The Election Timing Effect:

Evidence from a Policy Intervention in Texas

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This Draft: January 16, 2012

Abstract: Recent studies have argued that the low voter turnout that accompanies off-cycle elections could create an advantage for interest groups. However, the endogeneity of election timing makes it difficult to estimate its causal effect on political outcomes. In this paper, I examine the effects of a 2006 Texas law that forced approximately 20 percent of the state's school districts to move their elections to the same day as national elections. Using matching as well as district fixed effects regression, I estimate the causal effect of the switch to on-cycle election timing on average district teacher salary, since teachers and their unions tend to be the dominant interest group in school board elections. I find that school districts that were forced to switch to on-cycle elections responded by granting significantly lower salary raises to teachers, supporting the hypothesis that school trustees were less responsive to the dominant interest group after the switch.

* Thank you to Jonathan Rodden, Mike Tomz, Karen Jusko, Terry Moe, Jonathan Wand, Jeff Milyo, and Molly Cohn for their helpful comments on this project and to Tiffany Li for research assistance.

> The major danger in a light vote lies in the fact that highly organized groups, whether of the nature of old-fashioned city machines or of special interest groups of any type, will thereby be able to control the government, for the lighter the vote the easier it is for such groups to win. They have a solid nucleus of dependable voters. A small turnout does not result in the same percentage distribution of the vote among the various segments of the population as would be found in a large turnout (Adrian 1955, 72).

It is well established that the timing of elections affects voter turnout. Average turnout in midterm congressional elections runs 13 percentage points lower than turnout in congressional elections held concurrently with presidential elections (Jacobson 2001). Gubernatorial elections attract the most voters when they are held concurrently with presidential elections and the fewest voters when they are held in the odd-numbered years (Patterson and Caldeira 1983). The effect is even more pronounced for local government. For example, when municipal elections are held separately from state and national elections, voter turnout averages over 30 percentage points lower than when they are held concurrently with presidential elections (Hajnal and Lewis 2003).

Scholars have long suspected that low voter turnout creates electoral advantages for organized groups (e.g., Adrian 1955). However, in spite of the fact that securing off-cycle election timing is one of the most reliable ways to significantly lower turnout (e.g., Hajnal 2010), there is very little research that examines the link between the timing of elections and the extent to which interest groups can influence political and policy outcomes. This gap in the literature is notable, especially considering the number of governments in the U.S. that hold elections on days other than the Tuesday after the first Monday in November of even-numbered years: 80 percent of cities, the vast majority of school districts, and even a few U.S. states hold general elections at times other than national Election Day. Studies have established that far fewer voters participate in these elections than in comparable elections held concurrently with national elections (e.g., Caren 2007), but until recently, there were no empirical tests of whether this lower turnout leads to systematic differences in election outcomes and policy.

The last year has seen some empirical investigation of the effects of election timing (Anzia 2011, Berry and Gersen 2011), but like most studies that use observational data, they face challenges to establishing a causal link between off-cycle election timing, turnout bias, and interest group influence. First, there is potential for omitted variable bias. In cross-sectional analysis in particular, it is usually difficult to rule out the possibility that unobservable characteristics of the governmental units explain both the timing of their elections and the degree to which interest groups influence their political outcomes. Analysis of within-unit changes in election timing and political outcomes can reduce the incidence of omitted variable bias, but most longitudinal studies still confront the problem of selection bias: if interest groups lobby for off-cycle election scheduling in the units where they expect it to help them most, then by comparing units with off-cycle elections to units with on-cycle elections, one risks overestimating the causal effect of election timing.

In this paper, I take advantage of a policy intervention in Texas to overcome these empirical challenges and to develop a conservative causal estimate of the effect of election timing on public policy. Prior to 2006, the trustees of all 1,032 independent school districts in Texas had the authority to choose to hold elections on either the second Saturday in May or on the Tuesday after the first Monday in November, and 99 percent of school districts used the May election date. In July 2006, the Texas legislature passed House Bill 1 (HB 1), which required all school districts in the state to combine their elections with either municipal elections – most of which are held in May – or county elections – all of which are held in November of even-numbered years. School districts that had incorporated municipalities within their borders were allowed to keep their off-cycle election schedules as long as those municipalities held off-cycle elections. However, at least 174 districts were forced to switch to on-cycle trustee elections as a result of HB 1, either because they contained no incorporated municipalities, or because the municipalities within their borders held elections in November of even-numbered years.

I estimate the effect of election timing by comparing the pre- and post-HB 1 policy outputs of two groups of districts: those that were forced to switch to on-cycle elections (treatment districts) and those that were allowed to retain off-cycle elections (control districts). Since teacher unions tend to be the dominant interest group in school district elections (Hess and Leal 2005, Moe 2005), I use teacher salaries in the district to measure variation in interest group influence within and across school districts. First, I use matching to estimate the effect of the forced switch to on-cycle elections on teacher salary growth rates. I find that teacher salaries grew by approximately 0.75 percentage points less in treatment districts following implementation of HB 1 than in the matched control districts. Second, I model district teacher salaries using fixed effects regression, partialling out the effect of unobservable, time-invariant district characteristics. I find that relative to average annual changes in teacher salaries across the state, teacher salaries in treated districts were 1.3 percent lower following the switch to on-cycle elections. This finding strongly supports the hypothesis that school trustees were less responsive to the dominant interest group once they were forced to hold on-cycle elections.

1. Election Timing, Voter Turnout, and Interest Group Influence

Across the U.S., voter turnout varies considerably by the type of election: Turnout in presidential elections is higher than turnout in midterm congressional elections (Jacobson 2001), turnout in primary elections tends to be lower than turnout in general elections (Ranney 1972), and turnout in local government elections is usually much lower than turnout in state and national elections (Bridges 1997, Weimer 2001, Wood 2002). However, when local elections are combined with state and national elections, turnout in local races is considerably higher

(Aldrich 1993). Hajnal and Lewis (2003), for example, find that turnout in off-cycle city elections averages over 30 percentage points lower than in city elections held during presidential elections. Similarly, Hess (2002) finds that turnout in local school board elections tends to be much lower when they are not held concurrently with other elections.

The near-consensus in the American politics literature is that low voter turnout does little to affect election outcomes (e.g., Highton and Wolfinger 2001, Wolfinger and Rosenstone 1980), but almost all studies that draw such conclusions examine voters and nonvoters in presidential and congressional elections – when turnout is at its highest (Hajnal and Trounstine 2005). By contrast, several studies that examine state and local elections find that turnout levels *do* have consequences for political representation and policy. Hajnal and Trounstine (2005), for example, find that lower turnout works to reduce racial minority representation on city councils and in mayors' offices. Hill and Leighley (1992) find that low turnout in state elections leads to tax and welfare policies that favor the upper class. Dunne, Reed, and Wilbanks (1997) argue that off-cycle election timing in school bond referenda increases the proportion of "yes" voters who turn out to the polls, and Meredith (2009) provides evidence that agenda setters strategically schedule these referenda for *on*-cycle elections if high-turnout electorates are more likely to approve the bonds. Furthermore, Berry (2009) suggests that the reduced voter turnout of off-cycle elections increases the proportion of high demanders at the polls in special district elections.

In an earlier paper (Anzia 2011), I built on this literature and argued that off-cycle election timing increases the influence of organized interest groups in elections. That advantage works through two channels. The first channel is based on the notion that individuals who have an immediate stake in the outcome of an election are more likely to participate in that election than individuals who have less at stake (Rosenstone and Hansen 1993, Verba, Schlozman, and

Brady 1995), and, moreover, that many individuals who have a large stake in politics are members of special interest groups (e.g., Fiorina 1999). The logic is simple: When an election is shifted from on-cycle to off-cycle, overall voter turnout decreases. However, the people with a large stake in the election outcome participate at high rates *regardless* of when the election is held, and thus the decrease in turnout comes disproportionately from those who have less at stake. Purely on the basis of their individual incentives, then, the members of interest groups whose central policy issues are the responsibility of officials chosen in the election cast a greater proportion of the ballots when the election is held off-cycle as opposed to on-cycle.[1]

Second, off-cycle election timing enhances the effectiveness of interest groups' mobilization efforts. Specifically, organized interest groups can take advantage of the lower turnout that comes with off-cycle election timing by targeting potential supporters and encouraging them to participate. When turnout is low, each supportive voter mobilized by the interest group is more important to the outcome than she would be in an on-cycle election. Thus, if a group mobilizes the same number of voters in an off-cycle election as it would in an on-cycle election, those voters make up a greater proportion of the electorate in the off-cycle context.

Ultimately, the consequences of off-cycle election timing for election outcomes and policy depend on the types of policies at stake and the dynamics of interest group activity. The most straightforward scenario is one in which an interest group seeks a policy for which benefits are concentrated (among group members), costs are diffuse, and over which the group faces little to no organized competition.[2] In such a setting, a shift from on-cycle to off-cycle election timing

[1] Of course, some policy "stakeholders" may not be members of interest groups, and they would also be expected to have greater presence in off-cycle elections.

[2] As Wilson (1995, 331-337) argues, it is precisely the policy areas where benefits are concentrated and costs are distributed where one is likely to find highly organized beneficiaries facing little to no organized competition. The same is true when a policy has concentrated costs and distributed benefits: those who stand to pay the costs tend to be more motivated and better organized than the beneficiaries.

should suppress voter turnout more among those who stand to bear the cost of the policy than among the beneficiaries, producing an electorate with a median voter more favorable to the interest group's policy position (Dunne et al. 1997). Moreover, off-cycle election timing in such a context would enhance the effectiveness of the group's mobilization efforts without also aiding any opposition group. For both of these reasons, the electorate as a whole should be more favorably disposed toward the group's policy goals in off-cycle elections.

