The Political Consequences of Controversial Education Reform: Lessons from Wisconsin's Act 10*

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Abstract

We study the electoral consequences of Wisconsin's Act 10, a controversial law that weakened teachers' unions and enabled flexible teacher pay. Exploiting variation in the timing of implementation of the reform across districts, we first show that it raised student test scores, reduced union revenues, and created winners and losers among teachers in terms of pay. We then show that the reform increased the vote share for the incumbent GOP governor by about 2 pp and lowered Democratic campaign contributions from teachers and unions. The electoral gains were driven by districts with *ex ante* stronger unions and more potential winners among teachers and students.

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1 Introduction

Political effects are of first-order importance for education reforms. Efforts to counteract stagnant achievement or unequal access often generate controversy. Accountability, teacher performance pay, school vouchers, and charter schools are just a few examples of public education policies at the center of heated debates for years. Controversial reforms may be hard to implement and sustain. Elected officials, uncertain about a contentious reform's political consequences, may be reluctant to promote it or fight for its survival. Without political support, even successful programs run the risk of being abandoned in the face of backlash or watered down into irrelevance (Bold et al., 2018).

Yet the political effects of controversial education reforms have so far remained understudied, particularly in the U.S. context. This is likely due to a number of obstacles. First, policies are not made in a vacuum. Education reforms are often driven by other forces that also affect how people vote, such as the government's budgets or the state of the economy. Second, these reforms may happen at the same time as other policy changes that may impact voting. Third, many policies fail to achieve their intended effects, or are never evaluated at all. Lastly, education reforms usually take effect across an entire state simultaneously. This implies that, in most cases, there is not enough exogenous variation to isolate the impact of the educational reform on electoral outcomes.

In this paper we overcome these obstacles by studying the political effects of Act 10, a GOP-led reform enacted in 2011 in the U.S. state of Wisconsin. The law drastically cut the powers and resources of teachers' unions, granting districts substantial freedom to set teachers' pay. The reform was extremely controversial: even prior to its passage, intense protests erupted near the state capitol by teachers and other school stakeholders. Importantly, the reform's provisions could only be implemented after the expiration of each districts' pre-existing collective-bargaining agreements (CBAs). This generates quasi-random variation in the timing of implementation across districts.

Act 10 was controversial. Despite being presented as a budget-cutting bill, the law was widely perceived as a direct attack on teachers. Protests erupted across the state before, during, and after the law went into effect, culminating in a recall election against the governor held in June of 2012. Walker won the election, becoming the first incumbent to win a gubernatorial recall election in U.S. history. He then won reelection in the regular gubernatorial election of 2014. These two elections provide us with a clear measure of the electorate's support for the elected official most strongly

associated with the policy.

To identify Act 10's political effect, we exploit the staggered timing of implementation across districts in an event-study framework. We also implement a difference-in-differences framework, comparing how the change in electoral outcomes after 2011 differed across districts whose CBA expired in 2011 and after 2011.

We implement this strategy using data from various sources. Our primary outcomes rely on electoral data at the ward level, including the share of votes for each party's candidates in state elections between 2002 and 2014, obtained from the Wisconsin Legislative Technology Services Bureau. We geolocate wards inside school districts to link these outcomes to district CBA expiration dates, which we hand-collected from union contracts, school board minutes, and newspaper articles. As an alternative measure of electoral support, we use data on campaign contributions from the Database on Ideology, Money in Politics, and Elections (DIME). We further link these data to district-level information on students' demographics and test scores, teachers' demographics and salaries, and districts' turnover and retention rates, all constructed using records of the Wisconsin Department of Public Instruction. We also collect unions' financials (such as total revenues and dues collected) from their IRS Form 990 and information on recertification elections from the Wisconsin Employment Relations Commission.

We first show the effect of Act 10 on students and teachers. Students broadly benefited from it: the reform raised test scores significantly, especially for poorer students. Among teachers, however, the reform created clear sets of winners and losers. Act 10 gave districts substantial flexibility in setting teacher pay based on factors other than seniority and academic credentials. We show that this translated into a decline of the average teacher's salary and fringe benefits. However, it also flattened the relationship between salary and experience. As a result, it led younger and less credentialed teachers to earn more, and older, more experienced teachers to earn less than they would have prior to the reform. Previous work has also shown that Act 10 helped districts attract and retain high-quality teachers (Biasi, 2021).

We then present our main results: Act 10 increased the vote share for the incumbent governor. In districts exposed to the reform, GOP vote share in the 2012 recall election rose by about 2 percentage points (pp) three to four years after the CBA expiration relative to prior elections (or 4% of the control mean). This share remained similarly higher in the 2014 election. These effects rep-

resent a meaningful fraction of the margin of victory in each election (7 and 5 percentage points, respectively). Event-study estimates support these results and indicate that, two and four years after a CBA expiration, the share of GOP votes had increased by 4.0% and 6.1%. Reassuringly, event-study models show no pre-trends in outcomes in the years leading to an expiration. These electoral effects coincided with a decline in grassroots enthusiasm among Democrats as measured by campaign contributions: event-study estimates show that the law caused a sharp decline in the number of individual donations to Democratic candidates in the subsequent elections — particularly from teachers — and only a small increase in GOP donations. Consistent with the incumbent governor's efforts to claim credit for the reform, we find only small and insignificant electoral effects on presidential, House, or Senate elections. We do not find any effects on turnout.

These electoral gains were not equally distributed across districts; they were driven by places which stood to gain more from the reform. In particular, we find that the effects on GOP vote shares were significantly larger in districts with a larger share of young and less experienced teachers, who saw their salaries increase after Act 10. Yet, we do not find effects to be smaller in places with a larger share of older teachers, indicating that it is winners—rather than losers—that are driving our results. Similarly, we find that the reform won more votes in districts whose students stood to gain more from it: those with high rates of disadvantaged students (whose test scores rose most *ex post*) and teacher turnover (which was alleviated by pay flexibility). These teacher, student, and district characteristics are only mildly correlated with one another, indicating that these differences in effects capture distinct margins of response.

The law's effect on unions also appears to have helped drive its electoral effects. At its core, Act 10 directly targeted unions' capacity for collective bargaining and organization, and succeeded in diminishing their political power. Tax data indicate that union revenues per teacher fell by over 50%, with these declines concentrated among those with highest revenues *ex ante*. Unions' campaign contributions to the Democratic party also declined. This loss in power and political particpaion appears instrumental in shaping electoral results: Increases in GOP votes were largest in places where union revenues and campaign contributions were highest *ex ante*.

Taken together, our results indicate that controversial education reforms such as Act 10 can yield electoral benefits to the politicians and parties that promote them. These benefits appear strongly linked to the gains and losses produced by the reform on various stakeholders. Our findings also

support the hypothesis that unions play a pivotal role in shaping policy and politics (Anzia, 2011).

Our paper contributes to the nascent literature on the electoral effects of education policies. While the political obstacles to school reform are well-known among education economists, they are rarely treated as an object of scientific inquiry. A few existing studies examine the voting outcomes of education policies (Sandholtz, 2023; Dias and Ferraz, 2019; Litschig and Morrison, 2013; Harding, 2015). None of these, though, take place in a democracy as well-established as the U.S., where political communication in the media environment strongly shapes the policy discourse (DellaVigna and Kaplan, 2007; Gentzkow et al., 2011). An important exception is Cook et al. (2020), which finds that the entry of charter schools in Ohio reduced voter turnout in school board elections, though it did not affect candidates' vote shares.

Our study also informs the literature on changes to teacher compensation. Various studies have shown positive effects of teacher performance pay on teacher selection and student learning (Muralidharan and Sundararaman, 2011b, e.g.). However, these schemes can be politically contentious (Dee and Wyckoff, 2015). In practice, several implemented performance pay programs have provided bonuses for all teachers to avoid creating a set of losers (Leaver et al., 2021). Our work contributes to this literature by studying the political effect of a large-scale change in teacher compensation that resulted in lower pay for many teachers.

Lastly, we contribute to the the broader literature on the electoral effects of public policies. Previous studies have found that governments can win votes by building large, salient infrastructure projects, as well as by implementing direct redistributive transfers.[1] A related but distinct set of studies has looked at the political impact of fiscal policies, with mixed findings (Alesina et al., 1998; Brender and Drazen, 2008; Fetzer, 2019; Arias and Stasavage, 2019; Ardanaz et al., 2020). Here, we demonstrate that even budget-reducing public policies that create winners and losers can generate political support for incumbent politicians (and thus be politically viable) provided that the size of the winning constituency is large enough (Muralidharan and Sundararaman, 2011a; Bruns et al., 2019; Finger, 2018a).

[1]Studies on the positive electoral effects of infrastructure include Huet-Vaughn (2019); Voigtländer and Voth (2014); Butler and Boudot Reddy (2023); Leff Yaffe et al. (2023); Harding (2015); Garfias et al. (2021); work examining the electoral effect of redistributive transfers includes Manacorda et al. (2011); De La O (2013). See Hartmann and Sandholtz (2023) and Golden and Min (2013) for recent reviews.

2 Background: Wisconsin's Act 10

2.1 Wisconsin Public Schools and Teachers' Unions Before 2011

Public schools in Wisconsin enroll approximately 820,000 students each year and employ 60,000 full-time equivalent teachers. Its students are more socio-economically advantaged than the rest of the country: only 37% of them were entitled to a free or reduced-price lunch in 2011 (compared with a national average of 48%, U.S. Department of Education, 2011) and 76% were white (compared with 52% nationwide). Grade-4 students performed better in 2011 than the national average in both Math and Reading. Teachers' average salary equaled $57,395 in 2009-10, just below the national average of $61,804 (in 2016-17 US dollars, National Education Association, 2017).

In 1959, Wisconsin became the first state to grant public employees the right to collectively bargain, thanks to legislation promoted by Democratic Governor Gaylord Nelson (Stein and Marley, 2013). Since then, public-sector unions have enjoyed remarkable power and have been a key player in shaping the working conditions of employees in this sector.

Among the larger public-sector labor organizations were teacher unions, representing approximately 20% of all state and local employees in Wisconsin. The primary job of teachers' unions was to collectively bargain with each school district over teachers' pay and working conditions, such as safety, scheduling, and benefits. In particular, unions would negotiate over teachers' base pay and steps-and-lanes salary schedules, which specify the pay of each teacher as a function of seniority and academic credentials. This in turn ensured that teachers with similar seniority would be paid the same and that each teacher would enjoy a raise each year, leaving out the possibility of employer discretion in pay setting.

2.2 Act 10

The public-sector union landscape changed abruptly in February of 2011, when the Republican state legislature—led by Governor Scott Walker—passed the Wisconsin Budget Repair Bill, also known as Act 10. Initially proposed to close a projected $3.6 billion budget deficit, the reform dramatically changed the power and relevance of public-sector unions and the way that teacher pay and fringe benefits are decided.

First, the reform made it significantly harder for unions to operate, requiring them to hold re-certification elections every year and to obtain favorable votes from a majority of members of each bargaining unit. It also prohibited unions from automatically collecting dues from employees' pay-checks. As a result of this change, union membership had dropped by over 30% by 2015 (Schulz, 2023).

Second, the reform sought to cut school districts' costs by curbing the growth in teacher salaries and by requiring teachers to contribute more to their fringe benefits. In particular, the reform marked the end of collective bargaining over salary schedules. This implies that school districts became free to adjust teachers' pay at the level of the individual teacher, rather than setting it in strict accordance to the salary schedule. In addition, Act 10 capped the yearly growth in negotiated base pay to the rate of inflation. The reform also required teachers to pay half of their total pension contributions each year, equal to 5.8% of their pay (from an initial contribution rate of zero). It also mandated teachers to cover at least 12.5% of their healthcare premiums. Both these changes implied a reduction in take-home pay for all teachers (Biasi, 2021, 2024).

Importantly, though, the provisions of Act 10 provisions did not go into vigor immediately after its passage. Instead, the collective bargaining agreements (CBAs) districts had signed with unions prior to Act 10 remained valid until their expiration. This implies that the provisions of the law could only be applied after a CBA expiration. Due to historical reasons, expiration dates differed across Wisconsin districts. Out of 247 districts for which CBA expiration dates are known (see Section 3), a total of 198 had CBAs that expired in 2011, 20 in 2012, and 7 in 2013. Some districts extended the validity of their pre-existing CBAs by either one or two years; when considering extensions, 109 CBAs ended in 2011, 97 in 2012, 36 in 2013, 3 in 2014, and 2 in 2016 (Appendix Figure A1; Baron, 2018; Biasi, 2021; Biasi and Sarsons, 2022).

2.3 The Aftermath of Act 10: Protests and a Recall Election

Since its inception, Act 10 was a controversial piece of legislation. Protests began in Madison even before it was voted and quickly grew in size, with an estimated 100,000 participants through the month of February who occupied the State Capitol for weeks. Schools in Madison were forced to close due to the large numbers of teachers who called in sick to protest. Prior to its vote in the State Senate, a group of Senate Democrats left Wisconsin to stall the approval of the law. Even without

them, the Assembly managed to pass the bill on March 10, 2011, and the Governor signed it into law on the next day.

Efforts to revert the bill and stop Scott Walker's anti-union sentiment continued until 2012. Already in March 2011, a lawsuit was filed against the state regarding the bill, claiming unconstitutionality due to the presence of fiscal provisions. The State Supreme Court eventually upheld the bill in June 2011; protesters had continued demonstrating outside the State Capitol until that point. The attempts to overturn Act 10 culminated with a gubernatorial recall election in June 2012, which saw Scott Walker oppose Democrat Tom Barrett, who had already run for governor in 2010. Walker defeated Barrett by 7 pp, becoming the first governor in U.S. history to win a recall election.

3 Data

Our empirical analysis draws data from multiple sources. We describe here each set of variables, along with their sources.

3.1 Electoral Results

Our primary outcome are vote shares for the incumbent Republican governor between 2002 and 2014 in each locality. Our main analysis focuses on gubernatorial elections; we examine vote shares for the GOP party in Presidential, House, and Senate elections in robustness checks. We draw information on votes from the Wisconsin Legislative Technology Services Bureau (2011) (WLTSB), hosted by the University of Wisconsin. We measure vote share at the level of the ward (a voting district analogous to election districts or precincts in other states). Wisconsin had 6,634 wards in 2011. Ward boundaries are drawn by municipal governments and typically change after each decennial census. Since our differences-in-differences and event-study analyses compare election results over time and across Census decades, we use electoral data provided by the WLTSB with outcomes harmonized at the level of 2011 wards.

3.2 Collective Bargaining Agreements

We classify districts according to their exposure to the consequences of Act 10 as of 2012 with a novel dataset (also used in Biasi, 2021; Biasi and Sarsons, 2022) that contains the expiration of each

district's CBA pre-dating Act 10 and any extension to CBA validity. These data, described in greater detail in Biasi and Sarsons (2022), draw information from multiple sources, including union contracts, districts' employee handbooks, school board meetings minutes, and local news sources.[2] When available, we prioritize information from union contracts, school board minutes, and handbooks. In cases where these documents are unavailable, we complement with information from online local news sources.

The database of CBA expirations covers 70% of all enrolled students (in 247 of the 428 districts in the state). Most school districts in the state are "unified," i.e., oversee education at both the primary and secondary level. A few are elementary or secondary districts, which overlap with one another geographically. To avoid ambiguity in treatment status for districts with jurisdiction over different levels of schooling in the same geographic area, we limit attention to unified school districts. Our final dataset thus covers 236 unified school districts with CBA information, containing 4,989 wards. Figure A2 shows the location of districts with known expiration dates.

3.3 Teacher Records

We supplement vote shares and CBA expirations with information on the characteristics of teachers in each school and ward. These data are from the *PI-1202 Fall Staff Report - All Staff Files* for the years 2006-2017, maintained by the Wisconsin Department of Public Instruction (WDPI). The files list all individuals employed by the WDPI in each year; for each person, they list their full name, birth year, the identifier of the hiring agency (typically a school district) and working agency (usually a school), the job position (e.g., teacher), the full-time equivalency (FTE) units, total gross salary, highest education degree, and years of teaching experience. We focus on regular teachers and exclude long- and short-term substitutes and subcontractors. We use the available unique teacher identifier to calculate teacher turnover at the school and district level. For comparability, we express salaries in FTE units by dividing them by the FTE worked by each teacher.

[2]Union contracts typically directly report the date of the expiration of the agreement. Post-Act 10 school board minutes also mention whether a contract was set to expire in 2011, as the board would have had to make a number of decisions in that instance. The presence of an early version of district employee handbooks is also useful to establish when the post-CBA pay regime was introduced (which typically coincides with the date of the earliest handbook at the latest).

3.4 Student Records

We link CBA information to records from the state longitudinal student system, containing individual-level student data on demographic characteristics (gender and race/ethnicity), school and grade attended in each year, an indicator for eligibility for a free or reduced-price lunch (which we use as a measure of socio-economic status), and test score results in Math on the state standardized test.[3] This information is available for the years 2006-2016.

3.5 Census Data

We draw information on each ward's age distribution in the population from decennial Census data. We use information reported at the block level and aggregate to the ward level, by mapping each census block to the ward within which its centroid falls.

3.6 Campaign Contributions

We draw information on geo-coded campaign contributions from the Database on Ideology, Money in Politics, and Elections (DIME, Bonica, 2023). For each campaign contribution, this dataset lists the recipient as well as the contributor's name, address, and occupation. We limit our analysis to contributions to Wisconsin gubernatorial races that originate inside Wisconsin. Using geolocated addresses, we are able to map campaign contributions to the school districts in which they originated. Names and occupations also allow us to identify contributions from teachers and teacher unions. Specifically, we compute total contributions and those made by teachers (using information on contributors' occupation) and by teachers' unions (searching for unions' names from the Wisconsin Employment Relation Commission, or WERC, among contributor names via fuzzy matching).

