Election Timing and Public Policy

Quarterly Journal of Political Science, 2011, 6: 103–135

Election Timing and Public Policy*

Christopher R. Berry[1] and Jacob E. Gersen[2]

[1] *Associate Professor of Public Policy, University of Chicago, USA; crberry@uchicago.edu*
[2] *Professor of Law, Harvard University, USA; jgersen@law.harvard.edu*

ABSTRACT

There are nearly half a million elected officials in American local governments, and the timing of local elections varies enormously even within the same state. Some local elections are held simultaneously with major federal and state races, while others are held at times when no higher level elections coincide. We analyze the effect of election timing by exploiting 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 while election timing produces dramatic changes in voter turnout, resulting changes in public policy are modest in size and not robust statistically.

* We are grateful for useful discussion and comments from Stephen Ansolabehere, Ethan Bueno de Mesquita, Anne Joseph O'Connell, Paul Peterson, and Martin West. Excellent research assistance was provided by Sarah Anzia, CC Dubois, Patrick Giamario, Monica Groat, Masataka Harada, William Sullivan, and Lindsay Wilhelm.

Supplementary Material available from:
http://dx.doi.org/10.1561/100.00010070_supp
MS submitted 21 September 2010; *final version received 1 August 2011*
ISSN 1554-0626; DOI 10.1561/100.00010070

Elections are perhaps the most-studied institution of modern democracies. With illuminating and often excruciating detail, political scientists and lawyers have long analyzed how electoral rules and processes influence the performance of political institutions. Yet, with a few notable exceptions, there is a comparative paucity of scholarship focusing on the timing of elections. Were electoral timing either conceptually unrelated to democratic legitimacy or empirically inconsequential perhaps this relative dearth of attention would be understandable, but electoral timing is neither. Like other institutional conditions that affect the costs of political participation, when elections are held can affect the level and nature of group participation in the political arena and thereby also affect policy outcomes.

Election timing in parliamentary systems has spawned some interest (Smith, 2003, 2004), but less so in the United States, where the timing of elections for federal office is tightly regulated and exhibits only modest variation across political jurisdictions. Both scholars and politicians have long understood that electoral behavior in *off-years* — years without Presidential elections — differs from participation during years with Presidential elections, but this has sensibly been the starting and stopping point for thinking about electoral timing in the U.S. federal context.

Given that there are more than 500,000 elected officials in the United States and fewer than 600 of them are federal officials, state and local electoral settings provide richer fodder for empirical analysis (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 different times of the year. In some localities there is at least one local government election in 11 months of the year (Souzzi, 2007). Amidst this great heterogeneity, one widely known and well accepted fact is that turnout in local elections is notably higher when those elections are held concurrently with major national or state races (Hajnal *et al.*, 2002).

Equally important, the legal regimes that govern the timing of local elections vary significantly across jurisdictions (Berry and Gersen, 2009). State statutes sometimes set the actual date for all local government elections. Elsewhere, the timing of elections is left partially or entirely to the very government bodies subject to the elections. If electoral timing predictably affects participation differently across groups, then elected officials could conceivably use electoral timing to manipulate voter participation and ultimately substantive public policy downstream (Dunne *et al.*, 1997).

Most of the small literature on turnout and election timing is based on cross-sectional comparisons of jurisdictions with different election schedules. Our research design is a within-jurisdiction analysis that 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 produced more than a 150 percent increase in voter turnout in school board elections. These dramatic changes in turnout in similar elections over time do not stem from differences in the underlying substance of the elections themselves. We observe elections within the same political jurisdiction under conditions of high and low voter turnout and test for resulting changes in policy outcomes. Specifically, we 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 small, and often statistically insignificant, effects on substantive education policies. Indeed, our preferred specification shows zero effects. We cannot say whether this null result arises because voters in an election with low turnout have preferences similar to those of voters in the high turnout case, but we can say that the effect of dramatically increased voter turnout on policy is modest, at best. Thus, using a new and different empirical approach that focuses on policy 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 (e.g., Highton and Wolfinger, 1999).

Election Timing, Selective Participation, and Public Policy

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 *et al.*, 1997). As participation costs 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 and 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 (declining) costs of participation drive out (attract) potential voters from an election selectively, the substantive political preferences of actual voters may diverge from the political preferences of nonvoters in the jurisdiction.

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 those of voters in the November school board election (cf. Rubinfeld and Thomas, 1980; Rubinfeld, 1977; Berry and Gersen, 2010), but also that the differences between actual voters and the pool of potential voters in the jurisdiction will be larger for the oddly timed election than the November election. Indeed, a couple of excellent papers have already explored these ideas in the context of school *bond* elections (Dunne *et al.*, 1997; Meredith, 2009), showing that bonds are more likely to pass during elections held off-cycle, due to the differing, more supportive electorate that goes to the polls.

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 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.[1] Therefore, the first policy outcome we analyze is

[1] 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

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.[2]

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.[3] Thus, in addition to 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 three good reasons to expect that these 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 (Ellcessor and Leighley, 2001; Highton and Wolfinger, 2001; Verba *et al.*, 1995). Second, in the context of local government specifically, some versions of the Tiebout

---

include Babcock and Enberg (1999), Baugh and Stone (1982), Bellante and Long (1981), Courant *et al.* (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 *et al.* (1988), Gregory and Borland (1999), and Stone (2002).