As a result, officials elected in off-cycle elections should be more responsive to the organized group than officials elected in on-cycle elections. Strictly speaking, this difference in responsiveness could occur in two ways: First, in off-cycle elections, the interest group could have greater success in replacing incumbents who are unfriendly to their goals with new, more supportive elected officials. Alternatively, a shift to off-cycle election timing could induce sitting elected officials to be more responsive to the interest group if those officials expect that the group will suddenly be more important to their reelection efforts. Regardless of whether the interest group succeeds in replacing incumbents or inducing incumbents to be responsive (or both), however, the policy prediction is the same: policy should be more favorable to the group when elections are held off-cycle than when they are held on-cycle.

Of course, not all interest groups seek policies with concentrated benefits and distributed costs. If the policy issue at stake is one where voters on both sides are highly motivated to participate, the individual-level effect of off-cycle election timing might result in little to no change to election outcomes. It is even possible that *on*-cycle electorates could be more favorable to a group's policy position (Meredith 2009). For example, in an environment where pro-choice voters are more highly motivated to participate than pro-life voters, a pro-life group might actually fare better when turnout is high. In the most general sense, then, a group benefits from off-cycle election timing if its members and supporters are more likely to weather the overall decrease in voter turnout than the eligible voters who oppose the group's policy goals.

Also, when two organized groups compete over policy, the group-level effect of off-cycle election timing cannot create advantages for both groups simultaneously. In such cases, off-cycle election timing increases the importance of both groups' mobilization efforts, but it is the group with greater organizational capacity – meaning greater ability to mobilize supporters (e.g., more financial resources or more volunteers) – that should see policy shift in its favor as a result.

It is more difficult to make predictions about how election timing will affect political outcomes when individuals on both sides of an issue are highly motivated to turn out, or when two or more groups compete over policy, but election timing still has great potential to tip the balance of power between them. An example helps to illustrate.[3] Consider a scenario in which two organized groups, the Developers and the Environmentalists, are at odds over land-use policy. Each group nominates a candidate for the upcoming city election and seeks to maximize the vote share received by that candidate. The question is: which candidate wins greater vote share when the election is held off-cycle rather than on the same day as a presidential election?

If the Developers have greater organizational capacity than the Environmentalists, then on the basis of the group-level effect alone, the Developers' candidate stands to win greater vote share in an off-cycle election than in an on-cycle election.[4] However, separating the city election from the presidential election also demobilizes voters whose interests lie primarily in the highly visible presidential race. If the Developers care more deeply about the land-use issue than the

[3] This example is adapted from one I have presented in previous work (Anzia, forthcoming).

[4] This assumes that the groups maximize vote share. Groups' mobilization efforts could also be endogenous to the election schedule. For example, a group may not need to mobilize as many voters in an off-cycle election as in an on-cycle election to secure a comfortable victory margin. Empirically, then, the similarity of the group's vote share in off-cycle and on-cycle elections would mask the fact that the group *could* have performed much better in the off-cycle election if it had mobilized to its full capacity.

Environmentalists, then the Developers' candidate stands to win greater vote share in an off-cycle election than in an on-cycle election *both* because fewer Environmentalists vote in an off-cycle election *and* because the Developers can dominate the smaller electorate by mobilizing more supportive voters. The Environmentalists' candidate, on the other hand, performs better in an on-cycle environment, in which the presidential electorate is more favorable to its position and in which the superior mobilization ability of the Developers has a more muted effect.

In contrast, if the Developers are the dominant organizational force in the city but tend to have members who are less animated by the land-use issue than the Environmentalists, the Developers' preferences over the election schedule are mixed. The Developers favor off-cycle election timing if they anticipate that they can secure greater vote share for their candidate by overwhelming the small, off-cycle electorate with many mobilized Developer supporters. However, if the Developers' members are sufficiently less enthusiastic about the land-use issue than the Environmentalists, the Developers' candidate could win a larger percentage of the vote in an on-cycle election. In this way, the individual- and group-level effects of off-cycle election timing can pull in opposite directions, and the net impact of election timing on the candidates' vote shares depends on which of the two countervailing forces is stronger.

Table 1 summarizes the predictions for which election schedule brings greater success to the Developers' candidate. The vertical dimension characterizes the relative enthusiasm of Developers and Environmentalists on the land-use issue, and the horizontal dimension depicts the strength of the Developers' organizational capacity relative to that of the Environmentalists. In the middle category, where the groups are equally well organized and their members are equally motivated, election timing makes no difference to the Developers' candidate's vote share. Wherever the Developers are either weaker than or equal to the Environmentalists in organizational capacity but are less reliable as voters, the Developers fare better under on-cycle elections. In cases where the Developers are stronger than or equal to the Environmentalists in organizational capacity but are more animated on the land-use issue, the Developers' candidate wins more vote share in off-cycle elections. When the groups' members are equally motivated, the Developers' candidate does better under on-cycle elections when the Developers are organizationally weaker and off-cycle elections when the Developers are organizationally stronger. In the top left and bottom right corners of Table 1, the net effect of off-cycle election timing depends on which of the two countervailing forces is stronger.

[Table 1 about here]

Thus, even when groups seek policies that do not have concentrated benefits and diffuse costs, or when groups work at cross-purposes, changes to election timing still have potential to tip the balance of power in favor of one group or its rival. Notably, however, the theory's prediction is clearest when a group's members are more highly motivated to participate than their rivals and when the group faces relatively weak organized competition.

2. Empirical Strategy

The empirical analysis in this paper focuses on just such a context – a context in which one group tends to be more motivated and better organized than its opposition: school board elections. Teachers and other school district employees have greater incentive to participate in school board elections than the average eligible voter, since by getting involved in school board elections, district employees can help to select the very people who set their salaries, benefits, and working conditions (Moe 2006). Also, of the groups that tend to be active in school board elections, teachers and their unions are the most consistent: In a nationwide survey of school board members, Hess (2002) and Hess and Leal (2005) find that teacher unions top the list of groups cited as active in local school board elections. Focusing on California, Moe (2005) finds that teacher unions are *the* most influential group in elections in at least half the districts in the state. Moreover, in districts where other groups are cited as influential, such as parent-teacher associations and other district employee unions, those groups are typically allies of teacher unions and are thus unlikely to compete with teachers over policy (Haar 2002, Lieberman 1997, Moe 2005). The main potential rivals to teacher groups – business groups – are less frequently active (Hess and Leal 2005), and besides, education is only one of the policy areas that they might focus on, whereas for teachers, it is *the* focus (Moe 2005).

Furthermore, teacher unions across the country share many of the same policy goals, which simplifies the task of identifying a dependent variable. Notably, one of the main goals of teacher unions is to secure higher compensation for teachers.[5] Since teacher compensation is largely determined by the board members who are elected in school district elections, we can expect it to fluctuate within and across districts depending on how influential teachers are in those elections. Consequently, if off-cycle elections allow teachers to have greater influence than on-cycle elections, school board members in districts with off-cycle elections should better compensate teachers than board members in comparable districts with on-cycle elections.

2.1. Challenges for Causal Inference

A comparative study of teacher compensation in school districts with on-cycle and off-cycle elections must also address the concern that election schedules are not randomly assigned to school districts. Rather, for most states in the U.S., school board election timing is uniform within states, as mandated by the state government (Anzia 2011). This makes empirical analysis difficult, since any attempt to use cross-state variation to estimate the effect of election timing on

[5] See, for example, Hoxby (1996), Lieberman (1997), and Moe (2011). See also Greg Toppo, "Teachers union asks for higher salaries," *USA Today*, July 4, 2005.

teacher salaries must somehow rule out the possibility that state-level factors confound the estimated relationship – many of which may not be observable.

The problem of omitted variable bias is lessened when one can leverage within-state variation in the timing of school board elections, since state-level factors that are correlated with the choice of election timing and teacher salaries can be held constant. However, this empirical setup – which is one that I have used to test the theory in previous work (Anzia 2011) – does not fully address the problem. Specifically, for most states in which school board election timing varies within the state, the choice of election timing is at the discretion of the officers of an individual school board. If unobservable characteristics of a school district – such as teacher union strength – make it more or less likely to adopt a certain election schedule *and* influence its teacher compensation policies, estimates of the effect of election timing will be biased.

The situation is improved if one can leverage within-district changes in election timing to estimate the effect, which is the general approach taken by Berry and Gersen (2011). If, for example, teacher union strength is constant over the time period being examined, then a longitudinal model with school district fixed effects would eliminate teacher union strength as a potential source of omitted variable bias. But even in such a district fixed effects model, the possibility of selection bias remains. If teachers only lobby for off-cycle election timing in the districts where they think they will benefit from it, then by simply comparing districts where they secured off-cycle election timing to those where they did not make the effort, one would overestimate the average effect of off-cycle election timing. Since most within-district changes in election timing are the result of decisions made by elected officials within the district itself, it is possible that the officials who *chose* to alter election timing did so precisely *because* they anticipated that it would have certain effects on the size and composition of the electorate. The problem this poses to causal inference is this: if officials in one district change elections to off-cycle while officials in another district opt for a continuation of on-cycle elections, we might not expect officials in those districts to make similar policy decisions, even in the absence of changes to election timing.

The empirical design I use in this paper takes advantage of a 2006 Texas state law that forced some Texas school districts to move their elections to on-cycle while allowing others to retain their pre-existing off-cycle election schedules. By examining changes within districts over time, I largely overcome the problem of omitted variable bias. Furthermore, since the rule the state used to assign school districts to on-cycle or off-cycle schedules was objective, teachers and administrators in districts forced to on-cycle elections were unable to alter the mandate. Thus, the design reduces the incidence of selection bias.

