2009). Among local governments, moreover, there is enormous heterogeneity with respect to when elections are held. Some localities hold all elections on the same day in November; other local political jurisdictions hold elections for different offices on entirely separate days during at different times of the year. In some localities there is at least one local government election in eleven months of the year (Souzzi 2007). Amidst this great heterogeneity, one widely known and well accepted fact is that

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<sup>2</sup> Halberstam and Montagnes (2009) compare the voting records of US Senators first elected in presidential election years relative to those first elected during midterm election years and find that the former exhibit more ideologically extreme voting patterns. Although their analysis does not speak to policy outcomes directly, they do show a clear linkage between concurrent elections and post-election behaviors of politicians which may have important policy implications.

turnout in local elections is notably higher when those elections are held concurrently with major national or state races (Hajnal et al. 2002).

While most of the literature on turnout and election timing is based on cross-sectional comparisons of jurisdictions with different election schedules, our analysis is based on a within-jurisdiction analysis over time. Specifically, we exploit a change in the law which led to massive increases in turnout in school board elections in California. Because we can observe policy outcomes within the same electoral environments, indeed the exact same jurisdictions, under conditions of high and low voter turnout, we can more directly link policy changes to changes in political participation. Rather than extrapolating from the preferences of voters to the preferences of nonvoters, from preferences to election outcomes, and from election outcomes to policy, we can simply compare policy outcomes before and after the change in election timing.

In considering the relevant policy outcomes for our analysis, we work from a simple model of voter behavior. We assume that whenever an election is held, there will be some citizens who are indifferent between voting and not voting. For this group of citizens, the benefits of voting are roughly equal to the costs of political participation. As participation costs increase, these voters will stop participating and as a result, the median voter in the group of actual voters will change. Similarly, as participation costs decrease, some citizens who were unwilling to bear the costs of voting previously may choose to participate, again changing the identity of the median actual voter in the election. That is, the observed or actual median voter is endogenous to the political participation cost structure (Dunne, Reed, and Wilbanks 1997). As participation cost rise, the voters who continue to participate in elections should be those with the most at stake in the outcome. Here, and elsewhere, we refer to this as *selective participation*

(Berry & Gersen 2010; Berry 2009): the pool of actual voters in a given election is a selective function of voter interest—potential gains or losses from the electoral outcome. Because rising costs of participation drive out potential voters from an election selectively, the substantive political preferences of actual voters should diverge from the political preferences of nonvoters in the jurisdiction. Importantly, this a comparative claim. As between two otherwise identical hypothetical elections, the pool of actual voters will differ as a function of the participation costs. The higher are the costs of participation, the greater the predicted divergence between the preferences of voters and non-voters.

To illustrate, consider two elections for school board membership. The first takes place in April and is the only election on that day. The second takes place in November on the same day and at the same location as elections for other local, state, and national offices. The selective participation framework suggests that the preferences of the voters in the oddly timed school board election will not only be different from the pool of voters in the November school board election (cf. Rubinfeld and Thomas 1980; Rubinfeld 1977; Berry and Gersen 2010), but also that the distance between the median voter and the pool of potential voters in the jurisdiction will be larger for the oddly timed election than the November election. Changes in participation costs associated with the timing of elections, therefore, provide a particularly natural way to shed light on the voter-nonvoter problem. Indeed, a couple of excellent papers have already explored these ideas in the context of school *bond* elections (Dunne, Reed, and Wilbanks 1997; Meredith 2009), showing that bonds are more likely to pass during elections held off-cycle, due to the differing, and more supportive electorate, that goes to the polls. Berry (2009) extends the logic from school bond elections to elections for governing boards.

In the case of school board elections, it is widely acknowledged that teachers unions are the single most influential interest group (Hess 2002). Moreover, Moe (2006) has shown that teachers are two- to seven-times more likely to vote in school board elections than are other citizens. The selective participation framework suggests that special interest voters—for example, union members—will be more influential in off-cycle than on-cycle elections. A standard measure of the political influence of public sector unions is the salary of public employees.<sup>3</sup> Therefore, the first policy outcome we analyze is teacher salaries. Specifically, we ask whether the salary schedules negotiated between school boards and union representatives are more favorable when districts operate on low-turnout, off-cycle election schedules.<sup>4</sup>

Importantly, the selective participation argument is not a normative one. When participation is most costly only the voters who care most intensely about the issue at stake will turn out. On the one hand, special interests may use their electoral influence to secure particularistic benefits for themselves at the expense of nonvoters. On the other hand, special interests are likely to be precisely those voters with the most information and the greatest expertise regarding the issue at stake, and their participation may result in better candidates being elected (or worse candidates being voted out), ultimately leading to better public policy. Which of these two effects dominates in any given case is an empirical question.<sup>5</sup> Thus, in addition to

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<sup>3</sup> We follow a significant literature in using public employee's salaries as a dependent variable in an analysis of political influence. The related literature is vast, but important contributions include Babcock and Enberg (1999), Baugh and Stone (1982), Bellante and Long (1981), Courant, Gramlich, and Rubinfeld (1979), Ehrenberg and Goldstein (1975), Farber (1986), Fogel and Lewin (1973), Freeman (1986), Freund (1973), Kleiner and Petree (1988), O'Brien (1992, 1994), Summers (1973), and Rose and Sonstelie (2006). Reviews of the literature, though now somewhat dated, are provided by Aaron, Najita, and Stern (1988), Gregory and Borland (1999), and Stone (2002).

<sup>4</sup> Trounstine (2010) finds that municipal employees in cities with off-cycle elections earn more than those in cities with on-cycle elections, and Anzia (forthcoming) reports similar findings for teachers, although both analyses are strictly cross-sectional.