[2] 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.

[3] This basic tradeoff — namely that delegating to those with expertise may generate better decisions but also 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).

model suggest that policy is shaped by interjurisdictional competition more than by electoral politics (Perroni and Scharf, 2001; Sprunger and Wilson, 1998; Rausher, 1998; Rose-Ackerman, 1983; Sonstelie and Portney, 1978). 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.[4] Third, voter turnout is itself partly a function of government policy, and if elected officials stray too far from general public sentiment, these deviations could induce erstwhile nonvoters to come to the polls in response. In other words, the *threat* of increased voter participation puts a limit on the extent to which elected officials may cater to special interests even in off-cycle elections (Berry, 2009, Ch. 3). Ultimately, these too are empirical questions, and we seek to shed light on them in the next section.

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 2008. 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.[5]

The second and more important reason for analyzing California is that there has 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 other local and state government elections were held in even-numbered years. In 1986, 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

[4] For an extended discussion of these ideas, see Berry (2009, Chap. 7).

[5] 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).

an unqualified democratic good.[6] Because of a then-recent change allowing other special government districts to shift the date of their elections, had the bill failed, school districts would have 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.[7] 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 2008.

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 of election timing compares outcomes from one set of jurisdictions holding even-year elections to outcomes from a different set of jurisdictions holding odd-year elections (e.g., Trounstine, 2010; Anzia, forthcoming). The

---

[6] 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 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.

[7] 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).

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 (see Table 1). While it is, of course, possible to control for measurable

Table 1. Comparison of even- and odd-year districts.

	Mean	Standard deviation	T-stat (p-value)
Step-10 teacher salary			
Odd	$ 63,056	$ 444	3.24
Even	$ 61,180	$ 370	(0.0012)
Population density (county)			
Odd	721	51	2.38
Even	564	42	(0.018)
Avg. wage per job (county)			
Odd	$ 36,056	$ 523	−0.38
Even	$ 36,328	$ 499	(0.707)
Pct. Pop 65 and over			
Odd	0.12	0.002	−0.428
Even	0.12	0.002	(0.66)
Pct. owner occupied housing			
Odd	0.65	0.01	0.07
Even	0.65	0.01	(0.94)
Pct. Families with children			
Odd	0.53	0.005	1.21
Even	0.52	0.004	(0.225)
Pct. free/reduced lunch eligible			
Odd	0.38	0.01	0.78
Even	0.36	0.01	(0.435)
Total students			
Odd	5,975	449	1.88
Even	4,752	470	(0.06)

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.

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.

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 *et al.* (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 *et al.* (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 in this section 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.[8] Because 94 percent of school district elections took place in November, we 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

Table 2. Summary of school board election turnout.

Year	Median turnout (%)	Number of elections
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

Note: Results for November elections.

[8] 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 largest in the state (e.g., Los Angeles and San Francisco Unified). For consistency, we also exclude these districts form 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.

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.[9] 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 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.[10] 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

[9] We obtained data the 1990 and 2000 US Censuses and linearly projected values for other years.

[10] 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 U.S. House elections, because they always coincide with either a presidential or gubernatorial election. We can, however, identify the marginal effect of U.S. Senate elections due to their staggered timing. For example, there was a senatorial election in 2000 and 2004, but not in 2002.

Table 3. Election timing and voter participation: school boards, 1996–2004.

	Model 1 OLS	Model 2 OLS	Model 3 FE	Model 4 FE
Odd year election	-0.204^{***}		-0.222^{***}	
	(0.012)		(0.041)	
Election day — President		0.219^{***}		0.231^{***}
		(0.014)		(0.041)
Election day — Governor		0.160^{***}		0.174^{***}
		(0.013)		(0.040)
Election day — US Senetor		0.015^{*}		0.024^{***}
		(0.009)		(0.005)
Ln (total population)	-0.101^{***}	-0.100^{***}	0.095	0.087
	(0.006)	(0.006)	(0.086)	(0.084)
% Black/African American population	0.235	0.230	−0.370	−0.397
	(0.148)	(0.147)	(0.395)	(0.390)
% American Indian/Alaska native population	-0.640^{***}	-0.649^{***}	−1.135	−0.879
	(0.129)	(0.131)	(1.636)	(1.684)
% Asian, native Hawaiian and other pacific islander population	0.021	0.017	0.030	−0.051
	(0.084)	(0.084)	(0.224)	(0.222)
% Other race population	−8.379	−8.658	1.091	0.989
	(5.930)	(5.892)	(3.881)	(3.897)
% Hispanic/Latino population	-0.227^{***}	-0.230^{***}	-0.357^{*}	-0.388^{**}
	(0.054)	(0.054)	(0.194)	(0.193)
% Persons 65+ years old	1.542^{***}	1.513^{***}	0.508	0.609
	(0.382)	(0.383)	(0.941)	(0.916)
Ln (Ave. household income)	0.309^{***}	0.307^{***}	−0.033	−0.044
	(0.032)	(0.033)	(0.072)	(0.072)
% Owner-occupied housing units	−0.207	−0.205	0.091	0.053
	(0.130)	(0.130)	(0.364)	(0.362)
% Families and subfamilies with own children	0.612^{**}	0.598^{**}	0.639^{*}	0.689^{*}
	(0.287)	(0.287)	(0.371)	(0.356)
Constant	-2.155^{***}	-2.334^{***}	−0.616	−0.628
	(0.378)	(0.382)	(0.816)	(0.824)
Number of observations	4,105	4,105	4,105	4,105
R2	0.363	0.368	0.046	0.085

Note: Standard errors clustered by district reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

U.S. Senate election coincident with a presidential or gubernatorial election is negligible.[11]

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 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, or Native Americans. 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. Too 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 noncitizens, a simple count of the voting age population is particularly likely to overstate the number of eligible voters where there are many Hispanics.