2.2. A Quasi-Experiment: Texas House Bill 1

Prior to 2005, the Texas Election Code allowed for four election dates: the first Saturday in February, the first Saturday in May, the second Saturday in September, and the first Tuesday after the first Monday in November. All elections throughout the state had to be held on one of the established election dates.[6] When the Texas legislature decided in 2005 to eliminate the February and September uniform dates and move the May uniform date to the second Saturday in May, the move was relatively uncontroversial since 97 percent of governmental units in the state held elections either in May or September. Among school districts specifically, 99 percent held their regular trustee elections in May.[7]

In July 2006, state legislators passed HB 1, which for the first time required that all school districts throughout the state hold joint elections with either their parent county or with a

[6] Party primaries and school tax and bond elections were exceptions to this rule.
[7] Texas Association of School Boards (TASB), personal communication, December 3, 2008.

municipality partially or wholly within the school district's borders. Since most municipalities in Texas hold elections in May, the majority of Texas school districts were able to retain their existing May election dates by combining their elections with overlapping municipalities. However, a sizeable number of school districts did not have incorporated cities within their borders, and those districts had no choice but to combine their trustee elections with county elections in November of even-numbered years. Another set of districts had incorporated municipalities within their borders but could not keep their May election dates since the overlapping municipalities held November elections. As long as those municipalities held elections in November of odd-numbered years, a school district could keep its elections separate from state and national elections. However, if the overlapping municipal elections were held in November of even-numbered years, those school districts had to combine elections with the counties. Therefore, with the passage of HB 1, several Texas school districts were forced to shift trustee elections to the same day as presidential elections and gubernatorial elections.[8]

I group school districts into four categories based on how their elections were affected by HB 1 (Collins and Best 2007):

Type 1 districts: School districts that have an incorporated municipality within district boundaries, and that municipality holds elections in May. These school districts had the option of either having a joint election with the municipality in May (off-cycle) or having a joint election with the county in November of even-numbered years (on-cycle).

Type 2 districts: School districts that have a municipality within district boundaries, and that municipality holds elections in November of odd-numbered years. These districts had the option of either holding a joint election with the municipality in November of odd-numbered years (off-cycle) or with the county in November of even-numbered years (on-cycle).

[8] TASB reported that a few districts voluntarily moved their elections from May to November even though they could have retained their May election schedules. However, neither TASB nor the Secretary of State's office kept track of which districts switched voluntarily and which districts were forced to switch (TASB, personal communication, December 15, 2008). With the strategy I explain below, I drop districts that switched voluntarily from the analysis. Also, note that even after 2006, districts were allowed to hold bond elections and proposition elections on either the May or November uniform date.

Type 3 districts: School districts that have a municipality within district boundaries, and that municipality has elections in November of even-numbered years. These districts were forced to move their elections to November of even-numbered years (on-cycle).

Type 4 districts: School districts that do not have an incorporated municipality within district boundaries. These districts were forced to combine elections with the counties in November of even-numbered years (on-cycle).

For the empirical analysis, I take advantage of the fact that a large number of Texas school districts had no choice but to hold on-cycle elections after 2006, whereas other districts maintained discretion over whether to conduct on-cycle or off-cycle elections. The former group, composed of type 3 and type 4 districts, makes up the treatment group, and the latter set of districts, including type 1 and type 2 districts, makes up the control group. My key design innovation is that the school trustees in type 3 and type 4 districts had no choice but to move to on-cycle elections, which meant that interest groups in those districts could not lobby for an advantageous school trustee election schedule. Moreover, the assignment of districts to treatment and control conditions was purely a function of whether or not the school district had a municipality within its borders and when that municipality held its elections. Importantly, assignment was not explicitly a function of interest group strength in the district.

That said, the assignment rule established by HB 1 was created only after Texas Republicans tried for two years – unsuccessfully – to move *all* school trustee elections in Texas to November of even-numbered years. One early such attempt was HB 855, which would have required all school trustee elections to be held at the same time as state and national general elections. The primary supporters of the bill were state Republicans and taxpayer organizations, who argued that it would increase voter participation in school elections as well as reduce the cost of holding elections for school boards across the state.[9] The proposal was opposed by state

[9] Texas House of Representatives, House Committee on Elections, 79th Legislature Broadcast Archives, March 16, 2005, available online at http://www.house.state.tx.us/media/welcome.php. HB 733, which

teacher unions, some school trustees, and the Texas Association of School Boards (TASB). The TASB representative who testified in committee argued that the higher November turnout would merely expose school trustee elections to uninformed voters. A representative from one of the state's three teacher unions explained that on-cycle trustee elections would strip districts of local control and make it more expensive for school board candidates to wage campaigns. A school trustee from one district defended his district's May elections by explaining:

> "What we get in Spring Branch, and in most districts around the state, is an educated voter voting in our May election…I think what you get is…the people who really care about the issues and who are passionate about their district."[10]

In response to the testimony of the opposition, Texas Rep. Dan Gattis replied:

> "I think it's a little disingenuous at times to say, 'We want to make sure that we have informed voters.' No you don't. You want to make sure that you have *your* voters – your voters that are going to come and vote for your issue…They mean the voters that they know they can turn out to vote for their deal."[11]

HB 855 was eventually dropped, and subsequent bills that proposed moving all trustee elections to November of even-numbered years were quietly snuffed in committee. The joint election provision that was later slipped into HB 1 in the summer of 2006 was a watered down election timing measure that only affected the elections of 20 percent of school districts in the state. State teacher unions and school board representatives testified against HB 1 in committee hearings, but the bill was first and foremost a bill that provided property tax relief and created new fiscal and academic accountability programs for school districts, so testimony did not focus on the election timing provision.[12] After HB 1 was passed in 2006, Republicans renewed their

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would have moved all bond elections in the state to November of even-numbered years, was considered by the committee on the same day. Many of the parties that testified against HB 733 were the same as those that testified against HB 855.

[10] Ibid.

[11] Ibid.

[12] Texas State Senate, Senate Committee on Finance, Video/Audio Archives, 79th Session, May 1, 2006, available online at http://www.senate.state.tx.us/75r/senate/commit/c540/c540.htm.

attempts to force larger numbers of districts to on-cycle elections, but those attempts failed to gain traction in the legislature.

In sum, HB 1 created a rule for assigning districts to treatment and control conditions that applied uniformly to all 1,032 independent school districts throughout the state. Importantly, though, the creation of that rule in the legislature was likely the result of concessions made to the teacher unions and school trustees who opposed the move to on-cycle elections. Furthermore, even though the rule for assigning districts to treatment and control conditions was objective, it was not necessarily orthogonal to district attributes that were correlated with district teacher compensation. For example, since larger and more urban districts are more likely to have strong unions *and* to overlap with incorporated municipalities, the political clout of teachers might be greater in control districts than in the districts forced to on-cycle elections. To account for these differences between treatment and control districts, I use both matching techniques – which address selection on observable district characteristics – and fixed effects regression – which also addresses selection on time-invariant unobservable district characteristics – to estimate the effect of election timing on teacher salaries. I describe both strategies in the following sections.

2.3. School Board Elections and Teachers in Texas

Since my empirical analysis focuses on a single state, it is also important to evaluate whether Texas is a suitable testing ground for studying teacher influence in school elections. For starters, Texas is different from most states outside the South in that it prohibits collective bargaining for public school teachers. In one key respect, this difference is helpful for the empirical analysis: School board members in districts with collective bargaining are bound by contracts that typically remain in place for three years, and so even if election timing changes, school boards might not be able to change teacher compensation levels until the existing contract expires. In Texas, however, school boards can change teacher salaries every year. Thus, I can look for changes in teacher compensation levels immediately following the treatment.

Still, because the state prohibits collective bargaining for teachers, the rate of teacher union membership in Texas is not as high as in states like California. However, that is not to say that the rate of teacher union membership in Texas is low. To the contrary, a full 65 percent of public school teachers in Texas are members of unions (Moe 2011). Moreover, it does not appear that teacher unionization in Texas is limited to large urban districts: I assembled data on Texas public school teachers from the 1999, 2003, and 2007 Schools and Staffing Surveys (SASS) conducted by the National Center for Education Statistics (NCES) and calculated the percentage of teachers in each district who are members of unions, and my estimates indicate that the median percentage for the subset of Texas districts included in the SASS sample is 64%. Moreover, teacher unions are present in *at least 95%* of the districts.[13] Thus, even without collective bargaining, it seems that a majority of Texas teachers are members of unions.

Of course, without a study of school board politics specific to Texas, I cannot know for sure whether teachers are as politically active in Texas as they are elsewhere. As of now, the only existing studies of teacher and teacher union participation in school board elections have either been conducted in California (Moe 2005, 2006) or using a national sample of districts (Hess and Leal 2005). However, there is good reason to expect that teachers in Texas *would* be politically active in school board elections. Like everywhere else, school trustees in Texas set policies that directly affect the lives of teachers. It is therefore in the interest of teachers to help elect school trustees who will make teacher-friendly policies. And while Texas school board politics may feature groups that could be competitors of teachers – such as business or anti-tax groups – those competitors are interested in many different policy areas – not just education.

[13] The details of how I calculated these figures are described in the online appendix.

Thus, to the extent that teachers in Texas face organized competition over teacher compensation policies, that competition is probably inconsistent and relatively weak in comparison.

With that said, the main advantage of focusing the empirical analysis on Texas is that its state legislature passed HB 1. State legislatures very rarely pass measures that change school district election timing, and so the Texas case presents an unusual opportunity for a clean empirical study using observational data.

3. Data

Since no state-level entity kept track of how each Texas school district was affected by the law, I employed a combination of strategies to classify the districts in the state as one of the four types described above. To create an exhaustive list of type 4 districts, I overlayed a shapefile of the boundaries of the 1,211 incorporated municipalities in Texas onto a shapefile of the boundaries of 1,023 Texas school districts.[14] I then used the intersect feature of ArcGIS to identify all areas of intersection between the independent school districts and incorporated municipalities, which enabled me to identify 150 independent school districts that do not overlap with any part of an incorporated municipality. These 150 districts form the comprehensive set of type 4 districts, which were all forced to switch to on-cycle elections following HB 1.