3.7 Union Finances

We extract measures of union finances from tax records. The Internal Revenue Service of the United States requires most tax-exempt organizations, including labor unions, to fill Form 990 (officially, the "Return of Organization Exempt From Income Tax"). The form reports revenues, expenses, assets,

[3] Wisconsin schools used the Wisconsin Knowledge and Concepts Examination (WKCE) for the years 2007–2014 and the Badger test for the years 2015–2016. The WKCE was administered in November of each school year, whereas the Badger test was administered in March. To account for this change, for the years 2007–2014 we assign each student a score equal to the average of the standardized scores for the current and the following year.

and liabilities. We accessed the database of digitized Form 990s made available by the National Center for Charitable Statistics (NCCS) of the Urban Institute (2007-2016) and searched Wisconsin union names as they appear in WERC. We were able to link 99 districts to the records of 52 unions. We use revenues per member, defined as total revenues (primarily coming from membership dues and reported on the forms) divided by the total number of teachers in each union's represented districts as a measure of unions' strength prior to the passage of Act 10.

3.8 Union Recertification Elections

We collect information on union recertification election dates and outcomes from the records of WERC. These records are available starting from 2011, when the first post-Act 10 recertification elections took place, and include elections called by teacher unions and unions of other local employees. For each election wave, these records report the name of the bargaining unit, the name of the school district or other local government, the number of union members eligible to vote, and the number of favorable and opposing votes to the recertification. We calculate the vote share in favor of the recertification as the ratio between the number of favorable votes and the number of members of the bargaining unit.

3.9 Summary Statistics

Table 1 shows summary statistics of the main variables in our sample. The sample contains 134 districts whose CBAs had expired in 2011 and had not been extended and 102 districts whose CBAs were still valid in 2012. Column 1 contains all districts included in our analysis; columns 2 and 3 split the sample by whether districts' CBAs or their extensions had expired as of 2012.

On average, treated and control districts and wards are comparable in terms of population age (in particular the share of people aged 18 and below) and teacher characteristics. Treated districts have a smaller share of low-SES students and lower Math test scores. They have a larger share of GOP votes in the 2008 Presidential election and the 2010 Gubernatorial election, a larger number of GOP gubernatorial campaign donations, and a smaller number of donations to the Democratic Party. Naturally, they are also more likely to have held a union recertification election in 2011. Unions were less strong in treated districts, with revenues per member equal to $483 in 2011 compared with $842 in control districts.

4 Estimating The Impacts of Act 10

Our goal is to estimate the impact of the provisions of Act 10 on the political support for Governor Scott Walker, the main promoter of the reform. Walker was elected in 2010 and introduced the legislation that would become Act 10 in February of 2011. Our empirical strategy sets off from the fact that, by June 2012 (when Wisconsin voters were called back to vote in a gubernatorial recall election), the CBAs pre-dating Act 10 had expired in 134 districts and 3,242 wards but were still active (or had been extended) in 102 districts and 1,747 wards. Hence, in the latter group, the provisions of Act 10 had not yet gone into effect by the time people voted. For 5 of these districts (202 wards), the pre-Act-10 CBAs were still active by 2014, when Walker ran for reelection – providing yet another opportunity to measure voters' reactions to experiencing the law.

Event study Our primary identification strategy is an event study, which uses the staggered expiration of CBAs across districts to identify the impact of the reform. We implement it with the following model:

$$V_{jt} = \sum_{k=-6}^{4} \beta_k \mathbb{1}(t - E_{d(j)} = k) \; + \theta_j + \tau_t + \varepsilon_{jk} \tag{1}$$

where V_{jk} is the share of votes to the GOP governor in ward j in year t and E_d is the year of expiration of district d's CBA or extension. The vectors θ_j and τ_t contain ward and year fixed effects, respectively. Normalizing $\beta_0 = 0$, the parameters β_k estimate the average GOP vote share k years since an expiration, relative to the year of the expiration. We use similar models (with different sets of fixed effects) when estimating the impact of the reform on outcomes measured at the teacher, student, or district level. We describe these later in more detail. Since the treatment (i.e., the year of CBA expiration) is assigned at the level of the school district, we cluster standard errors at the district level.

Because Act 10 applied to all districts eventually, we do not have a group of "never treated" districts. The model in equation (1) is thus identified by comparing early-treated and late-treated districts. This strategy may yield biased estimates if treatment effects are correlated with the timing of the treatment (Sun and Abraham, 2021; Callaway and Sant'Anna, 2021; Borusyak et al., 2024). To probe the robustness of our estimates to this possibility, we implement the Extended Two-Way Fixed Effects approach of Wooldridge (2021): We modify equation (1) by allowing the parameters

β_k to be cohort-specific and then average them across cohorts. We present these estimates together with our main results in Section 6.

Difference in differences: We supplement our event studies with a simpler difference-in-differences strategy, which compares the share of votes for the GOP governor over time between wards in districts whose CBAs had expired in 2011 and had not been extended (the "treated" group) and wards in districts where the CBAs were still active by 2012 (the control group). We implement this strategy by estimating the following model via OLS on data up to 2012:

$$V_{jt} = \beta D_{d(j)t} + \theta_j + \tau_t + \varepsilon_{jt} \tag{2}$$

where $D_{dt} \equiv \mathbb{1}(t > E_d)$, all other variables are as before, and we consider elections in 2002, 2006, 2010, and 2012. The variable D_{dt} equals one if district d's CBA expired in 2011 and was not extended. The parameter of interest is β, which captures the change in votes for the GOP governor in treated districts—which had already been exposed to the consequences of Act 10 by 2012—relative to the control group, in 2012 relative to prior years.

We also estimate a dynamic version of equation (2):

$$V_{jt} = \sum_{\substack{t \in \{2002, 2006, \\ 2012, 2014\}}} \beta_s D_{d(j)s} + \theta_j + \tau_t + \varepsilon_{jt} \tag{3}$$

Normalizing β_{2010} to zero, the parameters β_s estimate the difference in GOP gubernatorial votes in districts with a CBA that had expired in 2011 relative to the control group, and relative to the difference between the two groups in 2010.

Identifying assumptions Our empirical strategy rests on the assumption that the political outcome of the GOP party in districts with CBAs that expired at different points in time would have evolved in similar ways had Act 10 not occurred. This assumption is primarily supported by the idiosyncratic nature of CBAs' expiration dates. In addition, we cannot detect any meaningful observable differences between districts with different expiration dates (Table 1).

Our analysis makes use of the timing of the CBA extensions, motivated by existing evidence showing Act 10 was only implemented after that point in each district (Biasi and Sarsons, 2022). One

potential concern with this choice is that districts may have extended the validity of the agreements in a way that is correlated with trends in the political views of their residents. Appendix Table A1 compares districts with and without a CBA extension on the basis of observable characteristics. The two groups are comparable in terms of teachers, students, and demographic characteristics. Districts with an extension have a higher share of FRPL students and showed less support for the GOP in pre-Act 10 elections (as measured by the vote share and campaign donations to each party). They also had stronger unions pre-reform as measured by revenues per teacher prior to 2011.

Despite these differences in levels, we show later that the two groups of districts had similar trends in all our outcome variables in the years leading to each expiration. We also replicate our main results ignoring the extension and classifying districts into treatment groups only according to the original CBA expiration dates. Our results are qualitatively similar (see Table 6, column 1, for the pooled event study and Appendix Table A3, column 1, for the difference-in-differences).

5 Winners and Losers of Act 10: Effects on Teachers and Students

To provide necessary context for understanding the political effects of Act 10, we begin by demonstrating the law's effects on the state's school system, focusing on teachers and students.

5.1 Effects on Teacher Compensation

Act 10 dramatically changed the way teachers pay is set by prohibiting teacher unions from engaging in collective bargaining over teacher salary schedules with each school district. In other words, the law gave districts full discretion over teacher pay. As the pre-reform salary schedule rewarded teachers for their seniority and academic credential, the law made it possible for some teachers with high experience and credentials to see their salaries grow at a slower rate or even decline, to the benefit of some of the less experienced and credentialed teachers.

We confirm this empirically by estimating equation 1 using individual teacher salaries and benefits as the outcome variable. Estimates of β_k indicate that teacher salaries fell significantly by approximately USD 1000 in the first year after the CBA's expiration (Appendix Figure A3, circle series). Teachers' fringe benefits fell even more, by around USD 2000 (Appendix Figure A3, panel (b), squares series).

While average compensation fell, it did not fall uniformly across teachers. To better characterize the winners and losers from Act 10 in terms of teacher salaries, in panel (a) of Figure 1 we plot teachers' average log salaries by age (left) and years of experience (right), separately for the years before and after each district's CBA expiration and controlling for year and district fixed effects. Prior to the reform, the salary-age profile was quite steep until age 45 and then flattened out; for example, 57-year-olds earned approximately 88% more than 24-year-olds. Similarly, the salary profile increased steeply with experience for the first 15 years of tenure. For example, teachers with 30 years of experience earned approximately 96% more than teachers with no experience.

Act 10 flattened these age and experience profiles, penalizing teachers over 57 or with 27 or more years of experience and benefiting teachers younger than 30. For example, after a CBA expiration 57-year-old earned 1% less than they did prior to the reform, and only 73% more than teachers aged 24. Similarly, teachers with 30 years of experience earned 8% less than they did before the reform, and only 61% less than teachers with 3 years of experience.[4]

These results illustrate how Act 10 created winners and losers (in terms of salaries) across teachers with different ages and experiences. In Section 7, we link the presence of winners and losers across districts in Wisconsin to the political consequences of the reform.

5.2 Effects on Student Test Scores

The changes in salaries introduced by Act 10 deeply affected the market for public-school teachers within the state, changing the pool of teachers working with students at each school (Biasi, 2021; Biasi et al., 2021). We now examine whether the reform had direct consequences for student learning, as measured by standardized student test scores in Math, by estimating a version of equation (1) with individual-level scores of grade 3-8 students as the outcome variable. We control for school and grade-by-year fixed effect and individual characteristics, such as gender, race and ethnicity, socio-economic status (proxied by an indicator for FRPL eligibility), English-learner status, and the presence of a disability.

We find that the reform significantly raised test scores over time. Scores grew from a 0.05 standard deviations (sd) higher level two years after a CBA expiration (relative to the expiration year)

[4] Appendix Figure A4 shows the cross-district distribution of the post-CBA average change in salary for teachers aged 63 and older and for those aged 27 and below. In 155 districts (63%) them the salary change for young teachers is larger than for older teachers, and for 130 (53%) the change in salaries for older teachers is negative.

to a 0.17 sd higher level five years later (Figure 1, panel (b), solid line). They increased significantly more for FRPL-eligible students, by 0.21 sd five years after an expiration compared with 0.10 sd for non-FRPL students (Figure 1, panel (b), dashed lines). These results are in line with Biasi (2021), who finds similar but smaller results using school-level average test scores. They are in partial contrast with those of Baron (2018), who finds negative short-run (i.e., 2-year) effects on aggregate high-school test scores in the short-run. Our analysis differs from Baron's in the population of students we examine (elementary and middle school) and in a longer time horizon of five years after a CBA expiration.

In principle, changes in students' learning environments may have lead families to sort across districts according to their preferences (à la Tiebout, 1956). We find no impact of Act 10 on aggregate enrollment and a small decline in the enrollment shares of FRPL and minority students (Appendix Figure A5).

6 The Political Effects of Wisconsin's Act 10

We now examine the political effects of Act 10. We focus on vote shares in gubernatorial elections and campaign donations to each party in these elections. We also probe the robustness of our estimates to different empirical models and assumptions.

6.1 Effects on The Share of Votes to the GOP Governor

Event study Exposure to Act 10 significantly increased the share of votes to the GOP in gubernatorial elections. This is evident in Figure 2 (circle series), which shows estimates of β_k in equation (1) with ward-level GOP vote shares as the dependent variable. The effect appears immediately after a CBA expiration and grows over time, reaching a 3 pp higher level 3-4 years after an expiration. This change corresponds to a 5.7% increase in the GOP vote share relative to a pre-Act 10 mean of 51.2%. It also corresponds to 43% of the winning margin in the 2012 election (equal to 6.8%, with a 53.08% vote share for Walker and a 46.28% share for his Democratic opponent Barrett). Reassuringly, estimates of β_k are close to zero and insignificant for $k < 0$, indicating the absence of differential pre-trends across wards who became exposed to the consequences of the reform at different times. These effects are summarized in Table 2, which shows averages of β_k for $k > 0$.

Effects are robust to the inclusion of either district (column 1) or ward fixed effects (column 2).

As previously mentioned, we do not have any "never-treated" districts (or wards) in our data because all districts eventually experienced the reform (that is, we can assign a E_d to all districts in our sample). This could bias two-way fixed effects estimates (like the ones we just presented) when the treatment is staggered and treatment effects are (i) heterogeneous across cohorts and (ii) correlated with treatment timing. To probe the robustness of our estimates to this possibility, we recognize that, within our time period of study (2002-2014), districts with CBAs expiring in 2014 and 2016 are *de facto* never treated. Then, we re-estimate equation (1) by setting all time-to-treatment indicators to zero for these cohorts, using both a two-way fixed effects model and the model proposed by Sun and Abraham (2021), robust to heterogeneity in treatment effects. The corresponding estimates, shown in Figure 2 as the triangles and hollow circles, respectively, are very similar to our baseline estimates and only slightly attenuated, with a GOP vote share change of 2.4 pp 3-4 years after an expiration.

Difference-in-differences As a second empirical strategy, we estimate difference-in-differences models.[5] Results from this model support the event studies. Estimates of the parameter β in equation (2), obtained including the 2012 recall election as the only post-reform election and controlling for ward fixed effects, indicate that wards in districts who had been exposed to the law by that time had a 2.0 pp (4%) higher GOP vote share compared with not-yet-treated districts (Table 3, column 2, significant at the 5% level). Estimates remain similar if we also include data from the 2014 gubernatorial election (columns 3 and 4), and they become much larger (at 5.3 pp) when we limit the control group to wards in districts whose agreements expired in either 2014 or 2016.

To examine pre-trends and the dynamics of the political effects of Act 10, Appendix Figure A7 shows estimates of the parameters β_s in equation (3). These estimates indicate that the share of votes to the GOP governor was on parallel trends in treated and control districts between 2002 and 2008 and it increased (although insignificantly) between 2008 and 2010. The share increased further in the treated districts in 2012 relative to the control group, by approximately 1.3 pp. This share remained at a significantly higher level (albeit smaller than 2012) in the 2014 election.

[5] Appendix Figure A6 shows the distribution of the GOP vote share in 2010 (panel (a)) and the change in this share between 2010 and 2012 (panel (b)), separately for treated districts (i.e., those with CBAs or extensions expiring in 2011) and controls.

6.2 Effects on Turnout

A possible mechanism behind the increase in GOP votes after Act 10 is a decrease in get-out-the-vote activities by teacher unions. These organizations are often seen as crucial vectors of voter mobilization, not just in the US but around the world (Flavin and Hartney, 2015; Larreguy et al., 2017). In the US, teacher unions tend to have a strong attachment to the Democratic party (Finger, 2018a). Since unions lost power after Act 10, they may have been less active in convincing people to show up to vote for the Democratic party. If this is the case, we should observe a reduction in turnout rates following Act 10.

We test for this mechanism using a measure of voter turnout as the dependent variable in our event study models, defined as the number of votes cast divided by the population over 18 in each ward. Estimates of β_k in equation (1), summarized in Table 4, show positive but insignificant effects of the reform on turnout, with confidence intervals that allow us to rule out impacts larger than 5 pp. While we cannot test for changes in the composition of voters, these estimates do not support the hypothesis of mobilization campaigns as an important channel behind the electoral effects of Act 10. Instead our results are consistent with previous work showing that campaign activity can change vote shares even without overall effects on turnout (Spenkuch and Toniatti, 2018).

6.3 Effects on campaign contributions

Next, we examine the impact of the law on campaign contributions made by donors. Other than representing an additional measure of political support, contributions allow us to separately identify changes in support to each party over time. Figure 3 (panel (a)) and Table 5 (columns 1-2) show event-study estimates of the effect of exposure to Act 10 on the total number of contributions to each party per person, measured at the district level and focusing on gubernatorial races.[6] The reform led to 33.1 fewer contributions to the Democratic party per 1,000 people 3 to 4 years after exposure (a ten-fold decline given an average contribution rate of 3.2 contributions per 1,000 people) and 4.6 additional contributions per 1,000 people to the GOP (a 33% increase). These results confirm an increase in relative support to the GOP party after Act 10; they also indicate that this change was largely driven by a decline in support for the Democratic party.

[6]To account for the different size of school districts across the state, in these specifications we weight observations by the number of people in each district.

Focusing on political contributions made by teachers, we find even larger negative impacts on contributions to the Democratic party, which fell by 50.8 contributions per 1,000 teachers in the district (a nearly 8-fold increase, Figure 3, panel (b), and Table 5, columns 3-4). Given that teachers were one of the groups most directly targeted by Act 10, this finding supports the hypothesis that the electoral changes we measure are in large part driven by a reaction to the law.

6.4 Spillovers onto Presidential, State, and House Elections

So far, we have focused on the impact of Act 10 on votes in gubernatorial races. Act 10 was Scott Walker's signature legislation, which he stridently defended and which defined his career (DaBruzzi, 2021). This likely made it difficult for other elected officials to claim credit for the law.

Yet, it is possible that the reform may have had spillover on other political races, such as presidential, house, or senate elections. We test this formally using the same event-study specification outlined in equation (1), using GOP vote shares in other races as the outcomes. We do not find the same positive effects we saw for gubernatorial races. If anything, estimates of β_k for $k > 0$ are negative, although imprecise (Appendix Figure A8 and Table A2). The political effects of Act 10 were therefore confined to gubernatorial races.