Policy Consequences: Employee Compensation

Employee compensation represents an obvious 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).

[11] 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 the top offices on the ballot.

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.[12]

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).[13] Individual districts have only limited ability to independently change the size of their budgets.[14] Nevertheless, within the top-line budget constraint, districts retain nearly complete latitude in setting teacher salaries (Rose and Sengupta, 2007).[15]

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.[16] 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

---

[12] See footnote 3 above for additional references using public employee salaries as a measure of union political influence.

[13] 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.

[14] 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.

[15] 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).

[16] On average in California, teacher compensation accounts for half of a district's total per pupil expenditures (Rose and Sengupta, 2007).

in 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 75th percentile district paid on average about 20% more than the 25th 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 CDE for each school district and each year from 1999 through 2008.[17] 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).[18] 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-in-differences analysis, we collected additional teacher salary data for 1987, the last year before the policy change took effect.[19] 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. We 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

---

[17] 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).

[18] Focusing on the starting salary, the highest salary, or the average salary yields comparable results to those presented below.

[19] 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.

election timing and those that did not. More formally, we estimate:

$$\ln(\text{salary}_i) = \alpha + \delta \ln(\mathrm{S}_{1987i}) + \psi \mathbf{X}_i + \varepsilon_i,$$

where salary_i denotes the average step-10 salary for district i over the period 1999 to 2008 and S_{1987i} represents the salary in district i the year before AB2605 took effect, which we also refer to as the district's *baseline* salary. $\mathbf{X}_i$ is a vector of district-level covariates that could influence teacher salaries, explained below. The vector ψ contains regression coefficients, α is the intercept, and ε_i is an error term.

We include the following variables in $\mathbf{X}_i$.[20] The average wage in the local labor market 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.[21] 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 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. These control variables, like the dependent variable, are measured as averages across the post-treatment years.

[20] Our selection of control variables was influenced by Rose and Sonstelie (2006) and Rose and Sengupta (2007).

[21] 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 not available annually.

Table 4. Election timing and teacher salaries.

	(1)	(2)	(3)
Even year election	-0.039^{***}	-0.021^{***}	-0.010^{*}
	(0.011)	(0.007)	(0.006)
Log 1987 step-10 salary			0.565^{***}
			(0.051)
Log county avg. wage		0.162^{***}	0.123^{***}
		(0.024)	(0.019)
Log population per Sq Mi		0.007^{**}	0.007^{***}
		(0.003)	(0.003)
Pct Pop 65 or older		0.275^{*}	0.191^{*}
		(0.146)	(0.106)
Pct Owner-occupied housing		-0.025	-0.012
		(0.039)	(0.031)
Pct families with children		0.222^{**}	0.208^{***}
		(0.089)	(0.067)
Pct free-reduced lunch eligible		-0.017	-0.015
		(0.021)	(0.017)
Log number of students		0.038^{***}	0.020^{***}
		(0.003)	(0.003)
Constant	11.031^{***}	8.912^{***}	3.583^{***}
	(0.010)	(0.257)	(0.546)
Number of districts	541	531	531
R2	0.065	0.646	0.762

Note: The dependent variable is the log of average step-10 teacher salary, 1999–2008. Robust standard errors reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

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 4 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 half to 2.2 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 1 percent less than salaries in odd-year districts, relative to their 1987 baseline levels, which is only about half as large as the cross-sectional estimates from model (2) and significant at only the 10 percent level.

While all of the estimated salary differences between even- and odd-year election districts are statistically significant, they are nevertheless fairly small substantively. With an average step 10 salary of $62,000 in 2008, the even-year salary differential of 1 percent amounts to about $620. While this amount may be substantial from the perspective of an individual teacher, the mean difference between the 75th and 25th percentile district salaries is more than 20 times as much. Moreover, that the within-district estimates are about 50 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.

Our main result can be displayed most transparently in a simple difference-in-difference table (see Table 5) showing the raw mean salaries for treatment and control districts — i.e., those that switched to on-cycle elections and those that did not, respectively — before and after the change in the law. The *after* column in Table 5 represents the average salary over the years 1999 to 2008, while the *before* column represents the last year of pre-treatment salary, 1987. All values are expressed in 2008 dollars. A cross-sectional study

Table 5. Difference-in-differences.