Type 3 districts also belong in the treatment group, but distinguishing type 3 districts from types 1 and 2 is challenging, because Texas does not have a central source of information on when municipal elections throughout the state are conducted. Therefore, I am unable to

[14] The 2008-09 school district boundaries shapefile is from the Texas Education Agency (TEA). The geographic information was collected by the GIS Staff of the Research Division of the Texas Legislative Council. There are 1,032 independent school districts in Texas according to the TEA directory of school districts for 2008-09, but there are only 1,029 districts in the shapefile, and 6 of them are common school districts. Since the HB 1 elections rule did not explicitly apply to common school districts, I exclude them from the analysis. Therefore, with 1,023 independent school districts in the shapefile, I lack data on 9 independent school districts. The shapefile for Texas municipalities comes from the Texas State Data Center and Office of the State Demographer, Texas 2009 TIGER/Line State Shapefiles, October 9, 2009, available online at http://txsdc.utsa.edu/txdata/. I removed all unincorporated places from that shapefile.

determine which municipalities hold regular elections in November of even-numbered years as opposed to May or November of odd-numbered years. As a next best alternative, I acquired a partial list of school districts that were forced to move their elections to November of even-numbered years from TASB. In its Election Advisory 2007-01, TASB asked all school districts to report whether they were forced to move their elections to November of even-numbered years as a result of HB 1. 97 school districts voluntarily reported to TASB that they had no choice but to hold on-cycle elections after the passage of HB 1, and of those, 24 were districts that I did not identify as type 4 districts using the procedure described above. Adding those 24 type 3 districts to the 150 type 4 districts, I identify a total of 174 school districts in the treatment group.

Using this approach, it remains possible that I have failed to identify some type 3 districts, since not all districts responded to the TASB request. I therefore pursue one additional strategy to ensure that I do not erroneously classify treatment districts as control districts. Each year, TASB asks school districts whether they would prefer to receive the May election calendar or the November election calendar. In 2008, a total of 982 districts requested one of the two calendars. From this, I can be fairly sure that all districts that requested the May election calendar hold May elections and therefore are type 1 districts. Since I cannot be sure whether the districts that requested the November calendar are type 1, type 2, or type 3 districts, I limit the control group to the 743 districts that hold May elections.[15]

The dependent variable for the analysis is district teacher salary. In Texas, the state education code establishes a minimum teacher salary schedule, but independent school districts adopt their own teacher salary schedules each year as part of the annual budget process, and most

[15] In addition, I read the individual local governance policies of 98 school districts that I was unable to classify, 7 of which stated that their elections are in November of even-numbered years. When I include those districts in the treatment group, the main results are unchanged. (See online appendix.) However, for the analysis here, I exclude those 7 districts because I want the treatment group to only include districts that were forced to switch to November even-year elections (not those that voluntarily switched).

of the state's districts pay teachers more than the state minimum.[16] While it would be desirable to have detailed annual salary schedule and benefits information for each district in Texas, unfortunately, no such data are available.[17] However, comprehensive data on average base teacher salary in each district and year are readily available through the Texas Education Agency (TEA). Average teacher salary has one major disadvantage in that it can change for reasons other than a change in the salary schedule. Still, since the data on average teacher salary are the best available, I use the TEA data files to compile annual average base teacher salary data for the panel of 1,023 independent school districts over seven years, from 2003-04 to 2009-10.[18] Salary figures and all other dollar values in the analysis are adjusted to 2009 dollars.

Since my argument is that election timing affects interest group influence by lowering voter turnout, an intermediate step for testing the theory is to establish that voter turnout in treatment districts increased following implementation of HB 1. As is true of most states, however, Texas does not have a statewide entity that compiles results from all school district elections held in the state. Rather, school trustee elections are administered by the counties, the municipalities, or the school districts themselves, which means that historical school trustee election results (including turnout statistics) in Texas are potentially kept by up to 2,497 different local governments, most of which do not report even recent election results on their websites. Therefore, collecting pre- and post-HB 1 turnout statistics for school trustee elections in Texas is a cumbersome task. For many districts, as I describe below, it is simply not possible.

[16] See TASB, 2010-11 TASB/TASA Teacher Report, available online at http://www.tasb.org/services/hr_services/salary_surveys/documents/tchr_highlights_landing.pdf.

[17] TASB conducts annual school district surveys that ask for detailed salary schedule information, but since only 60 to 70 percent of districts respond each year, those data cannot be combined into a complete panel. Even so, I have acquired three years of detailed salary survey data from TASB (2003, 2006, and 2009), and I use them in the robustness checks discussed in the following section.

[18] The salary data include base salary only; monies from other programs such as the incentive programs created by HB 1 are excluded. TEA, Information Analysis Division, personal communication, October 29, 2010.

However, there is little reason to expect that school elections in Texas would deviate from the empirical pattern established in the political science literature, which is that voter turnout is much higher in on-cycle local elections than in off-cycle local elections (see Hajnal 2010).[19] Even so, to boost confidence that this link in the argument is sound, I called or emailed officials in a total of 29 counties and 205 school districts in early June 2011 to request the returns of school board elections held between 2003 and 2011. Most of the officials I contacted either did not respond to the request or explained that they do not keep election returns more than 22 months old. However, with the returns I did receive, I was able to assemble a dataset of pre- and post-HB 1 turnout in 31 Texas school districts, 13 of which switched to on-cycle elections.

Lastly, since the empirical analysis utilizes a pre-post design, I must also determine *when* the consequences of HB 1 would have taken effect. If the election timing effect works solely through the replacement of sitting trustees with new trustees who are less responsive to teachers, the first sign of a difference in teacher salary in treatment districts would appear in 2009-10, the academic year following the first on-cycle election. However, based on the testimony of various school board members during the committee hearings on election timing changes, it seems clear that even prior to HB 1, school board members in Texas were well aware of the consequences of switching to on-cycle elections. They knew that on-cycle elections would drastically increase voter participation in their elections and that the composition of the electorate would be altered as a result. The same has been true in other states that have considered changes to school board election timing (e.g., Allen and Plank 2005). Therefore, I suspect that school trustees likely *anticipated* the effect of on-cycle election timing for the importance of teachers in elections – even before the first on-cycle election was held. If so, then any election timing effect would

[19] This holds even when one accounts for higher roll-off rates in on-cycle elections (e.g., Caren 2007).

appear as soon as school board members became aware of the implications of HB 1,[20] which was in the spring of 2007.[21] I therefore treat the 2007-08 academic year as the first year of the treatment. However, in my empirical tests, I also explore the possibility that election timing did not have an effect until after the first on-cycle election in November 2008.

4. Empirical Analysis

Figure 1 presents a map of the geographic location of the treatment and control districts that are used for the empirical analysis. The 174 treatment districts – all type 4 districts and the type 3 districts that I was able to identify – are colored dark grey. The 743 type 1 districts that I identified as having May elections are colored light grey. The remaining districts – 6 common school districts, 3 districts that do not hold trustee elections, and all districts that I was unable to classify as type 1, type 2, or type 3 – are left white. Aside from the fact that there are relatively few treatment districts in the urban areas in and around Dallas and Fort Worth, Austin, Houston, and San Antonio, treatment districts are well dispersed throughout the state.

[Figure 1 about here]

Moreover, as expected, voter turnout increased significantly in districts that shifted to on-cycle elections. Table 2 presents the results of two regressions of voter turnout on district and year fixed effects and an indicator for on-cycle election timing. The dependent variable in

[20] While incumbent defeat rates in school board elections tend to be low, they are higher than in U.S. House races (Berry and Howell 2007, Hess and Leal 2005), which suggests that sitting school board members often cannot safely ignore the policy pressure of organized groups in the electorate. Moreover, in California, Moe (2006) finds that teacher union endorsements are just as important to school board candidate success as incumbency.

[21] After HB 1 became law in 2006, it was unclear to officials in many affected districts whether they were required to change term lengths in order to comply with the joint elections rule. Between the summer of 2006 and the spring of 2007, many treatment districts simply rescheduled their May elections to November 2007, still an off-cycle election. To resolve the ambiguity, in April 2007, the Texas Attorney General officially interpreted HB 1 as requiring school districts in the treatment group to reschedule their trustee elections for November 2008. (See Texas Attorney General Opinion GA-535, 2007). That same month, the legislature passed SB 670, which allowed districts to change the term lengths of their trustees in order to comply with HB 1. Thus, the implications of HB 1 were fully evident to school districts starting in the spring of 2007.

column (1) is voter turnout as a percentage of adults in the district.[22] The results show that the average effect of switching to on-cycle elections was a 16 percentage point increase in voter turnout in the school board election. When I limit the analysis to the 14 districts for which I can calculate voter turnout as a percentage of registered voters,[23] as I do in column (2), the estimated coefficient on the on-cycle indicator is even larger – about 18 percentage points. Based on this analysis of a subset of districts, then, it is safe to assume that voter turnout increased in treatment districts as a result of the change to on-cycle elections.

[Table 2 about here]

However, treatment districts differ from control districts on the basis of some district-level attributes that tend to be associated with both teacher salaries and the political strength of teachers in the district. For example, only 31 percent of the districts assigned to the treatment group are urban or urban fringe districts, whereas 49 percent of control districts are classified as urban or urban fringe. Since urban districts generally pay higher teacher salaries than rural districts *and* tend to have stronger teacher unions, failure to account for these differences would result in biased estimates of the treatment effect.

[Figure 2 about here]

In addition, treatment districts tend to be smaller in size and slightly less affluent than control districts, and they also paid lower average teacher salaries in the pre-treatment period. I show these differences in Figure 2. In the top two rows of the left-hand column, I plot the distributions of logged district enrollment in 2005 and logged median family income in 2000 for

[22] The denominator is the number of adults in the district as of the 2000 Census. Three of the 31 school districts in this dataset conduct school board elections by electoral district, and since I do not have the Census figures broken down at the level of the school electoral district, I exclude those districts from the regression in column (1). Some districts that held at-large elections did not track the number of unique voters who participated in the school board elections, and in those cases, I estimated the number of ballots cast by dividing the total number of votes cast in the election by the number of positions up for election.
[23] Specifically, only 14 districts supplied the number of registered voters in the district for each election.

treatment and control districts.[24] The distribution of enrollment for treatment districts is clearly shifted to the left of the distribution for control districts. Moreover, due to the presence of a few control districts with extremely high values of median family income, income in treatment districts tends to be lower than in control districts. In addition, as the plot in the top right-hand corner shows, logged average teacher salaries in 2003 were slightly lower in treatment districts than in control districts. These differences between treatment and control districts pose a problem for the empirical analysis: Regardless of election timing, larger districts tend to have stronger teacher unions. Larger and more affluent districts also tend to pay higher teacher salaries than smaller, less affluent districts. And if treatment districts were starting out with slightly lower average teacher salaries even before the treatment, there might be something distinct about those districts that affected their salary growth after 2006.

