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. It also reduced Democratic campaign contributions, especially among those who initially opposed the law. Electoral gains were driven by districts with *ex ante* stronger unions and more potential winners among teachers and students.

JEL Classification: I20, P46, P11, J31, J45

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1 Introduction

The political effects of education reforms—defined as shifts in political endorsement for their proponents attributable to the reforms themselves—are of first-order importance to determine the success of these policies. Efforts to counteract stagnant achievement or unequal access often provoke vocal opposition from groups invested in the status quo, while the benefits tend to be ambiguous and diffuse (Olson, 1965). As a result, risk-averse public officials may rationally choose to uphold the status quo. Accountability, teacher performance pay, and school choice are just a few examples of public education policies which have generated enough controversy to discourage adoption, even when empirical evidence is promising. Without political support, even effective programs risk being diluted or abandoned in response to backlash (Bold et al., 2018).

Despite their potential significance, the political effects of education reforms have remained understudied, especially within the U.S. context. Several obstacles to identifying the causal effects of reform on political outcomes likely contribute to this gap in the literature. First, policies are not crafted in isolation. Education reforms are often packaged with other policies or correlated with factors that shape voters' political support, such as shifts in government budgets or broader economic conditions. Second, many education policies fail to achieve their intended effects or are never rigorously evaluated, making it difficult to assess or interpret downstream political effects. Finally, because education reforms are typically implemented statewide, there is often insufficient exogenous variation to isolate their causal impact on electoral outcomes, let alone to understand what drives these impacts.

We address these challenges by examining the political effects of Act 10, a controversial reform championed by a Republican governor in Wisconsin in 2011 that significantly curtailed the powers and resources of teachers' unions. Two features of this setting make it particularly well suited for our analysis. First, the reform's provisions could only be implemented after the expiration of each districts' pre-existing collective-bargaining agreements (CBAs), creating quasi-random variation in voters' exposure to the consequences of the reform. Second, the reform's passage was followed by a gubernatorial recall election — widely viewed as a referendum on Act 10 — as well as a regular election two years later.

We show that the reform was a political success for the incumbent governor, increasing his vote share by 3% in these elections (roughly 20% of his margin of victory). These political gains were larger in areas with ex ante stronger and more politically involved unions. They were also larger in districts that gained more from the reform, such as those with a larger share of teachers who experienced pay increases and students who stood to gain more from the policy change. Our findings highlight the importance of both concrete policy outcomes and the role of institutional actors in shaping the political consequences of the reform.

Act 10 weakened public sector unions' ability to bargain collectively, including over salaries, and the majority of public sector workers were teachers.[1] The law prompted waves of protests, culminating in a recall election against the governor in June of 2012, perceived by many as a referendum on Act 10 (Biskupic, 2013). The governor won the recall, then won reelection in the regular gubernatorial election of 2014. Ward-level electoral data from these two elections provide us with a clear and granular measure of the electorate's support for the elected official most strongly associated with Act 10.

Our empirical strategy to identify Act 10's political effects builds on the staggered implementation of the reform across Wisconsin school districts. The changes introduced by the new law only took effect after districts' pre-existing CBAs had expired. Expiration dates differed across districts due to long-standing differences in negotiation calendars, generating useful exogenous variation in the timing of implementation of the law and in voters' exposure to Act 10's consequences. We exploit this variation in an event-study framework. We complement these estimates with a difference-in-differences comparison of districts exposed to Act 10 prior to the 2012 election vs. districts whose agreements expired afterwards.

We implement this strategy using data from various sources. Our primary outcome is the share of votes for each party's candidates in state elections between 2002 and 2014, measured at the ward level. We geolocate wards inside school districts to link these data to district CBA expiration dates, which we hand-collected from union contracts, school board minutes, and newspaper articles. Data on individual campaign contributions provide another measure of political support (Bonica, 2023). 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' financial

[1] Fire and police unions were exempt from the law.

records (such as total revenues and dues collected) from their IRS Form 990.

We begin by showing the effect of Act 10 on unions, teachers, and students. Union were hurt by the law: revenues per member fell by over 50% following its implementation, a result likely attributable to a decline in the number of teachers actively paying dues after the end of automatic collection. The decrease was most pronounced in districts with higher dues per member ex ante. Among teachers, the reform created clear sets of winners and losers. By giving districts the capacity to set teacher pay based on factors other than seniority and academic credentials, the law flattened the relationship between salary and experience. As a result, it led younger and less credentialed teachers to earn more on average, and older, more experienced teachers to earn less than they would have prior to the reform. Students broadly benefited from Act 10. Four years after the expiration of a district's CBA, test scores had increased by 0.15 standard deviations (sd), a result primarily driven by economically disadvantaged students. A possible explanation for this finding is that pay flexibility helped districts attract and retain high-quality teachers (Biasi, 2021).

Turning to its political effects, we find that Act 10 significantly contributed to Walker's success at the ballot box. Event-study estimates indicate that 2-4 years of voter exposure to the consequences of the reform increased the share of votes for the GOP governor by 1.4 pp. These effects represent a meaningful fraction (20%) of the margin of victory in the 2012 and 2014 elections. Reassuringly, trends in GOP vote shares were parallel among districts exposed to the reform at different times. These results are corroborated by difference-in-difference estimates, which show that districts already exposed to the reform in 2011 saw the GOP vote share in the 2012 recall election rise by about 2 pp relative to not-yet-exposed districts. These electoral effects coincided with a decline in grassroots enthusiasm among Democrats as measured by individual campaign contributions. Event-study estimates show that exposure to the law caused a sharp decline in the number of individual donations to Democratic candidates — 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.

Since Act 10 was a statewide reform, the sizable political effects of exposure to the law that we document may appear surprising. Even voters who were exposed later likely already knew about the changes introduced by the reform in 2011. We argue that our findings can be rationalized by most people holding rather negative priors of the reform's effects and then positively updating their beliefs upon exposure. This argument is supported by three pieces of evidence. First, a sentiment analysis of news reports of Act 10 in 2011-12 shows that such reporting was overwhelmingly negative. Second, the effects of exposure were stronger in ex ante more Democratic wards, where people were likely to hold more negative priors on the reform. Lastly, the post-exposure reduction in Democratic campaign contributions was especially pronounced among the 900,000 people who had signed the petition to initiate the recall election, and who presumably most strongly opposed Act 10 when it was signed into law.

In the final section of the paper, we investigate the factors that may explain the political effects of Act 10 by comparing Governor Walker's electoral gains across school districts with varying pre-reform characteristics. These gains were not uniformly distributed; rather, they were larger in districts with a stronger union presence and a larger constituency who gained from the reform. Specifically, we find that electoral benefits were significantly greater in districts where unions had previously provided financial support to Democratic campaigns (a proxy for political involvement) and charged higher membership fees (an indicator of union strength). Similarly, effects were larger in districts with a higher share of young, less experienced teachers (who experienced salary increases after Act 10) and in areas with higher teacher turnover (which stood to benefit more from the introduction of flexible pay — see Biasi, 2021). Finally, districts with a greater proportion of disadvantaged students, whose test scores improved the most post-reform, also saw larger electoral gains. Notably, these union, district, teacher, and student characteristics are only mildly correlated with one another, suggesting that they capture distinct dimensions of the response to the reform. Using machine-learning techniques, we find that electoral gains were generally larger in [XXXDESCRIBE PLACES], suggesting XXX.

Overall, our results indicate that controversial education reforms such as Act 10 can generate electoral benefits for the politicians and parties that champion them. Our findings support the hypothesis that unions play a pivotal role in shaping both policy and politics (Anzia, 2011), and they reveal a strong link between a reform's electoral gains and its varied impacts on different stakeholders. Back-of-the-envelope calculations suggest that our estimates are reasonable in magnitude, given the share of the electorate that consists of parents and teachers. Although we do not observe electoral spillovers into other (non-gubernatorial) contests, we do find a correlation between the district-level effects of exposure to Act 10 on Walker's vote share and the 2016 vote share for GOP presidential candidate Donald Trump. This relationship implies that the political consequences of education reform may be long-lasting.

Contribution to the literature Our paper contributes to several strands of the literature. The first has studied reforms of collective bargaining, teachers' labor markets, and teacher pay. Most of this work has focused on the implications for students and has found mixed effects. Some studies on teacher unionization found no effects on student outcomes (Lovenheim, 2009), while others have shown negative effects (Hoxby, 1996; Lovenheim and Willén, 2019; Foy, 2024a). Studies of changes in teacher performance pay have also reached contrasting conclusions (see Neal, 2011; Pham et al., 2021, for reviews). Unionization and performance pay tend to be politically contentious policies (Dee and Wyckoff, 2015); in recognition of these constraints, some implemented performance pay programs have opted to provide bonuses for all teachers to avoid creating a set of losers (Leaver et al., 2021). We extend this literature in an understudied yet important direction: we causally identify and estimate the impacts of a large-scale change in collective bargaining rules and teacher compensation on political outcomes, vital dimensions of the feasibility of reform. Additionally, our findings also contribute to the literature on the effects of Act 10 on teachers and students (Baron, 2018; Biasi, 2021; Biasi et al., 2021; Biasi and Sarsons, 2022; Biasi, 2024; Foy, 2024a). In particular, we demonstrate that weakening the bargaining power of teacher unions and introducing pay flexibility for teachers can lead to important increases in student learning, especially among disadvantaged students.

Our study also informs the literature on the electoral effects of public policies. Research on voters' reactions to general fiscal policy has found mixed results. Brender and Drazen (2008) find no strong cross-country correlation between spending and incumbent reelection; Carreri and Martinez (2021) find that voters in Colombian municipalities reward the reduction of wasteful spending; and Fetzer (2019) shows that UK voters punished austerity. Studies on the electoral effects of specific policies show that large, salient infrastructure projects tend to win votes (Huet-Vaughn, 2019; Butler and Boudot Reddy, 2023; Voigtländer and Voth, 2021; Harding, 2015; Leff Yaffe et al., 2023; Garfias et al., 2021), as do direct redistributive transfers (Manacorda et al., 2011; De La O, 2013). This is consistent with theoretical predictions of relatively large electoral effects for more visible projects

(Mani and Mukand, 2007). Our paper contributes to this literature by showing that voters can notice and reward a policy that improves the quality of public services — even in the absence of visible capital investments, and even when it reduces overall public spending. It also demonstrates that the size of private benefits can be an important driver of the way people vote (Méndez and Van Patten, 2022).

Lastly, our paper contributes to a 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 the object of empirical inquiry. A few existing studies examine the voting outcomes of education policies (Sandholtz, 2023; Dias and Ferraz, 2019; Litschig and Morrison, 2013; Cox et al., 2024). 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. We contribute to this literature by focusing on a setting that allows us to causally identify an education reform's political effects and shed light on what drives political support for it. By studying a reform of public-sector collective bargaining, we also contribute to the research on the role of unions in shaping public policies and electoral outcomes (Anzia and Moe, 2015; Hartney and Kogan, 2024). Our results indicate that support for Act 10 was related to the fall in union powers, confirming that unions can act as obstacles for the successful implementation of education policy reform (Moe, 2011; Hartney, 2022).

2 Institutional Background: Wisconsin Before and After Act 10

2.1 Wisconsin Public Schools and Teachers' Unions Before 2011

Public K-12 schools in Wisconsin enroll approximately 820,000 students each year and employ 60,000 full-time equivalent teachers. Compared to the national average, the student body is less diverse and more socioeconomically advantaged: 76% of students are white (compared with 52% nationwide) and 37% were eligible for a free or reduced-price lunch in 2011 (compared with a national average of 48%, U.S. Department of Education, 2011). In 2011, fourth-grade students outperformed the national average in both math and reading. The average teacher salary was $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 (including school teachers) the right to collectively bargain, following legislation promoted by Democratic Governor Gaylord Nelson (Stein and Marley, 2013). Since then, public-sector unions have wielded significant influence, playing a central role in shaping the working conditions of state employees.

Among the larger public-sector labor organizations are teacher unions, representing approximately 20% of all state and local employees in Wisconsin. Their primary function is to negotiate with school districts on behalf of teachers over issues such as pay, safety, scheduling, and benefits. Prior to Act 10, unions also negotiated teachers' base pay and steps-and-lanes salary schedules, which determined pay based on seniority and academic credentials. This system ensured that teachers with comparable seniority received the same pay and that each teacher received an annual raise, thereby limiting employer discretion in setting salaries.

2.2 Act 10

In February 2011, Wisconsin's public-sector union landscape changed abruptly when the Republican state legislature, led by Governor Scott Walke, passed the Wisconsin Budget Repair Bill, commonly known as Act 10. Designed to close a projected $3.6 billion budget deficit, the legislation substantially restructured both the influence of public-sector unions and the mechanisms for determining teacher pay and fringe benefits.

First, Act 10 imposed significant operational constraints on unions. It requires annual recertification elections, with each bargaining unit needing a majority vote to continue union representation, and it prohibited the automatic deduction of union dues from employees' paychecks. As a result, union membership fell by over 30% between 2011 and 2015 (Schulz, 2023).

Second, the reform sought to reduce school districts' costs by curbing the growth of teacher salaries and increasing the financial contributions teachers make towards fringe benefits. Notably, it ended collective bargaining over salary schedules, enabling school districts to set individual teacher pay rather than strictly following predetermined scales. In addition, Act 10 capped the annual growth of negotiated base pay to the rate of inflation. The law also required teachers to pay half of their total pension contributions (amounting to 5.8% of their annual pay, compared with a previous rate of zero), and 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, these provisions did not take effect immediately. Existing collective bargaining agreements (CBAs) between school districts and unions, signed prior to Act 10, remained in force until their expiration. Act 10's changes could therefore only be applied after a CBA expired. Due to historical factors, expiration dates varied across districts. Among 247 districts for which CBA expiration dates are available (see Section 3), 198 CBAs expired in 2011, 20 in 2012, and 7 in 2013. Some districts extended the validity of their pre-existing CBAs by one or two years; when acounting for these extensions, 109 CBAs expired 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 highly controversial. Protests erupted in Madison before the bill was voted and quickly escalated; by February, an estimated 100,000 demonstrators had occupied the State Capitol and continued to protest for weeks.[2] The unrest forced the closure of schools in Madison, as many teachers called in sick to join the protests. In an unprecedented move, a group of Senate Democrats even left Wisconsin in an effort to stall the bill's approval. Despite these dramatic events, the State Assembly passed Act 10 on March 10, 2011, and the Governor signed it into law the following day.

Efforts to overturn the legislation and counter Scott Walker's anti-union agenda persisted until 2012. A lawsuit challenging the bill's unconstitutionality, on the grounds that its fiscal provisions violated state law, was filed in March 2011. Although protests continued outside the State Capitol, the State Supreme Court ultimately upheld Act 10 in June 2011; protesters had continued demonstrating outside the State Capitol until that point. Attempts to reverse Act 10 reached its climax with a gubernatorial recall election in June 2012, which saw Scott Walker oppose Democrat Tom Barrett, who had already run for governor in 2010; close to a million people signed the petition (in 2011, Wisconsin had a population aged 18 and above of 4.2M). Walker defeated Barrett by 7 pp, becoming the first governor in U.S. history to win a recall election. He then secured reelection in 2014

[2] See "Union Changes In Wisconsin Spark Protests" by Shawn Johnson, National Public Radio, February 16, 2011. Available at https://www.npr.org/2011/02/16/133814271/union-changes-in-wisconsin-spark-protests.

against Democrat Mary Burke with a 6-percentage-point victory margin, before ultimately being unseated in 2018 by then-Superintendent of Public Instruction Tony Evers. As Act 10 was Walker's signature policy, the 2012 and 2014 elections offer a unique opportunity to evaluate the political consequences of the bill on his proponent's election odds.[3]

3 Data

Our empirical analysis uses a dataset compiled drawing data from multiple sources. We describe here each set of variables, along with their source.