6.5 Robustness

We probe the robustness of our results to a set of choices we made in our empirical analysis.

Ignoring CBA Extensions Our main specifications consider districts to be exposed to Act 10 after the expiration of their pre-existing CBAs *or any extensions districts may have granted*. This choice is motivated by the fact that the effects of Act 10 on teachers (and therefore students) could only materialize after this point (Biasi and Sarsons, 2022). Yet, extensions are decided by the school districts, and one could worry that they are endogenous. To test whether our results are sensitive to this choice, we re-estimate our main models considering districts as exposed only after the expiration of the CBA, ignoring extensions. Our results remain unchanged (see Table 6, column 1, for the pooled event study and Appendix Table A3, column 1, for the difference-in-differences).

Excluding Milwaukee Estimates also remain robust when we exclude Milwaukee, the largest district in the state and an outlier in terms of enrollment (see column 2 of Table 6 for the pooled

event study and column 2 of Appendix Table A3 for the difference-in-differences).

Focusing on wards fully aligned with districts In our main specification, we assign each ward to a district (and hence to a treatment status) based on the district in which its centroid falls. Wards that span district boundaries would then be assigned a treatment status that does not correspond precisely to what all the voters living in the ward experienced. This could add measurement error to our explanatory variable of interest, biasing its estimate towards zero. To assess this possibility, in column 3 of Table 6 and column 3 of Appendix Table A3 we re-estimate our main event-study and difference-in-differences models on the subsample of 2,938 wards (56% of the total) that are completely circumscribed within a district. Estimates are largely unchanged.

7 Drivers of The Political Effects of Act 10: Winners and Losers

The impacts of Act 10 on Wisconsin's education system were dramatic, but uneven across districts (Biasi, 2021). The political effects of the reform also differed across districts and wards (Appendix Figure A6). In an attempt to understand the drivers of these political effects, we now investigate whether the electoral consequences of the law were related to how it impacted teachers, students, and school districts more generally.

7.1 Teachers: Salary Gains and GOP Support

Among all public-sector employees, Act 10 predominantly impacted teachers. Teachers are also a large potential voting bloc: In our data, they constitute about 2% of voters in the average school district. Yet, the consequences of the reform were not the same for all teachers. As shown in Section 5, older and more experienced teachers (who were rewarded by a seniority-based salary schedule pre-reform) experienced a decline in their salaries. Younger and less experienced teachers (who used to be penalized by such schedule) experienced a salary increase instead.

To test whether the electoral gains experienced by the GOP are related to the gains and losses experienced by teachers, we now test for differences in electoral effects across districts with different shares of teachers who are winners and losers from the reform in terms of salaries. To do this, we calculate the shares of teachers who (i) have 3 or fewer years of experience (who on average gained from the reform, Figure 1, panel (a)) and (i) have 21 or more years of experience (who on average

lost from the reform). To avoid confounding our estimates with endogenous movements of teachers across districts following Act 10, we calculate these shares using district data from 2010-2011, i.e., immediately preceding the passage of Act 10.

Table 7 (panel (a)) shows pooled estimates of equation (1) for districts in different quartiles of the distribution of low-experienced teachers. In districts in the top quartile, exposure to Act 10 increased the share of votes to the GOP by 1.9 pp (3.8%, column 3, significant at 1%). This is a much larger increase compared with that experienced by districts in the second and third quartiles (0.9 pp, column 5, or 1.8%, significant at 10%) and those in the first quartile (with an estimate very close to zero and insignificant, column 6). Estimates of the full set of parameters β_k in equation (1), shown in Figure 4 for districts in the top and bottom quartiles, confirm these results and indicate that districts in both groups were on similar trends leading to a CBA expiration, supporting our identification assumptions. Results remain robust if we split the sample according to each district's share of young and old teachers, defined as those aged 27 and younger and 55 and older, respectively (Appendix Table A4).

These results indicate that the impact of Act 10 was larger in districts with a larger share of teachers who were more likely to be winners from the reform in terms of salaries. Importantly, though, we do not register any electoral losses in districts with few low-experience teachers.

Next, we test for differences across districts with a different share of losers from the reform, i.e., teachers with high experience. Pooled estimates of equation (1), shown in panel (b) of Table 7, do not show large differences across districts with a share of high-experience teachers in each of the four quartiles. For example, on average GOP vote shares increased by 1.2 pp in districts with a share in the bottom quartile (column 3), and 1.3 pp in districts with a share in the top quartile (significant at 5%, column 6). Results are similar if we split the sample according to the share of teachers aged 55 or older (Appendix Table A4).

Taken together, these results indicate that the share of teachers who gained from Act 10 was an important driver of the political gains obtained by the GOP governor in the 2012 and 2014 elections. Importantly, the share of teachers who lost seems less important in determining these political gains, indicating that these teachers did not punish the governor at the ballot box.

7.2 Students: Share of FRPL, Test Scores, and GOP Support

Share of FRPL students Through changes in teachers' working conditions, Act 10 also affected students (Baron, 2018; Biasi, 2021).[7] In Section 5, we showed that the implementation of the law raised test scores, primarily for disadvantaged students (i.e., those eligible for a FRPL). The share of FRPL districts varies across Wisconsin districts, with a mean of 0.37, a median of 0.36, and a standard deviation of 0.14 in 2010, and districts that enrolled a higher share of FRPL students experienced larger gains on average. If parents were able to anticipate the effects of the reform on their children, the electoral gains obtained by the GOP after Act 10 across districts could have been related to future test score gains.

To test this hypothesis, in panel (a) of Table 8 we re-estimate our pooled event studies separately for districts in different quartiles of the state distribution of the share of FRPL students.Our findings confirm this hypothesis. Averages of β_k for $k > 0$ are significantly larger for districts in the top quartile, and equal to 2.5 pp (or 5.2% of the mean, column 6, significant at 1%). Instead, they are smaller and insignificant for districts in the three lowest quartiles (columns 4 and 5). Figure A9 shows the corresponding estimates of β_k in equation (1), obtained separately for districts in the top and bottom quartiles, and it confirms the findings of larger effects in districts in the top quartile.

Test scores In panel (b) of Table 8, we also test whether electoral gains were related to ex ante test scores. Exposure to Act 10 led to a 2.3 pp (4.8%) increase in the share of votes to the GOP in districts in the bottom quartile of the average test score distribution (column 4, significant at 1%). Instead, it had no effect in districts in the other three quartiles. These findings confirm a relationship between expected gains for students and the electoral gains enjoyed by the GOP after Act 10.

7.3 Districts: Teacher Scarcity and GOP Support

One of the ways in which Act 10 impacted school districts, schools, and students is by allowing districts to attract teachers with higher pay, opening up a "market" for public-school teachers (Umhoefer and Hauer, 2016; Biasi, 2021; Biasi et al., 2021). This may have especially helped districts that had been contending with high turnover or a tighter market (with a high number of vacancies relative

[7] While Baron (2018) found that the reform reduced student test scores on average, Biasi (2021) finds instead positive effects on some districts, primarily due to movements of teachers across districts along observable characteristics (such as experience and teaching quality) and exit of low-quality teachers from the profession.

to unemployed teachers) prior to the reform.

In Table 9, we test whether these districts experienced larger increases in GOP vote shares after they became exposed to Act 10. Panel (a) shows pooled event-study estimates by quartiles of the share of teacher turnover in 2010-11 in each district, defined as the share of teachers who leave the district in those years. The effects of Act 10 appear significantly more pronounced in districts in the top quartile, with an average estimate of β_k for $k > 0$ equal to 1.4 pp (3%, column 6, significant at 1%). Effects are also statistically significant for districts in the second and third quartiles, but smaller at 1.1 pp (column 5, significant at 10%) and indistinguishable from zero for districts in the first quartile (column 4). Figure 5, which shows estimates of β_k in equation (1) for districts in the top and bottom quartiles, confirms that the GOP vote share was flat in both groups of districts prior to a CBA expiration; it then rose in districts in the top quartile and declined in those in the bottom quartile.

Panel (b) of Table 9 replicates panel (a) but groups districts into quartiles of a measure of teacher labor market tightness, defined as the number of teachers who exited the district in 2010 and those who joined. We find similar results as in panel (a), with districts in the top quartile experiencing a 1.5 pp (2.9%) increase in the share of GOP votes after they became exposed to Act 10 (column 6, significant at 5%); and districts in the three lowest quartiles experiencing only an insignificant increase (columns 4 and 5).

Taken together, these results confirm that the electoral effects of Act 10 were related to the size of the potential benefits experienced by the school districts in terms of teacher retention. They also provide additional evidence in line with the hypothesis that the impact of the law was driven by the size of the groups of winners and losers in each district.

It should be pointed out that, while characteristics such as the share of FRPL, test scores, the share of low-experience teachers, and teacher turnover are positively correlated with one another across districts, their correlations are rather low (with a maximum of 0.40 between turnover rates and the share of low-experienced teachers; Appendix Figure A10). These characteristics are thus capturing distinct dimensions along which Act 10 impacted the electorate across districts.

8 The Role of Teacher Unions

Act 10 was, at its very core, a reform of public-sector unions, and particularly teacher unions. These unions have been shown to play a fundamental role in policy and politics. Union strength, for example, negatively predicts the enactment of teacher performance pay policies (Finger, 2018b; Hartney and Flavin, 2011). Collective bargaining laws have been shown to increase teacher political activity and benefits, sometimes to the detriment of disadvantaged schools (Flavin and Hartney, 2015; Anzia and Moe, 2015, 2014).

In this section, we first illustrate the impact of the law on various measures of union strength and political participation. Then, we link these measures to the electoral effects of Act 10 across districts, to better understand the role of unions in shaping the political impact of the reform.

8.1 Union Revenues After Act 10

Act 10 deprived unions of their powers and made it significantly harder for them to operate. It prohibited them from collecting membership dues automatically, and it mandated that recertification elections would require a majority of all members (not just those present at voting). Using data from the Current Population Survey, Baron (2018) documents that the passage of the law was followed by a decline in union membership in the public sector.

To further demonstrate how Act 10 impacted unions and whether this played a role in the law's electoral success for Republicans, we use unions' financial records from tax data as measures of union strength. These data show a dramatic decline in union revenues following the implementation of the reform. Estimates of the parameters β_k in equation (1) with $k \in [-4, 4]$, obtained using the natural logarithm of each district union's revenues per member in each district's union each year as the dependent variable, imply that revenues had fallen by 55% 2-3 years after a CBA expiration (with an estimate of -0.8 two years post expiration and $\exp(-0.8) - 1 = -0.55$, significant at 1%, Figure 6, solid line).

Crucially, not all unions were affected in the same way. The negative revenue effect was almost entirely driven by unions with pre-Act 10 revenues above the state median (Figure 6, dashed lines).

In addition to making it harder for unions to collect their revenues, Act 10 introduced the requirement to recertify yearly by collecting a an absolute majority of favorable votes from all union

members in local union elections. Data from WERC indicate that 60 of 102 unions (59%) whose agreements were set to expire by 2011 sought recertification in the same year, including 69% of those with ex ante revenues above the state mean and 90% of those with revenues below the median. 95% of all unions that sought recertification obtained it.

The loss in power experienced by teacher unions impacted their participation in politics. Figure 7 and column 5 of Table 5 show event study estimates of equation (1), obtained using the number of contribution to gubernatorial races made by each district's unions. Virtually all union contributions (0.002 per 1,000 people in the district) were to the Democratic party, both before and after Act 10. Exposure to the reform dramatically lowered these contributions, by 0.02 per 1,000 people three to four years after a CBA expiration (an 8.5-times decrease). It is possible that a lower level of political activity by teacher unions in early-treated districts may have bolstered support for the GOP. We test this hypothesis next.

8.2 Ex Ante Union Power, Political Participation, and The Electoral Effects of Act 10

To evaluate the impact of the decline in union powers on the political effects of the reform, we split our sample of districts into those that had ex ante stronger unions – defined as those with revenues per member in 2011 above the state median, which experienced the greatest decline in revenues post-reform – and those with weaker unions.

To this end, panel (a) of Figure 8 and panel (a) of Table 10 show estimates of the parameters β_k estimated separately for wards in districts belonging to these two groups. Among districts with non-missing union information, the electoral effects of Act 10 are entirely driven by districts with ex ante stronger unions, which experienced a 2.3 pp (6%) increase in the GOP vote share on average after a CBA expiration (circle series in Figure 8 and column 2 in Table 10, significant at 5%). The same estimate is equal to an insignificant -0.1 pp for weaker unions (column 4). Because ex ante stronger unions were more likely to seek recertification, we also find that the effects of Act 10 were strongest in wards located in districts with unions that recertified in 2011 (Appendix Figure A11).

These findings suggest that the electoral gains experienced by the GOP may have been driven by a decline in union powers, larger in places with ex ante stronger and wealthier unions. To further understand whether these effects are also related to the political role played by unions prior to Act 10, in panel (b) of Table 10 and panel (b) of Figure 8 we test whether the electoral gains also differ

among wards in districts with unions that contributed to gubernatorial races prior to Act 10, and all other unions. We find that the increase in GOP vote share after the reform is largely driven by districts whose unions contributed to political campaigns prior to 2011.

Taken together, these results indicate that teacher unions, and their loss in powers and political participation, played a fundamental role in establishing the political impacts of Act 10. More generally, our findings point to unions as important institutions for both the design of educational policies and their political success.

9 Discussion and Conclusion

This paper studies the political effects of controversial education reforms. We use the passage of Act 10 in Wisconsin as a natural experiment, a law which dramatically changed the rules of collective bargaining in the public sector. The law revolutionized the way teachers are paid, gave districts new tools to attract and retain teachers, and ended up impacting students as well. It was also politically controversial, and its passage was followed by intense protests across the state. We quantify the political impacts of the reform on the GOP governor that proposed it by exploiting the staggered timing of the reform's implementation across districts, due to differences in the expiration dates of existing collective bargaining agreements (CBAs). Specifically, we compare the share of voters to the GOP governor in the 2012 recall election and in the 2014 regular election across districts that had been differentially exposed to the changes introduced by the law, due to the differential timing of expiration of their existing CBAs. We find evidence of parallel pre-trends: districts which exogenously experienced the policy change at different points in time voted similarly in all gubernatorial elections in the decade prior to their CBA expirations.

We find that early-treated districts were 2 percentage points (or 4%) more likely to vote for the Republican governor in the two elections directly following the reform. These electoral effects were concentrated in places that stood to gain more from the effects of the reform. These include districts and wards with a larger share of younger teachers (who saw their salaries increase more after Act 10); districts and wards with lower baseline test scores (which had larger potential gains to be achieved); and districts and wards with higher baseline teacher turnover (which had larger potential gains in terms of teacher retention).

Given the relevant role played by teacher unions in participating to local politics and shaping policy-making in the education sector, and since Act 10 was primarily a reform of public-sector union powers, we explore the extent to which the electoral effects we find are given by a decline in union powers as a consequence of Act 10. We first document that ex ante wealthier unions experienced a larger decline in revenues post-reform, but were also more likely to seek recertification in the immediate aftermath of the law's passage. Next, we show that most of the electoral gains enjoyed by the GOP occurred in districts with ex ante wealthier unions. We interpret these results as evidence that a decline in union powers can significantly shape the local political landscape and, consequently, the political effects of controversial reforms.

Our results bring new empirical evidence to a policy conversation rife with anecdote. Opponents of union reforms and various teacher compensation schemes have enjoyed significant press coverage. Yet, even many of these reforms' strongest advocates acknowledge that implementing them can be a significant political challenge. The nation's highest-profile early performance pay scheme, IMPACT in the Washington DC public school system, was widely credited in the press with toppling the mayor who championed it (Hopkinson, 2010; Chait, 2016). Despite originating with a different political party, Act 10 in Wisconsin was similarly controversial (Stein and Marley, 2013). When interpreting our results, it is useful to consider that measuring the overall impact of a statewide high-profile policy on any election is challenging, and even similarly controversial policies (or policies promoted by candidates with similar priorities) can differ substantially in their effects. Yet, our analysis shows that the marginal electoral impact of a decrease in union power and an increase in teacher pay flexibility was positive and significant, particularly in places that stood to gain more from the changes. This implies that, although policy reform often carries political costs, it can also yield political benefits. The work of researchers has the potential to affect these policy conversations — indeed, there is evidence for considerable scope for information on public policy to shift political attitudes for both voters and elected officials in various parts of the world (Butler et al., 2011; Hjort et al., 2021; Sandholtz and Vicente, 2024). Our findings emphasize how designing education policies that balance technocratic effectiveness and political feasibility is possible, and highlight an important role for institutions – such as public sector unions – in shaping the future of these policies.