Regarding other attributes that are likely correlated with teachers' political strength and average teacher salaries, the pretreatment values for treatment and control districts are similar. For example, teachers employed in more challenging work environments – for example, districts with more students for whom English is a second language – generally earn higher salaries that teachers who work in less challenging environments (see Martin 2010). Yet, as we can see from the bottom left hand panel of Figure 2, treatment and control districts differ very little on the basis of the percentage of the student body that was Hispanic in 2005. Furthermore, if it were the case that teacher salaries were growing at significantly different rates in treatment and control districts *prior* to HB 1, it would be difficult to attribute any post-2006 salary difference to HB 1. However, the lower right hand panel of Figure 2 demonstrates that this was not the case: in spite of the differences in salary levels between treatment and control districts in 2003, there are only

[24] Data for both variables, as well as for the data on percent Hispanic, are from the Common Core Data (CCD) files from the NCES. Median family income is adjusted to 2009 dollars.

small differences between treatment and control districts in the distributions of average within-district teacher salary growth from 2003 to 2006.[25]

The first method I use to handle the pretreatment covariate imbalance between treatment and control districts is matching. I match treatment districts to control districts on the basis of their Metropolitan Status Codes (MSCs), their logged enrollment in 2005, and their logged median family income in 2000. I use exact matching for the MSCs so that rural districts in the treatment group are only matched to rural districts in the control group, and likewise for urban and urban fringe districts. In order to achieve balance on district enrollment and income, I use a caliper equal to one-tenth of a standard deviation for both variables, discarding all treatment districts that fail to find acceptable matches based on this distance criterion.[26] The dependent variable is the percentage growth in district average teacher salary (in real terms) from 2006-07 to 2009-10, the period following the implementation of HB 1. All matching is one-to-one with replacement and is carried out using the Matching package in R (Sekhon 2011).

To provide a benchmark, the first panel of Table 3 presents the results when I use all the treatment and control districts to calculate the average treatment effect on the treated (ATT).[27] Prior to carrying out any matching, I estimate a treatment effect of about -1 percentage point: districts that were forced to switch to on-cycle elections increased salaries by 1 percentage point less than districts that kept their May elections. This difference is statistically significant at the 5 percent level. However, the treatment and control groups are not balanced on enrollment, income, the percentage of students who are African American, or pretreatment salary level.

[25] In addition, treatment districts were no more likely to offer medical insurance or retirement benefits to their teachers, and they were less likely to offer dental and life insurance benefits. See online appendix.
[26] For all larger calipers that I tried, I did not achieve balance on either district enrollment or district median income. See online appendix for details.
[27] The number of control districts in this case is 742 instead of 743 because the TEA data are missing an average salary value for one district in 2006.

Using a Kolmogorov-Smirnov (K-S) test, which tests for differences in the overall distributions of a variable in two groups, I also reject that the distributions of percent Hispanic and percent Native American are the same in the treatment group as in the control group. I do, however, find that there is *no* difference between the rates of average teacher salary growth in treatment and control districts prior to the implementation of HB 1. Therefore, whereas treatment and control districts had statistically indistinguishable teacher salary growth rates prior to HB 1, after the election timing change was implemented, treatment districts increased salaries 1 percentage point less than control districts.

[Table 3 about here]

The lower half of panel 2 of Table 3 presents the same balance statistics after I carry out the matching procedure described above. I successfully match 106 treatment districts to 88 unique control districts and achieve balance on all of the critical covariates. Specifically, using the matched subset of districts, for enrollment, income, student demographics, and pretreatment salary, I fail to reject the null hypothesis that the average treatment group values are equal to the average control group values. In addition, a K-S test fails to reject the null hypothesis for enrollment, income, district demographics, and pretreatment salary, demonstrating that the distributions within matched treatment and control districts on those variables are exchangeable. As before the matching procedure, I find no significant differences between the means and distributions of average teacher salary growth in treatment and control districts prior to HB 1.

Using this comparable set of 194 districts, I estimate a treatment effect of -0.75 percentage points, statistically significant at the 10 percent level (p=.06). On average, therefore, districts that were forced to switch to on-cycle school trustee elections increased their teachers' salaries by 0.75 percentage points *less* following HB 1 than a set of districts of the same size, income, urbanicity, and pretreatment salary that were allowed to keep their off-cycle elections in May. This result supports the hypothesis that the dominant interest group in school district elections exerts less influence in on-cycle elections than in off-cycle elections.

To ensure that the result in panel 2 is not driven by the inclusion of the small number of type 3 districts in the treatment group, I present in panel 3 the results from the same analysis but excluding type 3 districts. Recall that type 3 districts are those that have incorporated municipalities within their borders but that were nonetheless forced to switch to on-cycle elections because those municipalities hold elections in November of even-numbered years. If there is some unobserved property of districts whose municipalities hold on-cycle elections that makes them different from control districts whose municipalities hold off-cycle elections, then the two types of districts would not be exchangeable. However, when I exclude the type 3 districts from the analysis,[28] the effect of the forced switch to on-cycle school trustee elections decreases by a mere tenth of a percentage point: I still estimate a -0.63 percentage point effect of on-cycle election timing on average district teacher salary, significant at the 5 percent level.

The results from the matching analysis are consistent with the hypothesis that teachers are less influential in on-cycle elections. However, the matching only accounts for potential confounders that are observable. One might be concerned that there are unobservable characteristics of treatment districts that not only differ from those of control districts but that also determine why their teacher salaries increased at lower rates after 2006. For example, the residents of type 3 and type 4 districts might place lower priority on education spending than residents of type 1 and type 2 districts such that they responded to the economic downturn by granting lower salary raises to teachers. The estimates in Table 3 do not account for treatment

[28] In order to achieve balance on the covariates here, I also have to match on pretreatment salary levels and growth. I use a caliper of 1.5 standard deviations for these variables.

and control group differences in residents' preferences over spending on teacher salaries – measures of which do not exist – and therefore cannot rule out this possibility. Furthermore, certain characteristics of districts that influence whether or not they contain an incorporated municipality, such as the political influence of private developers or population density (see Burns 1994), might also influence the way districts set teacher salary policy after 2006.

In order to account for potential differences between treatment and control districts such as district preferences and propensity to incorporate, I model within-district changes in average teacher salary from 2003-4 to 2009-10 using district fixed effects regression. This approach allows me to estimate the effect of the forced switch to on-cycle elections while partialling out the effects of any time-invariant district characteristics. The model is as follows:

$$\ln(salary_{it}) = \alpha_i + \delta_t + \beta(OnCycle_{it}) + X_{it}\psi + \varepsilon_{it}$$

Subscript i denotes the school district, and t denotes the year. *On Cycle*$_{it}$ is the primary independent variable of interest. It equals 1 for all treatment districts from 2007-08 to 2009-10 and 0 otherwise. The α_i are district fixed effects, and X_{it} is a matrix of district characteristics that vary year to year. The δ_t are year dummy variables, which control for annual statewide trends in logged average teacher salaries (in real terms). β and ψ are regression coefficients, and ε_{it} is an error term. Because the errors are likely correlated within districts over time, I cluster the standard errors by school district (Bertrand, Duflo, and Mullainathan 2004).

The inclusion of district fixed effects allows me to partial out the effects of any unobservable district characteristics that do not vary over time, but there are several time-variant district characteristics that likely affect yearly changes in district teacher salary policies. To this end, I have collected data on a number of school district characteristics that I expect to influence both teacher salaries as well as whether a district fell into the treatment group or control group with the passage of HB 1. I assembled annual data on district enrollment using the NCES CCD files from 2003-04 to 2007-08 as well as TEA enrollment records for 2008-09 and 2009-10, since teacher salaries generally grow as districts increase in size. Teacher salaries also increase with district income, but data on median family income are only available at the school district level for years in which the decennial census is conducted. As a substitute, I use the annual TEA data on total assessed property value in each district to control for increases in district income over time. Because the dependent variable is average teacher salary, and more experienced teachers are paid higher salaries than less experienced teachers, I use TEA data on the average number of years of teacher experience in each district and year to control for seniority.[29] Lastly, since teachers who work in districts with more minority students tend to earn higher salaries (e.g., Martin 2010), I use the same sources to compile measures of the percentage of enrolled students who are African American, Hispanic, Asian or Pacific Islander, and Native American.

The results of the fixed effects regression are presented in Table 4.[30] The first column presents a simple model that includes only *On Cycle*, the district fixed effects, and the year dummy variables on the right hand side. The result is similar in magnitude to that of the results in Table 3: relative to annual changes in average teacher salary throughout Texas, districts in the treatment group paid teachers 0.9 percent *less* in base salary once they were forced to conduct on-cycle elections. The estimate of the coefficient on *On Cycle* is statistically significant at the 5 percent level, lending support to the hypothesis.

[Table 4 about here]

[29] The teacher experience figures track how long a given teacher has been working for the Texas public school system, not the number of years he has been in a particular position.
[30] The number of observations in column (1) is 6418 rather than 6419 (917 districts x 7 years) due to missing average teacher salary data for one district in 2006. In addition, I am missing property values data for 11 district-years, which explains the N in column (2).

In column (2), I add the full set of control variables, including logged enrollment, logged assessed property value, average teacher experience in the district, and the ethnic composition of the district. The result is striking: once treatment districts were forced to consolidate their elections with national elections in November of even-numbered years, the school trustees in those districts granted teachers significantly smaller salary increases than they had given in earlier years. Specifically, relative to annual trends in average salary throughout the state, treatment districts paid teachers 1.3 percent less after implementation of HB 1. 1.3 percent of the average teacher salary in the average district in 2007 amounted to $560, a non-negligible amount of money for an individual who makes $43,000 per year in base salary. Moreover, the effect of on-cycle election timing is statistically significant at the 1 percent level. This result provides strong support for the prediction that the switch to on-cycle election timing decreased the influence of teachers in the elections.