3.1 Electoral Results

Our primary outcome are vote shares for the Republican candidate in gubernatorial and federal elections held between 2002 and 2014. Our main analysis focuses on gubernatorial race; we examine Presidential, U.S. House, and U.S. Senate races in robustness checks. We draw information on vote counts 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

To determine when school districts became subject to the consequences of Act 10, we use a hand-collected dataset (previously employed in Biasi, 2021; Biasi and Sarsons, 2022) containing the expiration dates of each district's pre-Act 10 collective bargaining agreement (CBA) as well as any extensions thereof. The dataset, described in greater detail in Biasi and Sarsons (2022), was assembled from multiple sources, including union contracts, districts' employee handbooks, school board

[3] Searching for the term "Scott Walker" on Google Trends for the period 12/01/2010 to 12/01/2018 (the period Walker was in office) yields "2011 Wisconsin Act 10" as the third most searched related topic, preceded by "Recall election," "Trade union," and "Tony Evers."

meetings minutes, and local news reports.[4] When available, we prioritize information from union contracts, school board minutes, and handbooks. In cases where these documents are unavailable, we supplement with information from online local news sources.

The database of CBA expirations covers 247 of the 428 districts in the state, enrolling 70% of all students. While most school districts in the state are "unified" (i.e., they oversee education at both the primary and secondary level), a few operate solely at the elementary or secondary level. Since elementary and secondary districts may geographically overlap, we focus exclusively on unified school districts to avoid ambiguity in treatment status. Our final dataset covers 236 unified school districts with CBA information, spanning 4,989 wards. Figure A2 shows the location of districts for which expiration dates are known.

3.3 Teacher Records

We complement our data on vote shares and CBA expirations with detailed individual-level information on public-school teachers. Our teacher data come from the PI-1202 Fall Staff Report – All Staff Files maintained by the Wisconsin Department of Public Instruction (WDPI) and span the years 2006–2017. These files provide annual records for all WDPI employees, including full name, birth year, hiring agency identifier (typically the school district), working agency (usually the school), job position (e.g., teacher), full-time equivalency (FTE) units, total gross salary, highest degree earned, and years of teaching experience. We focus exclusively on regular teachers, excluding long- and short-term substitutes and subcontractors. We use the unique teacher identifier available in the dataset to calculate teacher turnover at the school district level. To facilitate comparability, we express salaries in FTE units.

3.4 Student Records

We also link CBA information with records from the state longitudinal student system, which contains individual-level data on student demographics (gender and race/ethnicity), the school and grade attended in each year, eligibility for a free or reduced-price lunch (our proxy for socioeco-

[4]Union contracts typically specify the CBA expiration date directly. Post-Act 10 school board minutes often note whether a contract was scheduled to expire in 2011, as boards needed to address related decisions. Early versions of district employee handbooks also help establish when the new post-CBA pay regime was implemented.

nomic status), and Math test scores on the state standardized exam.[5] This dataset covers the years 2006-2016.

3.5 Census Data

We construct the age distribution of each ward's population from decennial Census records. Specifically, we aggregate population counts from the census block level to the ward level by assigning each block to the ward in which its centroid is located.

3.6 Campaign Contributions

We obtain data on political campaign contributions from the Database on Ideology, Money in Politics, and Elections (DIME, Bonica, 2023). For each contribution, the dataset lists the recipient's and contributor's name, address, and occupation. We restrict our analysis to contributions made to Wisconsin gubernatorial races that originate within the state. Using geolocated addresses, we assign each contribution to the corresponding school district. Contributor names and occupations also allow us to identify donations made by teachers and teacher unions. Specifically, we compute total contributions and those made by teachers (based on occupation information) and by teachers' unions (by performing fuzzy matching of contributor names against union names provided by the Wisconsin Employment Relations Commission).

3.7 Recall Petition Signers

Over 900,000 Wisconsin residents signed the petition to initiate the 2012 special election to recall Governor Scott Walker—46% of the total votes cast in the 2010 gubernatorial election and one out of every five voting-age residents. The state agency overseeing the process of verifying signatures, the Government Accountability Board (GAB), made these signatures available online in PDF format. Conservative organizations crowdsourced an effort to digitize these documents, posting the resulting names on a searchable website called iverifytherecall.com. The GAB subsequently released its own searchable website of petition signers in March of 2012 (Foy, 2024b). We match these

[5]Wisconsin schools administered the Wisconsin Knowledge and Concepts Examination (WKCE) for the years 2007–2014 and the Badger test for the years 2015–2016. The WKCE was held in November of each school year, whereas the Badger test was took place 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.

databases to campaign contribution data as a measure of contributors' *ex ante* opposition to Act 10 and Governor Walker. We designate campaign contributors as petition signers if and only if their names are a precise match to a name which appears in one of the aforementioned databases. By this measure, 4% of petition signers' names match to the name of a campaign contributors to the Wisconsin gubernatorial election in 2012, and 29% of the Wisconsin campaign contributions to that race came from individuals whose name matched that of a petition signer. As expected, the vast majority of contributions from petition signers were to the Democratic candidate.

3.8 Union Finances

We obtain measures of union finances from tax records. The Internal Revenue Service requires most tax-exempt organizations, including labor unions, to file Form 990 (the "Return of Organization Exempt From Income Tax")., which reports revenues, expenses, assets, and liabilities. We accessed a database of digitized Form 990s provided by the National Center for Charitable Statistics (NCCS) of the Urban Institute (2007-2016) and searched Wisconsin union names as they appear in WERC. Through this process, we linked 99 districts to the records of 52 unions. As a measure of union strength prior to the passage of Act 10, we compute revenues per member as total revenues (primarily from membership dues) divided by the total number of teachers in the districts represented by each union.

3.9 Summary Statistics

Table 1 shows summary statistics of the main variables in our sample. The sample contains 134 districts whose CBAs expired in 2011 and was not 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, early and late-treated districts 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. 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 Act 10's provisions on the political support for Governor Scott Walker, the principal advocate of the reform. Walker was elected in 2010 and introduced the legislation that later became Act 10 in February 2011. Our empirical strategy makes use of the variation in the timing of collective bargaining agreement (CBA) expirations. By June 2012 (when Wisconsin held a gubernatorial recall election), the CBAs pre-dating Act 10 had expired in 134 districts (3,242) wards but were still active (or had been extended) in 102 districts (1,747 wards). Hence, in those areas, the provisions of Act 10 had not yet gone into effect by the time people voted. Moreover, in 5 of these districts (202 wards), the pre-Act-10 CBAs were still active by 2014, when Walker ran for reelection, providing an additional opportunity to measure voters' responses to the reform.

Event study Our primary identification strategy exploits the staggered expiration of CBAs across districts within an event study framework. We estimate 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} denotes the share of votes to the GOP governor in ward j in year t and E_d represents the year of expiration of district d's CBA (or its 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 change in 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 in more detail later. Since the treatment (i.e., the year of CBA expiration) is assigned at the school district level, we cluster standard errors at that level.

Because Act 10 ultimately affected all districts, we do not have a "never treated" group. The model in equation (1) is thus identified by comparing early-treated districts with those treated later. This approach may yield biased estimates if treatment effects vary systematically with the timing of the treatment (Sun and Abraham, 2021; Callaway and Sant'Anna, 2021; Borusyak et al., 2024). To assess the sensitivity of our estimates to such heterogeneity, we perform robustness checks using the estimator proposed by Sun and Abraham (2021), which produces unbiased estimates in the presence of heterogeneous treatment effects.

Difference in differences: We complement our event study estimates with those from a simpler difference-in-differences model, which compares the share of votes for the GOP governor over time between districts whose CBAs (or extension) had expired in 2011 (the "treated" group) and those with active CBAs in 2012 (the control group). We estimate the following model via OLS:

$$V_{jt} = \beta D_{d(j)} \mathbb{1}(t > 2011) + \theta_j + \tau_t + \varepsilon_{jt} \tag{2}$$

where D_d equals one if district d's CBA expired in 2011 without extension and all other variables are defined as before. We consider all gubernatorial elections held between 2002 and 2018. The parameter of interest is β, which captures the differences in the share of votes for the GOP governor between treated and control districts after 2011, relative to the pre-reform period.

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

$$V_{jt} = \sum_{\substack{t \in \{2002,2006,\\ 2012,2014,2018\}}} \beta_s D_{d(j)} \mathbb{1}(t = s) + \theta_j + \tau_t + \varepsilon_{jt} \tag{3}$$

Normalizing β_{2010} to zero, the parameters β_s estimate the difference in GOP gubernatorial votes between treated and control districts in year s, relative to the difference in 2010.

Identifying assumptions Our empirical strategy relies on the assumption that, absent Act 10, outcomes in districts with CBAs that expired at different points in time would have followed similar trends. This assumption is primarily supported by the idiosyncratic nature of CBAs' expiration dates. Table 1 shows no meaningful observable differences between districts with varying expiration dates.

Our analysis considers the timing of the CBA extensions (not just the original expirations), motivated by evidence that Act 10 was implemented only after these extensions ended (Biasi and Sarsons, 2022). One potential concern with this choice is that districts may have extended the validity of their agreements in ways that correlate with changes in the political attitudes 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 free and reduced-price lunch (FRPL) students, lower support for the GOP in pre-Act 10 elections (as measured by the vote share and campaign donations to each party), and stronger unions pre-reform (as measured by revenues per teacher prior to 2011).

Despite these level differences, we show later that the both groups followed similar trends in our outcome variables in the years leading up to each expiration. Moreover, our findings remain qualitatively similar when we replicate our main results by classifying districts solely based on the original CBA expiration dates, ignoring extensions (see Table 5, column 1, for the pooled event study and Appendix Table A5, column 1, for the difference-in-differences analysis).

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

To contextualize our analysis of the political effects of Act 10, we begin by demonstrating its impact on the Wisconsin's educational system, with a focus on unions, teachers, and students.

5.1 Effects on Teacher Unions: Revenues and Political Participation

Act 10 significantly curtailed union powers and made it significantly harder for them to operate. The reform prohibited unions from collecting automatically membership dues and mandated annual recertification elections with a favorable majority of all members necessary to recertify (not just those present at voting). Using data from the Current Population Survey, Baron (2018) documents that the enactment of the law was followed by a decline in public sector union membership in the.

To further document how Act 10 impacted unions, we use unions' revenues per members—extracted from tax forms—as measures of union strength. Revenues experienced a dramatic decline following the implementation of the reform. Estimates of the parameters β_k in equation (1) with $k \in [-4, 4]$, obtained each district's union revenues per member as the dependent variable, show 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, Figure **??**, hollow circles, significant at 1%). Not all unions were affected in the same way. Unions with ex ante revenues per member above and below the state median saw a similar proportional decline (Figure **??**, full markers), which implies that the loss in revenues was larger in absolute terms in ex ante richer (and stronger) unions (Appendix Figure A3).

A loss in union revenues also curtailed unions' ability to participate to politics. We show this using information on contributions made by unions to gubernatorial races over time. Estimates of β_k in equation (1), obtained using the number of campaign contributions made by a district's union in each year, show that virtually all union contributions (0.002 per 1,000 people in the district) were to the Democratic party, both before and after Act 10. However, 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). XXXADD FIGURE HERE

5.2 Effects on Teacher Compensation

By prohibiting teacher unions from engaging in collective bargaining over teacher salary schedules with each school district, Act 10 dramatically changed how teachers pay is se and gave districts full discretion over teacher pay. Under the pre-reform salary schedule, teachers were rewarded for their seniority and academic credentials. The law thus made it possible for some teachers with high experience and credentials to see slower (or even negative) salary growth while less experienced and credentialed teachers could benefit.

To study the average impacts of Act 10 on teacher compensation, we estimate equation 1 using individual teacher salaries and benefits as the outcome variable. Estimates of β_k indicate that teacher salaries dropped significantly, by approximately $1000 in the first year after the CBA's expiration (Appendix Figure A4, circles series). Teachers' fringe benefits declined even more, by around $2000 (Appendix Figure A4, panel (b), squares series).

Although average compensation fell, the effect was not uniform across teachers. To better identify the winners and losers from Act 10 in terms of teacher salaries, in panel (a) of Figure 1 we plot the natural logarithm of teachers' 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; for example, 57-year-olds earned approximately 88% more than 24-year-olds. Similarly, the salary profile increased sharply 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.[6]

These results illustrate how Act 10 created clear winners and losers in terms of salaries based on teacher age and experience. In Section 7, we link the distribution of benefits for teachers across Wisconsin districts to the political consequences of the reform.

5.3 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 the direct consequences of the reform on student learning, measured by standardized student test scores in Math. Specifically, we estimate a version of equation (1) with individual-level scores of grade 3-8 students as the outcome variable, controlling for lagged test scores, 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. Relative to the expiration year, scores increased by 0.05 standard deviations (sd) two years after a CBA expiration an 0.17 sd higher level five years later (Figure 1, panel (b), solid line). The improvement was more pronounced for FRPL-eligible students, whose scores rose 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 findings align with Biasi (2021), who reports similar but smaller results using school-level average test scores. In contrast, Baron (2018) finds negative short-run effects on aggregate high-school test scores. Our analysis differs from Baron's by focusing on elementary and middle school students and by examining a

[6] Appendix Figure A5 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.

longer five-year time horizon after a CBA expiration.

In theory, 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 only a small decline in the enrollment shares of FRPL and minority students (Appendix Figure A6).

Taken together, the results from this Section indicate that Act 10 reduced union powers and revenues, rewarded some teachers at the expense of others, and raised student test scores. We return to these findings when discussing the political consequences of Act 10, which we investigate next.

6 The Political Effects of Wisconsin's Act 10

To 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 present event-study estimates and probe the robustness of our findings to different empirical models and assumptions.

6.1 Effects on The Share of Votes to the GOP Governor

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 and controlling for ward fixed effects. 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 53.1% vote share for Walker and a 46.3% 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, where we pool data before and after each expiration assuming $\beta_k = 0$ for $k \leq 0$ and a constant 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 (ie., 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.[7]

Difference-in-differences As an alternative empirical strategy, we estimate difference-in-differences models that consider districts with CBAs and extensions expiring in 2011 as the treated group.[8] Results from this model confirm findings from 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, in which Walker also won (columns 3 and 4). The estimates become larger (at 5.3 pp) when we include 2014 and limit the control group to wards in districts whose agreements expired in either 2014 or 2016 (columns 5 and 6).

To examine pre-trends and the dynamics of the political effects of Act 10, Appendix Figure A8 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 2006. It then increased (although insignificantly) between 2006 and 2010; this pre-trend, though, appears driven by the school district of Milwaukee and disappears if we exclude this district (as we show in the square series of Figure A8). The share of votes to Walker increased further in treated

[7]Pooled event study estimates considering the 2014 and 2016 cohorts as never treated are shown in columns 3 and 4 of Table 2.

[8]Appendix Figure A7 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.

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.

6.2 Effects on Campaign Contributions

Next, we examine the impact of the law on campaign contributions to gubernatorial races made by individual 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)) shows estimates of the parameter β_k in equation (1), obtained using district-level counts of contributions to each party per 1,000 people as the dependent variable (pooled estimates are in columns 1-2 of Table 4). 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 (unweighted estimates are largely unchanged). Estimates imply that 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 pre-reform) and to 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.

The drop in campaign contributions to the Democratic party was even larger among teachers, by 50.8 contributions per 1,000 teachers (a nearly 8-fold decrease, Figure 3, panel (b), and Table 4, 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.

6.3 Rationalizing Our Findings: Changes in Voters' Beliefs

Our results so far indicate that exposure to Act 10's consequences triggered an increase in political support for the GOP governor. At a first glance, these findings may appear surprising: All Wisconsin voters—including those in late-treated districts—knew that Act 10 would eventually be implemented, so one may expect an immediate electoral responses in all districts. However, we argue that although everyone knew about the policy, voters only learned about its real consequences and shifted their beliefs once the CBAs expired. Three pieces of evidence support this argument.