	Before	After
Treatment	60,517	60,845
Control	62,516	63,433
Difference	1,999	2,588
Difference-in-differences	589	

Note: *Treatment* denotes districts that switched to on-cycle elections; *Control* those that did not. *Before* denotes the last year of pre-treatment salary, 1987; *After* is the average across post-treatment years, 1999 to 2008. All values are adjusted to 2008 dollars.

would observe only the *after* column. Based on the post-treatment data alone, one would conclude that there is a roughly $2600, or 4 percent, differential in salaries between districts holding on-cycle versus off-cycle elections. Indeed, these figures are nearly identical to those reported in Anzia's (forthcoming) cross-sectional analysis, which she interprets as the effect of election timing. However, as Table 5 also makes clear, there was already a $2000 difference in salaries even *before* the change in the law, when all districts held off-cycle elections. The timing of elections obviously cannot account for the pre-treatment salary difference, but may explain the difference between the before and after salaries for treatment relative to control districts. This difference-in-difference is only about $600, or just under 1 percent of the average salary. These results are substantively the same as those shown in Table 4, which account for the influence of time-varying covariates.

Aside from election timing, 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.

Robustness: Matching and IV Estimates

As a robustness exercise, we repeat our analyses using matching methods, which allow us to effectively restrict 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 might not put much stock in the linear extrapolations required to produce the regression estimates shown above. We produced estimates by three different methods: nearest neighbor matching (Abadie and Imbens, forthcoming), kernel-based matching (Becker and Ichino, 2002), and the doubly robust estimator of Robins *et al.* (1995) (see Lunceford and Davidian, 2004). For each method, we produced two estimates, shown in Table 6: model (A) matches on all covariates from model (2) of Table 4; model (B) adds the baseline salary. In each case, model specification (A) recovers differences between even- and odd-year districts of roughly 2 percent.[22] Adding the 1987

[22] We produced the estimates in Stata using the following commands, respectively: nnmatch (Abadie *et al.*, 2004), pscore (Becker and Ichino, 2002), and dr (Emsley *et al.*, 2008). Additional details are available on request.

Table 6. Matching estimates.

Nearest neighbor		Kernel		Doubly robust	
(A)	(B)	(A)	(B)	(A)	(B)
-0.016^{**}	-0.008	-0.029^{**}	-0.016	-0.021^{***}	-0.009
(0.007)	(0.006)	(0.011)	(0.013)	(0.007)	(0.006)

Note: Table 6 reports matching estimates of the effect of election timing on teacher salaries using three different methods: nearest neighbor matching, kernel-based matching, and doubly robust estimation. See text for details. Specification (A) includes all the control variables listed in model (2) of Table 4. Specification (B) adds baseline (1987) salary as a control. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

salary reduces the estimated election timing effect by about half for each of the matching methods. Thus, the matching estimates are in line with the comparable regression estimates, indicating that the results are not being driven by functional form assumptions. The matching exercise also reaffirms that controlling for pre-treatment differences in salary substantially reduces the strictly cross-sectional estimates. Indeed, the matching estimates that include the baseline salary all fall short of statistical significance at conventional levels.

A bigger concern than functional form is endogeneity. 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, there is endogenous selection into the treatment and a natural worry is 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, because 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 unmeasured changes in districts over time might be correlated with both election timing and teacher salaries. Recall that a 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 for other reasons — 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 pre-treatment the number of elected offices in the county should affect post-treatment teacher salaries, other than through its potential effect on election timing.

Our IV results are shown in Table 7. The instrument performs well in the first stage, yielding an F-statistic of 74 and showing, as predicted, that districts in counties with more elected officials prior to the passage of AB 2605 were significantly less likely to change to on-cycle elections. Meanwhile, the estimated effect of election timing in the second stage is zero, although it is imprecisely estimated, with a 95% confidence interval that ranges from -3% to 3%. While we cannot reject the hypothesis that election timing has no effect on teacher salaries, we also cannot reject the hypothesis that the IV results are equal to the OLS results ($p = 0.53$). That said, assuming our exclusion restriction is valid, the IV analysis indicates that election timing has no measurable effect on teacher salaries and that the results shown in Tables 4–6 are biased upward due to endogeneity.

Table 7. Instrumental variables estimates.

	First stage	Second stage
Even year election		0.0007
		(0.017)
Log num. county elected officials (1987)	−0.236***	
	(0.028)	
Log 1987 step-10 salary	−0.368	0.574***
	(0.270)	(0.055)
Pct pop 65 or older	−0.684	0.198*
	(0.705)	(0.106)
Pct Owner-occupied housing	0.097	−0.011
	(0.207)	(0.031)
Pct families with children	−0.182	0.212***
	(0.424)	(0.066)
Pct free-reduced lunch eligible	0.047	−0.015
	(0.108)	(0.017)
Log number of students	−0.018	0.020***
	(0.020)	(0.003)
Log county avg. wage	0.032	0.119***
	(0.160)	(0.020)
Log popualtion per Sq Mi	0.039	0.007***
	(0.025)	(0.003)
Constant	4.934*	3.517***
	(2.878)	(0.571)
First-stage F-stat for instrument	73.65	
Endogeneity test of election timing		0.43
p-value		0.51

Note: The even-year election dummy is instrumented with the log number of county elected officials in 1987. The F-statistic is from the Angrist and Pishke multivariate test. The endogeneity test is the difference in Sargan-Hansen statistics. See Baum *et al.* (2007) for an explanation of both tests. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Policy Consequences: Test Scores

The preceding section explored the possibility that teachers exert greater influence in school board elections held in odd years and subsequently are able to extract better deals during negotiations with a board they helped to select. The results were mixed, at best, and arguably our most persuasive analysis, the IV, showed effectively a zero effect of election timing on teacher salaries. Another possibility — one with 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, may be 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.[23] Changing elections to coincide with major state and federal races, therefore, may increase participation by less knowledgeable voters, thereby diminishing the overall quality of school governance. If this hypothesis is correct, then odd-year districts might exhibit an edge in student test scores due to having better governance.