<sup>5</sup> This basic tradeoff—namely that delegating to those with expertise may generate better decisions but also

teacher salaries, we also analyze student test scores. If off-cycle elections encourage participation by a more informed electorate, schools may ultimately perform better. If so, then we should expect to see student test scores decline following a change to on-cycle elections.

Before turning to the data, however, we note at least two good reasons to expect that our hypothesized effects might not, in fact, materialize. First, the selective participation thesis may simply be wrong. If the decision to vote is motivated by some factor that is unrelated to policy preferences—say, the sense of “duty” to vote—then voters may be a fairly representative sample of the electorate regardless of the timing of the election (Elcissor and Leighley 2001; Highton and Wolfinger 2001; Verba et al 1995). Second, in the context of local government specifically, some versions of the Tiebout model suggest that policy is shaped by interjurisdictional competition more than by local politics (Perroni and Scharf 2001; Sprunger and Wilson 1998; Rausher 1998; Rose-Ackerman 1983; Sonstelie and Portney 1978). If local governments compete with each other for an increased tax base, then the “right” bundle of public goods, taxes, and spending should be provided in each jurisdiction. Although this view is itself sometimes contested (e.g. Epple and Zelenitz 1981), a common theme in the local political economy literature is that “voting with your feet” makes voting at the ballot box superfluous.<sup>6</sup> Ultimately, these are empirical questions, and we seek to shed light on them in the next section.

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gives the expert some latitude to exploit the principal—is a very general problem and a core element of literature on mechanism design (Mas-Colell et al. 1995). The rationale for delegating authority to committees in Congress exhibits similar concerns (Shepsle and Weingast 1994). Delegating some policymaking authority to specialized committees may be an efficient way for the chamber to generate informed policies, but committees may also use their informational advantages strategically to benefit their members rather than the chamber (Krehbiel 1991; Gilligan and Krehbiel 1987).

<sup>6</sup> For an extended discussion of these ideas, see Berry (2009, chap. 7).

#### IV. Empirical Analysis

We focus our analysis on local government elections in California for two reasons. First, there is a rich archive of electoral data available from the Center for California Studies at Sacramento State University. As explained below, this archive enables us to analyze thousands of local elections spanning 1996 through 2006. In most other states, by contrast, election data are maintained at the local level and must be collected on a cumbersome county-by-county basis.<sup>7</sup>

The second and more important reason for analyzing California is that there has recently been a large scale change in the timing of school board elections in the state. Prior to 1986, school district elections were held in odd-numbered years, while most local government and state government elections were held in even-numbered years. In the mid-1980s, the California Assembly passed Assembly Bill (AB) 2605, which authorized school districts to consolidate elections of board members with primary or general elections held in the county in which the district is located. The bill seems to have been overwhelmingly supported and the legislative history reveals that virtually all of the political rhetoric focused on the cost savings that would accrue from election consolidation and on the possibility of increasing voter turnout—generally described as an unqualified democratic good.<sup>8</sup> Because of a then-recent change allowing other special districts to shift the date of their elections, had the bill failed, school districts would have

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<sup>7</sup> An exception is South Carolina, which “is the only state that centrally collects precinct-level election data for local school board races” (Berry and Howell 2007).

<sup>8</sup> The Republican Analysis of AB 2605, California State Assembly, Assembly Elections and Reapportionment Committee (Aug. 22, 1986), explains that consolidated elections will increase voter turnout and thereby reduce the power of special interests like teachers’ unions. The Senate Rules Committee (July 3, 1986) noted that the bill would lead to cost savings by allowing for the consolidation of elections. Some supporters thought the bill would “would provide a broader base of support for the public school system” (Letter from Jeffrey N. Hamilton, Superintendent, Fort Jones Union Elementary School District, to Johan Klehs, Chairperson, Assembly Elections and Reapportionment Committee (Apr 4, 1986). Others emphasized cost savings (Letter from Bob L. Blacett, District Superintendent, Modoc Joint Unified School District, to Johan Klehs, Chairperson, Assembly Elections and Reapportionment Committee (Apr 2, 1986); Letter from James M. Donnelly, Director, Governmental Relations, to

been the only special district legally required to hold elections in odd years. As a result, at least one member of the legislature was concerned that school boards would be forced to pay all of what had been shared election costs.<sup>9</sup> The modest debates in the press mirror these same concerns (e.g. Maeshiro 2005). The little opposition to the bill that did emerge was generally focused on a provision of the law that required approval from the board of supervisors of the county in which the school board changing election dates was located. Some administrators thought the decision should be left to the school boards alone.

Following the passage of AB 2605, California experienced a widespread shift in the timing of school district elections. Whereas all school board elections were held in odd years prior to the change in the law in 1986, our estimates indicate that roughly two-thirds of the state's districts had changed their election dates to even years by 2006.

The changes in local election timing were enabled by changes in state policy, namely the passage of AB 2506. Because these statewide changes were exogenous from the perspective of individual local jurisdictions, we have a sort of “natural experiment” that allows us to estimate the effect of election timing on political participation and policy outcomes. Indeed, a major distinguishing feature of our analysis is that we are able to observe electoral and policy outcomes within a jurisdiction over time before and after a change in election timing that results in massive increases in turnout. The advantages of this *differences-in-differences* approach are significant when compared to a traditional cross-sectional analysis. A cross-sectional analysis compares outcomes from one set of jurisdictions holding even-year elections to outcomes from a different

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Johan Klehs, Chairperson, Assembly Elections and Reapportionment Committee (Feb 27, 1986). These letters are part of the legislative history of the bill and on file with the authors.