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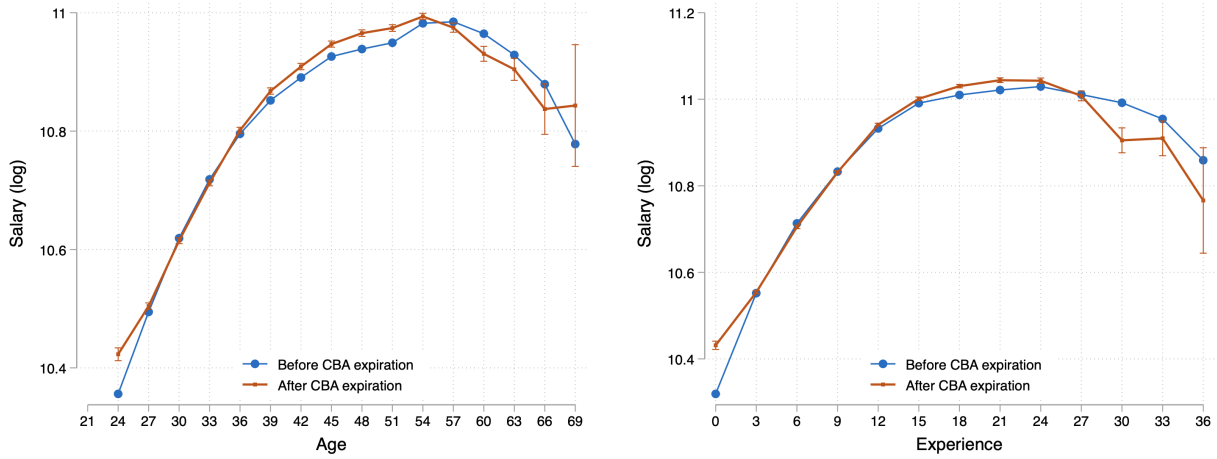
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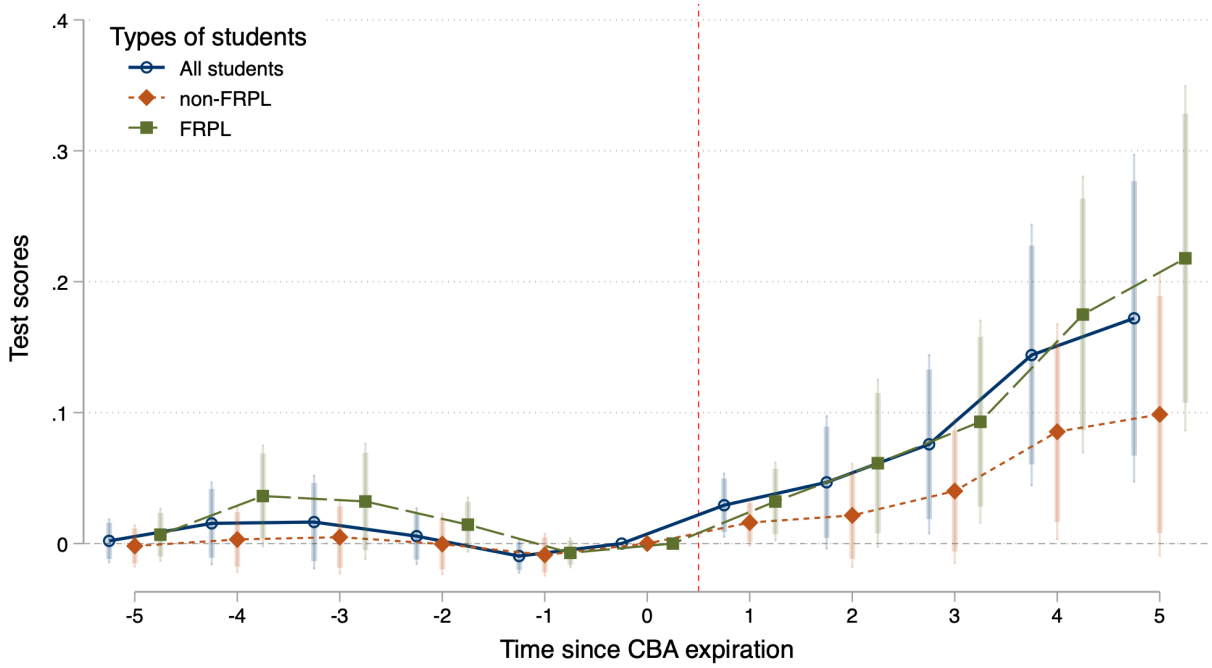
Figures

Figure 1: Effects of Act 10 On Teachers and Students

(a) Teacher pay by age and experience

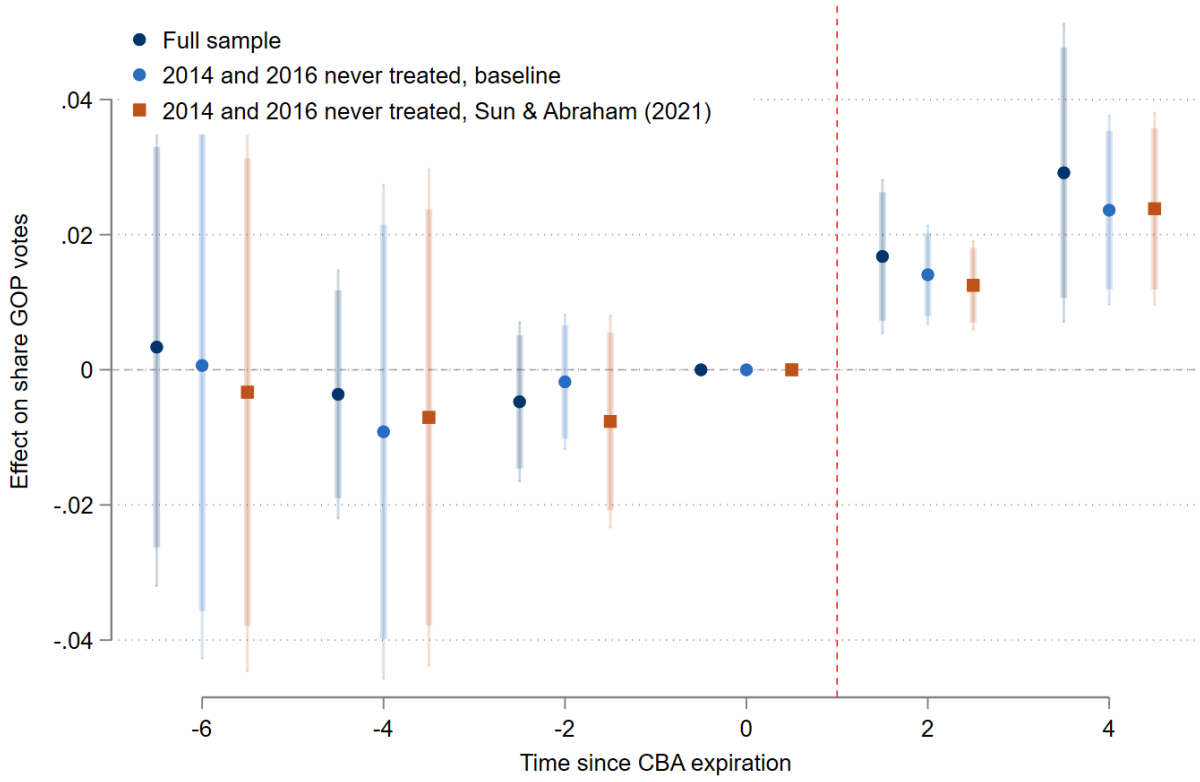


(b) Student test scores



Notes: Panel (a) shows estimates and 90% and 95% confidence intervals of age indicators (left) and experience indicators (right) on a regression of salaries on district and year fixed effects, for the two years preceding and the two years following the expiration of each district's CBA or its extension. Panel (b) shows estimates and 90% confidence intervals of the coefficients β_k in equation (1), using individual-level Math test scores of grade 2-8 students and controlling for student demographics, school, and grade-by-year fixed effects. The solid line shows estimates for the full student sample. Diamond markers denote estimates for FRPL-eligible students, and squared markers denote estimates for all other students. In all figures, confidence intervals are obtained using standard errors clustered at the district level.

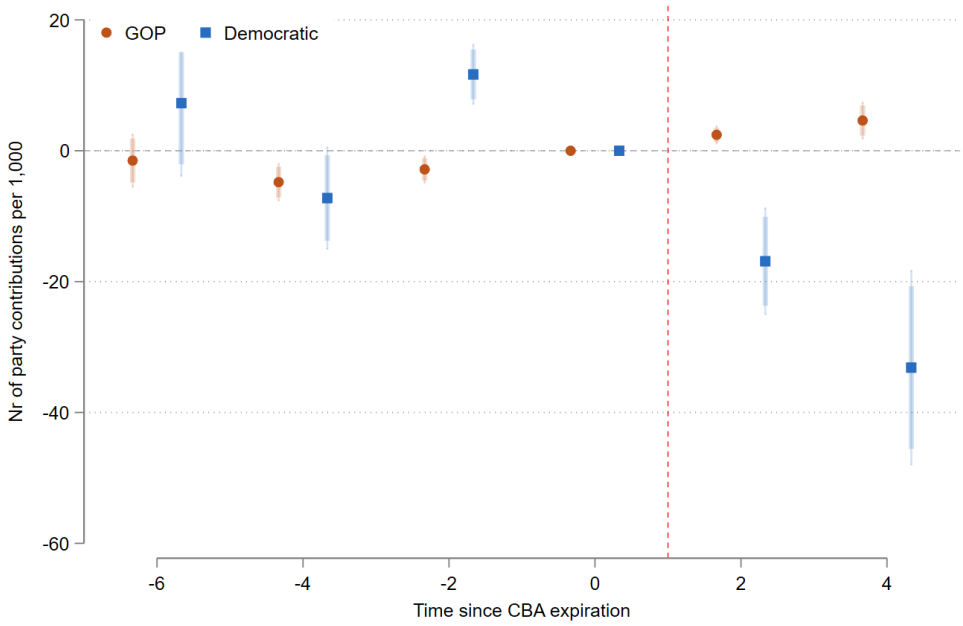
Figure 2: Political Effects of Wisconsin's Act 10: Event Study Estimates



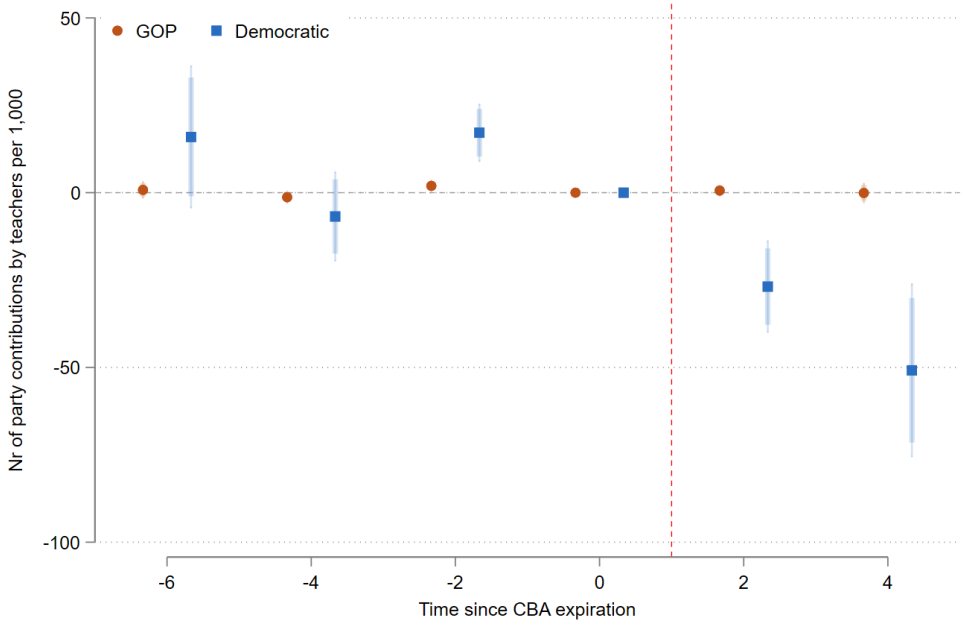
Notes: The blue circles show estimates and 90% and 95% confidence intervals of the coefficients β_s in equation (1), estimated using the GOP vote share in gubernatorial elections in each ward and year as the dependent variable and controlling for ward and year fixed effects. The triangles show analogous estimates, considering the 2014 and 2016 CBA expiration cohorts as never treated. The hollow squares show estimates of the model of Sun and Abraham (2021), obtained considering the 2014 and 2016 CBA expiration cohorts as never treated. Confidence intervals are obtained using standard errors clustered at the district level.

Figure 3: Campaign Contributions to Political Parties, Total and for Teachers: Event-Study Estimates

(a) Total number of contributions

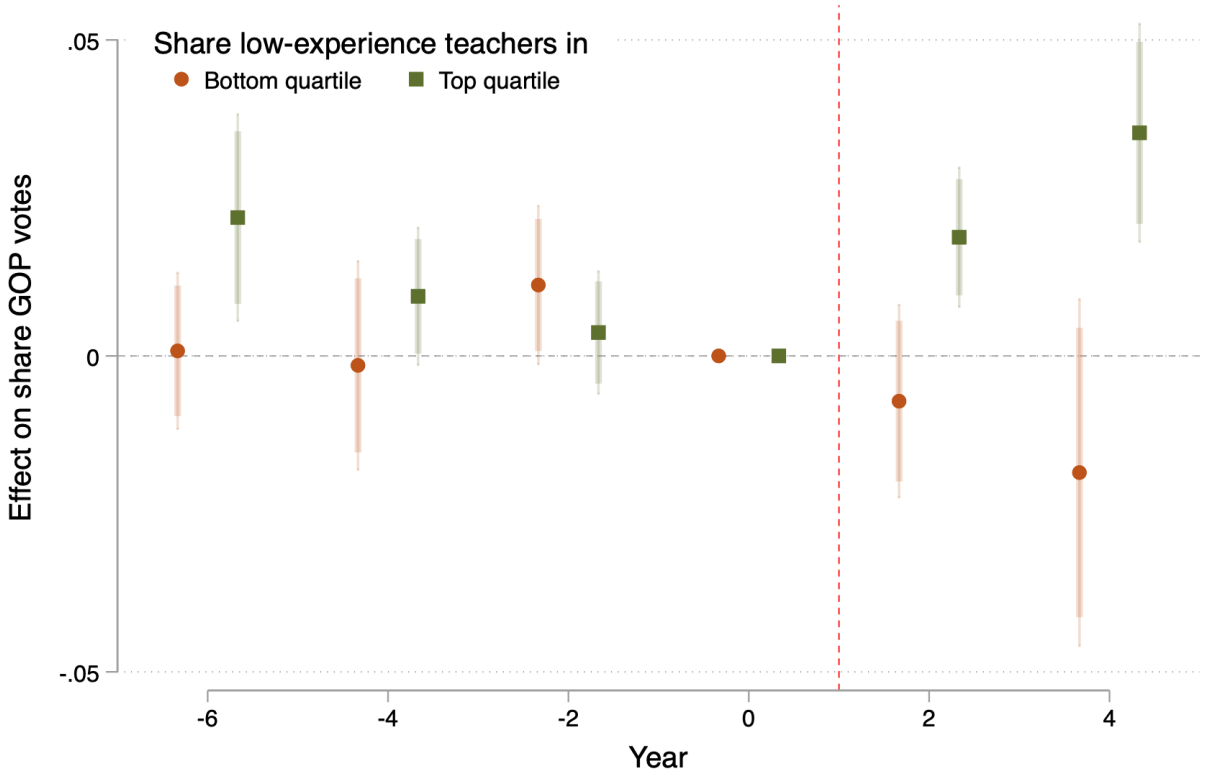


(b) Contributions by teachers



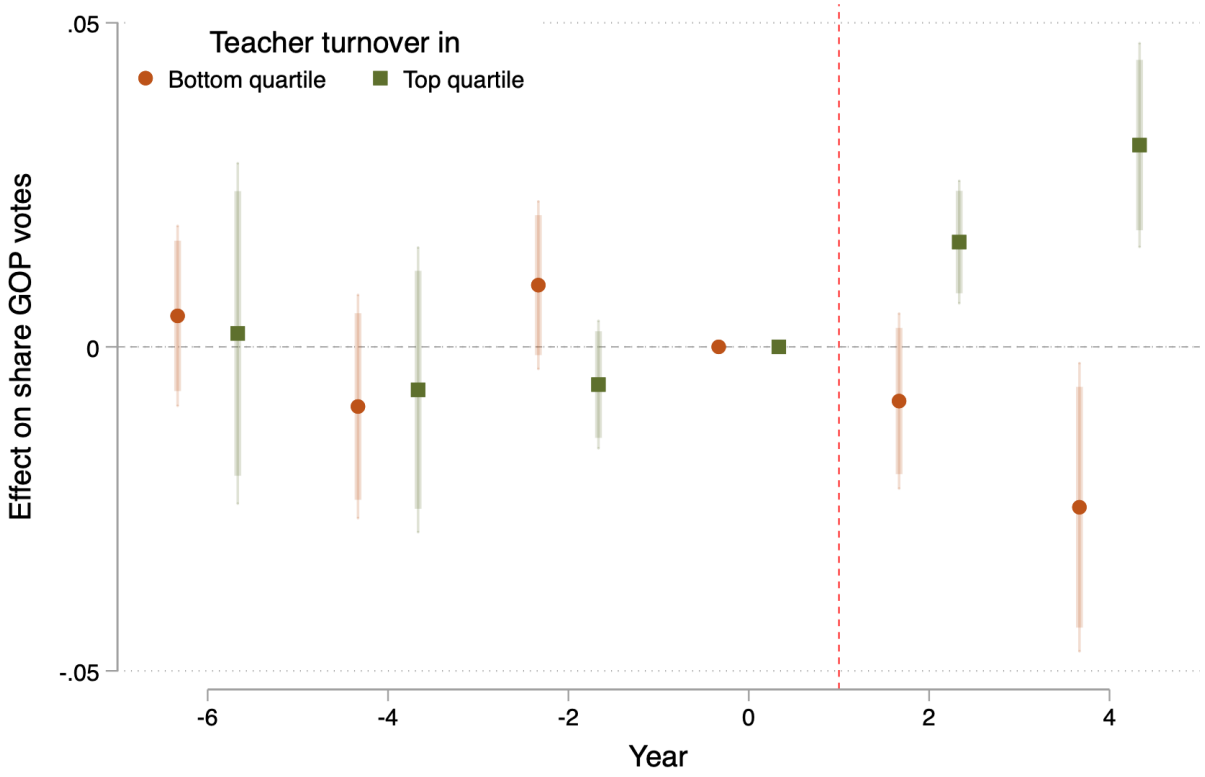
Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_s in equation (1), obtained using the number of contributions per 100 people to each party in the district. Panel (a) shows all contributions; panel (b) shows contributions made by teachers, per 100 teachers in the district. Observations are weighted by district population. Confidence intervals are obtained using standard errors clustered at the district level.