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 statewide 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.[24]

We begin by regressing API percentile scores on the election timing indicator, which is effectively a test of the difference of means between

---

[23] Chingos *et al.* (2010) find that parents are better informed about school performance than are other voters.

[24] The CDE provides decile rankings of schools — that is, a classification of schools into deciles of performance on the API. We obtain similar results when we use the CDE decile rankings; however our percentile rankings generate somewhat more precise estimates.

Table 8. Election timing and test scores.

	(1) OLS	(2) OLS	(3) FE
Even year election dummy	7.246***	1.830**	−1.712
	(2.661)	(0.770)	(1.908)
Pct free/reduced lunch students		−0.479***	−0.049
		(0.023)	(0.200)
School characteristics index		1.013***	0.307*
		(0.050)	(0.159)
Pct African American		−0.137***	0.151
		(0.047)	(0.337)
Log enrollment		0.987*	7.087
		(0.504)	(7.149)
Constant	46.024***	−101.037***	−53.297
	(2.036)	(11.621)	(57.404)
Number of observations	31,311	27,629	630
R2	0.016	0.825	0.051

Note: 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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

even- and odd-year districts. The results, shown in model (1) of Table 8, 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 African American, and a school characteristics index (SCI) provided by the CDE.[25] With the addition of

[25] 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.

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.[26] 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).

Implications

Our empirical analysis yields three main results. First, when school board elections are held to coincide with state and national elections, turnout is dramatically higher, on the order of 150 percent higher. Second, our best estimates indicate that there is no causal relationship between election timing and teacher salaries. Although salaries are higher in districts where elections are held off-cycle, the difference is small in size and not robust corrections for endogeneity. Third, election timing is not associated with a robust change in student achievement. From the perspective of education policy, these findings are important in and of themselves.

More generally, the results also have implications for a related debate about the relationship between voters and nonvoters in elections. Put simply, would public policy change if voter participation increased dramatically? 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

[26] We cannot estimate changes relative to baseline, pre-treatment levels because test scores are not available prior to 1999.

nonvoters such that neither the outcomes of elections nor resulting public policies would change even if all eligible voters participated in politics?[27]

There are three dominant views in political science about the relationship between voters and nonvoters. First, 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). A second view is that the voting public actually has significantly different preferences from the nonparticipating public. For example, 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. Related 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 toward low income populations (Hill and Leighley, 1992, 1995). A third view agrees with the descriptive claim that voters and nonvoters are different, but raises 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 *et al.*, 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.

The conventional approach to these issues relies on survey data to compare the partisanship and policy preferences of voters with those of nonvoters, makes extrapolations as to how nonvoters would have voted (if they had voted), and asks whether their hypothetical votes would have changed election outcomes. While this approach is sensible and productive, it also suffers from three notable limitations. First, it assumes that unobservable differences between voters and nonvoters — 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 nonvoter differ in some unmeasured way, then it may not be the case a nonvoter would make

---

[27] Andrew Gelman provides an accessible and informative introduction to these questions: http://www.stat.columbia.edu/~cook/movabletype/archives/2007/12/what_difference.html.

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 nonvoters to turnout, other aspects of the campaign might change accordingly. For instance, if candidates changed their platforms or tactics to appeal to erstwhile nonvoters, 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.

Our analysis mitigates many of these challenges. While judging the substantive magnitude of the observed effects in our data is inevitably somewhat subjective, they support the conventional view that outcomes would not change importantly if everyone voted (e.g., Highton and Wolfinger, 2002; Citrin *et al.*, 2003). Arguably our most persuasive results come from the IV analysis, which indicates no effect of election timing. IV results notwithstanding, even the results that merely control for baseline salary yield estimates of only about 1 percent, which is equivalent to roughly $600. While this amount may or may not be viewed as large from the perspective of an individual teacher, it seems fair to say that a (at most) 1% increase in salary associated with a 150% increase in turnout is a *very* small elasticity. Indeed, even if we allow for the possibility that the effects are compounded over time, 1% observed over 10 to 20 years post-treatment is a miniscule rate of appreciation. If turnout changes this large are necessary to drive a policy shift this small, 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, while the salary differential is negligible, we cannot rule out 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 many layers of government can produce significant aggregate consequences for public sector budgets. Thus, if a small but significant election timing effect were observed in all the special purpose jurisdictions in a given locality, the aggregate 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. On this last point, we note that a new analysis by Anzia (2011), using a research design reminiscent of our own, finds a comparable effect (about 1 percentage point) of election timing on teacher salaries in Texas.