<sup>9</sup> Assemblyman Richard Robinson noted that “without enactment of AB 2605, school districts could . . . be left to pay the full costs for conducting the expensive, low-turnout elections in the off years” ) Letter from Richard Robinson, Assemblyman, 72d District, to George Deukmajian, Governor, State of California (Aug. 21, 1986).

set of jurisdictions holding odd-year elections. The differences between the two types of jurisdictions may be attributable to the effect of election timing, but the differences may also be due to other factors that differ systematically between jurisdictions holding even- versus odd-year elections. For example, California school districts that hold elections in even years are smaller and less urban than districts that hold elections in odd years, and have a lower proportion of students that are eligible for free or reduced-price lunch, an indicator of poverty (see Table 1). While it is, of course, possible to control for measurable district attributes in a statistical analysis, it is not possible to control for the unobservable aspects of the districts that are also correlated with election timing and voter participation (for example political interest or social capital). The policy change in California allows us to examine outcomes within the same district before and after a change in election timing. As long as other attributes of the district do not change before and after the shift in election timing, we can be more confident that the observed differences in outcomes are the result of the electoral regime.

Our analysis proceeds in two steps. First we examine the effect of election timing—specifically, the concurrence of major state and federal elections—on turnout in school board elections. Next, we investigate the effect of election timing on two related policy outcomes: teacher salaries and student test scores.

#### *A. Timing and Turnout*

That turnout in local elections is higher when they coincide with major national and state races is hardly a controversial proposition. For example, Hajnal, Lewis, and Louch (2002) found that turnout in California municipal elections roughly doubles (from about 18 to 35 percent of



adult residents) when those elections coincide with a presidential or gubernatorial election. Based on a national survey, Hess (2002) finds that turnout among registered voters in school board elections averages about 44 percent when those elections are concurrent with higher level offices, but only 26 percent when they are held separately. Like most of the literature, these two studies rely on cross-sectional data. A noteworthy exception is Townley, Sweeney, and Schneider (1994), who analyze changes in turnout within school districts in Riverside County, California, after many of those districts changed their election time from odd to even years. Their results are broadly consistent with the cross-section literature. They find that districts that changed their election timing experienced between a doubling and tripling of turnout in subsequent elections. Our empirical analysis of turnout essentially generalizes the latter study to include the entire state and extends the time frame with an additional decade's worth of election data.

We collected data on voter turnout from the California Elections Data Archive (CEDA) maintained by the Center for California Studies at Sacramento State University. The archive contains data on candidates, ballot designations, and vote totals for all county, municipal, school district, and community college elections held between 1996 and 2006. In total, we obtained data on over 4,900 school district elections held during this time period. CEDA contains the number of votes cast for each candidate in each election. Based on this information, we computed voter turnout as the total number of votes cast in the election divided by the voting age population in the jurisdiction.<sup>10</sup> Because 94 percent of school district elections took place in November, we

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<sup>10</sup> We did not have access to data on the number of registered voters in the jurisdictions, so we rely on the number of voting-age residents. In addition, we had to drop observations from districts in which elections were held by ward rather than at large because we did not have census data by school district election area from which to compute the voting age population. As a result, we lose about 10 percent of districts, some of which are among the

excluded other months from our analysis. Roughly two-thirds of school district elections were held in even years. As shown in Table 2, elections held in odd years garnered less than half the level of voter participation as those held in even years— 13% versus 33% on average—and this differential was evident throughout all the years studied.

In order to confirm that the average turnout differentials are not result of differences in other attributes of the jurisdictions that hold their elections at different times, we ran a series of regression models controlling for population characteristics thought to influence voter turnout.<sup>11</sup> Specifically, we control for population size, as well as the racial and age composition of the jurisdiction. In addition, we control for the homeownership rate and the fraction of families with children, which are expected to be especially important determinants of participation in local elections. We emphasize that these variables measure the aggregate attributes of the population in the jurisdictions, not the attributes of individual voters, and therefore the usual cautions regarding the ecological fallacy apply (e.g., King 1997).

Table 3 shows the results of the turnout analysis. Models (1) and (2) show the regression of turnout on election timing and jurisdictional demographics. The coefficient for the odd-year dummy variable in model (1) is highly significant statistically and, at negative 20 percentage points, nearly equal to the simple difference in means. In other words, controlling for population demographics does not alter the basic story about turnout differentials between even and odd years.

Of course, we do not suggest that the evenness of the election year, per se, causes

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largest in the state (e.g., Los Angeles and San Francisco Unified). For consistency, we also exclude these districts from the second stage (i.e., salary and test score) analyses. However, our results do not change notably if we include these districts in the second stage. Complete results are available on request.

<sup>11</sup> We obtained data the 1990 and 2000 US Censuses and linearly interpolated values for the intermediate years.

differences in voter participation. Rather, we hypothesize that the concurrence of major state and federal races in even years draws voters to the polls who otherwise might not vote in local elections. This hypothesis is tested more directly in model (2), which substitutes dummy variables for presidential, gubernatorial, and senatorial election years in place of the catchall odd year dummy variable.<sup>12</sup> The results indicate that turnout in school district elections is roughly 22 percentage points higher in presidential election years and 16 percentage points higher in gubernatorial election years, relative to odd years. The marginal effect on turnout of holding a U.S. Senate election coincident with a presidential or gubernatorial election is negligible.<sup>13</sup>

Models (3) and (4) of Table 3 introduce school district fixed effects, thereby isolating within-district differences in turnout between even and odd years. Identification in the fixed effects models comes from two sources. First, some districts held elections for school board seats in both even and odd years, usually due to the need for a special election to fill a vacant seat. Second, some districts changed their election timing from even to odd years during the course of our study period, as explained above. In both cases, we are able to observe how turnout differs within the same district between even and odd years. This specification purges the results of any time-invariant differences between districts that hold their elections on different schedules. The results do not change significantly from the OLS models. The only notable difference is that the senatorial election dummy becomes statistically significant—though remaining substantively

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In addition, we linearly projected values forward through 2004.