First, the media coverage of Act 10 was overwhelmingly negative, particularly in the aftermath of its passage. A sentiment analysis of national and local newspaper articles containing the words "Act 10" and "school" and published between March of 2011 and December of 2012, which we conducted using the large-language model ChatGPT 4.0, indicates that 51% of these articles portrayed the law negatively, 24% neutrally, and 25% positively (Appendix Figure A9).

Second, the electoral impacts of exposure to Act 10 were most pronounced in districts with a lower share of GOP votes in the 2010 gubernatorial election. Figure 4 shows estimates of β_k in equation (1) separately for districts with a 2010 share of votes for the GOP governor above and below the state median. Two years after a CBA expiration, the share of votes for Walker rose by 2.2% in districts with a 2010 share below the median, while it did not change in the rest of the state. This result is confirmed when we allow the effect of exposure to Act 10 to vary linearly with the 2010 GOP vote share (Appendix Table A2). This finding suggests that the reform persuaded votes who were not already strong supporters, rather than merely energizing existing supporters.

We confirm this conclusion with our third piece of evidence, which shows that even people who signed the petition to hold the 2012 gubernatorial recall election increased their relative support to Walker after gaining exposure to Act 10's consequences. Signing the petition is a predictor of individuals' opposition to Act 10 and Walker: Signers were three times as likely as non-signers to have contributed to the Democratic campaign in the 2010 election (52% vs. 17%). Yet, these contributions dropped after exposure. Estimates β_k in equation (1), obtained using partisan gubernatorial campaign contributions of petition signers and non-signers, show that negative effect of Act 10 on Democratic contributions was even larger for signers (a 6-fold decline, compared with a 2-fold increase for non-signers, Figure 5). This result further confirms that our main results are due to a shift in political attitudes towards the reform driven by direct exposure to its real consequences.

Turnout A potential mechanism for the rise in GOP votes after Act 10 is a change in voter turnout, for example due to a decrease in get-out-the-vote activities by teacher unions. However, our data do not support this channel. We constructed a measure of turnout by dividing the number of votes cast in each election by the adult population (age 18 and over) in each ward. Estimates of β_k in equation (1), using turnout as the dependent variable, are positive but insignificant for $k > 0$. The confidence intervals rule out impacts larger than 4.5 pp (or 10% of the control group mean; see pooled event study estimates in Appendix Table A3). Although we cannot test for changes in the composition of voters, these estimates do not support the idea that changes in voter mobilization are a major factor behind the electoral effects of Act 10. Instead, our results align with previous research showing that campaign activity can change vote shares even overall turnout remains unchanged (Spenkuch and Toniatti, 2018).

6.4 Spillovers onto Presidential, State, and House Elections

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

Nevertheless, the reform may have generated spillovers on other political races, such as federal presidential, house, or senate elections. We test this formally using the same event-study specification outlined in equation (1), using GOP vote shares in these races as the outcomes. Although the results tend to be imprecise, we generally do not observe the same positive effects as in gubernatorial races. In fact, estimates of β_k for $k > 0$ are negative, albeit imprecise (Appendix Figure A10 and Appendix Table A4). These findings suggest that the political effects of Act 10 were mostly confined to gubernatorial races. The one notable exception is the GOP vote share in the presidential race four years after Act 10, corresponding for most school districts to the 2016 presidential election. We discuss this finding in the conclusion.

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 5, columns 1-2, for the pooled event study and Appendix Table A5, columns 1-2, 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 5 for the pooled event study and column 2 of Appendix Table A5 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 5 and column 3 of Appendix Table A5 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 Political Effects: Benefits of Act 10

Our results thus far indicate that Act 10, although controversial, was a political win for its proponent. We now turn to the potential drivers of these positive effects. The hypothesis we test in this section is rooted in retrospective voting theory (Fiorina, 1978; Healy and Malhotra, 2013), which suggests that citizens cast votes based on their perceived gains from a policy. To test this, we examine how variations in the real-world impacts of Act 10 across districts relate to its differential political effects at the district and ward levels (shown in Appendix Figure A7). In particular, we focus on the law's consequences on teachers, students and their families, and school districts.

7.1 Benefits for Teachers: Salary Gains

Among all public-sector employees, teachers were most impacted by Act 10. Teachers are also a non-trivial voting bloc, accounting for roughly 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, younger and less experienced teachers (previously penalized by a seniority-based salary schedule) experienced a salary increase, whereas older and more experienced teachers (rewarded by such a schedule) experienced a decline.

To examine whether the GOP's electoral gains are related to the gains and losses experienced by teachers, we test whether districts with varying shares of teacher "winners" and "losers" exhibit different political effects. Specifically, we calculate the share of teachers in each district who, as of 2011, either (i) had three or fewer years of experience (average winners), or (ii) had 21 or more years of experience (average losers). We then re-estimate our main event studies on subsamples of districts with different shares of winners and losers.

Our estimates indicate that the increase in the GOP vote share in gubernatorial elections was driven entirely by districts with a high share of winners. Four years after a CBA expiration, districts with a share of winners above the state median saw the GOP vote share rise by 6 pp (equivalent to 85% of the vote margin, Figure 6, panel (a), square series), while districts with a winner share below the median experienced no significant change (hollow circle series). This result remains robust when we define winners as teachers aged 27 years old or younger (Appendix Figure **??**).

Panel (a) of Table 6 (columns 1-3), further confirms this result by splitting the sample into quartiles based on the share of winners. Specifically, we re-estimate equation (1) by quartile of the share of winners assuming $\beta_k = 0$ for $k < 0$ and a constant β_k for all $k > 0$. After a CBA expiration, the GOP vote share increased by 3.4 pp in districts in the top quartile, by 0.9 pp in districts in the two middle quartiles, and remained unchanged in districts in the bottom quartile. Column 4 of the same table pools data from all districts and allows the impact of a CBA expiration to vary semi-parametrically by quartile of the share of winners. These estimates show that the political gains experienced by the GOP governor were concentrated in districts with a large share of winning teachers.

Next, we test whether the effects of the reform were also concentrated in districts with a low share of losers. Unsurprisingly, the shares of winners and losers are negatively correlated; however, the correlation is quite low at -0.3. Our analysis shows that the increase in the GOP vote share was concentrated in districts with a share of losers below the median. In these districts, the GOP vote share increased by 4.3 pp (57% of the vote margin) four years after a CBA expiration (Figure 6, panel (b), square series). It was instead indistinguishable from zero in districts with a share of losers above the median (hollow circle series). Splitting the sample by quartile of the share of losers yields similar findings (Table 6, panel (b)). Importantly, we do not observe evidence of negative political impacts of Act 10 in districts with a high share of losers: Estimates of β_k remain positive even in districts in the top quartile (Table 6, panel (b), columns 1 and 4).

7.2 Benefits for Students: Number, Socio-Economic Disadvantage, and Test Scores

In Section 5, we showed that the reform benefited students by substantially increasing test scores, particularly for disadvantaged students (i.e., those eligible for a FRPL). We now investigate whether these student benefits can be linked to the electoral gains experienced by the GOP governor.

If improvements for students were a key factor behind the GOP's electoral success, they should be larger in areas where voters have a stronger stake in public education—for example, in districts with a higher share of households with school-age children. To test this hypothesis, we re-estimate equation (1) in subsamples of districts grouped according to their share of households with children younger than 18. In 2010, this share varied substantially across districts, ranging from 24% at the 5th percentile to 40% at the 95th percentile, with a median of 32%. Our estimates show that, four years after a CBA expiration, the GOP vote share increased by 4.1 pp in districts with a share of household with school-age children above the state median, although there is evidence of a pre-trend (Figure 7, panel (a) square series). By contrast, districts with a share below the median experienced a smaller increase of 1.8 pp (circle series). We confirm this result in columns 1-3 of Table 7, where we allow the impact of exposure to Act 10 to vary by quartile of the share of households with school-age children.

Next, we examine whether the GOP's political gains were larger in districts with a higher proportion of FRPL students, who benefited more from the reform (as shown in Figure 1, panel (d)). We find that, four years following a CBA expiration, districts with a share of FRPL students above the state median saw the GOP vote share rise by 5.1 pp (Figure 7, panel (b), square series), whereas districts below the median experienced no significant change (circle series). In columns 4-6 of Table 7 we confirm that this result holds when we allow the impact of Act 10 to vary by quartile of the FRPL student share.

Finally, Appendix Figure A12 and Table A6 show that the political effects of Act 10 were larger in districts with lower average test scores in 2011, which likely had more to gain from improvement to the educational system. Together, these results provide evidence that the reform's positive effects on students contributed to the GOP governor's electoral success following Act 10.

7.3 Benefits for Districts: Teacher Turnover

Act 10 also affected school districts by altering teacher labor markets. By enabling districts to attract teachers with higher pay, the reform effectively opened up a competitive "market" for public-school teachers (Umhoefer and Hauer, 2016; Biasi, 2021; Biasi et al., 2021). This dynamic may have particularly benefited districts that were previously challenged by high teacher turnover. If the GOP's electoral gains were linked to these benefits, we would expect larger effects in districts with higher pre-reform rates of teacher turnover.

Figure **??** shows evidence supporting this hypothesis. We split the sample between districts above and below the state median rate of teacher turnover in 2010. Four years following Act 10, districts with teacher turnover rates above the median experienced a 4.6 pp increase in the GOP vote share (square series), whereas districts below the median showed no significant change (circle series). We confirm this finding in Table 8, where we allow the impact of the law to vary by quartile of teacher turnover. These findings reinforce the notion that the political gains following Act 10 were partly driven by benefits accruing to school districts through improved teacher recruitment and retention, consistent with a theory of retrospective voting.

Correlations between dimensions of heterogeneity An important question is whether the various dimensions of heterogeneity we have examined—namely, the share of low- and high-experience teachers, the share of households with school-age children, the share of FRPL students, and teacher turnover—capture distinct aspects of school districts. Our analysis reveals that, while some of these variables are correlated, the correlations are relatively low, with a maximum magnitude of XX (see Appendix Figure A14). This suggests that each measure provides unique information about district characteristics and helps us better understand the multiple channels through which Act 10 influenced electoral outcomes.

8 The Role of Teacher Unions

Our results so far suggest that the perceived benefits from Act 10 played an important role in influencing voter behavior. In this section, we examine an additional possible explanation: changes in the powers and influence of teacher unions. Unions have played a key role in shaping both policy and political outcomes, particularly within the field of education. For example, teachers' unions have challenged reforms such as teacher performance pay (Finger, 2018; Hartney and Flavin, 2011), advocated for higher salaries and benefits (Anzia and Moe, 2015), and boosted political participation among teachers (Flavin and Hartney, 2015). Therefore, it is plausible that a reform such as Act 10, which stripped public-sector unions of many of their powers, diminished their ability to influence voter preferences, ultimately benefiting the GOP. We test this hypothesis here.

8.1 Unions' Political Participation and GOP Support

In Section 5, we showed that unions' political participation—as measured by campaign contributions—declined following the passage of Act 10. However, only 11% of districts in our sample had unions who contributed pre-reform, all of whom went to the Democratic party. If the decline in union's political activity contributed to the GOP's electoral gains after Act 10, we would expect larger gains in districts where unions were politically active pre-reform (and then became inactive) compared to districts where unions were never politically active.

To empirically test this hypothesis, we re-estimate equation (1) separately for districts with unions that contributed to gubernatorial races prior to 2011 and for districts where unions never contributed. These estimates confirm that the political gains were concentrated in districts with previously active unions. In these districts, the GOP vote share increased by 7 pp four years after a CBA expiration (Figure **??**, panel(a), square series). The vote share did not change in districts without prior union contributions (circle series).

We also test whether these effects operate through unions' ability to mobilize voters, i.e., by influencing turnout. Appendix Figure **??** (panel (a)) show estimates of equation (1) using the turnout rate as the dependent variable, separately for districts with and without politically active unions. Consistent with the overall finding of no significant effects of Act 10 on turnout (see Appendix Figure XX), we observe no changes in turnout after the passage of the law in either of the two groups. This suggests that the reduced influence of unions after Act 10 affected how people voted—by shifting their preferences—rather than altering voter participation.

8.2 Union Revenues and GOP Votes

Act 10 weakened unions in part by prohibiting the automatic collection of dues from employees' paychecks, making it more difficult for them to generate revenue. As shown in panel (a) of Figure 1, union revenues per teacher fell following a CBA expiration. Moreover, Appendix Figure A3 reveals that this decline was larger in dollar terms in districts that initially had higher revenues per member. Higher pre-reform revenues reflect, at least in part, unions that were better positioned to extract dues. Therefore, the greater decline in districts with higher ex ante revenues suggests a larger loss of union power for these districts.

To assess how the decline in union powers influenced the political effects of the reform, we split our sample in two groups: districts with ex ante stronger unions (those with revenues per member in 2011 above the state median, which experienced the most pronounced revenue declines post-reform) and districts with ex ante weaker unions. Estimates of equation (1) indicate that the electoral effects of Act 10 were substantially larger in districts with initially stronger unions. In these districts, the GOP vote share increased by 3 percentage points four years after a CBA expiration (see Figure **??**, square series; Table **??**, column 2, significant at 5%), whereas they do not change in districts with weaker unions (Table **??**, column 4).

These findings suggest that the GOP's electoral gains can be linked to the decline in union power, with more pronounced effects in areas that initially housed stronger and wealthier unions. Overall, the results from this section results underscore that teacher unions—and their subsequent loss of power and political participation—played a crucial role in shaping the political impacts of Act 10. More broadly, our findings highlight the importance of unions as institutions that influence both educational policy design and political outcomes.

9 Discussion and Conclusion

9.1 Comparing potential mechanisms

The foregoing heterogeneity analyses provide supportive evidence that the electoral effects of Act 10 may depend on the *ex ante* propensity of unions, teachers, and students to gain or lose from the policy. To shed light on the degree to which these dimensions are likely to represent independent channels (as opposed to capturing the same underlying variation), we next include various dimensions of heterogeneity in the same regression. We use LASSO to select these channels in a data-driven way.

We start by including eight potential dimensions of heterogeneous impacts for which data are available, each defined at the district level:

- Fraction of teachers with less than three years of experience
- Fraction of teachers with at least 21 years of experience
- Whether the union made a political donation in 2010
- GOP gubernatorial vote share in 2010
- Fraction of students on free and reduced-price lunch (FRPL)
- Teacher turnover
- Student test scores
- Fraction of households with school-age children.[9]

We create dummy variables for whether each district is above the median for each of these dimensions of heterogeneity, then regress GOP gubernatorial vote share on an indicator for exposure to Act 10 as well as the interaction of this indicator with all the heterogeneity dummies, controlling for district and year fixed effects as well as interactions of each dimension of heterogeneity with year fixed effects (Athey et al., 2024; Ahrens et al., 2020). The results of this regression can be seen in Columns 1 and 2 of Table A14. We then apply LASSO to this regression model to select the most relevant interactions. LASSO identifies four interaction terms with non-zero coefficients: union donations, young teachers, FRPL, and GOP vote share. Columns 3 and 4 of Table A14 show the results from the same regression described above, but including only those four LASSO-selected dimensions of interactions.

Both analyses yield similar insights: the effect of Act 10 on GOP gubernatorial vote share tended to be higher in places with more young teachers and with lower pre-Act-10 GOP vote share. (The

[9]Fraction of households with school-age children defined as the fraction of households containing at least one child between the ages of 6 and 17 inclusive, using block-level data from the 2010 census.

model including all eight interactions also finds stronger positive effects in districts where unions made donations prior to Act 10, but these coefficients are small and insignificant in the LASSO-selected model.)