Conclusion

The relationship between electoral institutions and public policy is a core concern of modern political science. The timing of local government elections has received comparatively little emphasis in the literature and yet this type of institutional variation offers promising data for analyzing the impact of institutions on policy. Particularly because election timing is often controlled by the very governments subject to the elections at issue, an understanding of the effects of timing on policy outcomes is essential to evaluating the democratic legitimacy of local elections. If local governments could manipulate election timing to swing policy outcomes, the role of elections in facilitating accountability would obviously be called into question.

Our analysis is one of the first to speak directly to these questions. 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 electoral institutions and policy. Our analysis tests whether massive changes in voter participation are associated with changes in policy outcomes within the same jurisdiction. Our results suggest that the effects are at best modest and likely zero. While certainly not the final word, we hope these results contribute to the accumulating literature on the effects of electoral institutions on public policy.

References

Aaron, B., J. M. Najita, and J. L. Stern. (eds.). 1988. In *Public Sector Bargaining.* Washington, DC: The Bureau of National Affairs.

Anzia, S. F. Forthcoming. "Election Timing and the Electoral Influence of Interest Groups." *Journal of Politics* 73(2): 412–427.

Anzia, S. F. 2011. "The Election Timing Effect: Evidence from a Policy Intervention in Texas." Working paper, Stanford University.

Arthur, A. J. and G. V. Bass. 1974. *Schools, Taxes, and Voter Behavior: An Analysis of School District Property Tax Elections* (Rand).

Babcock, L. and J. Engberg. 1999. "Bargaining Unit Composition and the Returns to Education and Tenure." *Industrial and Labor Relations Review* 52(2): 163–178.

Bartels, L. M. 2009. "Economic Inequality and Political Representation." In: *The Unsustainable American State*, L. Jacobs and D. King, eds., New York: Oxford University Press, pp. 167–196.

Baugh, W. H. and J. A. Stone. 1982. "Teachers, Unions, and Wages in the 1970s: Unionism Now Pays." *Industrial and Labor Relations Review* 35(3): 368–376.

Bellante, D. and J. Long. 1981. "The Political Economy of the Rent-seeking Society: The Case of Public Employees and Their Unions." *Journal of Labor Research* 2(1).

Bennett, S. E. and D. Resnick. 1990. "The Implications of Nonvoting for Democracy in the United States." *American Journal of Political Science* 34(3): 771–802.

Berry, C. R. 2009. *Imperfect Union: Representation and Taxation in Multilevel Governments (Cambridge).*

Berry, C. R. and J. E. Gersen. 2009. "Fiscal Consequences of Electoral Institutions." *Journal of Law and Economics* 52(August): 469–495.

Berry, C. R. and J. E. Gersen. 2010. "The Timing of Elections." *University of Chicago Law Review* 77.

Berry, C. and W. Howell. 2007. "Accountability and Local Elections: Rethinking Retrospective Voting." *Journal of Politics* 69: 844.

Blais, A. 2000. *To Vote or Not to Vote?: The Merits and Limits of Rational Choice Theory.* Pittsburgh, PA: University of Pittsburgh Press.

Blais, A., E. Gidengil, R. Nadeau, and N. Nevitte. 2002. *Anatomy of a Liberal Victory: Making Sense of the Vote in the 2000 Canadian Election.* Peterborough, ON: Broadview Press.

Blais, A, E. Gidengil, N. Nevitte, and R. Nadeau. 2004. "Where Does Turnout Decline Come From?" *European Journal of Political Research* 43(2): 221–236.

Boskoff, A. and H. Zeigler. 1964. *Voting Patterns in a Local Election.* Philadelphia, PA: Lippincott.

Bridges, A. 1997. *Morning Glories: Municipal Reform in the Southwest.* Princeton, NJ: Princeton University Press.

California Department of Education (CDE). 2009. Academic Performance Index Reports: Information Guide. May. Available online at: http://www.cde.ca.gov/api/

Chingos, M., M. Henderson, and M. West. 2010. "Citizen Perceptions of Government Service Quality: Evidence from Public Schools." Working Paper, Harvard University.

Citrin, J., E. Schickler, and J. Sides. 2003. "What if Everyone Voted? Simulating the Impact of Increased Turnout in Senate Elections." *American Journal of Political Science* 47(1): 75–90.

Courant, P. N., E. Gramlich, and D. L. Rubinfeld. 1979. "Public Employee Market Power and the Level of Government Spending." *American Economic Review* 69(5): 806–817.

DeNardo, J. 1980. "Turnout and the Vote: The Joke's on the Democrats." *American Political Science Review* 74(2): 406–420.

Dunne, S. W., R. Reed, and J. Wilbanks. 1997. "Endogenizing the Median Voter: Public Choice Goes to School." *Public Choice* 93: 99–118.

Ehrenberg, R. G. and G. S. Goldstein. 1975. "A Model of Public Sector Wage Determination." *Journal of Urban Economics* 2(3): 223–245.

Epple, D. and A. Zelenitz. 1981. "The Implications of Competition among Jurisdictions: Does Tiebout Need Politics?" *Journal of Political Economy* 89: 1197.

Ellcessor, P. and J. E. Leighley. 2001. "Voters, Non-voters and Minority Representation." In: *Representation of Minority Groups in the U.S.: Implications for the Twenty-first Century*, C. E. Menifield, ed., Sanfrancisco, CA: Austin & Winfield.