<sup>12</sup> California gubernatorial elections occur in even years alternating with presidential elections. For example, there were presidential elections in 1996, 2000, and so on, while there were gubernatorial elections in 1998, 2002, etc. We cannot separately identify the effects of US House elections, because they always coincide with either a presidential or gubernatorial election. We can, however, identify the marginal effect of US Senate elections due to their staggered timing. For example, there was a senatorial election in 2000 and 2004, but not in 2002.

<sup>13</sup> We cannot definitively attribute the turnout differential in presidential or gubernatorial election years to the presence of those offices on the ballot. In principle, any office that follows the same schedule of elections would produce the same coefficient in the model. However, we think it reasonable to attribute the turnout differentials to

small—with the inclusion of the district fixed effects.

The control variables in Table 3 perform generally as expected. The cross-sectional results (models 1 and 2) indicate that turnout is lower in larger districts, and in districts with a higher proportion of Hispanics, Native Americans, or “other” races. Turnout is higher in districts with more people over the age of 65, more families with children, and higher incomes. However, all but one of these effects dissipates when district fixed effects are added in models (3) and (4). The exception is the percent Hispanic variable, whose effect actually increases in the fixed effects specifications. To see why, recall that the dependent variable is defined as the number of votes over the voting age population. However, because they are disproportionately likely to be non-citizens, a simple count of the voting age population is particularly likely to overstate the number of eligible voters where there are many Hispanics.

### *B. Policy Consequences: Employee Compensation*

Employee compensation represents a natural dependent variable for a test interest group influence in school board politics (e.g., Baugh and Stone 1982; Dunne et al. 1996; Kleiner and Petree 1988; Rose and Sonstelie 2006). First, there is clear evidence of selective participation by teachers’ union members in school district elections (Moe 2006). Second, higher salaries are a universal and unambiguous goal for teachers and their unions. Third, teacher salaries follow a rigid pay scale based on qualifications and experience, and comprehensive data on the pay scales are available from the California Department of Education (CDE). Thus, while teacher salaries represent just one special interest policy objective, they are a particularly direct, easily

measurable, and unambiguous outcome for testing our theory.<sup>14</sup>

It is important to note that school districts do not have unfettered authority to set fiscal policy. Most states place limits on districts' fiscal autonomy, and California is extreme in the extent to which local budgets are determined at the state level (Hoxby 2001). As a result of voter-approved tax limits and court-ordered and legislative school finance reforms, the state government effectively determines local budgets and guarantees each district a roughly equal level of per pupil funding (Timar 2006).<sup>15</sup> Individual districts have only limited ability to independently change the size of their budgets.<sup>16</sup> Nevertheless, within the top-line budget constraint, districts retain nearly complete latitude in setting teacher salaries (Rose and Sengupta 2007).<sup>17</sup>

Each district determines its own salary schedule—that is, the salary paid to teachers with different combinations of education and experience—usually through a process of collective bargaining with union representatives. In other words, districts effectively decide how much of their budget to allocate to teacher compensation versus other expenditures.<sup>18</sup> In practice there is tremendous heterogeneity in teacher salaries among districts within the state. For example, in 2005, the most generous district, Los Gatos-Saratoga, paid \$80,040, while the least generous district, Potter Valley Unified, paid only \$42,733 for equivalently qualified teachers at step 10 in

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<sup>14</sup> See footnote 3 above for additional references using public employee salaries as a measure of union political influence.

<sup>15</sup> Categorical programs that provide supplemental funds for specific purposes, such as educating special-needs and low-income students or operating small schools, generate some variation in local revenue, meaning that per pupil spending is not perfectly equalized across districts.

<sup>16</sup> Schools may enhance their budgets by raising voluntary contributions, but Brunner and Sonstelie (2003) show that such contributions account for a very small share of the variation in funding across schools.

<sup>17</sup> Beginning in the 1999-2000 school year, the state mandated a minimum teacher salary of \$32,000, but the requirement was not binding for most districts (Loeb and Miller 2006).

<sup>18</sup> On average in California, teacher compensation accounts for half of a district's total per pupil expenditures (Rose and Sengupta 2007).

the salary schedule. Indeed, in every year of our study, the highest paying district offered a salary roughly twice as high as that of the lowest paying district for comparably qualified teachers. Meanwhile, the 75<sup>th</sup> percentile district paid on average about 20% more than the 25<sup>th</sup> percentile district in each year. Thus, despite limits on districts' fiscal independence, there is substantial variation in teacher compensation across districts that remains to be explained. In the concluding section of the paper, we return to these issues and discuss the generalizability of our results beyond California.

We obtained the certificated salary and benefit schedule (form J-90) from the California Department of Education (CDE) for each school district and each year from 1999 through 2005.<sup>19</sup> To identify comparable teachers across districts, we focus on those at step 10 in the salary schedule (BA degree plus 60 hours of continuing education), which is often taken to represent a "typical" teacher (e.g., Rose and Sengupta 2007).<sup>20</sup> This allows us to compare the salaries received by teachers with the same qualifications and experience in even-year and odd-year election districts.