Figure 4: Winners and Losers of Wisconsin's Act 10. Event-Study Estimates by Share of Low-Experience Teachers in District



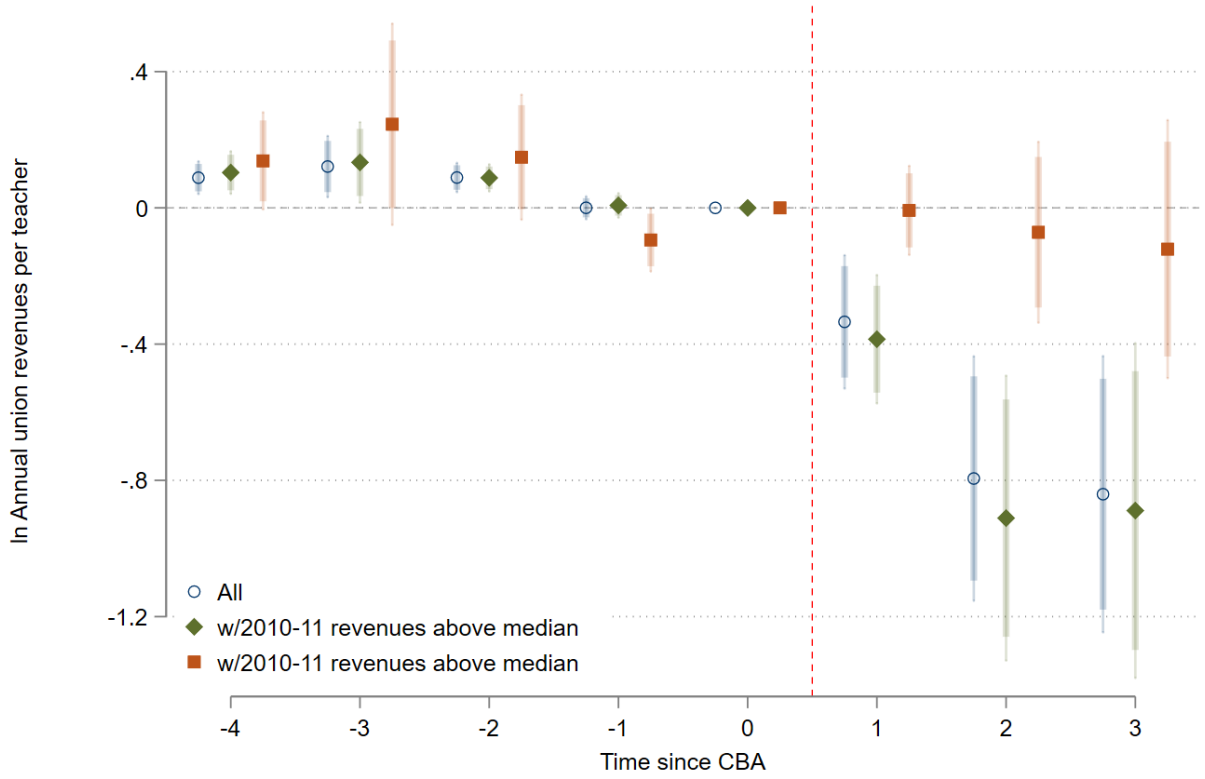
Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_s in equation (1), estimated using the GOP vote share in gubernatorial elections in each ward and year as the dependent variable and controlling for ward and year fixed effects. The squares show estimates for the subsample of districts in the top quartile of the state distribution of the 2010-11 share of low-experience (≤ 3 years) teachers, and the circles show estimates for districts in the bottom quartile. Confidence intervals are obtained using standard errors clustered at the district level.

Figure 5: Winners and Losers of Wisconsin's Act 10. Event-Study Estimates, by District Turnover



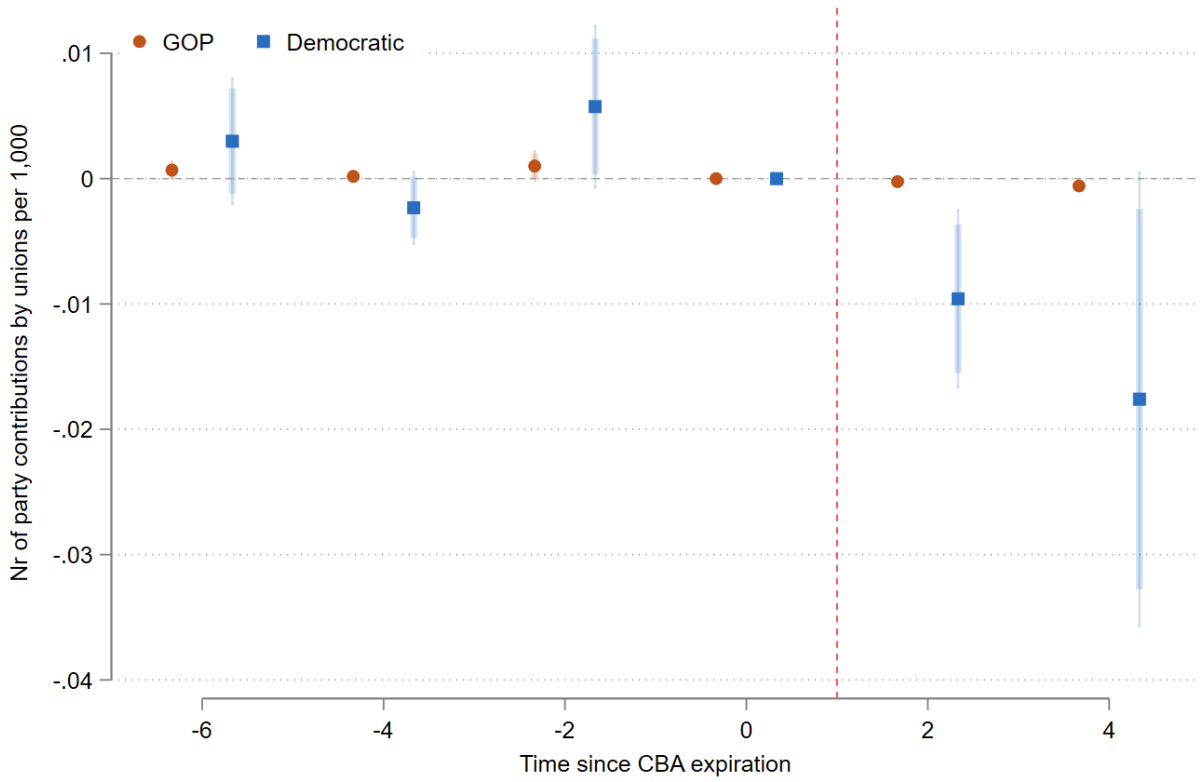
Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_s in equation (1), estimated using the GOP vote share in gubernatorial elections in each ward and year as the dependent variable and controlling for ward and year fixed effects. The squares show estimates for the subsample of districts in the top quartile of the state distribution of 2010-11 teacher turnover, and the circles show estimates for districts in the bottom quartile. Confidence intervals are obtained using standard errors clustered at the district level.

Figure 6: Effects of Act 10 on Union Revenues: Event Study Estimates



Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_k in equation (1), using district-year level union revenues per member as the dependent variable, for all unions (solid line) and separately for districts with revenues per member above and below the state median in 2010-11 (dashed lines). Observations are weighted by the number of teachers in each district. Confidence intervals are obtained using standard errors clustered at the district level.

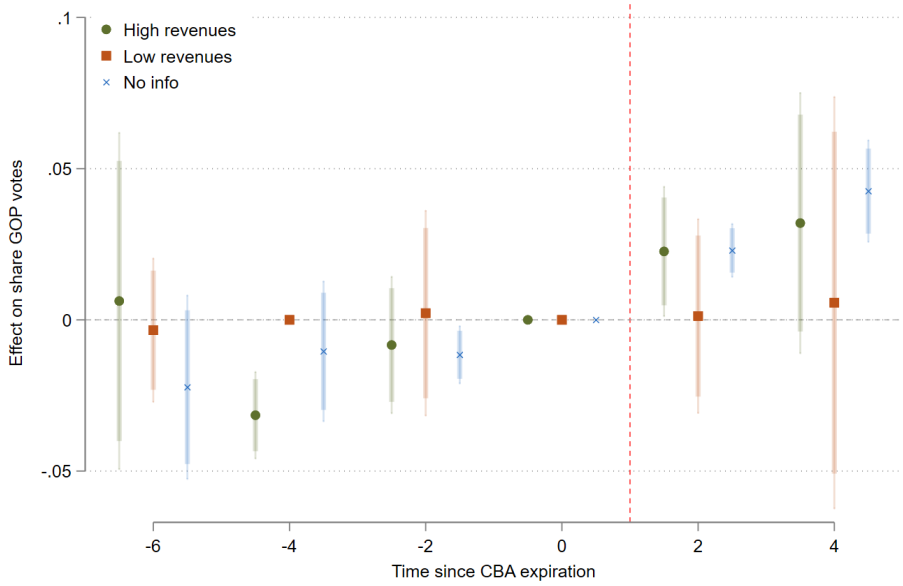
Figure 7: Campaign Contributions to Political Parties Made By Teachers' Unions: Event Study



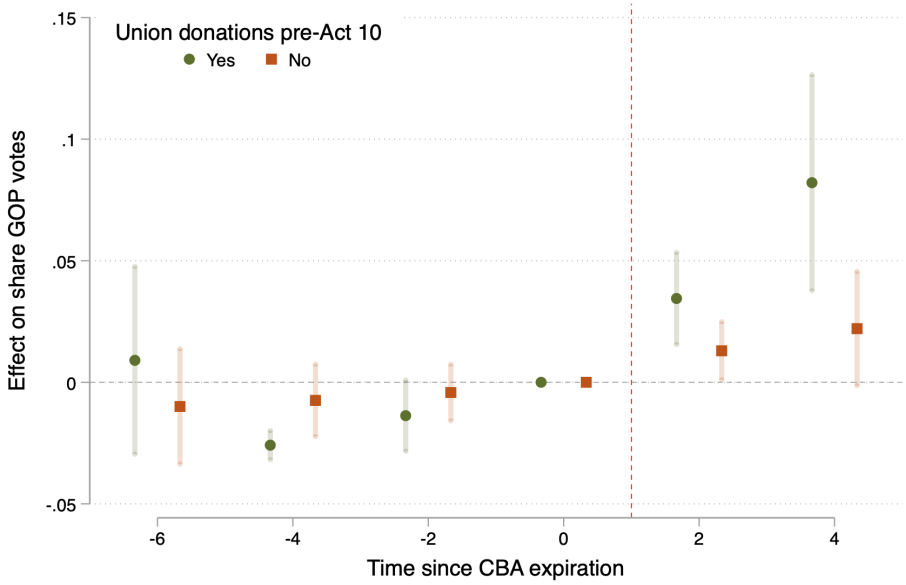
Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_s in equation (1), obtained using the number of contributions per 100 people made by each district's teacher union to each party. Observations are weighted by district population. Confidence intervals are obtained using standard errors clustered at the district level.

Figure 8: The Impact of Unions on GOP Votes. Event-Study Estimates, by Ex Ante Union Revenues and Campaign Contributions

(a) By ex ante revenues



(b) By ex ante campaign contributions



Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_s in equation (1), estimated using the GOP vote share in gubernatorial elections in each ward and year as the dependent variable and controlling for ward and year fixed effects. In panel (a), the circles show estimates for the subsample of wards in districts with teacher union revenues above the state median in 2011; the squares show estimates for wards with union revenues below the median; and the X's show estimates for wards for which we have no information on union revenues. In panel (b), the circles show estimates for wards in districts whose unions made campaign contributions to gubernatorial races prior to 2011 and the squares show estimates for wards in districts with no contributions. Confidence intervals are obtained using standard errors clustered at the district level.

Tables

Table 1: Wisconsin Wards and School Districts: Summary Statistics

		Expiration in	
	All districts	2011	After 2011
Teachers			
Teacher turnover rate, pre-2011	0.100	0.10	0.099
	(0.030)	(0.027)	(0.031)
Teacher mkt tightness, pre-2011	1.07	1.14	1.04
	(0.39)	(0.43)	(0.36)
Share teachers w/experience $<$ 3y	0.15	0.15	0.15
	(0.051)	(0.056)	(0.047)
Share teachers w/experience $>$ 21y	0.18	0.18	0.17
	(0.067)	(0.067)	(0.067)
Students			
Share low-SES (FRPL) students	0.40	0.33	0.44
	(0.18)	(0.16)	(0.18)
Std. test scores, Math	-0.0030	-0.0059	-0.0016
	(0.018)	(0.022)	(0.015)
Population age			
Share pop $<$ 18 yo	0.23	0.23	0.23
	(0.067)	(0.062)	(0.070)
Political views			
Share GOP Governor votes, 2010	0.54	0.60	0.51
	(0.15)	(0.12)	(0.16)
Share GOP President votes, 2008	0.44	0.50	0.41
	(0.14)	(0.12)	(0.14)
100 * Donations pp to Dem	0.43	0.38	0.47
	(0.51)	(0.52)	(0.50)
100 * Donations pp to GOP	1.34	1.60	1.20
	(0.84)	(1.13)	(0.58)
Unions			
Teacher union held election in 2011	0.34	0.67	0.15
	(0.47)	(0.47)	(0.36)
Share yes votes to union, 2011	0.75	0.74	0.77
	(0.10)	(0.11)	(0.077)
Union revenues per teacher, pre-2011	733.9	482.8	842.2
	(1169.0)	(481.7)	(1348.2)
Number of wards	4,989	3,242	1,747
Number of districts	236	134	102

Notes: Means and standard deviations (in parentheses) of variables used in the analysis. The first column shows statistics on the full sample of districts included in the analysis; the second column restricts attentions to districts included in the analysis with CBAs or extensions that expired in 2011; and the third column restricts attention to districts with CBAs or extensions that expired after 2011.

Table 2: Political Effects of Wisconsin's Act 10: Pooled Event Study Estimates

	All districts		2014 and 2016 never treated	
	(1)	(2)	(3)	(4)
Exposed	0.014***	0.014***	0.014***	0.014***
	(0.005)	(0.005)	(0.005)	(0.005)
District FE	Yes	No	Yes	No
Ward FE	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
Mean dep. var. control	0.476	0.476	0.476	0.476
N	21240	21233	21240	21233
Clusters (districts)	236	236	236	236
R-squared	0.73	0.94	0.73	0.94

Notes: The dependent variable is the share of GOP votes in gubernatorial elections in each ward and year. We show estimates and standard deviations of the parameter β_k in equation (1), where we constrain $\beta_k = 0$ for $k < 0$ and β_k to be the same across all $k > 0$. Columns 1 and 3 controls for year and district fixed effects; columns 2 and 4 control for ward and year fixed effects. Columns 3 and 4 consider the 2014 and 2016 expiration cohorts as never treated. Standard errors in parentheses are clustered at the district level. * ≤ 0.1; ** ≤ 0.05; *** ≤ 0.01.

Table 3: Political Effects of Wisconsin's Act 10: Difference-in-Differences Estimates

	All districts				Only clean controls	
	(1)	(2)	(3)	(4)	(5)	(6)
CBA after 2011 * post 2011	0.021**	0.020**	0.019**	0.019**	0.053***	0.054***
	(0.010)	(0.010)	(0.009)	(0.009)	(0.019)	(0.018)
District FE	Yes	No	Yes	No	No	No
Ward FE	No	Yes	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Includes 2014	No	No	Yes	Yes	Yes	Yes
Mean dep. var. control	0.465	0.465	0.465	0.465	0.314	0.314
N	19645	19636	24550	24545	9596	9594
Clusters (districts)	236	236	236	236	107	107
R-squared	0.73	0.93	0.73	0.93	0.80	0.94

Notes: The dependent variable is the share of GOP votes in gubernatorial elections in each ward and year. We show estimates and standard deviations of the parameter β in equation (2), using as the treatment variable an indicator for districts with CBAs expiring in 2011, regardless of whether they were extended or not. Columns 1-2, and 4 are estimated on the sample of years until 2012; columns 3-6 on the sample until 2014. Columns 1, 3, and 6 control for year and district fixed effects; columns 2, 4, and 6 control for ward and year fixed effects. Columns 5-6 are estimated only on wards in districts in cohorts 2011, 2014, and 2016. Standard errors in parentheses are clustered at the district level. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.

Table 4: Effects of Wisconsin's Act 10 on Voter Turnout: Event Study Estimates

	All districts		Excluding Milwaukee
	(1)	(2)	(3)
Exposed	0.014	0.014	0.016
	(0.015)	(0.015)	(0.016)
District FE	Yes	No	No
Ward FE	No	Yes	Yes
Year FE	Yes	Yes	Yes
Mean dep. var. control	0.463	0.463	0.470
N	21313	21313	20021
Clusters (districts)	236	236	235
R-squared	0.02	0.27	0.26

Notes: The dependent variable is a measure of voter turnout, defined as the number of votes divided by the population over 18 in each ward. We show estimates and standard deviations of the parameter β_k in equation (1), where we constrain $\beta_k = 0$ for $k < 0$ and β_k to be the same across all $k > 0$. Column 1 controls for year and district fixed effects; column 2 controls for ward and year fixed effects. Standard errors in parentheses are clustered at the district level. * ≤ 0.1; ** ≤ 0.05; *** ≤ 0.01.