Farber, H. S. 1986. "The Analysis of Union Behavior." In: *Handbook of Labor Economics*, O. C. Ashenfelter and R. Layard, eds., Vol. 2. Amsterdam, The Netherlands: North-Holland.

Fischel, W. 2001. *The Homevoter Hypothesis: How Home Values Influence Local Government Taxation, School Finance, and Land-Use Policies.* Cambridge, MA: Harvard University Press.

Fogel, W. and D. Lewin. 1973. "Wage Determination in the Public Sector." *Industrial and Labor Relations Review* 410.

Freeman, R. B. 1986. "Unionism Comes to the Public Sector." *Journal of Economic Literature* 24(March): 41–86.

Freund, J. L. 1973. "Market and Union Influences on Municipal Employee Wages." *Industrial and Labor Relations Review* 391.

Gant, M. M. and W. Lyons. 1993. "Democratic Theory, Nonvoting, and Public Policy: The 1972–1988 Presidential Elections." *American Politics Research* 21(2): 185–204.

Gilens, M. 2005. "Inequality and Democratic Responsiveness." *Public Opinion Quarterly* 69(5): 778–796.

Gilligan, T. W. G and K. Krehbiel. 1987. "Collective Decisionmaking and Standing Committees: An Informational Rationale for Restrictive Amendment Procedures." *Journal of Law, Economics, and Organization* 3: 287.

Gregory, R. G. and J. Borland. 1999. "Recent Developments in Public Sector Labor Markets." *Handbook of Labor Economics* 3(3): 3573–3630.

Griffin, J. D. and B. Newman. 2005. "Are Voters Better Represented?" *The Journal of Politics* 67(4): 1206–1227.

Grofman, B., G. Owen, and C. Collet. 1999. "Rethinking the Partisan Effects of Higher Turnout: So What's the Question?" *Public Choice* 99(2): 357–376.

Hajnal, Z. L. and P. G. Lewis. 2005. "Where Turnout Matters: The Consequences of Uneven Turnout in City Politics." *Journal of Politics* 67(2): 515–535.

Hajnal, Z. L., P. G. Lewis, and H. Louch. 2002. *Municipal Elections in California: Turnout, Timing, and Competition*. Public Policy Institute of California.

Halberstam, Y. and B. Pablo Montagnes. 2009. "The Presidential Race for Office and the Persistent Electoral Bias It Creates: Evidence from Entry and Exit of Senators." Working Paper, Northwestern University.

Hess, F. 2002. *School Boards at the Dawn of the 21st Century: Conditions and Challenges of District Governance*. National School Boards Association.

Highton, B. and R. E. Wolfinger. 2001. "The Political Implications of Higher Turnout." *British Journal of Political Science* 31: 179.

Hill, K. Q. and J. E. Leighley. 1992. "The Policy Consequences of Class Bias in American State Electorates." *American Journal of Political Science* 36(May): 351–365.

Hill, K. Q., J. E. Leighley, and A. Hinton-Anderssson. 1995. "Lower-Class Mobilization and Policy Linkage in the U.S. States." *American Journal of Political Science* 39(1): 75–86.

Karnig, A. K. and O. Walter. 1983. "Decline in Municipal Voter Turnout: A Function of Changing Structure." *American Politics Quarterly* 11: 491.

King, G. 1997. *A Solution to the Ecological Inference Problem: Reconstructing Individual Behavior from Aggregate Data* Princeton.

Kleiner, M. M. and L. D. Petree. 1988. "Unionism and Licensing of Public School Teachers: Impact on Wages and Educational Output." In: *When Public Sector Workers Unionize*, R. B. Freeman and C. Ichniowski, eds., Chicago: University of Chicago Press.

Krehbiel, K. 1991. *Information and Legislative Organization (Michigan)*.

Leighley, J. E. and J. Nagler. 2009. "Electoral Laws and Turnout, 1972–2008." CELS 2009 4th Annual Conference on Empirical Legal Studies Paper.

Lijphart, A. 1997. "Unequal Participation: Democracy's Unresolved Dilemma." *American Political Science Review* 91(1): 1–14.

Loeb, S. and L. C. Miller. 2006. "A Review of State Teacher Policies: What Are They, What Are Their Effects, and What Are Their Implications for School Finance?" Institute for Research on Education Policy & Practice, School of Education, Stanford University, California.

Lunceford, J. K. and M. Davidian. 2004. "Stratification and Weighting via the Propensity Score in Estimation of Causal Treatment Effects: A Comparative Study." *Statistics in Medicine* 23(19): 2937–2960.

Lupia, A. and M. D. McCubbins. 1998. *The Democratic Dilemma: Can Citizens Learn What They Need to Know?* New York, NY: Cambridge.

Lutz, G. and M. Marsh. 2007. "Introduction: Consequences of Low Turnout." *Electoral Studies* 26(3): 539–547.

Maeshiro, K. 2005. "Big Changes for Schools? Larger Classes, Middle School Reorganization Mulled." *LA Daily News* 1(Feb).

Mas-Colell, A., M. D. Whinston, and J. R. Green. 1995. '*Microeconomic Theory*. Oxford.