The foregoing regression ignores the possibility of significant interactions between different dimensions of heterogeneity. To account for this, we then estimate a regression model which interacts the "exposed" dummy with each of the four LASSO-selected covariates individually and with the double and triple interactions between them. Columns 1 and 2 of Table A15 report the full results from this regression. We then perform a second LASSO on this fully-interacted regression model, and estimate the regression again including only the coefficients from this second LASSO selection. These results are shown in Columns 3 and 4 of Table A15.

For ease of exposition, we add together the interaction coefficients reported in Table A15 and report the linear combinations of interaction effects corresponding to the overall effect of Act 10 on districts which fall into each of the sixteen cells characterized by the fully saturated interaction of the four dimensions of heterogeneity originally selected by LASSO. That is, we add the appropriate interaction coefficients to report the main effect of Act 10 exposure on districts with a high share of young teachers, union donations, FRPL, and 2010 GOP vote share; a low share of each of those four measures; and all other possible combinations. Not all sixteen cells are populated. Where sparseness yields missing interaction coefficients, we assume these are zero. In adding up the interaction coefficients from the regression which includes only LASSO-selected coefficients from the fully-interacted model, we assume any interaction not selected by LASSO is also zero. The cell-by-cell coefficients are reported in Table A15. The results are broadly consistent with those from the previous heterogeneity analyses. Positive effects on GOP vote share appear in various types of districts, including those with many young teachers, many FRPL students, and low pre-Act-10 GOP vote shares. However, effects are largest and most significant in districts where all four measures are high (including, somewhat surprisingly, pre-Act-10 GOP vote share).

We plot these cell-by-cell effects in Figure A15, displaying the number of districts in each cell to the right of the estimate in parentheses. (Empty cells are excluded from the graph.)
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.

[BACK-OF-THE-ENVELOPE]

[SAY EXPLICITLY WHAT WE CAN EXTRAPOLATE TO OTHER CONTEXTS, EMPHASIZING ROLE OF UNIONS]

[TRUMP VOTE SHARE RESULT]

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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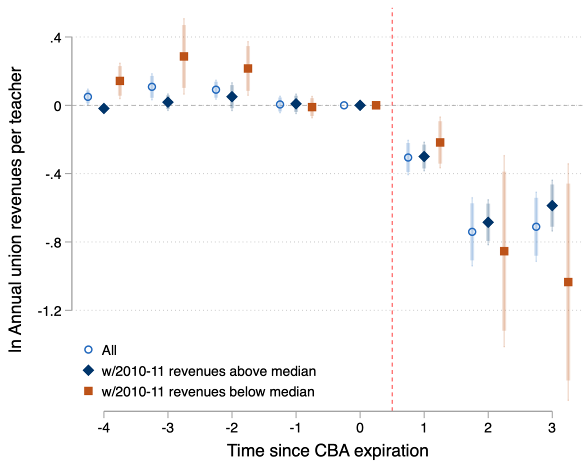
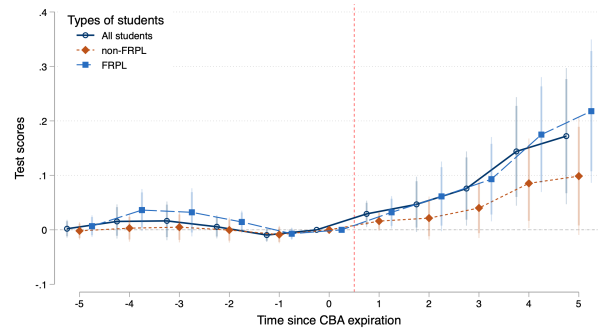
Figures

Figure 1: Effects of Act 10 On Unions, Teachers and Students

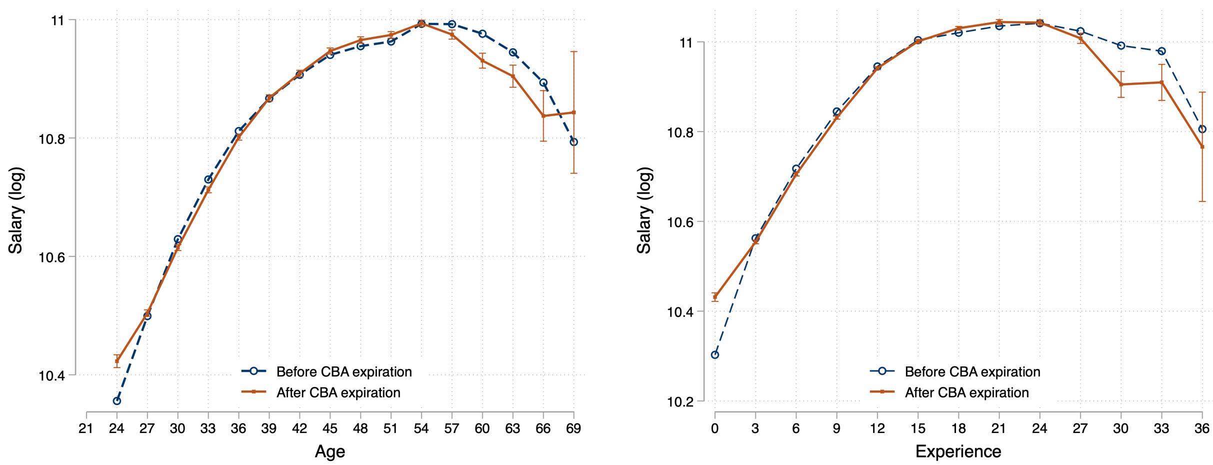
(a) Union revenues



(b) Student test scores

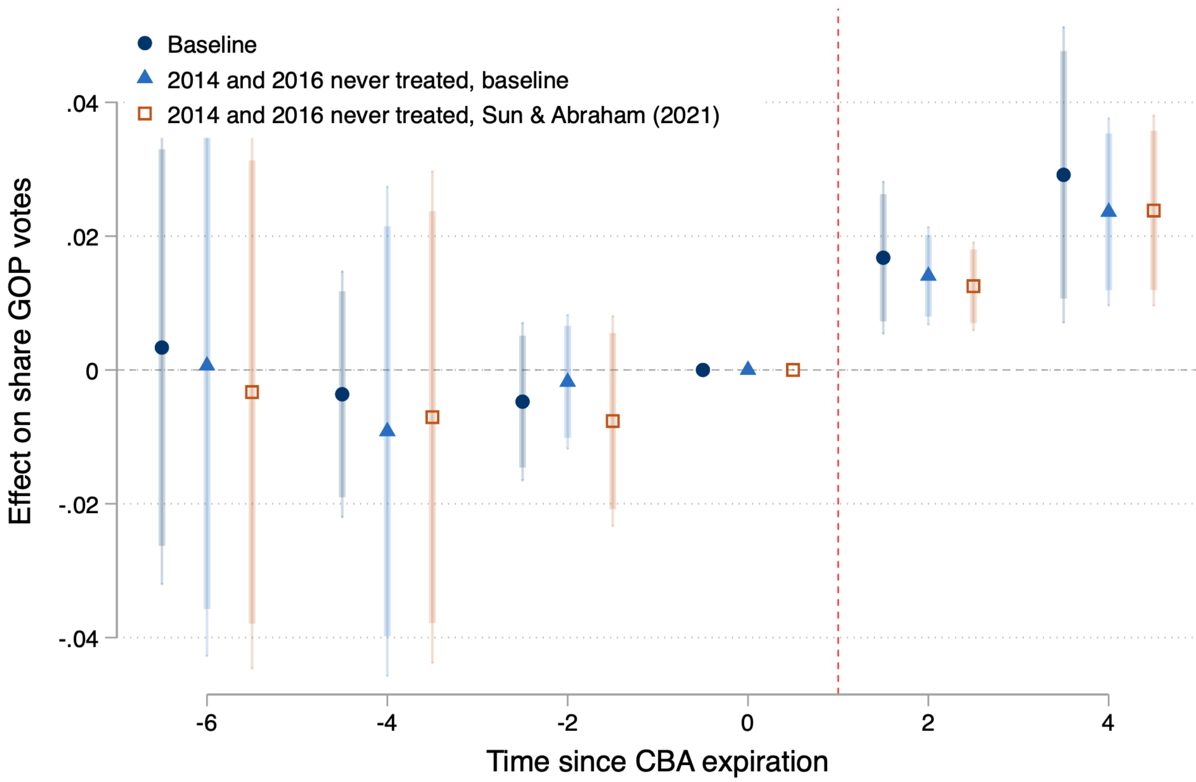


(c) Teacher pay by age and experience



Notes: Panel (a) shows estimates and 90% and 95% confidence intervals of the coefficients β_k in equation (1), using the natural logarithm of 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. 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, lagged test scores, 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. Panel (c) 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. In all figures, confidence intervals are obtained using standard errors clustered at the district level.

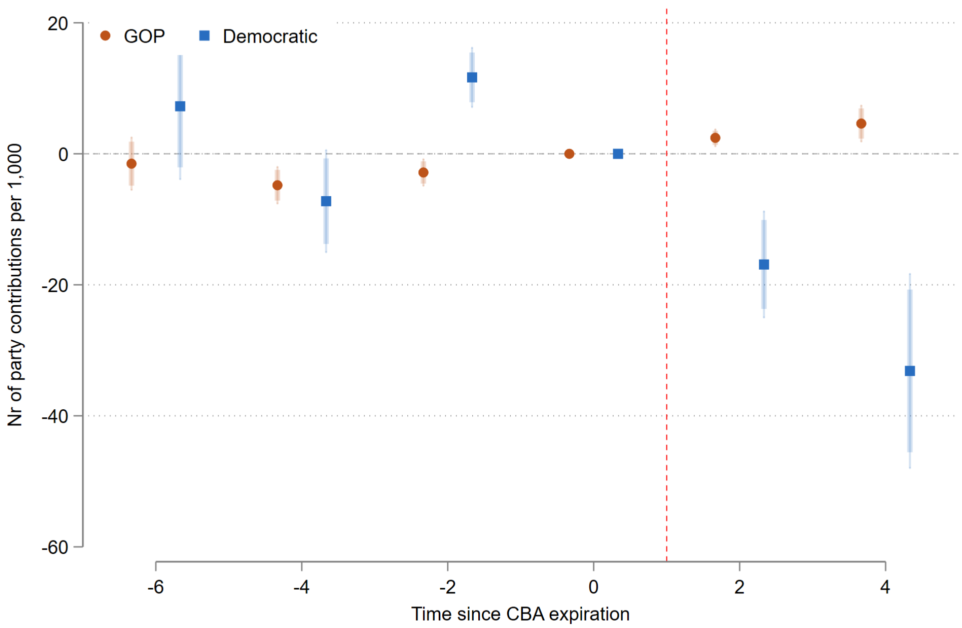
Figure 2: Political Effects of Wisconsin's Act 10: Event Study Estimates



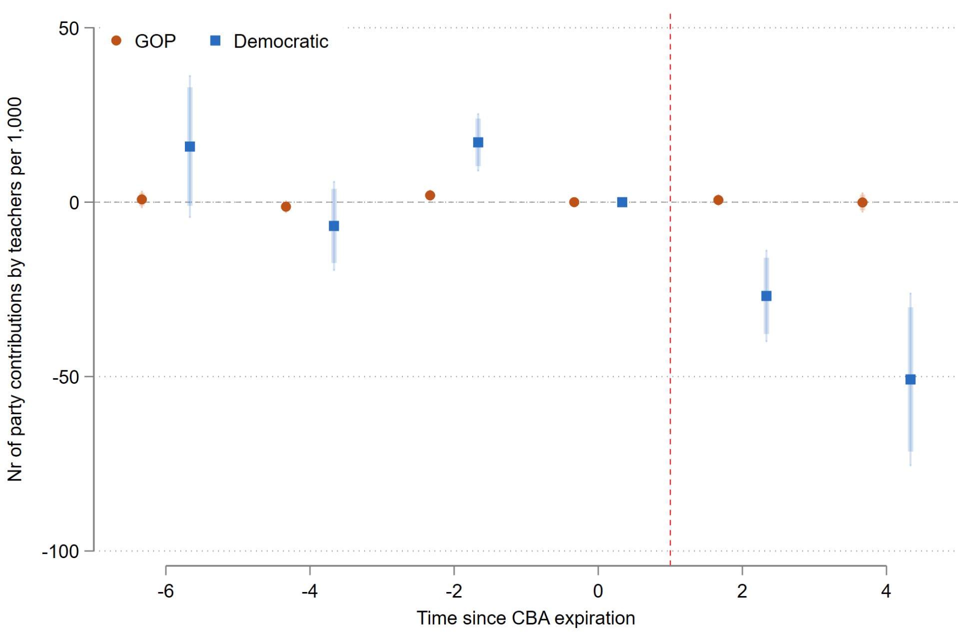
Notes: The blue circles show estimates and 90% and 95% confidence intervals of the coefficients β_k in equation (1), estimated using the GOP vote share in gubernatorial elections in each ward and year as the dependent variable and controlling for ward and year fixed effects. The triangles show analogous estimates, considering the 2014 and 2016 CBA expiration cohorts as never treated. The 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