The other independent variables behave as expected. Enrollment is positively correlated with average district teacher salary: a 1 percent increase in enrollment is associated with a 0.03 percent increase in average teacher salary. Likewise, rising property values are associated with increases in average district teacher salaries, as expected. As the average number of years of teacher experience in a district increases, teacher salaries rise as well, an effect that is statistically significant at the 1 percent level. While the proportion of students who are Native American or Asian does not appear to affect average teacher salaries, increasing proportions of African American and Hispanic students are associated with higher average teacher salaries.

In the model presented in column (3), I investigate the timing of the effect of on-cycle elections on average teacher salaries by including interactions between the treatment district indicator and all of the year dummy variables. Notably, there was no significant difference between the annual increases in average teacher salaries in treatment and control districts for 2003-4 to 2006-7, prior to HB 1: the coefficients on the interaction terms for all pre-treatment years are statistically insignificant. In 2007-8, however, while teacher salaries were lowered from 2006 levels (in real terms) in both treatment and control districts, the dip was significantly more pronounced in treatment districts. That trend continued in 2008-9, when the gap between treatment and control districts widened further. In 2009-10, the difference in growth between treatment and control districts slowed, and both increased average teacher salary by approximately 2 percent. The finding that there was a negative effect in the years between the announcement of the election timing change and the first on-cycle election suggests that sitting school board members became less responsive to teachers as a result of the switch.[31]

The results presented in columns (1) to (3) of Table 4 are robust to a variety of alterations in the district sample and model specification.[32] When I limit the analysis to the set of 194 matched districts, I estimate an effect of -0.7 percent, significant at the 10 percent level. The results are robust to the exclusion of type 3 districts, the exclusion of districts that pay the state minimum salary schedule, as well as to the inclusion (as control districts) of districts that I was not able to classify as treatment or control. When I use first differences rather than fixed effects regression, the effect is still negative and statistically significant. Finally, a battery of tests suggests that the results are not driven by fluctuation in teacher seniority over time, nor are they caused by differential effects of property tax reduction in treatment and control districts.

Moreover, it does not appear that the negative effect of on-cycle election timing is a product of within-district variation in teacher salaries. When I model specific steps of the salary

[31] With more detailed information on the school trustee candidates running in each election, one could conduct a better test of the alternative "replacement" hypothesis. In the absence of such information, my findings suggest that the election timing change induced sitting trustees to be less responsive to teachers.
[32] All the results described here are presented in the online appendix.

schedule for a subset of districts and years – thus comparing salaries for teachers with equal levels of education and experience – the negative impact of on-cycle election timing persists.[33] One might also worry that the negative effect on salaries could have been offset by increases in other, unmeasured components of teacher compensation, such as health insurance. However, my analysis of NCES expenditures data shows that the switch to on-cycle elections had no discernable effect on districts' expenditures on instructional employees' fringe benefits.

On the whole, then, these results strongly support the hypothesis that the forced switch to on-cycle elections decreased the influence of teachers and teacher unions in Texas school board elections. However, if there were similar decreases in spending in areas *not* coveted by teachers, one might hesitate to attribute the change in average teacher salary to decreased teacher electoral influence caused by on-cycle elections.[34]

One way of addressing concern about the mechanism would be to measure teacher electoral influence directly, perhaps by the percentage of voters mobilized by teacher organizations as a fraction of total active voters. Given that measures of teacher mobilization capacity are not available, one proxy for teacher mobilization strength is the percentage of teachers in a district who are members of unions. If unionization rates capture teacher organizational capacity, then the negative effect of *On Cycle* should be greater in more heavily unionized districts.

Even unionization, however, is hard to measure: there are no current, publicly available data on teacher union membership in all Texas school districts. As I explained above, it is possible to produce rough estimates of teacher union membership in a subset of districts using

[33] The salary schedule data were provided by TASB for a subset of districts in 2003, 2006, and 2009; the results are in the online appendix.
[34] Notably, the shift to on-cycle elections did not result in a significant decrease in total district spending. In a model of logged current expenditures on elementary and secondary education, the coefficient on *On Cycle* is negative but statistically insignificant. See online appendix.

the SASS data. For teacher unionization data on all school districts, however, the best available resource is the Census of Government from 1987 – the most recent year that the Census conducted its labor-management relations survey. Admittedly, this 20-year-old measure is a crude proxy for unionization rates today. Still, since it is the only available source for information on all of the districts, I use it to test whether the effect of the switch to on-cycle elections was larger (more negative) in more heavily unionized districts.[35] I interact the *On Cycle* indicator with the unionization rate, centered about its mean, to test whether the shift to on-cycle elections had a greater impact in districts with better organized teachers.

The results are presented in column (4) of Table 4. For districts with the mean level of teacher unionization in Texas in 1987, I estimate a statistically significant effect of on-cycle election timing of -1.3 percent. As expected, the coefficient on the interaction term shows that the effect of the switch to on-cycle elections was more pronounced for districts in which a greater proportion of teachers were unionized. Granted, the coefficient estimate on the interaction term is imprecise (p=0.106), and given the measurement error in the unionization variable, I do not put much stock in this result.[36] However, when I run the same regression on the subset of districts for which I have more current estimates of teacher unionization – using the SASS data as I described earlier – I find the same pattern. Those results are set out in column (5).[37] Specifically, the districts where more teachers are in unions are those where the switch to on-cycle elections had the largest impact, and the negative coefficient on the interaction term is

[35] The unionization rate equals the total number of instructional employees who were members of an employee organization divided by full-time equivalent instructional employees in 1987.

[36] Moreover, there is one district that strongly influences the estimated slope of the coefficient on the interaction term. When I exclude that district from the analysis, the estimated coefficient on the interaction term drops from -0.013 to -0.008. See online appendix.

[37] There are 359 districts in this model: 51 treatment districts and 308 control districts.

significant at the 10 percent level. Together, these results suggest that the districts with better organized teachers were disproportionately affected by the switch to on-cycle elections.

As an additional check on whether the negative effect is driven by the decreased influence of teachers, I use the original model to test whether the switch to on-cycle elections led to decreased school district spending on an item that teachers do *not* have a vested interest in expanding: general district administration. The dependent variable in column (6) of Table 4 is logged per pupil district expenditures on general administration, as reported by TEA actual financial reports.[38] Since teachers have little stake in increasing expenditures on general administration, we would not expect the switch to on-cycle elections to have an effect if it is indeed decreased teacher union influence that is at work. The result in column (6) is consistent with this expectation: the switch to on-cycle elections did not affect per pupil spending on general administration. This evidence supports the claim that the switch to on-cycle elections affected one of the main items sought by teachers – teacher salaries – but did not decrease spending on items that teachers should care little about.

Taken together, these results provide strong support for the prediction that off-cycle election timing enhances the influence of organized interest groups in elections. For decades, school districts in Texas had been accustomed to holding trustee elections in May, separately from state and national elections. As a school board trustee explained during the hearings on a predecessor of HB 1, these elections were truly "their" elections: typical turnout was less than 10 percent of registered voters. In 2006, when the Texas state legislature handed down a mandate for districts to combine their trustee elections with either city or county elections, at

[38] I do not have the TEA actual financial reports for the 2009-10 academic year, hence I am missing a year of data in the results presented in column (6). However, when I replace the dependent variable with the budgeted amounts for all years – for which I have figures for 2009-10 – the results do not change in any substantive sense. These results are in the online appendix.

least 174 districts had no choice but to move their elections to November of even-numbered years, the same time as presidential and gubernatorial elections. This was, no doubt, a massive change for those districts. Incumbent school trustees could expect more than twice as many voters to participate in future elections. I find that as soon as the joint election requirement was made clear to district officials, school trustees in the districts forced to on-cycle elections responded by granting smaller pay increases to teachers.

At first glance, 1.3 percent seems a small figure, definitely statistically significant but questionable in its substantive importance. Yet this is almost certainly an underestimate of the effect of on-cycle election timing on the relative influence of teachers in elections. As I described above, the election timing provision of HB 1 – which affected 20 percent of Texas school districts – only passed after multiple failed attempts by Republican state legislators to move *all* Texas school trustee elections to November of even-numbered years. Most of those bills were never considered in committee. The ones that were considered in committee were vigorously opposed by Texas school boards and teacher unions. The repeated failure of those bills no doubt persuaded Republican legislators to water down the election timing provision so as to only affect a small number of districts, which the teacher unions did not openly oppose. It is possible, if not likely, that the teacher unions let the election timing provision pass because they anticipated that it would only change election timing in districts where it was least likely to make a difference to their influence. If so, then the estimates presented in Table 4 are lower bounds on the effect of on-cycle election timing on teacher union electoral influence. Had the change affected the remaining 80 percent of Texas districts, the estimated effect would likely be larger.

But it is important not to make too little of the -1.3 percent effect that I do estimate. To an individual teacher who makes $43,000 per year, the loss of $560 is a noticeable decrease.

Moreover, current spending by Texas public school systems amounts to $35.6 billion every year, and over 45 percent of that is used to pay instructional salaries and wages (Census of Government 2007). If all districts in the state cut average teacher salary by 1.3 percent, the $16.27 billion spent on instructional salaries in Texas would decrease by almost *$211 million* – an amount that could either be reallocated within the districts or passed on to voters. In the context of Texas school district budgets, therefore, the consequences of forcing districts to switch from off-cycle to on-cycle school trustee elections are highly substantively significant.

5. Conclusion

A large fraction of the nation's 500,000 elected officials are elected at times other than national Election Day, when few races are on the ballot and voter turnout is low. In this paper, I have argued that the low voter turnout that accompanies these off-cycle elections creates an environment in which special interest groups have increased electoral influence. For interest groups that seek policies with concentrated benefits and dispersed costs and that are better organized than any competing group, I expect that off-cycle election timing works to their advantage: officials elected in off-cycle elections should make policies that are more favorable to those groups than officials elected in on-cycle elections.