Table 5: Campaign Contributions to Political Parties, Total and for Teachers: Pooled Event-Study Estimates

	GOP	Dem	Teachers, GOP	Teachers, Dem	Unions, Dem
	(1)	(2)	(3)	(4)	(5)
Exposed	1.673***	-7.815	1.583*	-12.755	-0.005
	(0.515)	(5.641)	(0.926)	(7.941)	(0.003)
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Mean dep. var. control	13.793	3.205	3.759	7.655	0.002
N	948	948	943	943	948
Clusters (districts)	216	216	215	215	216
R-squared	0.81	0.83	0.36	0.60	0.50

Notes: The dependent variable is the share of number of contributions to gubernatorial races per 1,000 people in each district to each party. We show estimates and standard deviations of the parameter β_k in equation (1), where we constrain $\beta_k = 0$ for $k < 0$ and β_k to be the same across all $k > 0$. Columns 1 and 3 5 examine donations to the GOP; columns 2, 4, and 5 examine donations to the Democratic party. Columns 1 and 2 use total donations per 100 people, columns 3 and 4 use donations by teachers per 100 teachers and column 5 uses donations by teacher unions by 100 people. All specifications control for year and district fixed effects. Observations are weighted by the number of people in each district. Standard errors in parentheses are clustered at the district level. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.

Table 6: Political Effects of Wisconsin's Act 10: Pooled Event Study Estimates, Robustness Checks

	Ignoring extensions	Excluding Milwaukee	Only fully aligned wards
	(1)	(2)	(3)
Exposed	0.019	0.010***	0.015***
	(0.014)	(0.004)	(0.006)
Ward FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Mean dep. var. control	0.476	0.503	0.449
N	21313	19941	12332
Clusters (districts)	237	235	231
R-squared	0.94	0.93	0.94

Notes: The dependent variable is the share of GOP votes in gubernatorial elections in each ward and year. We show estimates and standard deviations of the parameter β_k in equation (1), where we constrain $\beta_k = 0$ for $k < 0$ and β_k to be the same across all $k > 0$. Column 1 is estimated considering only CBA expirations and ignoring extensions to construct the exposure variable. Column 2 is estimated excluding Milwaukee. Column 3 is estimated on the subsample of wards that do not contain district boundaries. Standard errors in parentheses are clustered at the district level. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.

Table 7: Political Effects of Wisconsin's Act 10: Pooled Event Study, By Share of Low and High-Experience Teachers

Panel (a): By share of teachers with experience ≤ 3 in district						
	$\geq$ 75pctile	25-75pctile	$\leq$25pctile	$\geq$ 75pctile	25-75pctile	$\leq$25pctile
	(1)	(2)	(3)	(4)	(5)	(6)
Exposed	0.018**	0.009*	-0.000	0.019***	0.009*	0.000
	(0.007)	(0.005)	(0.007)	(0.007)	(0.005)	(0.007)
District FE	Yes	Yes	Yes	No	No	No
Ward FE	No	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var. control	0.505	0.492	0.527	0.506	0.492	0.527
N	5117	9776	5055	5114	9774	5053
Clusters (districts)	63	115	57	63	115	57
R-squared	0.67	0.77	0.61	0.93	0.94	0.87

Panel (b): By share of teachers with experience ≥ 21 in district						
	$\geq$ 75pctile	25-75pctile	$\leq$25pctile	$\geq$ 75pctile	25-75pctile	$\leq$25pctile
	(1)	(2)	(3)	(4)	(5)	(6)
Exposed	0.012	0.008*	0.012*	0.012	0.009*	0.013**
	(0.009)	(0.005)	(0.006)	(0.008)	(0.005)	(0.006)
District FE	Yes	Yes	Yes	No	No	No
Ward FE	No	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var. control	0.527	0.512	0.453	0.527	0.513	0.453
N	4995	10034	4919	4993	10030	4918
Clusters (districts)	73	115	47	73	115	47
R-squared	0.69	0.68	0.78	0.91	0.91	0.94

Notes: The dependent variable is the share of GOP votes in gubernatorial elections in each ward and year. We show estimates and standard deviations of the parameter β_k in equation (1), where we constrain $\beta_k = 0$ for $k < 0$ and β_k to be the same across all $k > 0$. In panel (a), we split the sample by the quartile of the share of teachers who had 3 or fewer years of experience in 2010-2011. In panel (b), we split the sample by the quartile of the share of teachers who had 21 or more years of experience. In both panels, columns 1 and 3 show estimates for the top quartile, columns 2 and 5 for the 2nd and 3rd quartile, and columns 3 and 6 for the bottom quartile. Columns 1-3 control for district and year fixed effects, and columns 4-6 control for ward and year fixed effects. Standard errors in parentheses are clustered at the district level. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.

Table 8: Political Effects of Wisconsin's Act 10: Pooled Event Study, by Share FRPL Students and Ex Ante Test Scores

Panel (a): By share of FRPL students in district						
	$\leq$ 25pctile	25-75pctile	$\geq$75pctile	$\leq$ 25pctile	25-75pctile	$\geq$75pctile
	(1)	(2)	(3)	(4)	(5)	(6)
Exposed	0.006	0.002	0.024***	0.006	0.003	0.025***
	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)	(0.007)
District FE	Yes	Yes	Yes	No	No	No
Ward FE	No	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var. control	0.536	0.517	0.477	0.536	0.517	0.477
N	4927	9150	4532	4926	9148	4528
Clusters (districts)	56	109	49	56	109	49
R-squared	0.85	0.61	0.52	0.96	0.89	0.90
Panel (b): By ex ante unconditional test scores in district						
	$\leq$ 25pctile	25-75pctile	$\geq$75pctile	$\leq$ 25pctile	25-75pctile	$\geq$75pctile
	(1)	(2)	(3)	(4)	(5)	(6)
Exposed	0.022**	0.005	0.001	0.023***	0.005	0.002
	(0.008)	(0.006)	(0.007)	(0.008)	(0.006)	(0.007)
District FE	Yes	Yes	Yes	No	No	No
Ward FE	No	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var. control	0.484	0.520	0.506	0.484	0.520	0.506
N	4697	9103	4713	4693	9102	4711
Clusters (districts)	65	99	49	65	99	49
R-squared	0.56	0.63	0.85	0.90	0.89	0.96

Notes: The dependent variable is the share of GOP votes in gubernatorial elections in each ward and year. We show estimates and standard deviations of the parameter β_k in equation (1), where we constrain $\beta_k = 0$ for $k < 0$ and β_k to be the same across all $k > 0$. In panel (a), we split the sample by the quartile of the share of FRPL students in the district in 2010-2011. In panel (b), we split the sample by the quartile of each district's average test scores. In both panels, columns 1 and 3 show estimates for the top quartile, columns 2 and 5 for the 2nd and 3rd quartile, and columns 3 and 6 for the bottom quartile. Columns 1-3 control for district and year fixed effects, and columns 4-6 control for ward and year fixed effects. Standard errors in parentheses are clustered at the district level. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.

Table 9: Political Effects of Wisconsin's Act 10: Effects by Ex Ante Teacher Turnover and Market Tightness

Panel (a): By teacher turnover						
	$\leq$ 25pctile	25-75pctile	$\geq$75pctile	$\leq$ 25pctile	25-75pctile	$\geq$75pctile
	(1)	(2)	(3)	(4)	(5)	(6)
Exposed	0.003	0.010*	0.013**	0.003	0.011*	0.014***
	(0.007)	(0.006)	(0.005)	(0.007)	(0.006)	(0.005)
District FE	Yes	Yes	Yes	No	No	No
Ward FE	No	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var. control	0.549	0.491	0.472	0.549	0.491	0.473
N	4979	9785	5272	4977	9784	5268
Clusters (districts)	59	104	72	59	104	72
R-squared	0.52	0.75	0.76	0.86	0.94	0.94
Panel (b): By market tightness						
	$\leq$ 25pctile	25-75pctile	$\geq$75pctile	$\leq$ 25pctile	25-75pctile	$\geq$75pctile
	(1)	(2)	(3)	(4)	(5)	(6)
Exposed	0.011	0.005	0.014**	0.010	0.005	0.015**
	(0.008)	(0.005)	(0.006)	(0.009)	(0.005)	(0.006)
District FE	Yes	Yes	Yes	No	No	No
Ward FE	No	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var. control	0.519	0.491	0.519	0.518	0.491	0.519
N	5037	9799	5143	5036	9796	5140
Clusters (districts)	63	107	64	63	107	64
R-squared	0.64	0.73	0.75	0.91	0.93	0.93

Notes: The dependent variable is the share of GOP votes in gubernatorial elections in each ward and year. We show estimates and standard deviations of the parameter β_k in equation (1), where we constrain $\beta_k = 0$ for $k < 0$ and β_k to be the same across all $k > 0$. In panel (a), we split the sample by the quartile of the district's turnover rate in 2010-11, defined as the share of teachers who leave the district in that year. In panel (b), we split the sample by the quartile of the district's teacher labor market tightness, defined as the ratio between the number of teachers who exit the district and the number of teachers who join. In both panels, columns 1 and 3 show estimates for the top quartile, columns 2 and 5 for the 2nd and 3rd quartile, and columns 3 and 6 for the bottom quartile. Columns 1-3 control for district and year fixed effects, and columns 4-6 control for ward and year fixed effects. Standard errors in parentheses are clustered at the district level. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.

Table 10: Political Effects of Wisconsin's Act 10: Effects by Ex Ante Union Revenues and Campaign Contributions

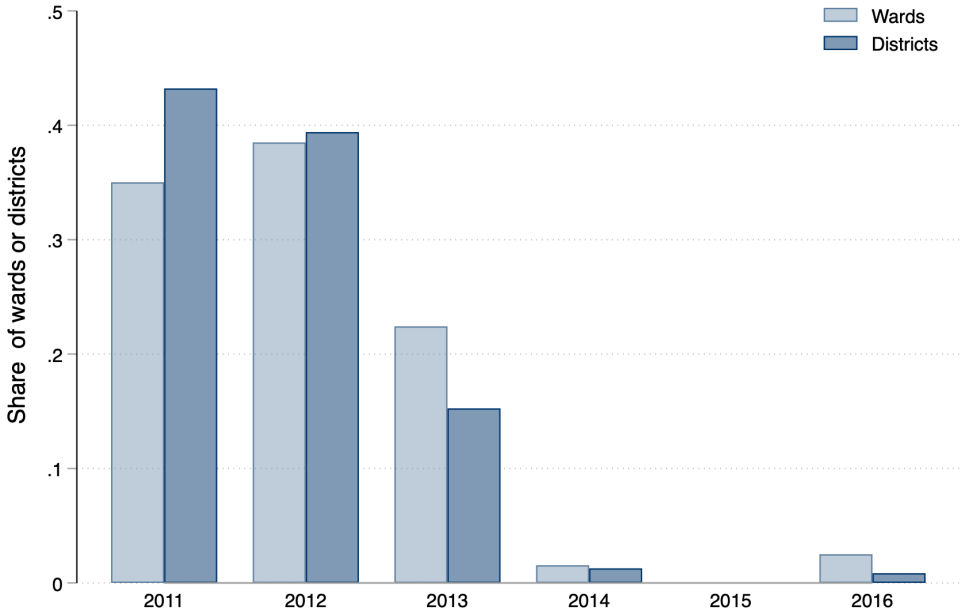
Panel (a): Effects by union revenues						
	above median		below median		missing	
	(1)	(2)	(3)	(4)	(5)	(6)
Exposed	0.023**	0.023**	-0.000	-0.001	0.010**	0.011**
	(0.009)	(0.009)	(0.006)	(0.007)	(0.005)	(0.005)
District FE	Yes	No	Yes	No	Yes	No
Ward FE	No	Yes	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var. control	0.380	0.380	0.517	0.517	0.518	0.518
N	5673	5673	4425	4424	11142	11136
Clusters (districts)	39	39	30	30	167	167
R-squared	0.69	0.96	0.74	0.93	0.68	0.90

Panel (b): Effects by union gubernatorial campaign contributions				
	w/donations	no donations	w/donations	no donations
	(1)	(2)	(3)	(4)
Exposed	0.024**	0.008**	0.024**	0.008**
	(0.008)	(0.004)	(0.008)	(0.004)
District FE	Yes	Yes	No	Yes
Ward FE	No	No	Yes	No
Year FE	Yes	Yes	Yes	Yes
Mean dep. var. control	0.311	0.511	0.311	0.512
N	2873	18367	2873	18360
Clusters (districts)	10	226	10	226
R-squared	0.72	0.68	0.97	0.91

Notes: The dependent variable is the share of GOP votes in gubernatorial elections in each ward and year. We show estimates and standard deviations of the parameter β_k in equation (1), where we constrain $\beta_k = 0$ for $k < 0$ and β_k to be the same across all $k > 0$. In panel (a), columns 1 and 2 show estimates for wards in districts whose unions had revenues above the state median in 2011; columns 3 and 4 show estimates for wards in districts with union revenues below the state median; and columns 5 and 6 show estimates for wards in districts with no information on union revenues. In panel (b), columns 1 and 3 show estimates for wards in districts whose unions made campaign contributions to gubernatorial races prior to 2011; columns 2 and 4 show estimates for wards in districts with no contributions. Columns 1, 3, and 5 control for district and year fixed effects, and columns 2, 4, and 6 control for ward and year fixed effects. Standard errors in parentheses are clustered at the district level. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.

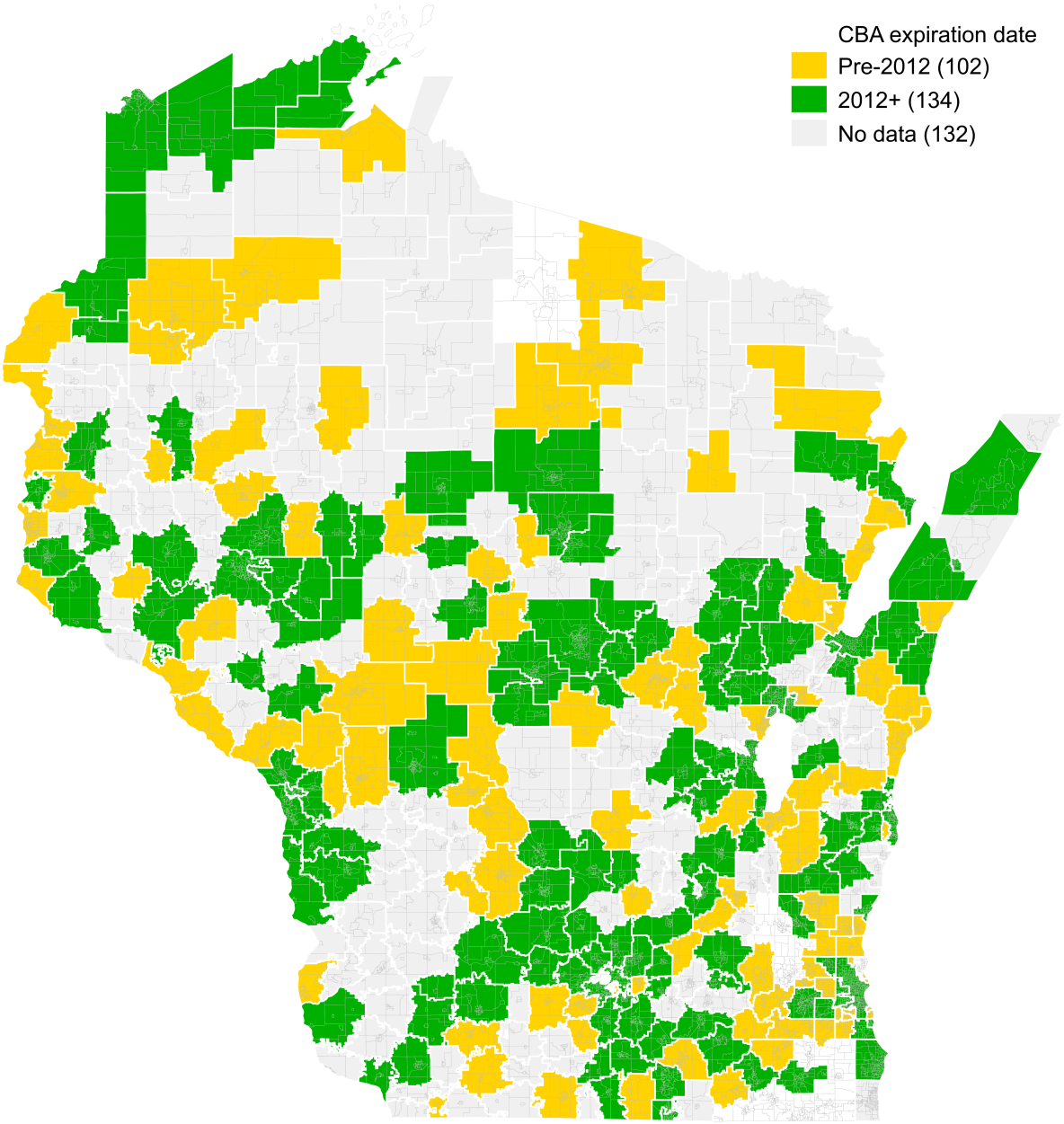
Appendix Tables and Figures

Figure A1: Distribution of Wards and Districts by CBA Expiration Dates



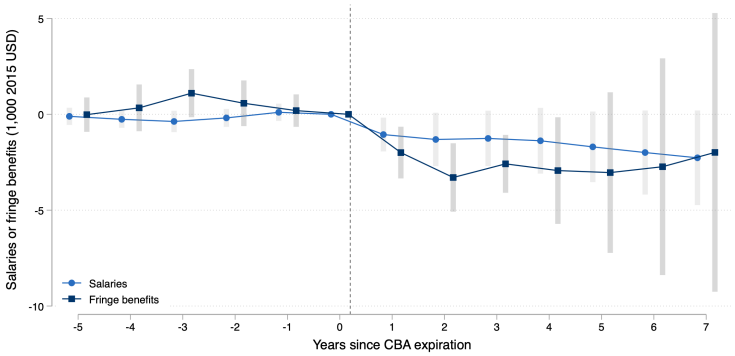
Notes: Share of wards and districts by date of expiration of the district's CBA or of its extension.