Note that the policy reform that allowed school districts to change their elections from odd to even years occurred in 1986, while the first year for which district-level salary data are electronically available is 1999. Therefore, we first observe the outcome of interest more than 10 years after the change in election timing may have occurred. By this time, most of the districts that were to change to even-year elections had already done so. In order to enable a differences-

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<sup>19</sup> 1999 is the earliest year of data available. The data are obtained by CDE from local school districts through a survey. Although participation in the survey is voluntary, the response rate is 84 percent of districts representing 98 percent of the state's students. The responses are checked by CDE and reconfirmed with the districts before publication (CDE 2006, p. 1).

<sup>20</sup> Focusing on the starting salary, the highest salary, or the average salary yields comparable results to those presented below.

in-differences analysis, we collected additional teacher salary data for 1987, the last year before the policy change took effect.<sup>21</sup> We collected the records from paper archives at the CDE and entered the data manually. As a result, we are able to estimate each district's change in salary relative to its baseline, or "pre-treatment" level. Thus, we are able estimate whether districts that switched to even year elections exhibited differential *changes* in salary relative to districts remaining on an odd-year election schedule. This approach effectively controls for (observable and unobservable) time-invariant attributes of districts that may differ between those that changed election timing and those that did not. We complement this analysis with a second differences-in-differences analysis using the relatively small number of districts—12 to be exact—that changed their election timing after 1999.

Throughout our analyses, we control for a variety of district level covariates that could influence teacher salaries.<sup>22</sup> We control for the average wage in the local labor market, which provides a rough index of regional differentials that districts must offer to be competitive in attracting teachers. We use the annual average wage in the county as estimated by the Bureau of Economic Analysis.<sup>23</sup> We control for the size of the district, using the natural log of the number of students, to account for the possibility that unions are stronger in larger districts and therefore would extract more generous compensation independently from the timing of elections (Rose and Sonstelie 2006). We control for population density to capture potential differences between more or less urban districts. In addition, we control for the fraction of students receiving free or

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<sup>21</sup> The state law was changed in 1986; the first year in which an even-year election could have been held was 1988. Therefore, 1987 is the last "pre-treatment" year.

<sup>22</sup> Our selection of control variables was influenced by Rose and Sonstelie (2006) and Rose and Sengupta (2007).

<sup>23</sup> In principle, we would prefer to use the average wage for a worker with education and experience comparable to that of the average teacher, as in Rose and Sengupta (2007). However, the Census data used by those authors are

reduced price lunch, according to the National Center for Education Statistics, because districts with more low-income students may be perceived as more challenging by teachers, requiring additional compensation (Rose and Sengupta 2007). We control for demographic factors that may influence the attentiveness of local voters to school board politics, namely: the fraction of the population that is over 65, the fraction of housing units that are owner-occupied, and the fraction of families with school-age children. These three variables are taken from the 1990 and 2000 Censuses and values are linearly interpolated for other years. Because costs may vary for different types of districts, we include dummy variables for elementary and high school districts. Unified districts (K-12), which enroll about 70% of pupils, are the omitted category. Finally, all models include year fixed effects to account for statewide trends over time in teacher salaries.

Model (1) of Table 4 reports the bivariate regression of teacher salary against election timing. Teachers working in districts where elections are held in even years earn roughly 5 percent less than those in districts with odd year elections. With the addition of relevant covariates in model (2), the election timing estimates drops by roughly one-third, to 3.4 percent.

The estimates in models (1) and (2) rely on cross-sectional comparisons between even- and odd-year election districts. As we suggested above, such estimates may be confounded by unmeasured differences between the two categories of districts. In model (3), we add the baseline (1987) teacher salary as a control variable, allowing us to estimate the differences-in-differences in salaries. The estimates in model (3) indicate that salaries in even year districts increased 2 percent less than salaries in odd-year districts, relatively to their 1987 pre-treatment levels. In model (4), we estimate a variation of the differences-in-differences model by making the



dependent variable the *change* between 1987 and 2004 salaries. Again the point estimate is roughly 2 percent.

As noted above, there are 12 districts that changed their election timing from odd to even years over the course of our study period. In model (5), we utilize data from these schedule-switching districts to identify the within-district change in teacher salaries before and after the change in election timing. Because we have so few observations and we are studying changes over a fairly short period of time, we do not include additional control variables in this model. Even with only 69 observations from 12 districts, the estimated effect of election timing is roughly equal in magnitude to the other within-district estimates, and the coefficient is significant at  $p < 0.10$ .

While all of the estimated salary differences between even- and odd-year election districts are statistically significant, they are nevertheless fairly small substantively speaking. With an average step 10 salary of \$54,000, the even-year salary differential of 2 percent amounts to about \$1000. While this amount may be substantial from the perspective of an individual teacher, the mean difference between the 75<sup>th</sup> and 25<sup>th</sup> percentile district salaries is ten times as much. Moreover, that the within-district estimates are about 40 percent smaller than the between-district estimates validates our concern that cross-sectional estimates, even within the same state and with a rich set of control variables, overstate the true effects.

Several of the control variables demonstrate significant relationships with teacher salaries. Districts in counties with higher average wages also pay higher teacher salaries, consistent with Rose and Sengupta (2007). In addition, larger districts pay higher salaries, as in Rose and Sonstelie (2006), as do more urban districts and those where there is a higher

proportion of families with school-age children.

*B1. Robustness*

As explained above, AB2605 was a reform that *allowed* school districts to change their election dates from even to odd years, but it did not *require* them to do so. As such, this is a situation in which there is endogenous selection into the treatment, and it is natural to worry that the districts that chose to change their election timing were otherwise prone to reduce teacher salaries for some reason. One response is to emphasize that our within-district analyses account for both observable and unobservable *time invariant* differences across districts. For example, we need not be concerned that the results above are an artifact of greater inherent fiscal conservatism among districts that changed their election timing, since such districts would have been expected to have lower teacher salaries even before the change in election timing.