Martinez, M. D. and J. Gill. 2006. "Does Turnout Decline Matter? Electoral Turnout and Partisan Choice in Canada." *Canadian Journal of Political Science* 39(2): 343–362.

Meredith, M. 2009. "The Strategic Timing of Direct Democracy." *Economics and Politics* 21: 159–177.

Moe, T. M. 2006. "Political Control and the Power of the Agent." *Journal of Law, Economics, and Organization* 22: 1.
Norrander, B. 1989. "Ideological Representativeness of Presidential Primary Voters." *American Journal of Political Science* 33(3): 570–587.
O'Brien, K. M. 1992. "Compensation, Employment, and the Political Activity of Public Employee Unions." *Journal of Labor Research* 13(1): 189–203. (Winter).
O'Brien, K. M. 1994. "The Impact of Union Political Activities on Public-sector Pay, Employment, and Budgets." *Industrial Relations* 33(3): 322–345.
Pacek, A. and B. Radcliff. "Turnout and the Vote for Left-of-centre Parties: A Cross-national Analysis." *British Journal of Political Science* 25(1): 137–143.
Pammett, J. H. and L. LeDuc. 2003. *Explaining the Turnout Decline in Canadian Federal Elections: A New Survey of Non-voters*. Elections Canada, Ottawa.
Perroni, C. and K. A. Scharf. 2001. "Tiebout with Politics: Capital Tax Competition and Constitutional Choices." *Review of Economic Studies* 68: 133.
Piele, P. K. and J. S. Hall. 1973. *Budgets, Bonds, and Bailouts* D.C. Heath.
Piven, F. F. and R. A. Cloward. 1998. *Why Americans Don't Vote*. New York: Pantheon Books.
Rauscher, M. 1998. "Leviathan and Competition among Jurisdictions: The Case of Benefit Taxation." *Journal of Urban Economics* 44: 59–67.
Robins, J. M., A. Rotnitzky, and L. P. Zhao. 1995. "Analysis of Semiparametric Regression Models for Repeated Outcomes in the Presence of Missing Data." *Journal of the American Statistical Association* 90: 106–121.
Rose-Ackerman. S. 1983. "Tiebout Models and the Competitive Ideal: An Essay on the Political Economy of Local Government." In: *Perspectives on Local Public Finance and Public Policy*, J. M. Quigley, ed., 23 (JAI Press).
Rose, H. and R. Sengupta. 2007. "Teacher Compensation and Local Labor Market Conditions in California: Implications for School Funding." Occasional Paper, Public Policy Institute of California.
Rose, H. and J. Sonstelie. 2006. "School Board Politics, School District Size, and the Bargaining Power of Teachers' Unions." Working Paper No. 2006.05, Public Policy Institute of California.
Rubenson, D., A. Blais, P. Fournier, E. Gidengil, and N. Nevitte. 2004. "Accounting for the Age Gap in Turnout." *Acta Politica* 39(4): 407–421.
Rubinfeld, D. and R. Thomas. 1980. "On the Economics of Voter Turnout in Local School Elections." *Public Choice* 35: 315.
Rubinfeld, D. 1977. "Voting in a Local School Election: A Micro Analysis." *Review Economics and Statistics* 59: 30.
Shepsle, K. A. and B. R. Weingast. 1994. "Positive Theories of Congressional Institutions." *Legislative Studies Quarterly* 19: 149.
Silver, B. D. and B. A. Anderson, "Who Over Reports Voting?" *American Political Science Review* 80(2): 613–624.
Sonstelie, J. C. and P. R. Portney. 1978. "Profit Maximizing Communities and the Theory of Local Public Expenditure." *Journal of Urban Economics* 5: 263.
Souzzi, T. R. 2007. *Special District Election Date Study: A Crazy Quilt*. Nassau County.
Sprunger, P. and J. D. Wilson. 1998. "Imperfectly Mobile Households and Durable Local Public Goods: Does the Capitalization Mechanism Work?" *Journal of Urban Economics* 44(3): 468–492.
Stone, J. A. 2002. "Collective Bargaining and Public Schools." In: *Conflicting Missions? Teacher Unions and Educational Reform*, T. Loveless, ed., Washington, DC: Brookings Institution Press.
Summers, C. W. 1973. "Public Employee Bargaining: A Political Perspective." Yale *Law Journal* 83: 1156.
Swaddle, K. and A. Heath. 1989. "Official and Reported Turnout in the British General Election of 1987." *British Journal of Political Science* 19(4): 537–551.

Townley, A. J., D. P. Sweeney, and J. H. Schmieder. 1994. "School Board Elections: A Study of Citizen Voting Patterns." *Urban Education* 29: 50.
Tucker, H. J. 2004. "Low Voter Turnout and American Democracy." Working Paper.
Trounstine, J. 2010. "Incumbency and Responsiveness in Local Elections." Working Paper, University of California-Merced.
Verba, S., K. L. Schlozman, and H. E. Brady. 1995. *Voice and Equality: Civic Voluntarism in American Politics*, Cambridge, MA: Harvard.
Wolfinger, R. E. and S. J. Rosenstone. 1980. *Who Votes?* New Haven, CT: Yale University Press.