Figure A2: Wisconsin unified school districts, by CBA expiration year



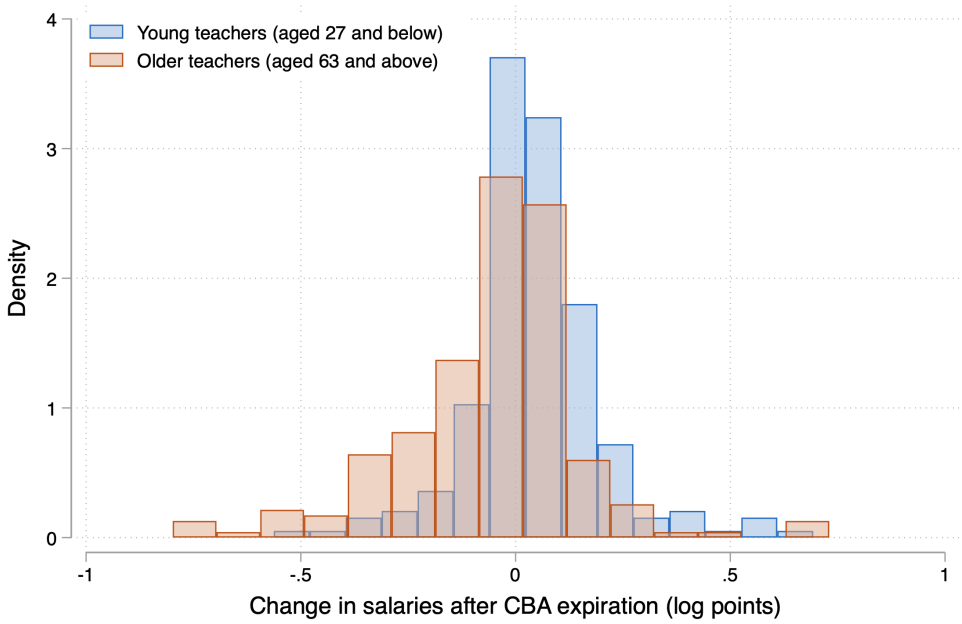
Notes: The map shows school districts by when their (extended) CBAs expired (pre-2012 vs 2012 or later). Non-unified school districts (elementary and secondary) not shown. School districts are delineated by thick white lines; wards are delineated by thin gray lines.

Figure A3: Changes in Teacher Compensation After Act 10: Salaries and Benefits



Notes: Estimates and 95% confidence intervals of the coefficients β_k in equation (1), obtained using individual-level salaries and fringe benefits as the dependent variable and controlling for district and year fixed effects. Confidence intervals are obtained using standard errors clustered at the district level.

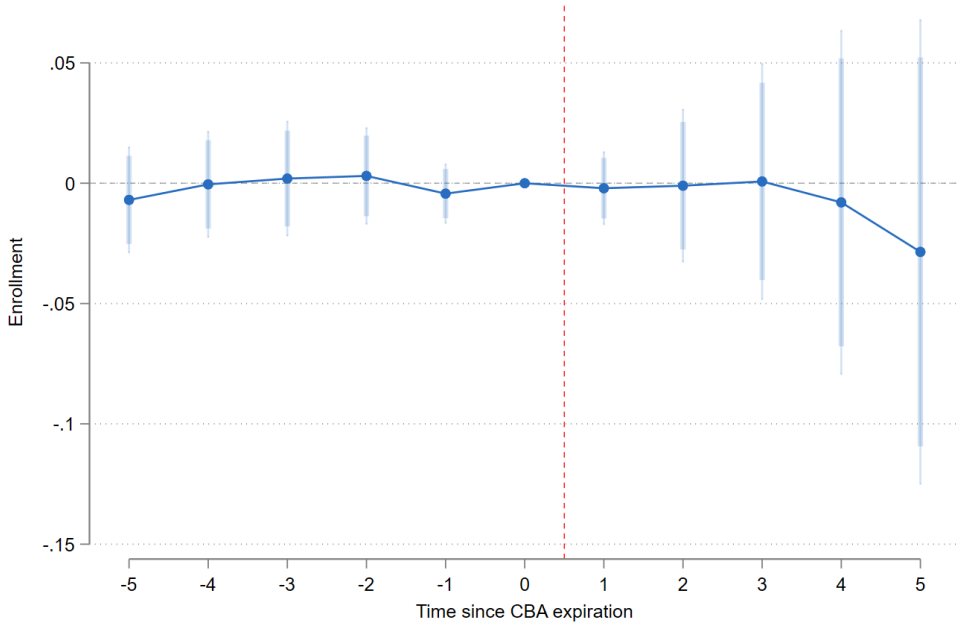
Figure A4: Distribution of Changes in Salaries, Before vs After A District's CBA Expiration



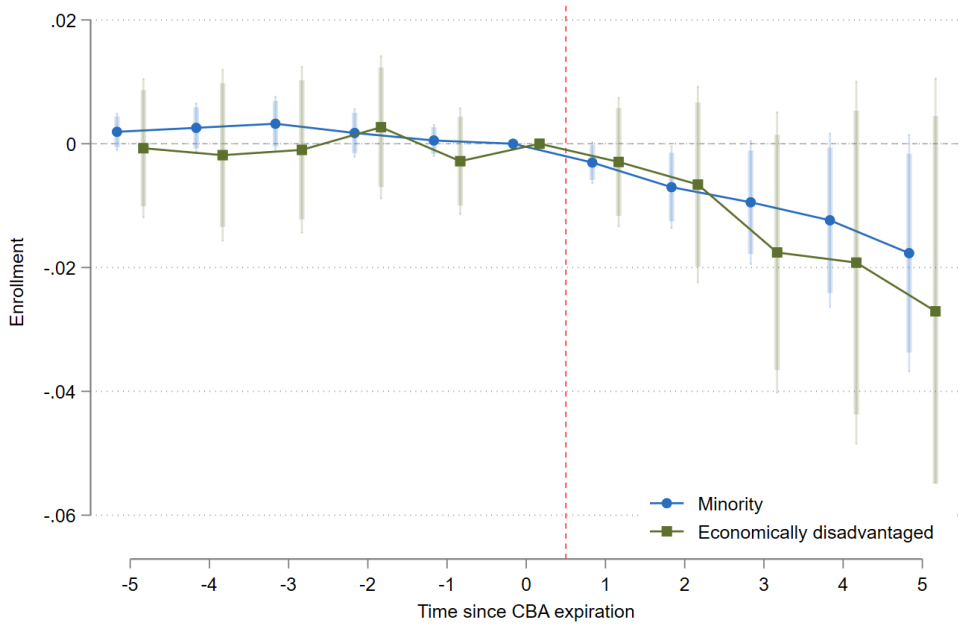
Notes: District-level changes in the logarithm of teacher salaries for teachers aged 63 and older (orange series) and those aged 27 and below (blue series), between the two years preceding and the two years following the expiration of each district's CBA or its extension.

Figure A5: Enrollment and Shares of Students in Demographic Groups: Event Study Estimates

(a) Enrollment



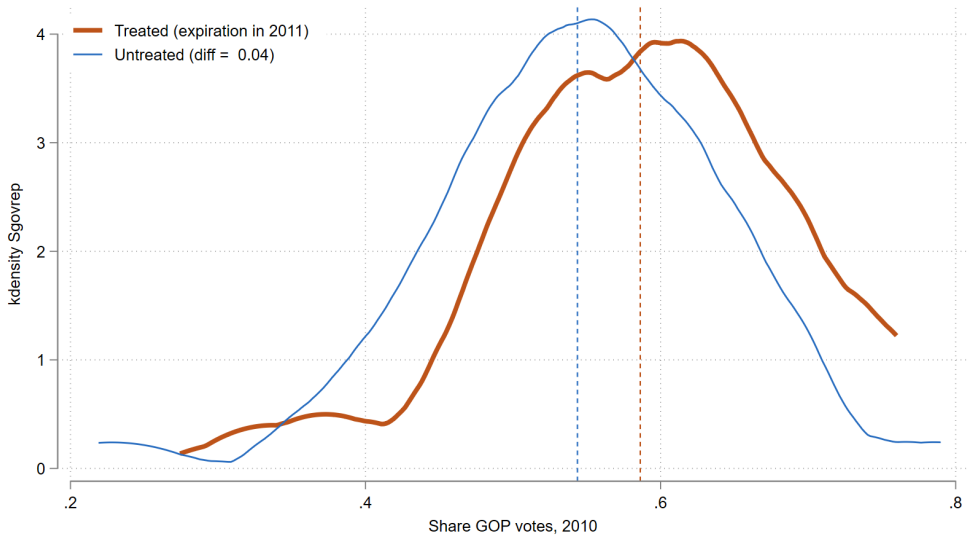
(b) Enrollment shares



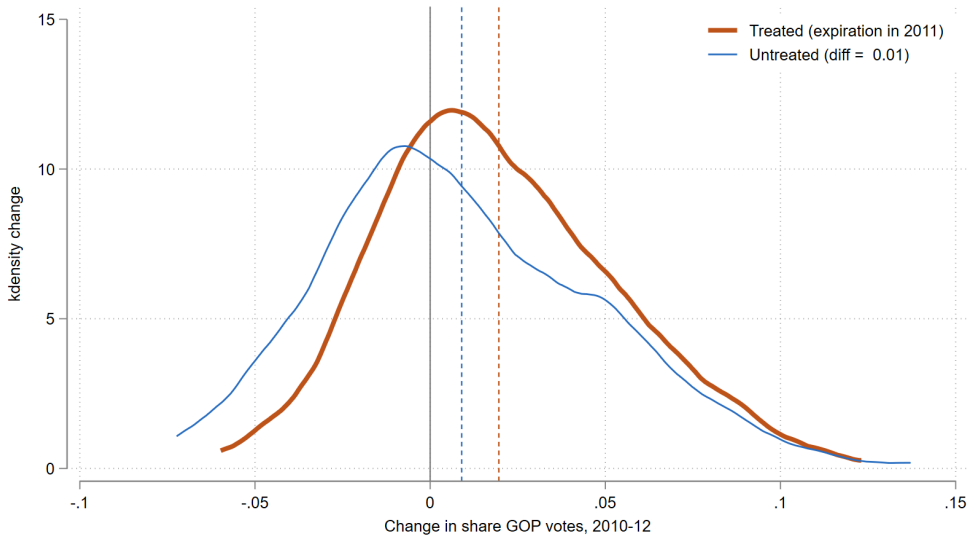
Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_k in equation (1), using log district enrollment (panel (a)) and district enrollment shares of FRPL and minority (Black or Hispanic) students (panel (b)). Confidence intervals are obtained using standard errors clustered at the district level.

Figure A6: Votes to GOP Governor in Treated and Control Districts: Baseline (2010) and 2010-2012 Change

(a) GOP vote shares in 2010 gubernatorial election

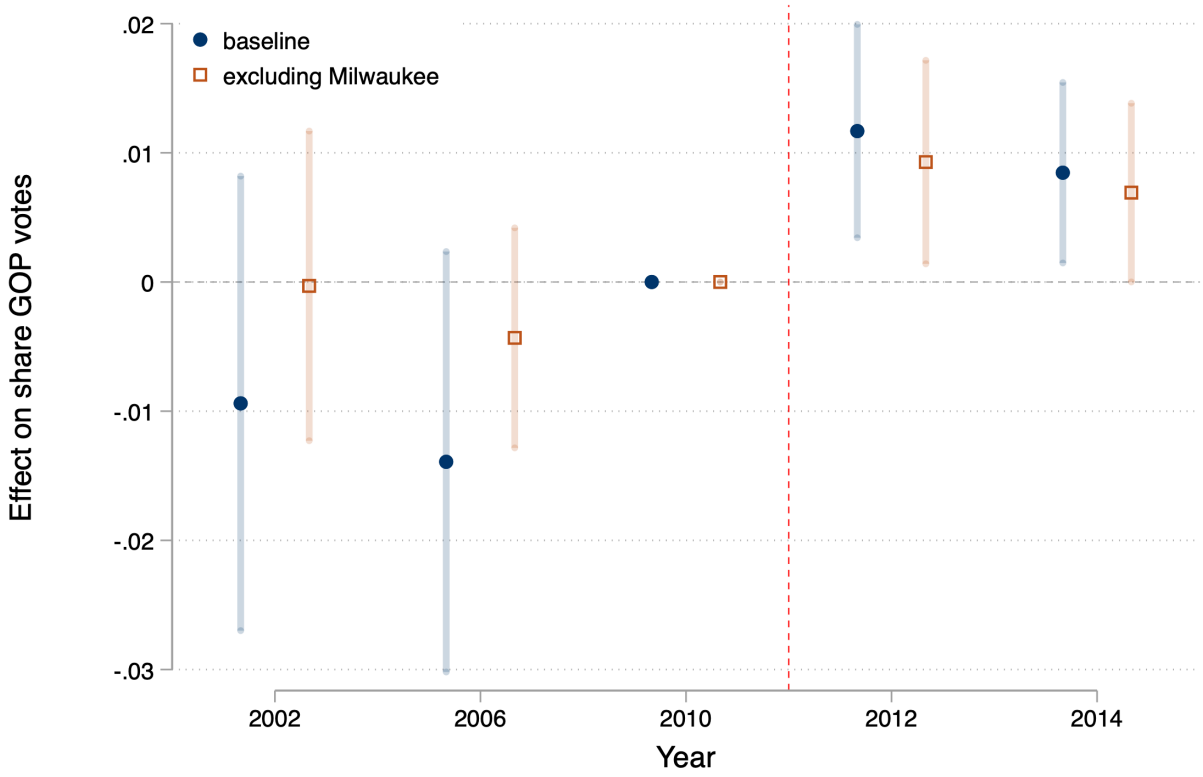


(b) Change in GOP vote shares, 2012 vs 2010



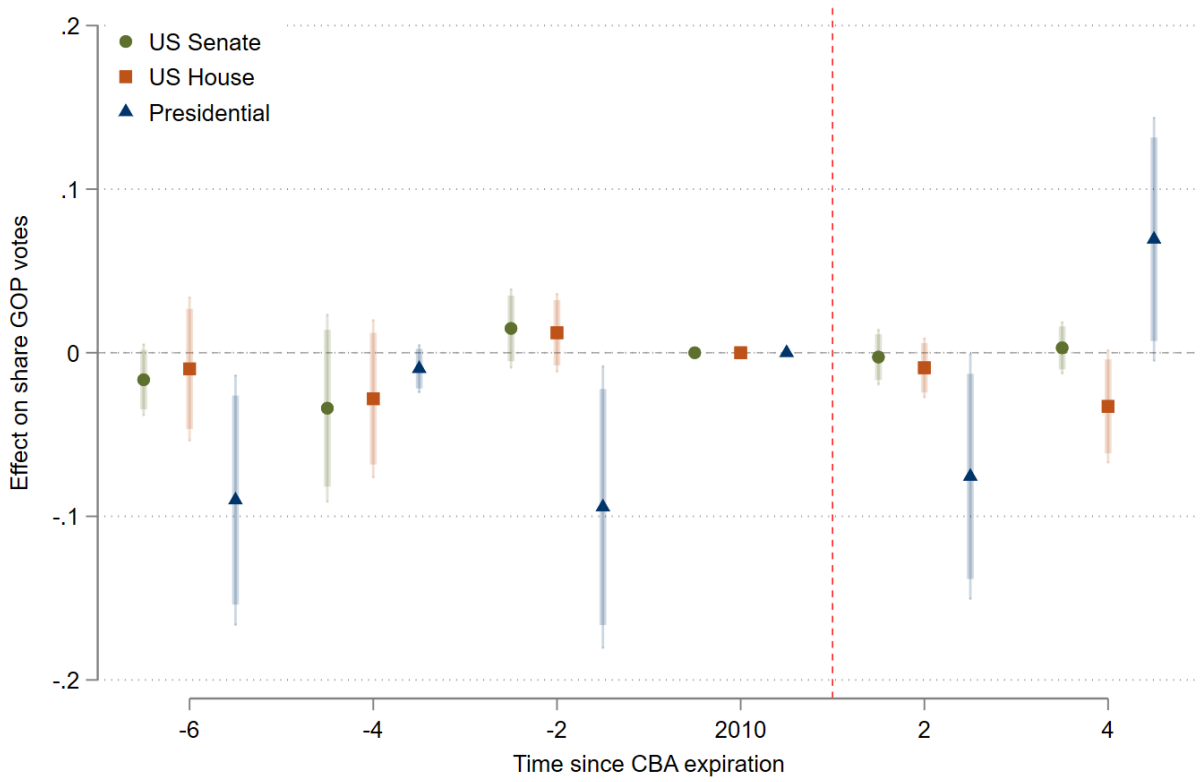
Notes: Panel (a) shows a distribution of the share of GOP votes in the 2010 gubernatorial election, separately for wards located in school districts with CBAs that expired in 2011 and were not extended (treated, thick line) and wards in districts with CBAs (or extensions) that expired after 2011 (untreated). Panel (b) shows the distribution of the 2010-2012 change in the share of GOP governor votes, separately for wards located in treated and untreated districts.