There may be a lingering concern, however, that changes in districts over time might be correlated with both election timing and teacher salaries. Recall that the primary motivation given in the journalistic accounts of AB2605 was to save money on election administration. Suppose that the districts that were most motivated to save money on election administration were also the most motivated to keep teacher salaries in check over time—due to changing needs to spend the funds on other expenses, say. Then the districts that changed to even-year elections might be those that were most likely to have held the line on teacher salaries even without the electoral change. In this case, our estimates could be biased upward.

Given that we have just argued that the effect of election timing on teacher salaries is small, we are not especially troubled by the prospect that those estimates may be biased upward.

If the true effects were even smaller, this would only strengthen our argument. Nevertheless, to explore these endogeneity concerns, we conducted an instrumental variables (IV) analysis. Our instrument relies on the fact that districts' proposals to change the time of their elections had to be approved by the county board. In several notable cases—for example, Los Angeles and San Bernadino—district proposals were rejected. A common reason given in rejecting districts' attempts to change their election dates was that the November general election ballot was already crowded and that adding more offices would unduly burden voters. Based on this experience, our instrument is the number of elected offices in the county as of 1987, which we obtained from the Census of Governments. Our reasoning is that counties with more elected offices in existence prior to passage of AB 2605 would be less likely to consolidate school district elections onto an already congested ballot. At the same time, we see no reason why the number of elected offices in the county should affect teacher salaries, other than through its potential effect on election timing. Our IV model (not shown) yields a coefficient of 1 percent for the election timing variable, but it is imprecisely estimated (standard error of 2 percent).<sup>24</sup> We thus cannot reject the hypothesis that the IV results are equal to the OLS results ( $p = 0.79$ ). The analysis therefore indicates no evidence of endogeneity.

As an additional robustness exercise, we repeated our analyses using matching methods. While matching does not address endogeneity concerns, it does allow us to test robustness by effectively restricting our comparisons to even-year and odd-year districts with overlap in the covariate distribution. In other words, if we were concerned that even-year and odd-year districts were so fundamentally different in observables that there was no common support, then we

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<sup>24</sup> The instrument performs well in the first stage, with an  $F$ -statistic of 98.21. Complete results are available on request.

might not put much stock in the linear extrapolations required to produce the regression estimates shown above. In any case, the concern seems unfounded, as matching estimates produce results quite similar to those shown above. Using the same set of covariates in model (3) of Table (4), nearest neighbor matching, kernel-based matching, and the “doubly robust” estimator of Robins, Rotnitzky, and Zhao (1995) (Lunceford and Davidian 2004) all recover differences between even- and odd-year districts of roughly 2 percent, which is in line with the comparable regression estimates.<sup>25</sup>

### *C. Policy Consequences: Test Scores*

The effect of election timing on teacher salaries might be taken as evidence that special interests exert a nefarious, if modest, influence in low-turnout elections. One possible reading of the data is that teachers dominate school board elections held in odd years and subsequently are able to extract better deals during negotiations with a board they helped to select. On the other hand, a more positive gloss might be that parents or pro-education interests more generally dominate odd-year, low-turnout school board elections. Such interests, possibly including unions, might prefer higher teacher salaries in the hopes of attracting better teachers and thereby improving educational outcomes for children. By the same token, it may be that voters in off-cycle elections are generally better informed about the performance of their local schools. For instance, parents and teachers may have first-hand information about school performance that allows them to better discern which incumbent board members are worthy of reelection and which need to be replaced.<sup>26</sup> Changing elections to coincide with major state and federal races,

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<sup>25</sup> Complete results are available on request.

<sup>26</sup> Chingos, Henderson, and West (2010) find that parents are better informed about school performance than are

therefore, may increase participation by less knowledgeable voters, thereby diminishing the overall quality of school governance. If either of these hypotheses is correct, then odd-year districts might exhibit an edge in student test scores due to having higher quality teachers, better governance, or both.

To investigate these issues, we analyze standardized test results on the state's Academic Performance Index (API) between and within districts in the same way that we did for teacher salaries. API scores are available beginning in 1999. We use school-level scores and match each school to its home district. We then assess whether schools in even-year election districts perform differently from schools in odd-year election districts. Because the formula used to compute the API can vary from one year to the next, the raw scores are not directly comparable over time (CDE 2009). Therefore, we normalized the scores to create percentile rankings across schools for each year. We computed the normalization separately for elementary, high school, and unified districts, so that each school is ranked with respect to others of the same type.<sup>27</sup>

We begin by regressing API percentile scores on the election timing indicator, which is effectively a test of the difference of means between even- and odd-year districts. The results, shown in model (1) of Table 5, reveal that even-year districts score 7.2 percentile points higher than odd-year districts on the API. Controlling for school-level observables, however, substantially reduces the estimated differential. Model (2) introduces the following independent variables: school size, the percent of students receiving free or reduced-price lunch, the percent

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other voters.

<sup>27</sup> The CDE provides decile rankings of schools—that is, a classification of schools into deciles of performance on the API. We obtain similar (and still significant) results when we use the CDE decile rankings; however our percentile rankings generate somewhat more precise estimates.

African American, and a school characteristics index (SCI) provided by the CDE.<sup>28</sup> With the addition of these controls, the estimated performance gap between even- and odd-year districts falls dramatically to 1.8 percentile points, but remains statistically significant. Finally, model (3) introduces district fixed effects, tying identification to within-district changes in performance from the 12 districts that changed election timing during the study period.<sup>29</sup> The point estimates in the final model are negative 1.7 percentile points, though nowhere near to being statistically significant.