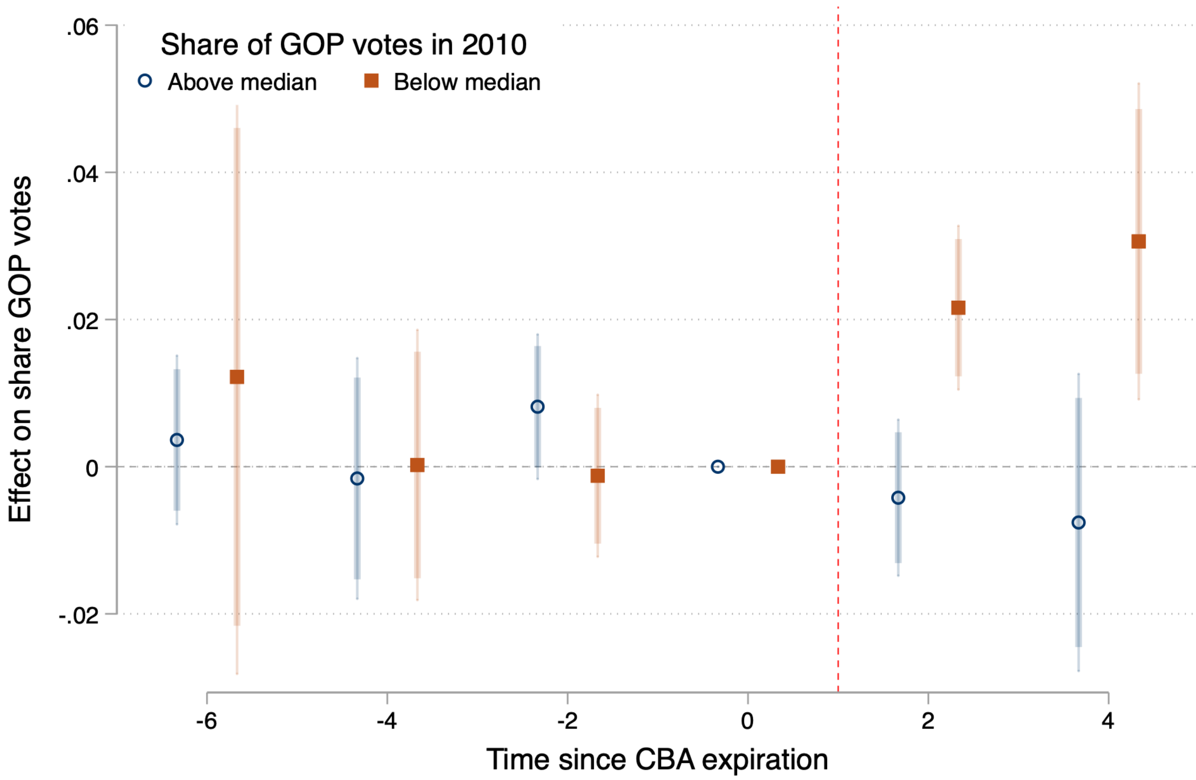


(b) Contributions by teachers



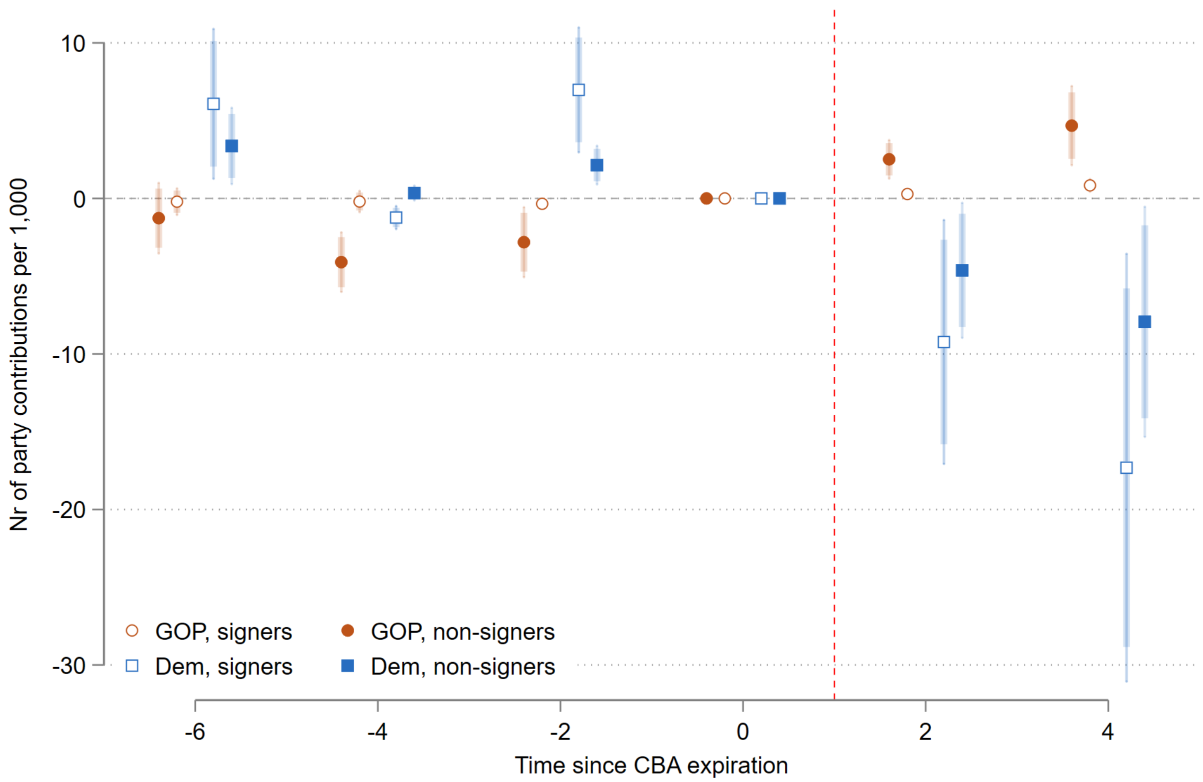
Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_k in equation (1), obtained using the number of contributions per 100 people to each party in the district. Panel (a) shows all contributions; panel (b) shows contributions made by teachers, per 100 teachers in the district.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

Figure 4: Event-Study Estimates by 2010 GOP vote share



Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_k 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 below the median of the state distribution of the 2010 gubernatorial GOP vote share, and the circles show estimates for wards above the median. Confidence intervals are obtained using standard errors clustered at the district level.

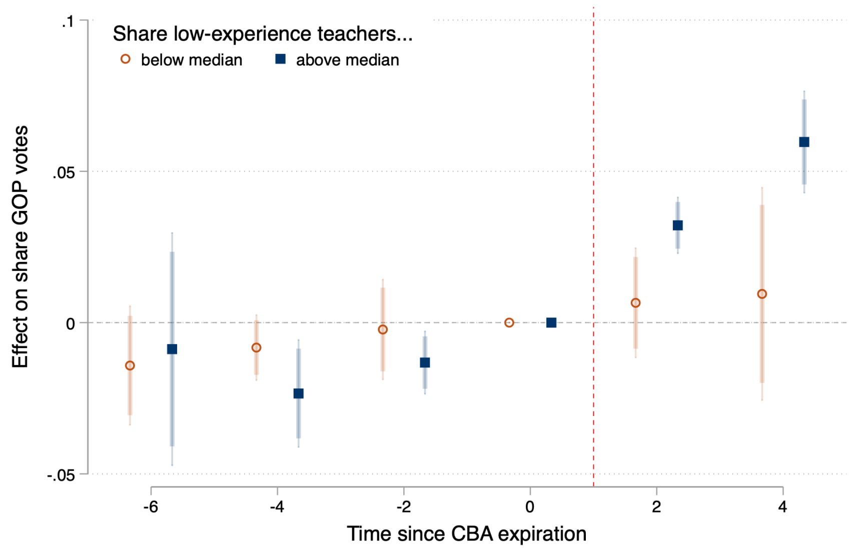
Figure 5: Campaign Contributions to Political Parties by Petition Signers: Event-Study Estimates



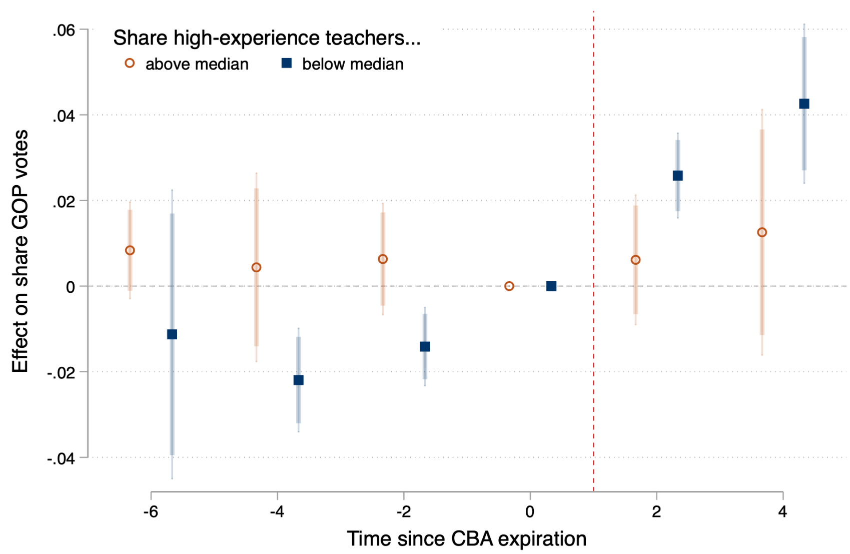
Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_k in equation (1), obtained using the number of contributions to each party, by people who did and did not sign the recall election petition, per 1,000 people in the district. Observations are weighted by district population. Confidence intervals are obtained using standard errors clustered at the district level.

Figure 6: Winners and Losers of Wisconsin's Act 10. Event-Study Estimates by Share of Low- and High-Experience Teachers in District

(a) By share of low-experience teachers (*winners*)



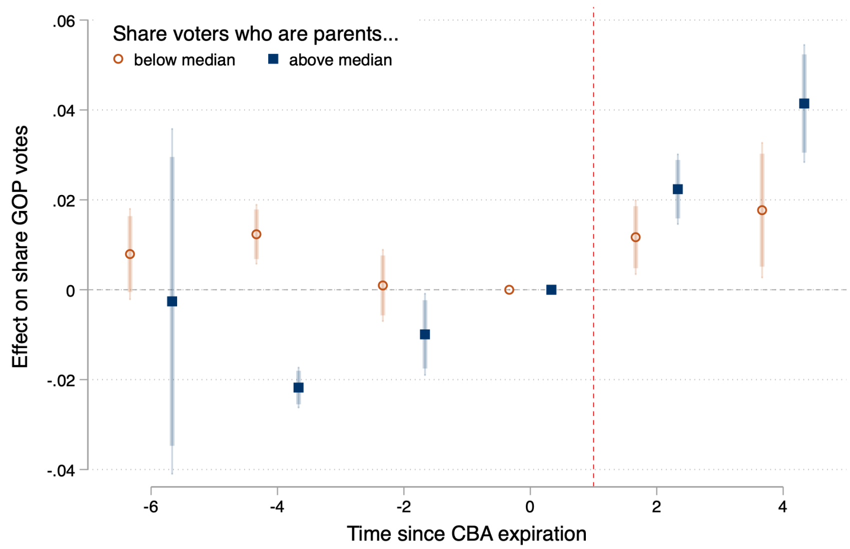
(b) By share of high-experience teachers (*losers*)



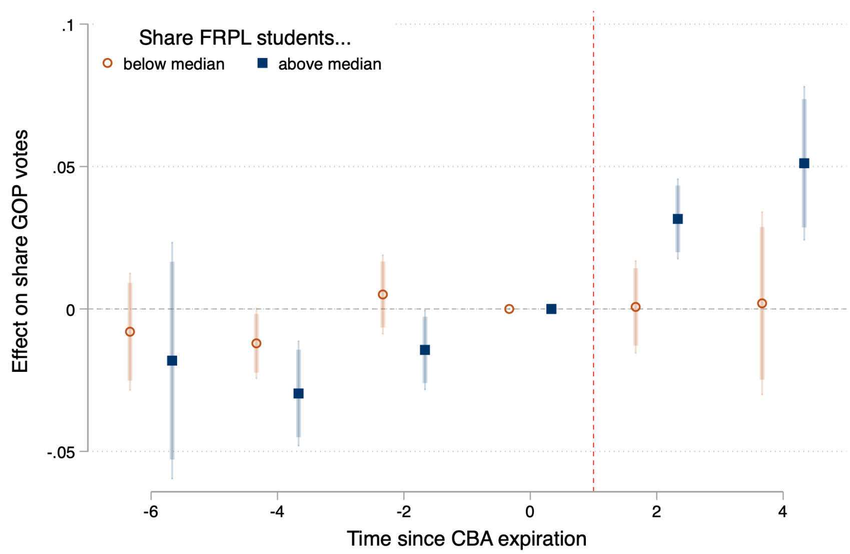
Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_k in equation (1), estimated using the GOP vote share in gubernatorial elections in each ward and year as the dependent variable and controlling for ward and year fixed effects. In panel a), the squares show estimates for the subsample of districts in with a 2010-11 share of low-experience (≤ 3 years) teachers above the state median and the circles show estimates for districts with a share below the median. In panel b), the squares show estimates for the subsample of districts in with a 2010-11 share of high-experience (≥ 21 years) teachers below the state median and the circles show estimates for districts with a share above the median. Confidence intervals are obtained using standard errors clustered at the district level.

Figure 7: Winners and Losers of Wisconsin's Act 10. Event-Study Estimates by Share of Households with School-Age Children and Share of FRPL Students

(a) By share of households with school-age children (aged 18 and younger)

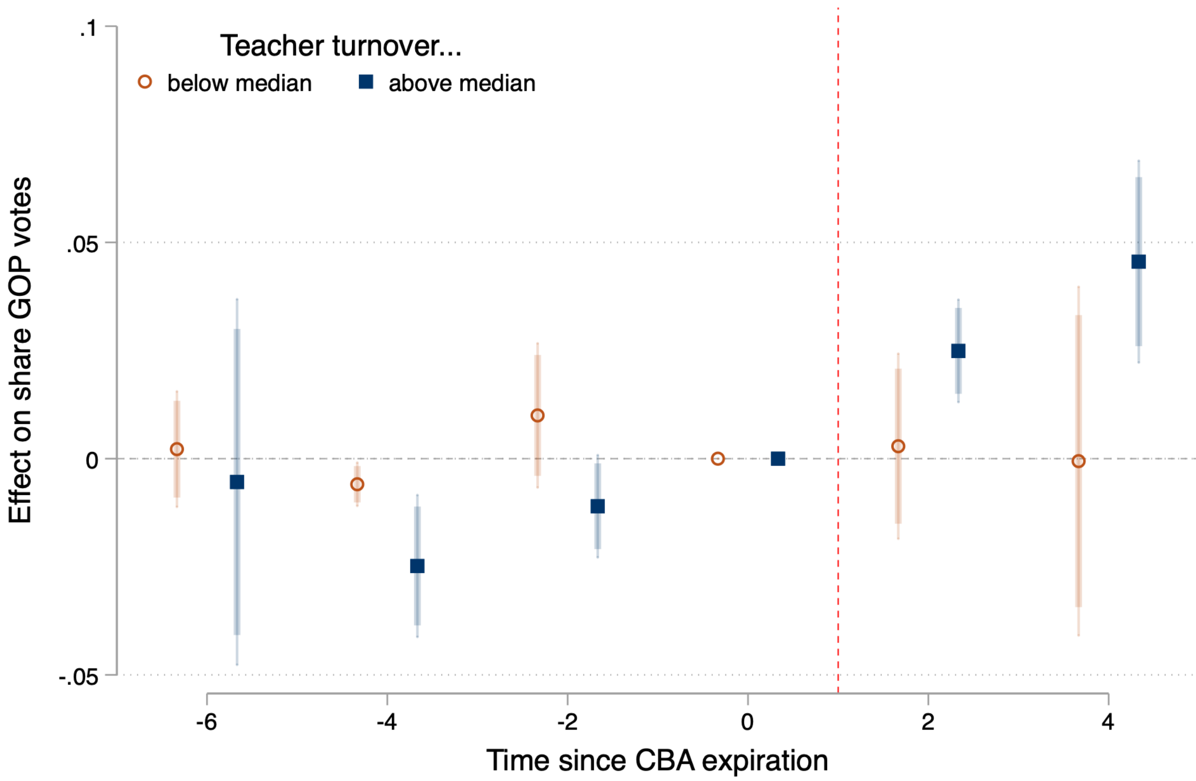


(b) By share of FRPL students



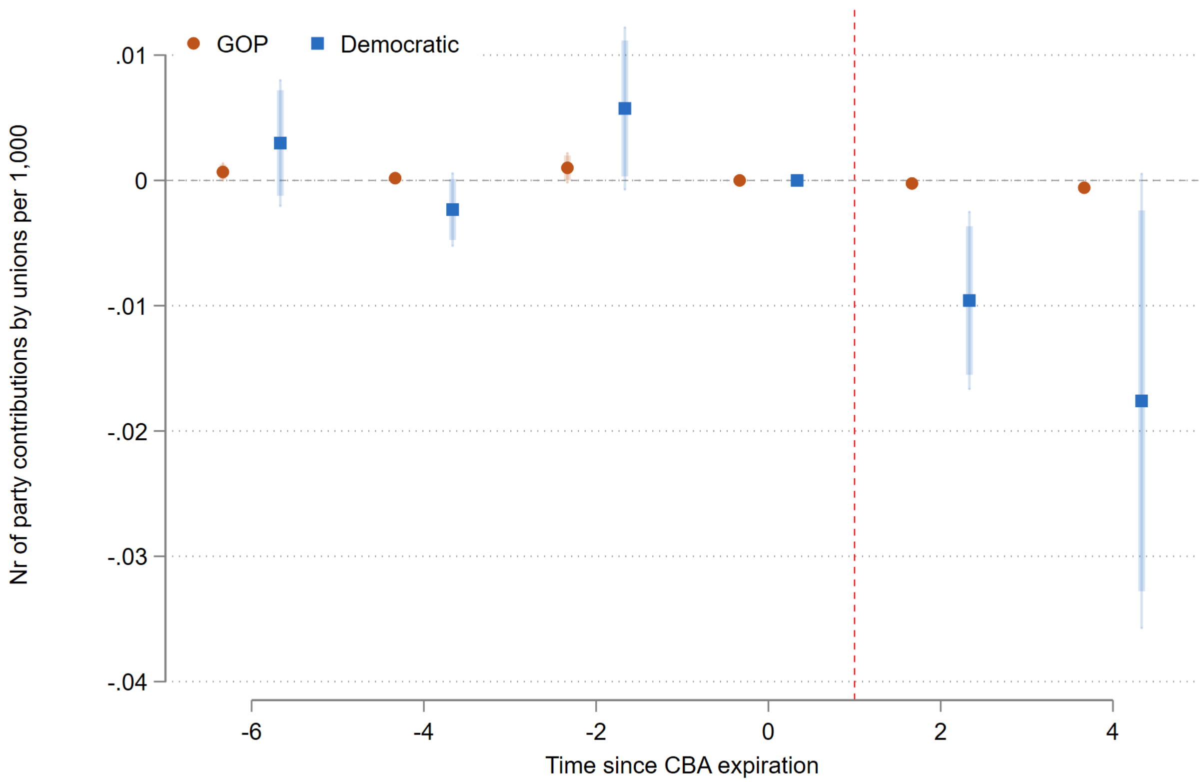
Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_k in equation (1), estimated using the GOP vote share in gubernatorial elections in each ward and year as the dependent variable and controlling for ward and year fixed effects. In panel a), the squares show estimates for the subsample of districts in with a 2010-11 share of households with school-age children (aged 18 and younger) above the state median and the circles show estimates for districts with a share below the median. In panel b), the squares show estimates for the subsample of districts in with a 2010-11 share of FRPL students above the state median and the circles show estimates for districts with a share below the median. Confidence intervals are obtained using standard errors clustered at the district level.