Figure A7: Political Effects of Wisconsin's Act 10: Dynamic Difference-in-Differences Estimates



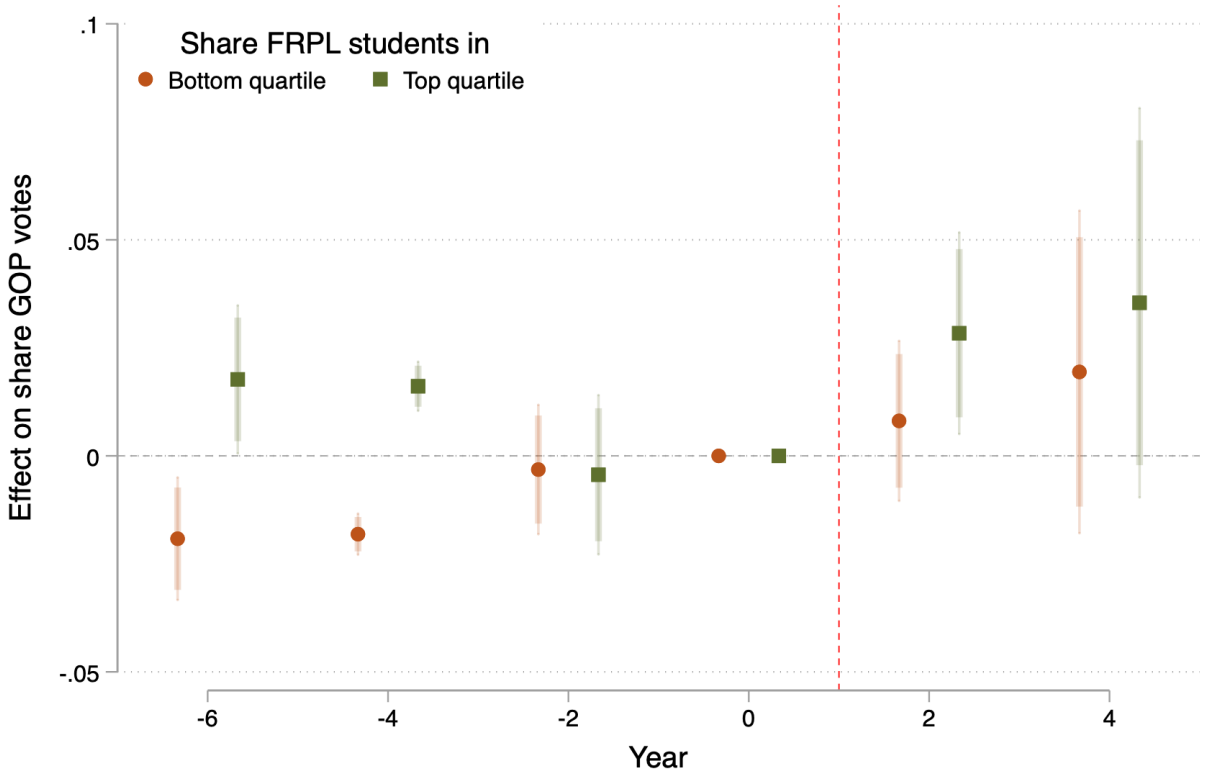
Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_s in equation (3), estimated on the full sample of districts (full circles), on the sample of districts that excludes Milwaukee Public Schools (hollow squares), and on the sample of wards that do not contain district borders (triangles). Confidence intervals are obtained using standard errors clustered at the district level.

Figure A8: Spillover Effects of Wisconsin's Act 10 onto Other Races: Event Study Estimates



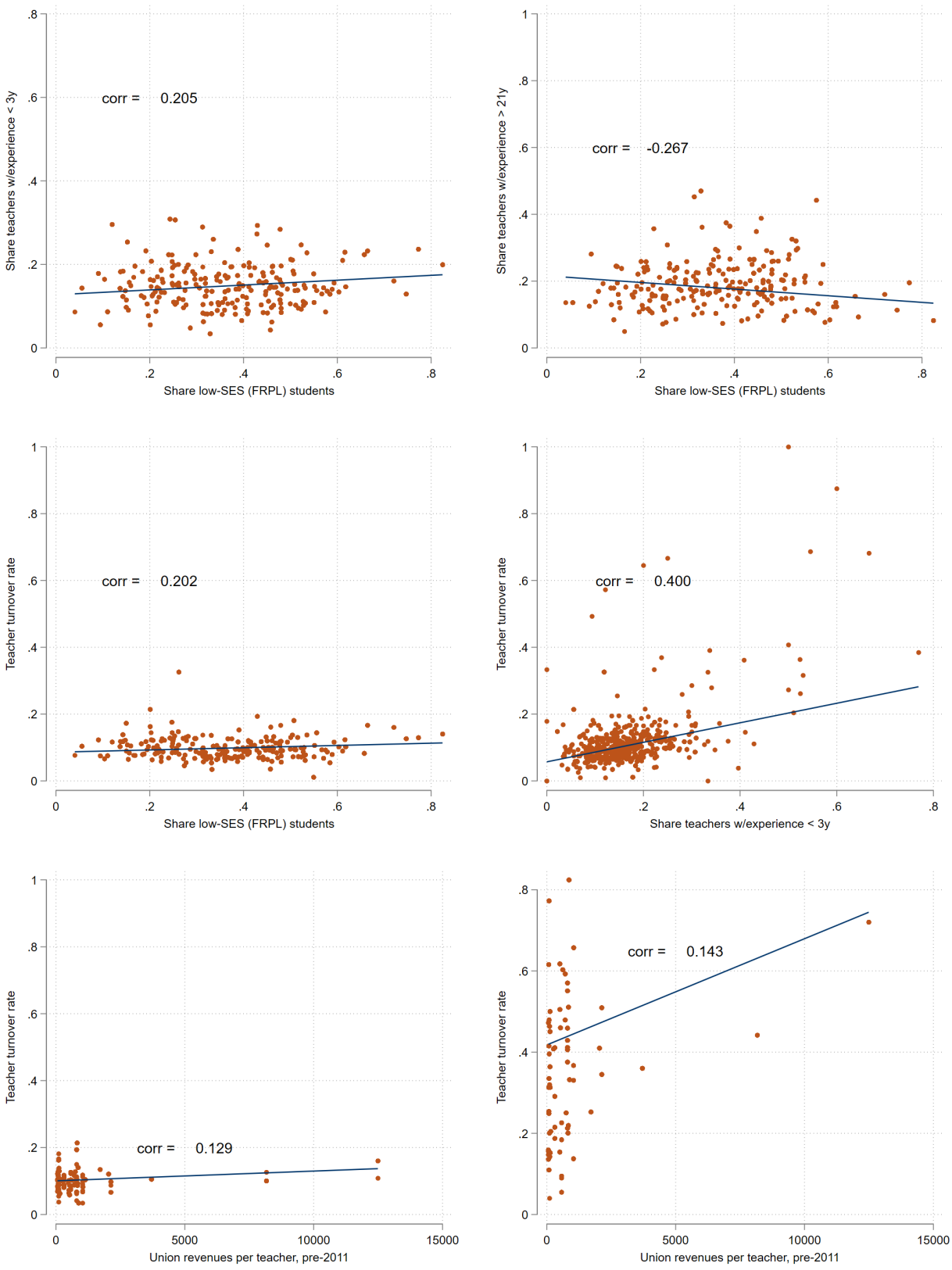
Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_s in equation (1), estimated using ward-level GOP vote shares for governor. Confidence intervals are obtained using standard errors clustered at the district level.

Figure A9: The Impact of Act 10 on GOP Votes: Event Study Estimates, by District-level SES



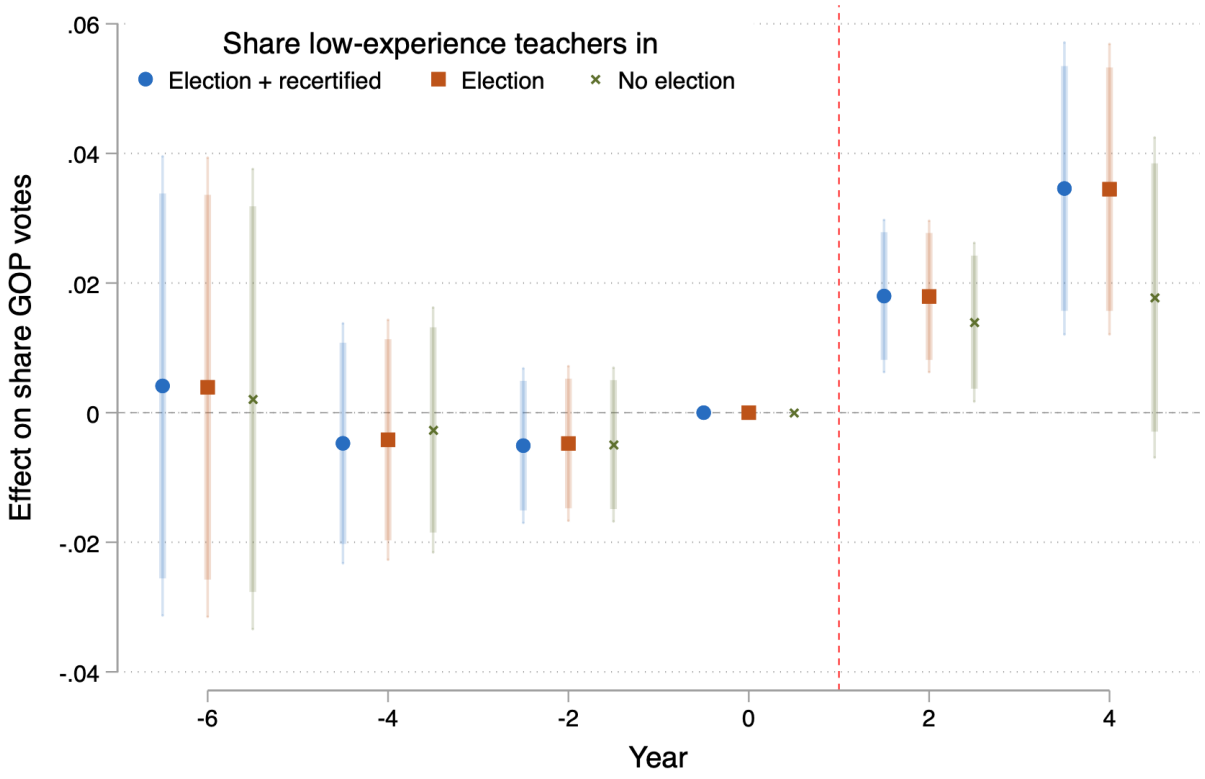
Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_s in equation (1), estimated using ward-level GOP vote shares in U.S. Senate, House, and Presidential elections (circles, squares, and diamonds, respectively). Confidence intervals are obtained using standard errors clustered at the district level.

Figure A10: Correlations Between District Characteristics



Notes: Correlations between various district characteristics.

Figure A11: The Impact of Unions on GOP Votes. Event-Study Estimates, by Ex Ante Union Revenues



Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_s in equation (1), estimated using the GOP vote share in gubernatorial elections in each ward and year as the dependent variable and controlling for ward and year fixed effects. In the circle series, we include only districts from the 2011 cohort that recertified in 2011 (and all the other cohort); in the square series, we include only districts from the 2011 cohort that held recertification elections in 2011 (regardless of the election result, and all the other cohort); and in the X series we include only districts from the 2011 cohort that did not hold any recertification elections. Confidence intervals are obtained using standard errors clustered at the district level.

Table A1: Wisconsin Wards and School Districts: Summary Statistics, By Presence of An Extension

		Expiration in	
	All districts	No extension	W/extension
Teachers			
Teacher turnover rate, pre-2011	0.100	0.10	0.099
	(0.030)	(0.050)	(0.032)
Teacher mkt tightness, pre-2011	1.07	1.13	1.04
	(0.39)	(0.48)	(0.37)
Share teachers w/experience $<$ 3y	0.15	0.15	0.15
	(0.051)	(0.065)	(0.049)
Share teachers w/experience $>$ 21y	0.18	0.19	0.17
	(0.067)	(0.076)	(0.065)
Students			
Share low-SES (FRPL) students	0.40	0.34	0.44
	(0.18)	(0.15)	(0.19)
Std. test scores, Math	-0.0030	-0.0059	-0.0011
	(0.018)	(0.022)	(0.015)
Population age			
Share pop $<$ 18 yo	0.23	0.23	0.23
	(0.067)	(0.061)	(0.071)
Political views			
Share GOP Governor votes, 2010	0.54	0.60	0.50
	(0.15)	(0.12)	(0.16)
Share GOP President votes, 2008	0.44	0.49	0.40
	(0.14)	(0.12)	(0.14)
100 * Donations pp to Dem	0.43	0.36	0.49
	(0.51)	(0.48)	(0.53)
100 * Donations pp to GOP	1.34	1.56	1.17
	(0.84)	(1.05)	(0.58)
Unions			
Teacher union held election in 2011	0.34	0.57	0.14
	(0.47)	(0.50)	(0.35)
Share yes votes to union, 2011	0.75	0.74	0.77
	(0.10)	(0.11)	(0.080)
Union revenues per teacher, pre-2011	733.9	479.1	879.6
	(1169.0)	(680.5)	(1375.2)
Number of wards	4,989	3,242	1,747
Number of districts	236	134	102

Notes: Means and standard deviations (in parentheses) of variables used in the model. The first column shows statistics on the full sample of districts included in the analysis; the second column restricts attentions to districts without a CBA extension; and the third column restricts attention to districts with a CBA extension.

Table A2: Political Effects of Wisconsin's Act 10: Pooled Event Study Estimates

	Senate		House		President	
	(1)	(2)	(3)	(4)	(5)	(6)
Exposed	-0.006	-0.005	-0.021	-0.020	-0.005	-0.005
	(0.004)	(0.004)	(0.013)	(0.013)	(0.006)	(0.006)
District FE	Yes	No	Yes	No	Yes	No
Ward FE	No	Yes	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var. control	0.409	0.409	0.467	0.467	0.448	0.448
N	24239	24234	35789	35782	19453	19448
Clusters (districts)	236	236	236	236	236	236
R-squared	0.78	0.93	0.64	0.71	0.72	0.92

Notes: The dependent variable is the share of GOP votes in U.S. Senate, House, and Presidential elections (columns 1-2, 3-4, and 5-6, respectively). We show estimates and standard deviations of the parameter β_k in equation (1), where we constrain $\beta_k = 0$ for $k < 0$ and β_k to be the same across all $k > 0$. Columns 1, 3, and 5 control for year and district fixed effects; columns 2, 4, and 6 control for ward and year fixed effects. Standard errors in parentheses are clustered at the district level. * ≤ 0.1; ** ≤ 0.05; *** ≤ 0.01.

Table A3: Political Effects of Wisconsin's Act 10: Difference-in-Differences Estimates, Robustness Checks

	Ignoring extensions	Excluding Milwaukee	Only fully aligned wards
	(1)	(2)	(3)
CBA after 2011 * post 2011	0.037*	0.010*	0.020*
	(0.020)	(0.006)	(0.010)
Ward FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Mean dep. var. control	0.465	0.489	0.438
N	24545	22930	14340
Clusters (districts)	236	235	231
R-squared	0.93	0.92	0.93

Notes: The dependent variable is the share of GOP votes in gubernatorial elections in each ward and year. We show estimates and standard deviations of the parameter β in equation (2), using as the treatment variable an indicator for districts with CBAs expiring in 2011, regardless of whether they were extended or not. Columns 1-2, and 4 are estimated on the sample of years until 2012; columns 3-6 on the sample until 2014. All specifications control for ward and year fixed effects. Column 1 is estimated considering only CBA expirations and ignoring extensions to construct the exposure variable. Column 2 is estimated excluding Milwaukee. Column 3 is estimated on the subsample of wards that do not contain district boundaries. Standard errors in parentheses are clustered at the district level. * ≤ 0.1; ** ≤ 0.05; *** ≤ 0.01.

Table A4: Political Effects of Wisconsin's Act 10: Pooled Event Study, By Share of Young (age 27 and below) and Old Teachers (age 55 and above)

Panel (a): By share of teachers aged 27 and below in district						
	$\geq$ 75pctile	25-75pctile	$\leq$25pctile	$\geq$ 75pctile	25-75pctile	$\leq$25pctile
	(1)	(2)	(3)	(4)	(5)	(6)
Exposed	0.009	0.008*	0.008	0.009	0.009**	0.007
	(0.007)	(0.004)	(0.009)	(0.007)	(0.004)	(0.009)
District FE	Yes	Yes	Yes	No	No	No
Ward FE	No	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var. control	0.524	0.499	0.492	0.524	0.499	0.493
N	5032	9830	5086	5028	9828	5085
Clusters (districts)	70	110	55	70	110	55
R-squared	0.70	0.75	0.66	0.92	0.94	0.88

Panel (b): By share of teachers aged 55 and above in district						
	$\geq$ 75pctile	25-75pctile	$\leq$25pctile	$\geq$ 75pctile	25-75pctile	$\leq$25pctile
	(1)	(2)	(3)	(4)	(5)	(6)
Exposed	0.013	0.009**	0.006	0.013	0.010**	0.006
	(0.009)	(0.004)	(0.009)	(0.009)	(0.004)	(0.009)
District FE	Yes	Yes	Yes	No	No	No
Ward FE	No	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var. control	0.518	0.497	0.501	0.517	0.497	0.501
N	4811	10123	5014	4809	10119	5013
Clusters (districts)	71	108	56	71	108	56
R-squared	0.64	0.76	0.69	0.92	0.94	0.90

Notes: The dependent variable is the share of GOP votes in gubernatorial elections in each ward and year. We show estimates and standard deviations of the parameter β_k in equation (1), where we constrain $\beta_k = 0$ for $k < 0$ and β_k to be the same across all $k > 0$. In panel (a), we split the sample by the quartile of the share of teachers aged 27 and younger in 2010-2011. In panel (b), we split the sample by the quartile of the share of teachers aged 55 and older. In both panels, columns 1 and 3 show estimates for the top quartile, columns 2 and 5 for the 2nd and 3rd quartile, and columns 3 and 6 for the bottom quartile. Columns 1-3 control for district and year fixed effects, and columns 4-6 control for ward and year fixed effects. Standard errors in parentheses are clustered at the district level. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.