Overall, we see little evidence to suggest that election timing, and by implication voter turnout, notably affects school performance. Most of the mean difference in performance between even- and odd-year districts can be adduced to differences in observable student characteristics. Even taking the estimates from model (2) at face value, however, a 2 percentile point differential is substantively quite small considering that the standard deviation in percentile scores is 29. Our findings are broadly consistent with those of Rose and Sonstelie (2006), who find no relationship between teacher salaries and student test scores in California (although they do not examine election timing).

## V. Implications & Caveats

Our empirical analysis yields three main results. First, when school board elections are

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<sup>28</sup> The SCI is a composite index, ranging from 100 to 200, computed by the CDE to represent the school's demographics. The components of the index include pupil mobility, pupil ethnicity, pupil socioeconomic status, teacher accreditation, class size, grade span, the percentages of gifted and disabled students, and the percentage of migrant students. For details of how the index is constructed, see CDE (2009, pp. 66-69). We experimented with using the component variables individually and found that they did not appreciably alter our estimates of the election timing dummy relative to using the more parsimonious SCI.

<sup>29</sup> We cannot estimate changes relative to baseline, pre-treatment levels because test scores are not available prior to 1999.

held to coincide with state and national elections, turnout is dramatically higher, on the order of 150 percent higher. Second, teacher salaries are between one and three percent higher when school board elections are held off-cycle. Third, neither the change in voter turnout nor the change in teacher salary is associated with a robust change in student achievement. From the perspective of education policy, these findings are of important in and of themselves. Our main interest, however, is in the implications of these results for the voter versus non-voters debate.

While judging the substantive magnitude of the observed effects is inevitably somewhat subjective, one obvious interpretation is that these results are of a piece with the conventional view that outcomes would not change importantly if everyone voted (e.g., Highton and Wolfinger 2002; Citrin et al. 2003). In the present case, while \$1000 may or may not be viewed as a large amount from the perspective of an individual teacher, it seems fair to say at a 2% increase in salary associated with a 150% increase in turnout is a very small elasticity. Indeed, if turnout changes this large are necessary to drive a substantive policy shift, it casts doubt on the idea that the more modest variation in turnout typically observed in general interest elections at the state or national level could be expected to generate major policy changes.

On the other hand, the analysis does demonstrate that changes in turnout, in fact, generate a robust measurable difference in a policy outcome. While the salary change is relatively small, it may be suggestive of potential effects along other unstudied dimensions. For example, if unions were also able to extract more favorable terms on tenure standards, working conditions, or other employment parameters not readily measured in this study, the aggregate effect on policy might be more consequential. Moreover, we have only examined one of the dozens of types of special-purpose local governments for which low-turnout, off-cycle elections are

commonplace. Berry (2009) argues that small increases in spending multiplied across multiple layers of government can produce significant aggregate consequences for public sector budgets. Thus, if a similar result were observed in all the special purpose elections in a given locality, the aggregate overall effects would obviously be much larger and more important from a policy perspective.

Aside from the magnitude of the effects, another important consideration is their generalizability. Indeed, one concern is that the effects we observe in California are particularly small because the state's school finance system leaves little room for local districts to alter the size of their budgets. On this question, two points are relevant. First, as explained above, districts have nearly complete latitude in setting teacher salaries and there is tremendous heterogeneity in salaries across districts within California. So lack of local discretion appears unlikely to be the primary explanation for the small observed effects. In addition, we note that two cross-sectional studies, one using national data (Trounstine 2010) and one using data from 8 states (Anzia forthcoming), find salary differences similar in magnitude to our own cross-sectional estimates (e.g., model (2) of Table 4). While we suspect that the cross-sectional estimates overstate the true size of the effects, for reasons elucidated above, that cross-sectional estimates from outside California comport with our own cross-sectional estimates suggests that the California system may not be so different as to limit the generalizability of the findings. That said, of course we place our stock on the within-district estimates rather than the cross-sectional estimates, and the only way to truly know whether those results generalize would be to replicate the study elsewhere using a comparable quasi-experiment of some kind.



## Conclusion

Understanding the relationship between political participation and policy outcomes is one of the core tasks of modern political science. Our analysis complements past studies of the preferences of voters and non-voters by analyzing the relationship between turnout and policy more directly. By focusing on a special purpose election, school boards, we are able to draw on conventional measures of education policy, including teacher salaries and student achievement. In addition, we are able to take advantage of much larger differences in turnout than are typically observed for national offices; in this case turnout more than doubles between even and odd years. Finally, in comparison to past studies based on cross-sectional comparisons, we are able to make stronger causal inferences about the connection between turnout and policy. By virtue of the quasi-experiment in California, we are able not only to compare electoral outcomes across jurisdictions, but also within the same jurisdiction over time. That is, our analysis tests whether massive changes in voter participation are associated with changes in policy outcomes within the same jurisdiction. While certainly not the final word, we hope these results contribute to the accumulating literature on the topic by casting new light on the voters versus nonvoters debate in political science. Returning to the motivating question of the paper—would policy outcomes change if everyone voted?—our qualified answer is, *yes but not radically*.

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Table 1. Comparison of Even- and Odd-Year Districts

Variable	Mean	Std. Err. of Mean	Diff. of Means T	(p - value)
<b>Step-10 Teacher Salary</b>				
Odd	\$ 53,634	\$ 408		3.71
Even	\$ 51,631	\$ 365		(0.0002)
<b>Population Density (county)</b>				
Odd	929	73		2.09
Even	724	65		(0.037)
<b>Avg. Wage per Job (county)</b>				
Odd	\$ 34,594	\$ 751		-1.14
Even	\$ 35,858	\$ 818		(0.256)
<b>Pct. Pop 65 and Over</b>				
Odd	0.12	0.003		-0.44
Even	0.12	0.003		(0.66)
<b>Pct. Owner Occupied Housing</b>				
Odd	0.65	0.01		0.49
Even	0.65	0.01		(0.63)
<b>Pct. Families with Children</b>				
Odd	0.54	0.006		1.83
Even	0.52	0.005		(0.068)
<b>Pct. Free/Reduced Lunch Eligible</b>				
Odd	0.36	0.02		1.57
Even	0.32	0.02		(0.117)
<b>Total Students</b>				
Odd	8,169	653		2.81
Even	5,875	497		(0.005)

Source: 2000 US Census for all variables except free/reduced lunch and total students, which come from NCES, and average county wage, which comes from the BEA.