Figure 8: 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_k 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 above the state meandian of 2010-11 teacher turnover, and the circles show estimates for districts below the median. Turnover is defined as the share of teachers who leave the district at the end of each year. Confidence intervals are obtained using standard errors clustered at the district level.

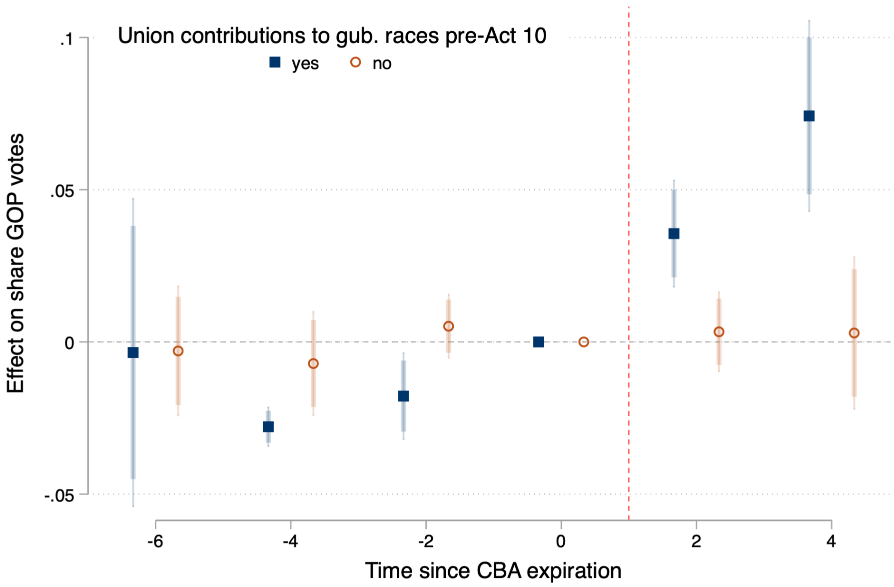
Figure 9: Campaign Contributions to Political Parties Made By Teachers' Unions: Event Study



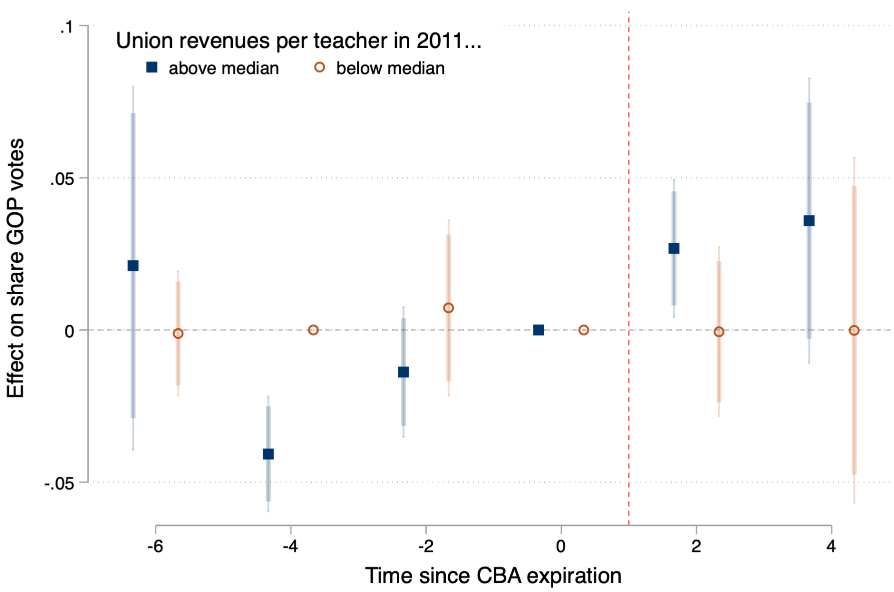
Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_k in equation (1), obtained using the number of contributions per 100 people made by each district's teacher union to each party. Observations are weighted by district population. Confidence intervals are obtained using standard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

Figure 10: The Impact of Unions on GOP Votes. Event-Study Estimates, by Ex Ante Union Campaign Contributions and Revenues

(a) By ex ante campaign contributions



(b) By ex ante revenues



Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_k 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 squares show estimates for wards in districts whose unions made campaign contributions to gubernatorial races prior to 2011 the circles show estimates for wards in districts with no contributions. In panel (b), the squares show estimates for the subsample of wards in districts with union revenues per teacher above the state median in 2011; the circles show estimates for wards with union revenues below the median. 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
Population age			
Share pop < 18 yo	0.23	0.23	0.23
	(0.067)	(0.062)	(0.070)
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	0.024	0.14	-0.034
	(0.33)	(0.29)	(0.34)
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. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 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: Campaign Contributions to Political Parties: Pooled Event-Study Estimates

	All		Teachers		Petition signers		Unions
	(1) GOP	(2) Dem	(3) GOP	(4) Dem	(5) GOP	(6) Dem	(7) Dem
Exposed	1.673***	-7.815	1.583*	-12.755	0.081	-4.922	-0.005
	(0.515)	(5.641)	(0.926)	(7.941)	(0.172)	(3.712)	(0.003)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var. control	13.793	3.205	3.759	7.655	2.333	1.626	0.002
N	948	948	943	943	948	948	948
Clusters (districts)	216	216	215	215	216	216	216
R-squared	0.81	0.83	0.36	0.60	0.71	0.81	0.50

Notes: The dependent variable is the 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, 3, and 5 examine donations to the GOP; columns 2, 4, 6, and 7 examine donations to the Democratic party. Columns 1 and 2 use total donations to each party per 1000 people in the district; columns 3 and 4 use donations by teachers to each party per 1000 teachers in the district; columns 5 and 6 use donations by 2012 gubernatorial recall petition signers per 1000 people in the district; and column 7 uses donations by teacher unions per 1000 people in the district. 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. * ≤ 0.1; ** ≤ 0.05; *** ≤ 0.01.

Table 5: Political Effects of Wisconsin's Act 10: Pooled Event Study Estimates, Robustness Checks

	Ignoring extensions		Excluding Milwaukee		Only fully aligned wards	
	(1)	(2)	(3)	(4)	(5)	(6)
Exposed	0.019	0.019	0.009***	0.010***	0.015**	0.015***
	(0.014)	(0.014)	(0.004)	(0.004)	(0.006)	(0.006)
District FE	Yes	No	No	No	No	No
Ward FE	No	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var. control	0.476	0.476	0.503	0.503	0.449	0.449
N	21320	21313	19948	19941	12338	12332
Clusters (districts)	237	237	235	235	231	231
R-squared	0.73	0.94	0.72	0.93	0.78	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 $k > 0$. All specifications control for year fixed effects; columns 1, 3, and 5 control for district fixed effects and columns 2, 4, and 6 control for ward fixed effects. Columns 1 and 2 are estimated considering only CBA expirations and ignoring extensions to construct the exposure variable. Columns 3 and 4 are estimated excluding Milwaukee. Columns 5 and 6 are 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 6: Political Effects of Wisconsin's Act 10: Pooled Event Study, By Share of Low and High-Experience Teachers

	By quartile of % *winners* (exp. ≤ 3)				By quartile of % *losers* (exp. ≥ 21)			
	Q1	Q2-Q3	Q4	All	Q1	Q2-Q3	Q4	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exposed	0.001	0.009**	0.034***		0.028**	0.010**	0.013	
	(0.007)	(0.004)	(0.012)		(0.013)	(0.005)	(0.008)	
Exposed * Q1				-0.001				0.028
				(0.009)				(0.018)
Exposed * Q2				0.002				0.012
				(0.008)				(0.009)
Exposed * Q3				0.016**				0.007
				(0.008)				(0.009)
Exposed * Q4				0.046***				0.018**
				(0.016)				(0.008)
Ward FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Year * qtile FE	No	No	No	Yes	No	No	No	Yes
Mean dep. var. control	0.525	0.492	0.404	0.476	0.397	0.493	0.528	0.476
N	5393	10579	5261	21233	5203	10703	5327	21233
Clusters (districts)	62	122	52	236	42	114	80	236
R-squared	0.87	0.94	0.96	0.94	0.96	0.93	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 constant across all $k > 0$. In columns 1-4, we split the sample by the quartile of the share of teachers who had 3 or fewer years of experience in 2010-2011. In columns 5-8, we split the sample by the quartile of the share of teachers who had 21 or more years of experience in 2011. Q1, Q2, Q3, and Q4 refer to the first, second, third, and fourth quartiles of each variable, respectively. Columns 1-3 and 5-7 control for ward and year fixed effects; columns 4 and 8 control for ward and quartile-year fixed effects. Standard errors in parentheses are clustered at the district level. * ≤ 0.1; ** ≤ 0.05; *** ≤ 0.01.

Table 7: Political Effects of Wisconsin's Act 10: Pooled Event Study, By Share of Households with School-Age Children and Share of FRPL Students

	By quartile of % HH w/school-age children				By quartile of % FRPL students			
	Q1	Q2-Q3	Q4	All	Q1	Q2-Q3	Q4	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exposed	0.017***	0.010*	0.022		0.005	0.011*	0.030***	
	(0.005)	(0.006)	(0.015)		(0.005)	(0.005)	(0.010)	
Exposed * Q1				0.012				0.007
				(0.008)				(0.007)
Exposed * Q2				0.006				0.013
				(0.009)				(0.012)
Exposed * Q3				0.014				0.009
				(0.010)				(0.008)
Exposed * Q4				0.036*				0.039**
				(0.018)				(0.017)
Ward FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Year * qtile FE	No	No	No	Yes	No	No	No	Yes
Mean dep. var. control	0.471	0.513	0.412	0.476	0.541	0.510	0.376	0.476
N	5216	10701	5316	21233	5526	10649	5058	21233
Clusters (districts)	62	122	52	236	62	131	43	236
R-squared	0.92	0.92	0.97	0.94	0.96	0.88	0.95	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 constant across all $k > 0$. In columns 1-4, we split the sample by the quartile of the share of households with school-age children (aged 18 and younger) in 2010. In columns 5-8, we split the sample by the quartile of the share of FRPL students in 2011. Q1, Q2, Q3, and Q4 refer to the first, second, third, and fourth quartiles of each variable, respectively. Columns 1-3 and 5-7 control for ward and year fixed effects; columns 4 and 8 control for ward and quartile-year fixed effects. Standard errors in parentheses are clustered at the district level. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.

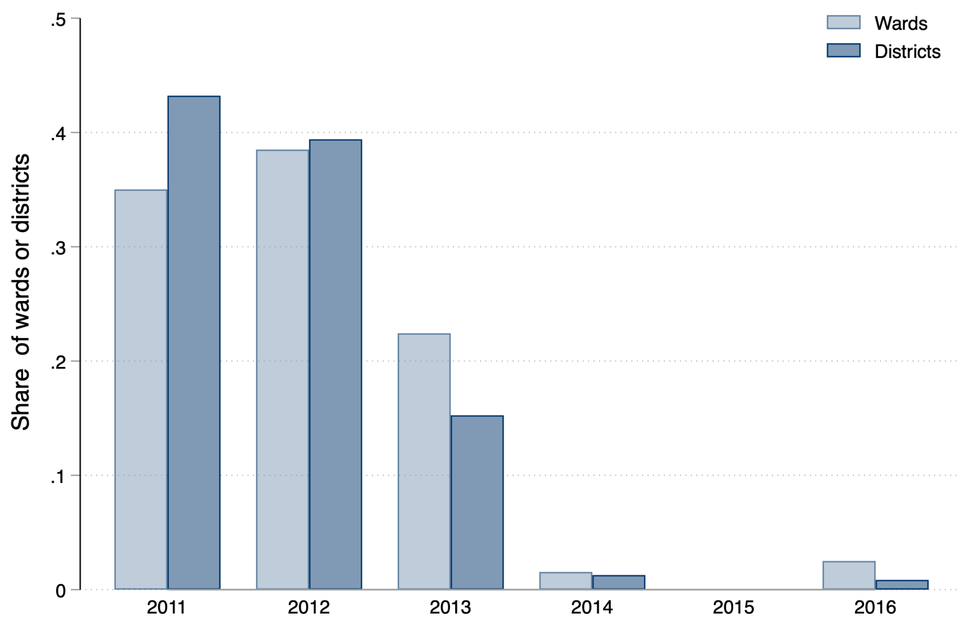
Table 8: Political Effects of Wisconsin's Act 10: Effects by Ex Ante Teacher Turnover

	≤25pctile		25-75pctile		≥ 75pctile	
	(1)	(2)	(3)	(4)	(5)	(6)
Exposed	0.004	0.004	0.012**	0.012**	0.032***	0.033***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.012)	(0.011)
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	0.549	0.549	0.487	0.487	0.367	0.367
N	5383	5381	10151	10149	5624	5621
Clusters (districts)	66	66	105	105	64	64
R-squared	0.55	0.87	0.73	0.93	0.76	0.97

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 $k > 0$. 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 at the end of the year year. Q1, Q2, Q3, and Q4 refer to the first, second, third, and fourth quartiles of each variable, respectively. Columns 1-3 control for ward and year fixed effects; column 8 controls for ward and quartile-year fixed effects. Standard errors in parentheses are clustered at the district level. * ≤ 0.1; ** ≤ 0.05; *** ≤ 0.01.