To test the theory in a context where its prediction is clearest, I have leveraged a 2006 Texas law that forced several of the state's school districts to combine their elections with state and national elections, while allowing others to retain their pre-existing off-cycle election schedules. The rule that the state legislature used to assign districts to different election timing conditions was objective, so that interest groups in the treated districts could not directly lobby for favorable election timing. Since I have examined districts within a single state as well as changes within districts over time, there is little cause for concern that omitted variables bias the estimates.

Using matching to achieve balance on district size, income, urbanicity, ethnic composition, and pretreatment salary, I have found that the rate of average teacher salary growth was approximately 0.75 percentage points lower after the policy intervention in districts that were forced to switch to on-cycle elections. Furthermore, by conducting a within-district analysis using fixed effects regression, I have found that treatment districts granted significantly smaller salary increases to teachers after the implementation of the new election timing law. This change cannot be attributed to changes in district size, income, ethnicity, or teacher experience. The results are therefore strongly consistent with the hypothesis that on-cycle elections decreased the influence of the dominant interest group in the school board elections.

The results of this quasi-experiment are striking, especially considering that these are likely conservative estimates of the causal effect of election timing on policy. The analysis is not invulnerable to criticism, however. Without a district-level measure of the mobilization capacity of teachers and teacher unions – for which even poor measures are hard to come by – I am not well able to evaluate how the effect of the switch to on-cycle elections varies with interest group strength. Moreover, the assignment of districts to treatment and control conditions was not random, which leaves open the possibility that there is some time-variant characteristic of districts that confounds the estimate of the treatment effect.

The latter possibility, however, is unlikely. The conditions created by HB 1 allow for cleaner estimation than the vast majority of studies that use observational data. To the extent that the results are biased, they are most likely biased in a conservative direction. Furthermore, while interest group strength and election activity are extremely difficult to measure, the focus on school district elections limits the possible alternative explanations for the findings. School boards make decisions that directly affect the lives and livelihoods of teachers and other school employees, and thus teachers have a large personal stake in school district policy. Moreover, teachers are well organized and highly active in politics (e.g., Moe 2011); the extant literature has demonstrated that they tend to be the dominant interest group active in school board elections (e.g., Hess and Leal 2005). This study shows that when school board elections are held at a time when voter turnout is high, school boards give smaller salary increases to teachers. Moreover, the negative impact is greater in districts where teachers are better organized.

The results here also accord with those found in other studies. In an earlier paper, I used a cross-sectional design in eight states which showed that teacher salaries are 1.5 to 4.2 percent higher in districts that hold off-cycle elections, depending on the experience and education level of the teacher (Anzia 2011). In a study of California districts, Berry and Gersen (2011) found a significant 1 percent effect by regressing log average teacher salary between 1999 and 2008 on an indicator for even-year elections and log teacher salary in 1987 – the year prior to when school districts were allowed to change their elections to on-cycle.[39] The Texas design I use in this paper finds similar effects but with an improved design – a design that leverages within-district changes in election timing, partials out potential time-constant sources of omitted variable bias, and reduces the possibility of selection bias.

While the empirical analysis of this paper has focused on school board elections, the potential implications of these findings for American government are far broader and open up a number of questions that are ripe for future research: What other organized groups benefit from

[39] The instrumental variables regression in the Berry and Gersen study yields a null effect of election timing, but in analyzing the data from that article, I found that the instrument for on-cycle election timing predicts teacher salaries in 1987, even before districts were allowed to change their election schedules to on-cycle. This suggests that it is unlikely that the instrument's effect on teacher salary works only through election timing.

off-cycle election timing, and under what conditions? Do city officials elected in off-cycle elections make policy that is more favorable to the dominant interest groups in city elections, whether those groups are real estate groups, business associations, or municipal employee unions? In state elections held in the odd-numbered years, are a larger percentage of voters interest group members or mobilized by interest groups than in state elections held in November of even-numbered years? Does the involvement of political parties in elections condition the extent to which off-cycle election timing helps interest groups? At a minimum, the political science literature has shown that voter participation in elections varies predictably with when they are held. This paper has shown that the lowering of voter turnout that accompanies off-cycle election timing does not occur uniformly across the electorate. Rather, it increases the presence of organized interest groups at the polls, with great potential to affect election outcomes and public policy.

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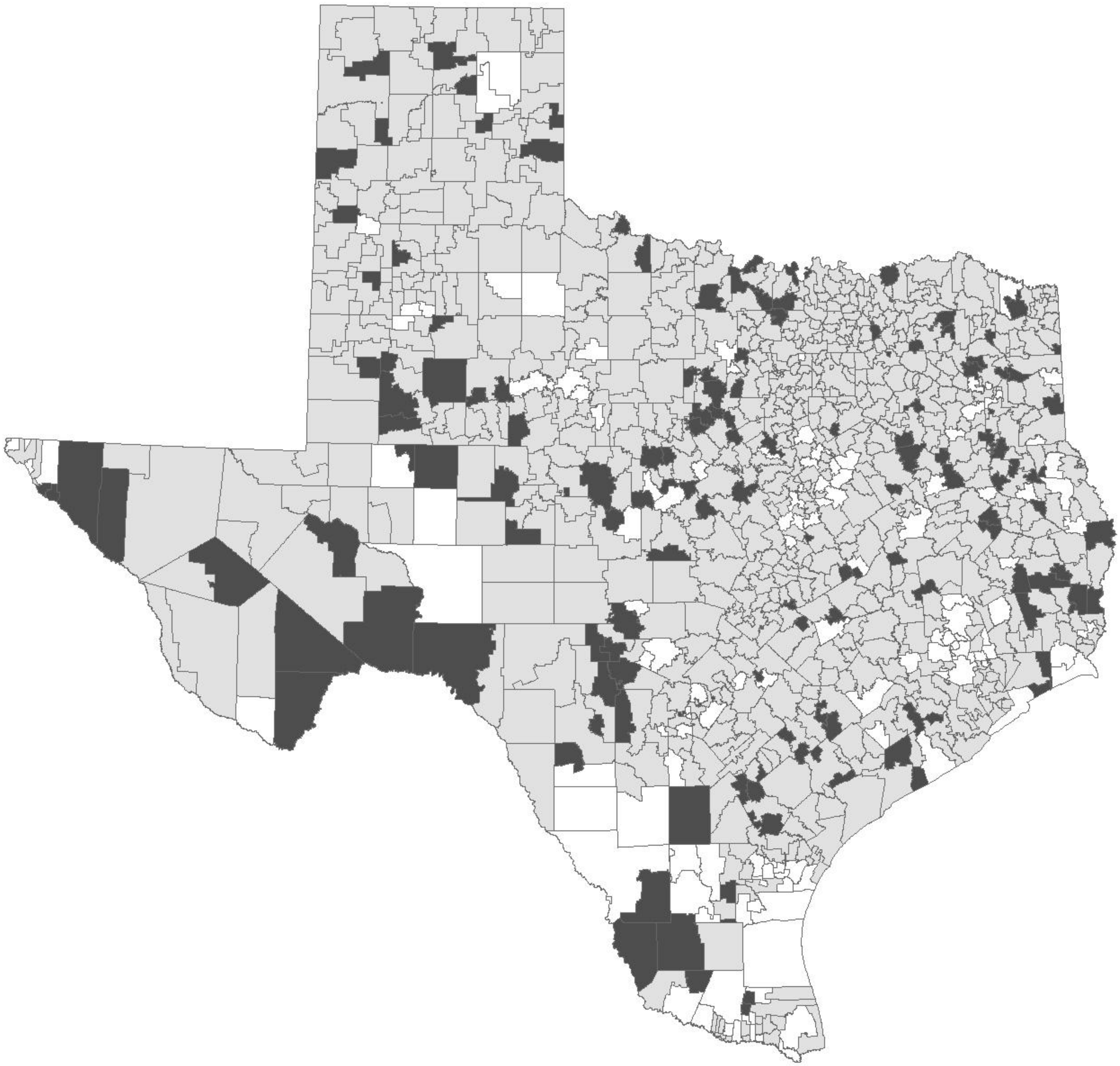
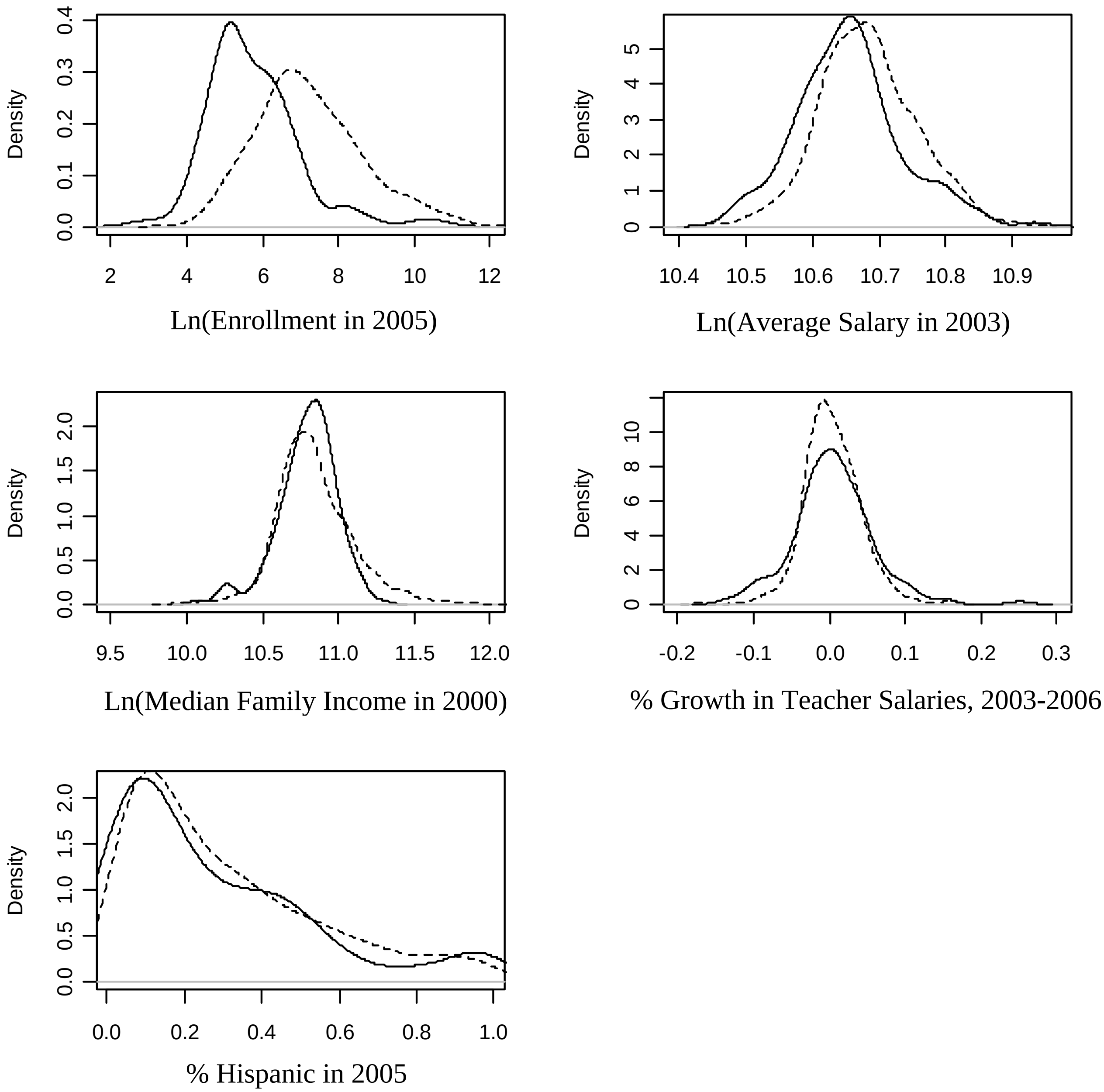
Figure 1: Treatment and Control School Districts, House Bill 1