Table 2. Summary of School Board Election Turnout

<b>Year</b>	<b>Median Turnout</b>	<b>Number of Elections</b>
1996	38%	577
1997	15%	332
1998	31%	566
1999	12%	326
2000	36%	519
2001	14%	334
2002	26%	594
2003	10%	312
2004	37%	545
All even years	33%	2801
All odd years	13%	1304
All years	22%	4105



Table 3. Election Timing and Voter Participation: School Boards

	<b>Model 1</b>	<b>Model 2</b>	<b>Model 3</b>	<b>Model 4</b>
	<b>OLS</b>	<b>OLS</b>	<b>FE</b>	<b>FE</b>
Odd year election	-0.194*** (0.012)		-0.219*** (0.028)	
Election Day - President		0.223*** (0.014)		0.240*** (0.029)
Election Day - Governor		0.155*** (0.013)		0.181*** (0.028)
Election Day - US Senator		0.009 (0.008)		0.023*** (0.005)
Ln( Total Population)	-0.102*** (0.006)	-0.102*** (0.006)	0.089 (0.081)	0.092 (0.080)
% Black/African American Population	0.212 (0.154)	0.230 (0.153)	-0.258 (0.360)	-0.294 (0.352)
% American Indian/Alaska Native Population	-0.686*** (0.139)	-0.668*** (0.140)	-0.166 (1.249)	-0.229 (1.338)
% Asian, Native Hawaiian and other Pacific Islander Population	0.007 (0.084)	0.001 (0.084)	-0.240 (0.214)	-0.280 (0.210)
% Other Race Population	-9.110* (5.299)	-8.985* (5.227)	1.716 (3.638)	1.999 (3.590)
% Hispanic/Latino population	-0.260*** (0.054)	-0.254*** (0.054)	-0.396** (0.155)	-0.413*** (0.155)
% Persons 65+ years old	1.485*** (0.364)	1.530*** (0.369)	0.764 (0.673)	0.857 (0.671)
Ln( Ave. Household Income)	0.300*** (0.032)	0.310*** (0.033)	-0.048 (0.064)	-0.027 (0.063)
% Owner-occupied Housing Units	-0.186 (0.133)	-0.182 (0.132)	0.178 (0.289)	0.129 (0.292)
% Families and Subfamilies with Own Children	0.657** (0.280)	0.677** (0.281)	0.437 (0.319)	0.494 (0.308)
Constant	-2.070*** (0.367)	-2.396*** (0.378)	-0.361 (0.742)	-0.842 (0.761)
Number of observations	4,656	4,656	4,656	4,656
R2	0.360	0.366	0.061	0.099

Standard errors clustered by district reported in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Table 4. Election Timing and Teacher Salaries

	(1)	(2)	(3)	(4)	(5)
Even year election	-0.050*** (0.012)	-0.032*** (0.008)	-0.020*** (0.006)	-0.024*** (0.008)	-0.027* (0.015)
ln(Baseline 1987 salary)			0.527*** (0.069)	-0.508*** (0.081)	
Log County Avg. Wage		0.119*** (0.029)	0.100*** (0.022)	0.102*** (0.026)	
ln(Population per sq. mile)		0.013*** (0.004)	0.010*** (0.003)	0.013*** (0.004)	
Interpolated % Persons 65+ years old		0.222 (0.144)	0.159 (0.100)	0.127 (0.109)	
Interpolated % Owner-occupied Housing Units		-0.047 (0.039)	-0.026 (0.031)	-0.010 (0.035)	
% Families and Subfamilies with Own Children		0.244*** (0.087)	0.229*** (0.062)	0.233*** (0.065)	
% Ratio of Free Lunch Eligible		-0.030 (0.020)	-0.025 (0.016)	-0.026 (0.020)	
Constant	10.899*** (0.009)	9.210*** (0.312)	4.058*** (0.730)	4.460*** (0.828)	10.952*** (0.003)
Number of observations	1,848	1,842	1,842	309	69
R2	0.120	0.648	0.752	0.428	0.825

The dependent variable is the natural log of the Step-10 salary except in model (4) where the dependent variable is the log difference between the 1987 and 2005 Step-10 salaries. Standard errors clustered by district reported in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table 5. Election Timing and Test Scores

	(1)	(2)	(3)
	OLS	OLS	FE
Even Year Election Dummy	7.246*** (2.661)	1.830** (0.770)	-1.712 (1.908)
Pct Free/Reduced Lunch Students		-0.479*** (0.023)	-0.049 (0.200)
School Characteristics Index		1.013*** (0.050)	0.307* (0.159)
Pct African American		-0.137*** (0.047)	0.151 (0.337)
Log Enrollment		0.987* (0.504)	7.087 (7.149)
Constant	46.024*** (2.036)	-101.037*** (11.621)	-53.297 (57.404)
Number of observations	31,311	27,629	630
R2	0.016	0.825	0.051

The unit of analysis is the school. The dependent variable is the school's percentile ranking on the API. Standard errors clustered by district reported in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

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