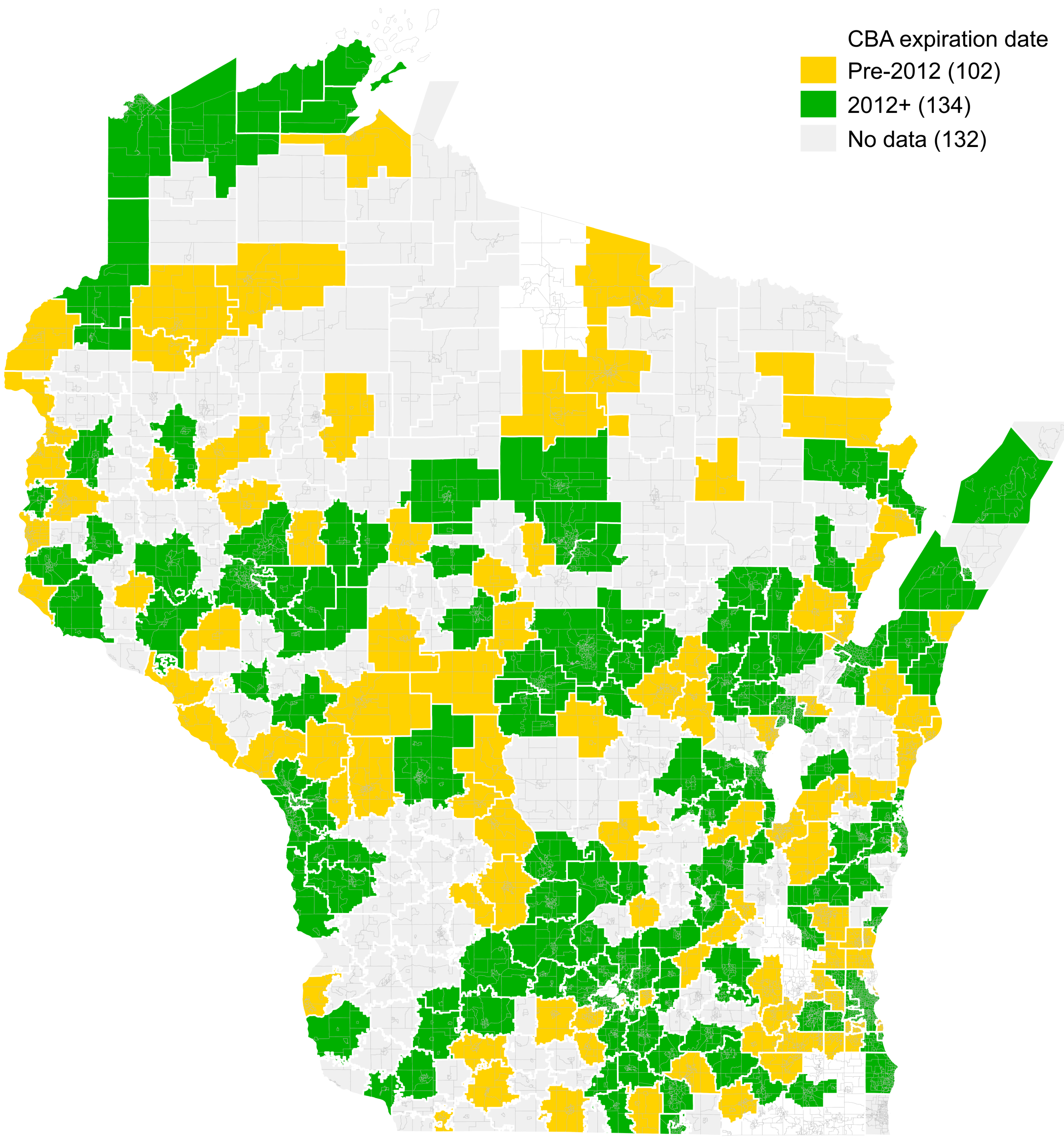
Appendix Tables and Figures

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



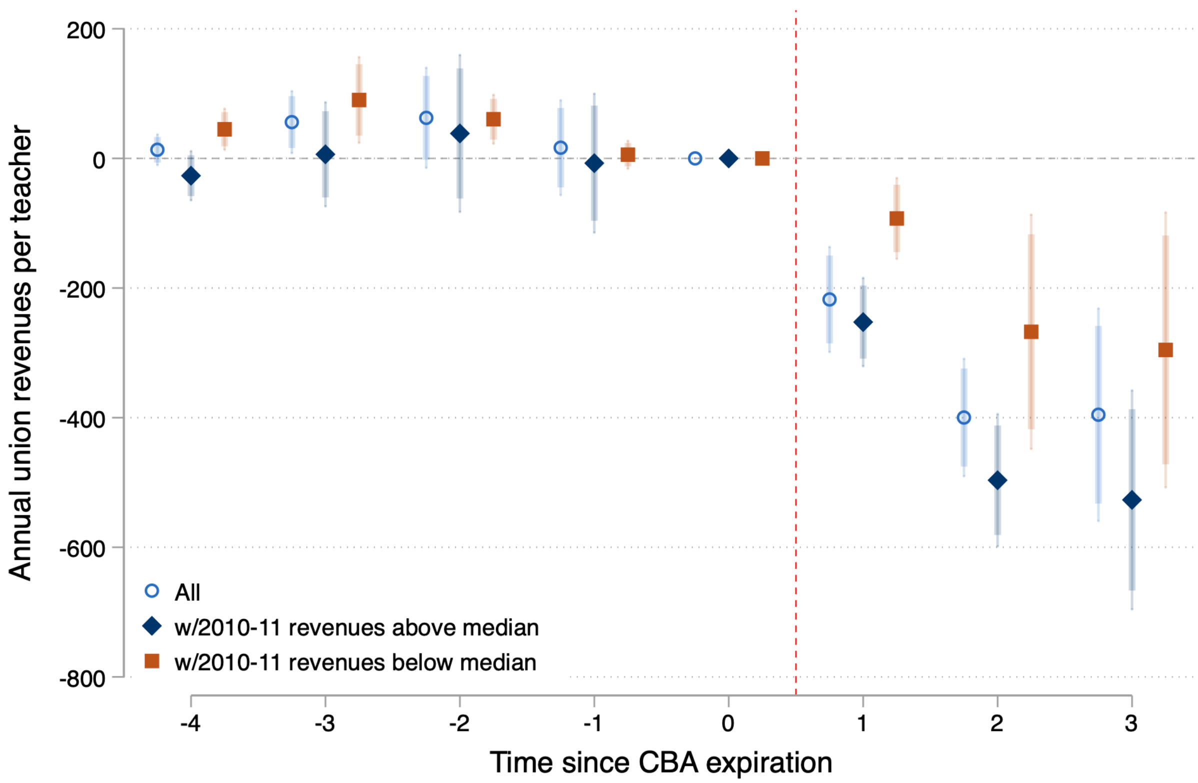
Notes: Share of wards and districts by date of expiration of the district's CBA or of its extension.

Figure A2: Wisconsin Unified School Districts, by CBA Expiration Year



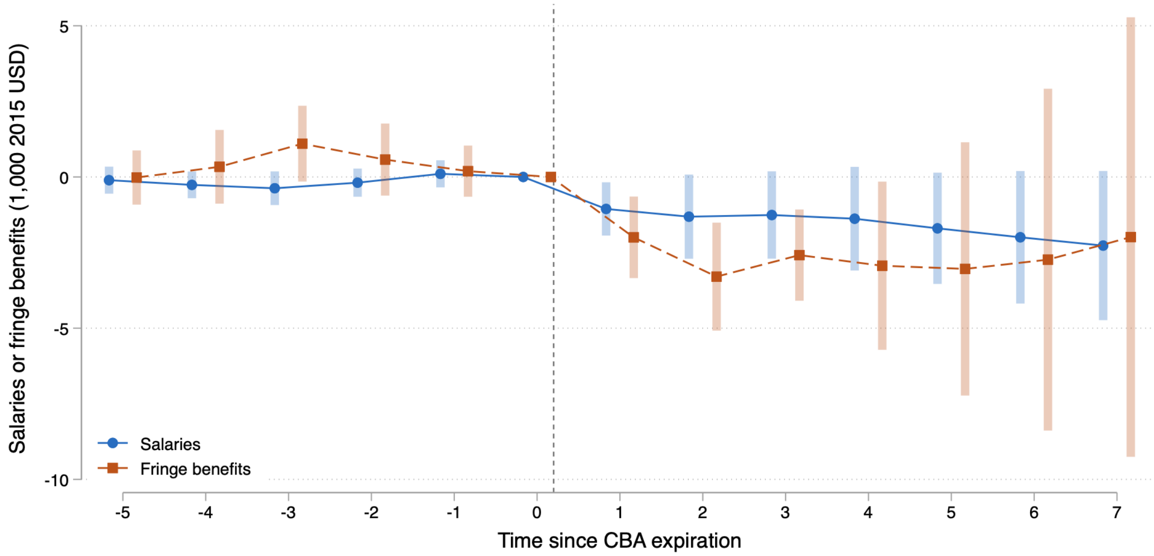
Notes: The map shows school districts by when their (extended) CBAs expired (pre-2012 vs 2012 or later). Non-unified school districts (elementary and secondary) not shown. School districts are delineated by thick white lines; wards are delineated by thin gray lines.

Figure A3: Effects of Act 10 on Union Revenues: Event Study Estimates



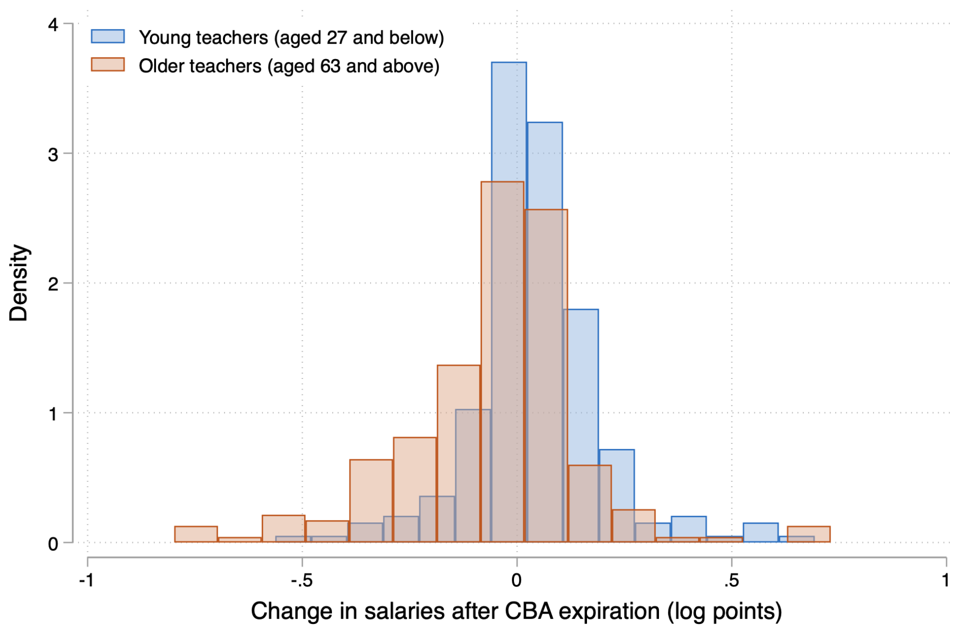
Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_k in equation (1), using district-year level union revenues per member as the dependent variable, for all unions (solid line) and separately for districts with revenues per member above and below the state median in 2010-11 (dashed 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 A4: 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.

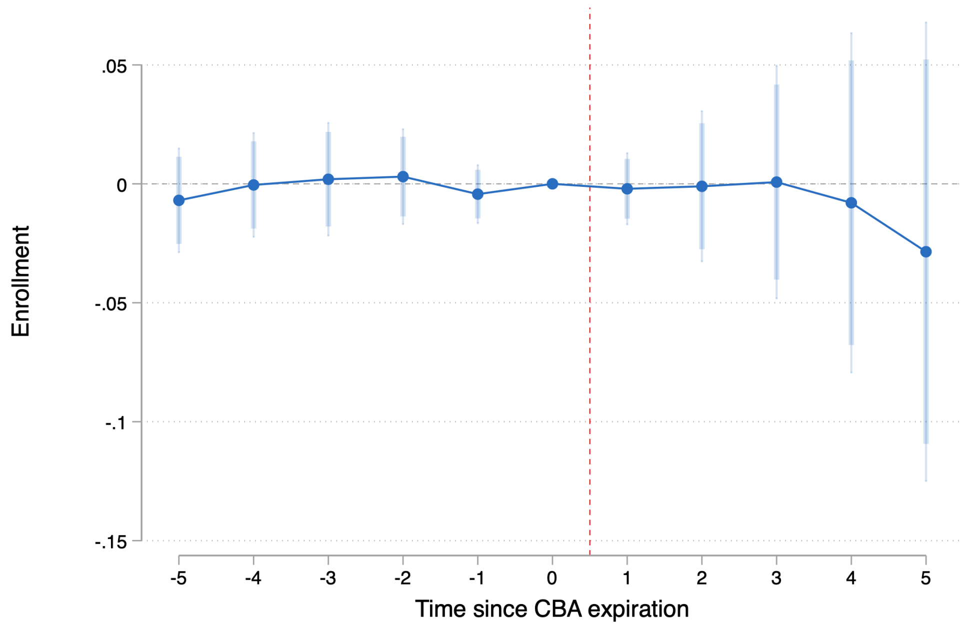
Figure A5: Distribution of Changes in Salaries, Before vs After A District's CBA Expiration



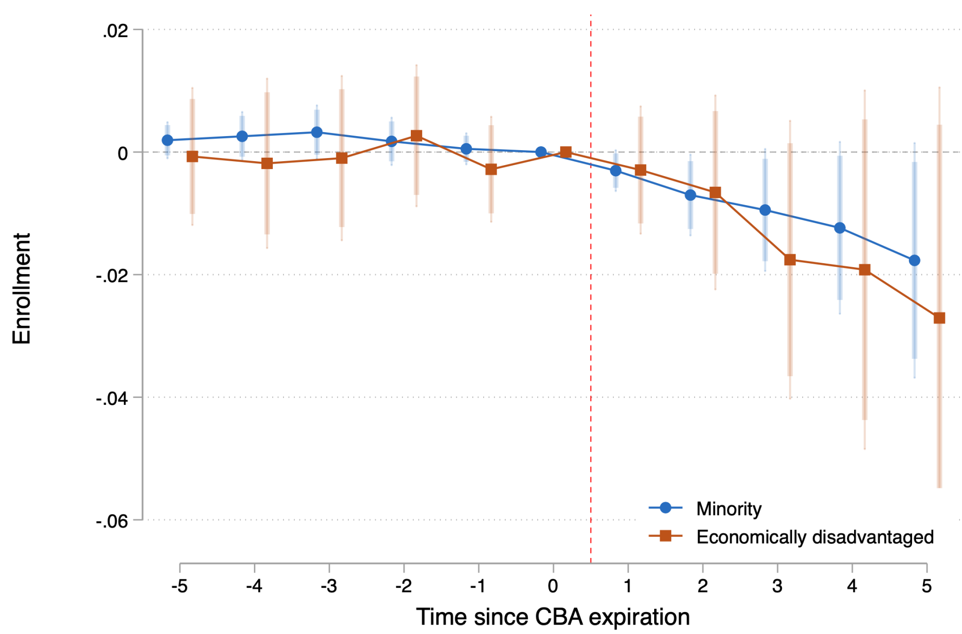
Notes: District-level changes in the logarithm of teacher salaries for teachers aged 63 and older (orange series) and those aged 27 and below (blue series), between the two years preceding and the two years following the expiration of each district's CBA or its extension.