Notes: The 174 treatment districts are shaded black. The 743 control districts used for the empirical analysis are shaded light grey. All other districts are left white: the 6 common school districts, 3 districts that do not hold school trustee elections, and all type 1, 2, and 3 districts that I was unable to classify.

Figure 2: Pretreatment Attributes of Treatment and Control Districts



Notes: The solid lines are the distributions of each variable for the treatment districts; the dashed lines are the corresponding distributions for control districts. See panel 1 of Table 3 for difference in means tests and Kolmogorov-Smirnov tests.

Table 1: Election timing condition that yields greater vote share for the Developers' candidate

	The Developers have stronger organization than the Environmentalists	The Developers and the Environmentalists are equal in organizational capacity	The Developers have weaker organization than the Environmentalists
Environmentalists are more motivated to participate than Developers	Depends on the relative size of the individual and group effects	On-Cycle	On-Cycle
Developers and Environmentalists are equally motivated	Off-Cycle	No difference	On-Cycle
Developers are more motivated to participate than Environmentalists	Off-Cycle	Off-Cycle	Depends on the relative size of the individual and group effects

Table 2: Election Timing and Voter Turnout

	% of Adults (1)	*% of Registered Voters* (2)
On Cycle	0.156	0.182
	(0.019)***	(0.032)***
Observations	110	65
Number of districts	28	14
R-squared	0.85	0.80

Notes: Robust standard errors clustered by district in parentheses. All models include district and year fixed effects. Dependent variable in column (1) is the percentage of adults who voted in the school board election. Dependent variable in column (2) is the percentage of registered voters who voted in the school board election.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3: Effect of On-Cycle Elections, Matching

Treatment Effect

	1. All Districts				2. Matched Districts				3. Matched Districts, Excluding Type 3			
	Mean, Treatment	Mean, Control	Difference in Means	T-test p-value	Mean, Treatment	Mean, Control	Difference in Means	T-test p-value	Mean, Treatment	Mean, Control	Difference in Means	T-test p-value
% Growth in Average Salary, 2006-2009	-0.20	0.77	-0.97	0.03	-0.59	0.16	-0.75	0.06	-1.324	-0.697	-0.627	0.05
Number of Districts	174	742			106	88			65	54		

Balance Statistics

	Difference in Means	T-test p-value	K-S Statistic	K-S Bootstrap p-value	Difference in Means	T-test p-value	K-S Statistic	K-S Bootstrap p-value	Difference in Means	T-test p-value	K-S Statistic	K-S Bootstrap p-value
Ln(Enrollment)	-1.49	0.00	0.50	0.00	0.001	0.93	0.04	1.00	0.006	0.58	0.05	1.00
Ln(Median Income)	-0.05	0.00	0.12	0.02	0.000	0.97	0.06	0.99	0.001	0.85	0.08	0.98
% Hispanic	-2.54	0.24	0.11	0.05	0.25	0.92	0.14	0.22	-1.09	0.74	0.20	0.13
% Black	-4.30	0.00	0.27	0.00	-0.42	0.63	0.08	0.88	-1.42	0.21	0.12	0.68
% Native American	0.19	0.28	0.27	0.00	0.04	0.89	0.10	0.50	0.25	0.56	0.09	0.87
Ln(Pretreatment Salary)	-0.03	0.00	0.20	0.00	0.01	0.22	0.12	0.37	0.004	0.46	0.11	0.82
Pretreatment Growth	0.06	0.88	0.08	0.24	-0.12	0.86	0.11	0.48	-0.40	0.23	0.12	0.68

Notes: I execute all matching using the Match function in the Matching package in R (Sekhon 2011). I require exact matches on districts' Metro Status Codes (i.e., urban, urban fringe, and rural) and use a caliper of one-tenth of a standard deviation for logged 2005 enrollment and logged 2000 median family income. Panel 1 (All Districts) uses all the districts shaded in Figure 1, i.e., both type 3 and type 4 districts in the treatment group and identified type 1 districts in the control group. Panel 2 (Matched Districts) includes matched districts only. Panel 3 presents the results excluding all type 3 districts from the matching procedure; it thus compares matched type 4 and type 1 districts. To achieve balance on the covariates in panel 3, I also matched on logged pretreatment salary and pretreatment growth using a caliper of 1.5 standard deviations. The dependent variable is the percentage growth in average district teacher salary from 2006-07 to 2009-10, in real terms. Pretreatment salary is the district's average teacher salary in 2003-04 (in 2009 dollars). Pretreatment growth is the percentage growth in average district teacher salary during the pretreatment period, 2003-04 to 2006-07 (in real terms). All t-test p-values for the ATT are p-values from a one-sided test, since I am testing a one-sided hypothesis. For all the balance statistics, the t-tests are two-sided.

Table 4: Effect of On-Cycle Elections

	(1)	(2)	(3)	(4)	(5)	(6)
			Ln(Average Teacher Salary)			*Ln(Gen. Admin)*
On Cycle	-0.009	-0.013		-0.013	-0.017	-0.001
	(0.004)**	(0.003)***		(0.003)***	(0.006)***	(0.016)
On Cycle * Unionization				-0.013	-0.036	
				(0.011)	(0.022)*	
Ln(Enrollment)		0.034	0.034	0.034	0.028	-0.753
		(0.009)***	(0.009)***	(0.009)***	(0.015)*	(0.048)***
Ln(Property Value)		0.022	0.023	0.023	0.023	0.12
		(0.006)***	(0.006)***	(0.006)***	(0.011)**	(0.027)***
Avg. Years Experience		0.013	0.013	0.013	0.01	-0.004
		(0.001)***	(0.001)***	(0.001)***	(0.002)***	(0.002)*
% Native American		0.029	0.032	0.023	0.063	-0.702
		(0.074)	(0.074)	(0.075)	(0.274)	(0.417)*
% Asian or Pacific Islander		0.19	0.181	0.182	0.114	0.86
		(0.118)	(0.119)	(0.118)	(0.173)	(0.677)
% Black		0.15	0.15	0.151	0.212	0.234
		(0.061)**	(0.061)**	(0.061)**	(0.095)**	(0.243)
% Hispanic		0.088	0.089	0.087	0.161	-0.258
		(0.027)***	(0.027)***	(0.027)***	(0.057)***	(0.144)*
2004	-0.02	-0.02	-0.02	-0.02	-0.019	-0.007
	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.002)***	(0.004)*
2005	-0.039	-0.042	-0.042	-0.042	-0.041	-0.007
	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.002)***	(0.006)
2006	0.004	-0.002	-0.002	-0.002	-0.004	0.009
	(0.001)***	(0.001)	(0.001)	(0.001)	(0.003)	(0.008)
2007	-0.005	-0.012	-0.014	-0.012	-0.014	-0.038
	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.004)***	(0.010)***
2008	-0.017	-0.027	-0.027	-0.027	-0.029	-0.097
	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.005)***	(0.012)***
2009	0.01	-0.007	-0.006	-0.006	-0.01	
	(0.002)***	(0.003)**	(0.003)**	(0.003)**	(0.006)*	
Treatment District * 2004			0.0004			
			(0.003)			
Treatment District * 2005			0.004			
			(0.003)			
Treatment District * 2006			-0.001			
			(0.003)			
Treatment District * 2007			-0.007			
			(0.004)**			
Treatment District * 2008			-0.014			
			(0.005)***			
Treatment District * 2009			-0.015			
			(0.005)***			
Observations	6418	6407	6407	6393	2509	5491
R-squared	0.88	0.91	0.91	0.91	0.93	0.96

Notes: Standard errors clustered by district in parentheses. Dependent variable is ln(average teacher salary) in (1)-(5) and ln(per pupil expenditures on general administration) in (6). On Cycle = 1 for treatment districts in 2007, 2008, and 2009; 0 otherwise. Hypothesis tests on On Cycle, On Cycle*Unionization, and Treatment*Year interactions for 2007-2009 are one-tailed; all other tests are two-tailed. Models include district fixed effects. * significant at 10%; ** significant at 5%; *** significant at 1%.