Figure A6: Enrollment and Shares of Students in Demographic Groups: Event Study Estimates

(a) Enrollment



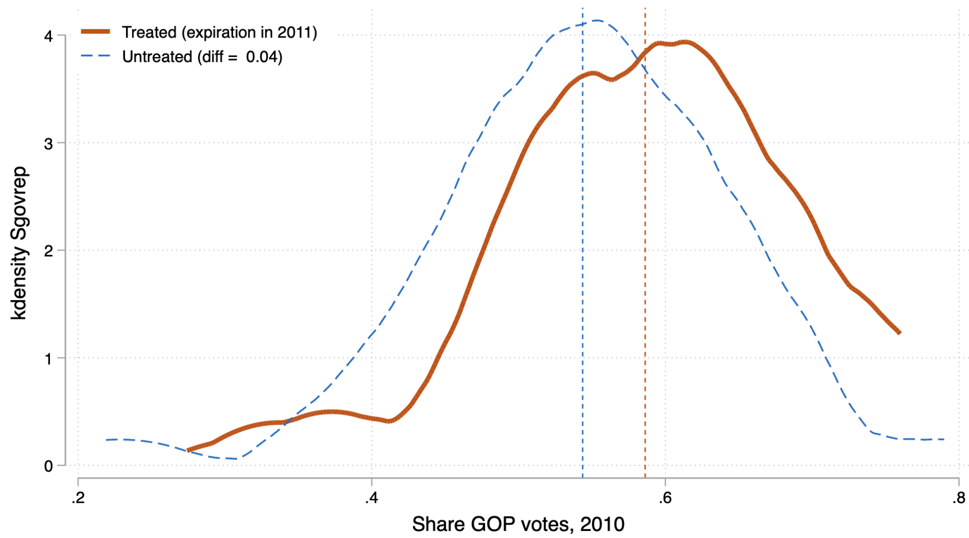
(b) Enrollment shares



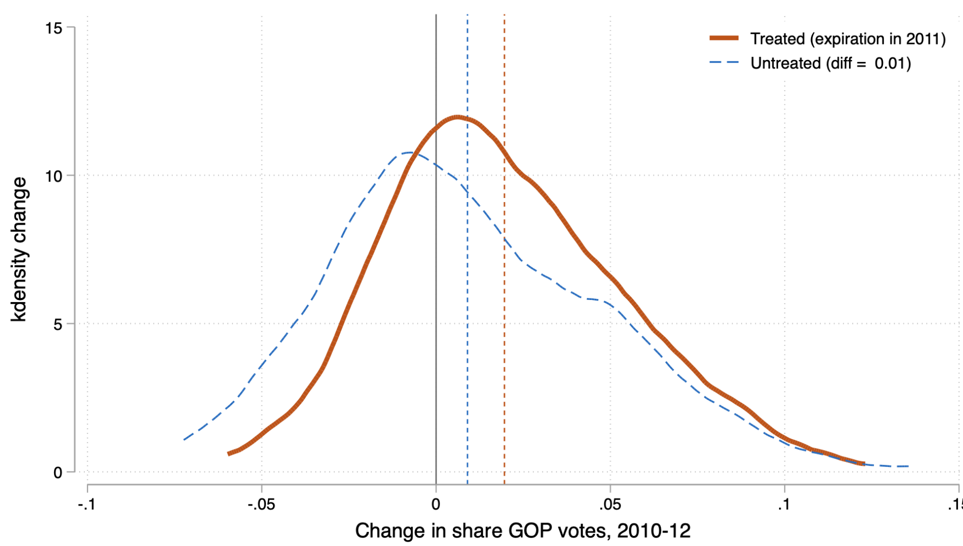
Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_k in equation (1), using log district enrollment (panel (a)) and district enrollment shares of FRPL (*Economically disadvantaged*) and Black or Hispanic students (*minority*, panel (b)). Confidence intervals are obtained using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

Figure A7: Votes to GOP Governor in Treated and Control Districts: Baseline (2010) and 2010-2012 Change

(a) GOP vote shares in 2010 gubernatorial election

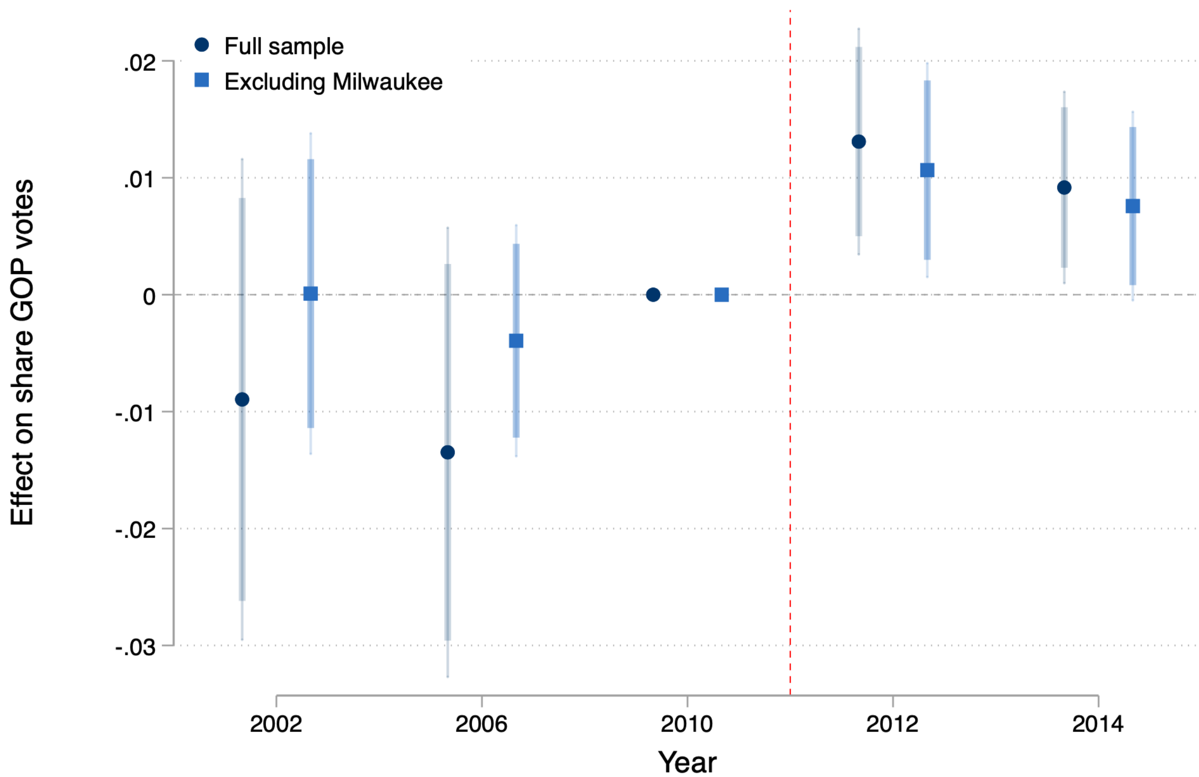


(b) Change in GOP vote shares, 2012 vs 2010



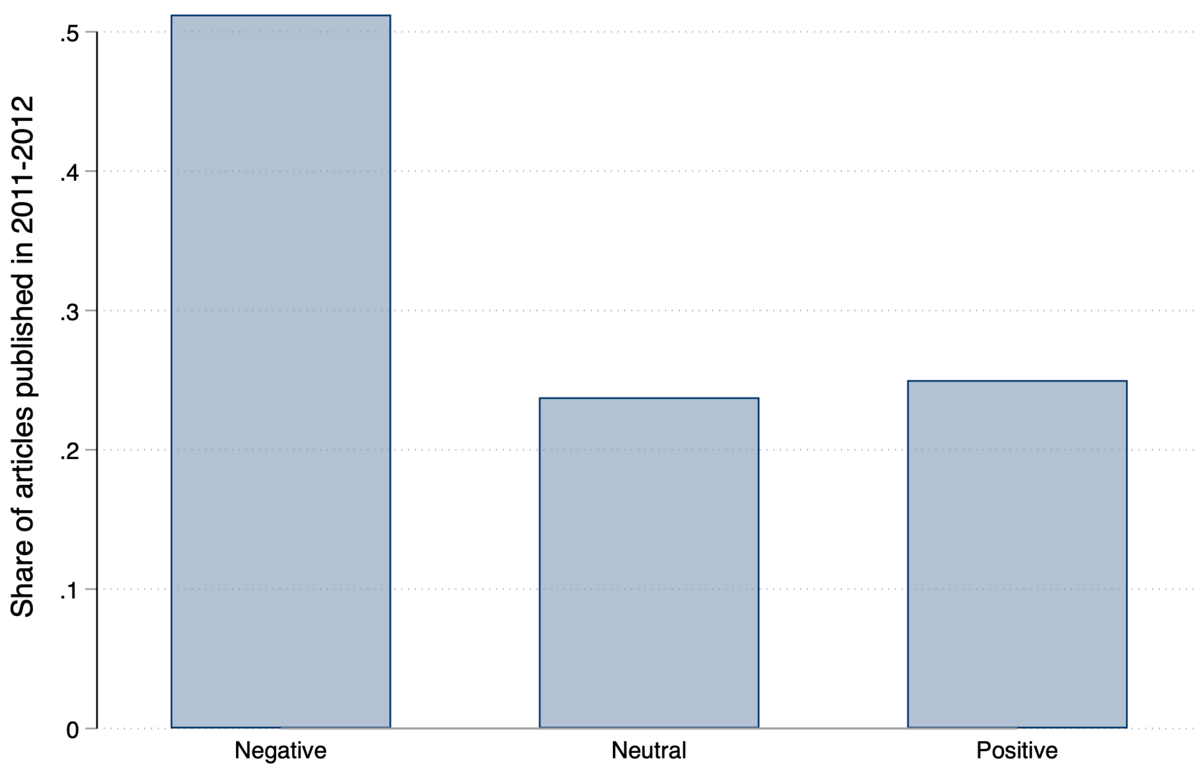
Notes: Panel (a) shows a distribution of the share of GOP votes in the 2010 gubernatorial election, separately for wards located in school districts with CBAs that expired in 2011 and were not extended (treated, thick solid line) and wards in districts with CBAs (or extensions) that expired after 2011 (untreated, dashed line). 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 A8: Political Effects of Wisconsin's Act 10: Dynamic Difference-in-Differences Estimates



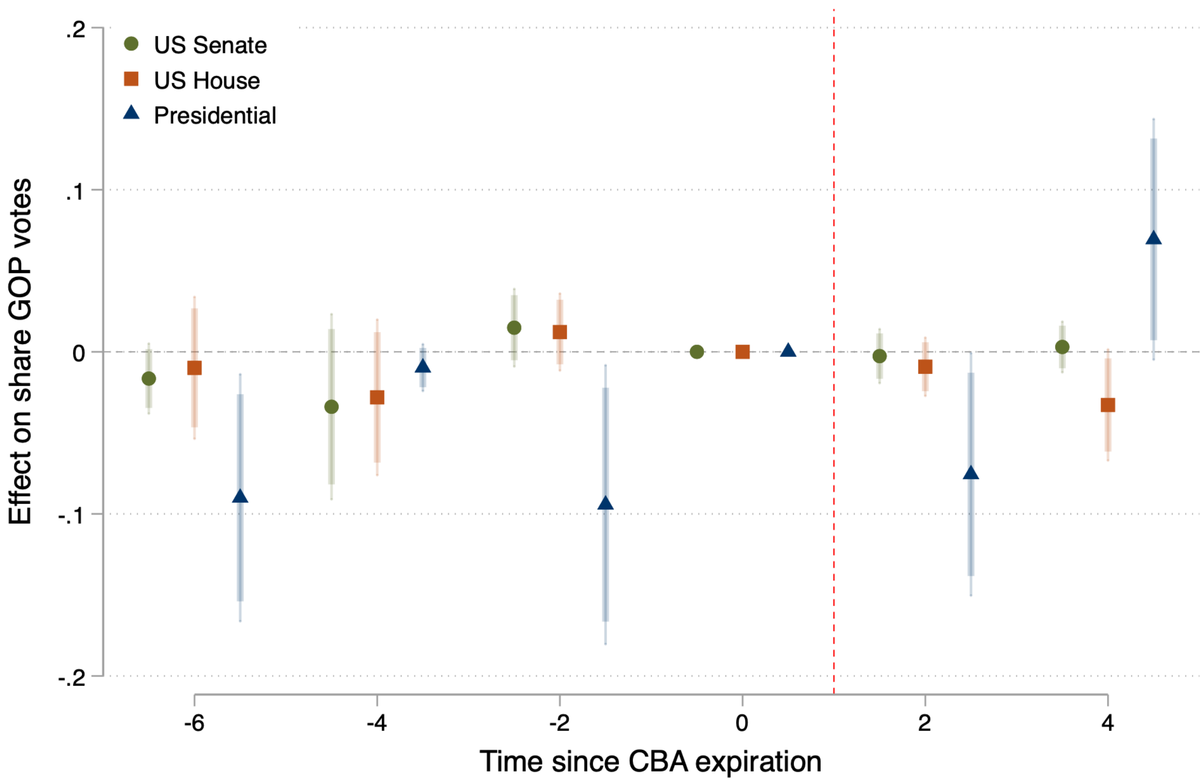
Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_k in equation (3), estimated on the full sample of districts (circles), and on the sample of districts that excludes Milwaukee Public Schools (squares). Outcome is Republican vote share in gubernatorial race. Confidence intervals are obtained using standard errors clustered at the district level.

Figure A9: Sentiment Analysis of Newspaper Articles on Act 10, 2011-12



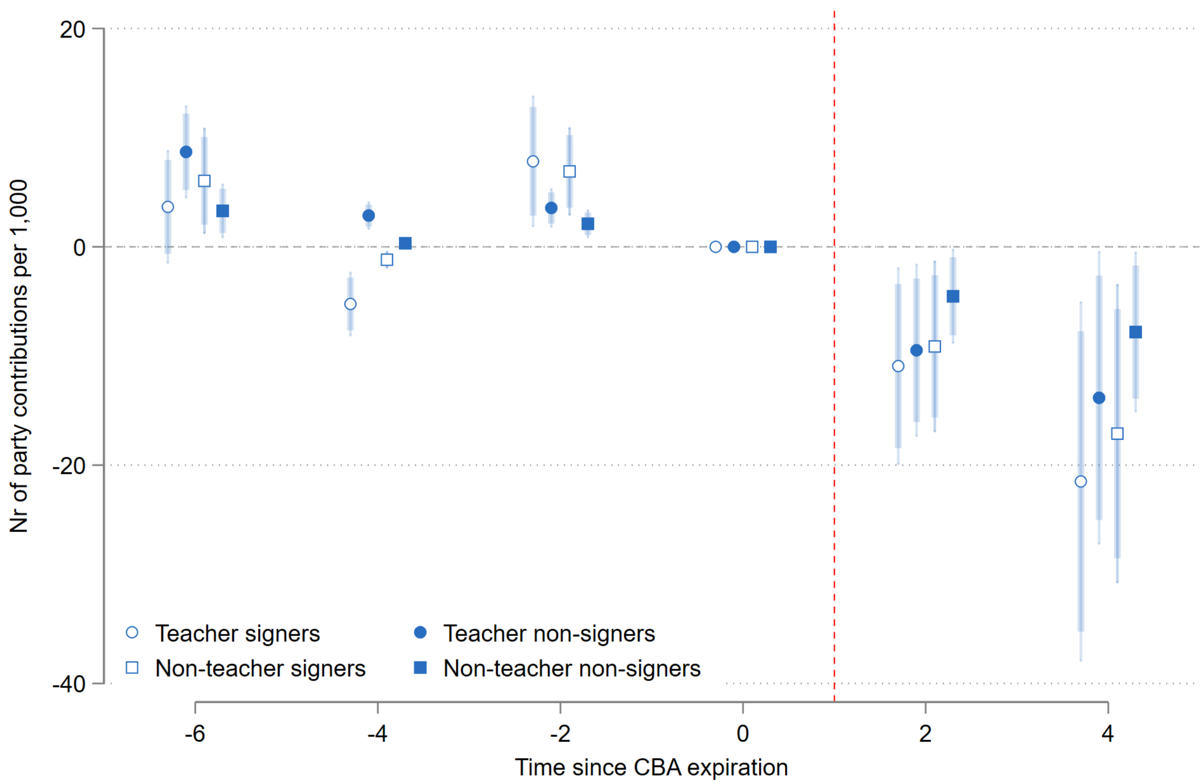
Notes: Share of articles in national and local newspapers published in 2011 and 2012 and containing the keywords "Act 10" and"school," by news sentiment. Sentiment analysis performed using the large-language model ChatGPT 4.0.

Figure A10: Spillover Effects of Wisconsin's Act 10 onto Other Races: Event Study Estimates



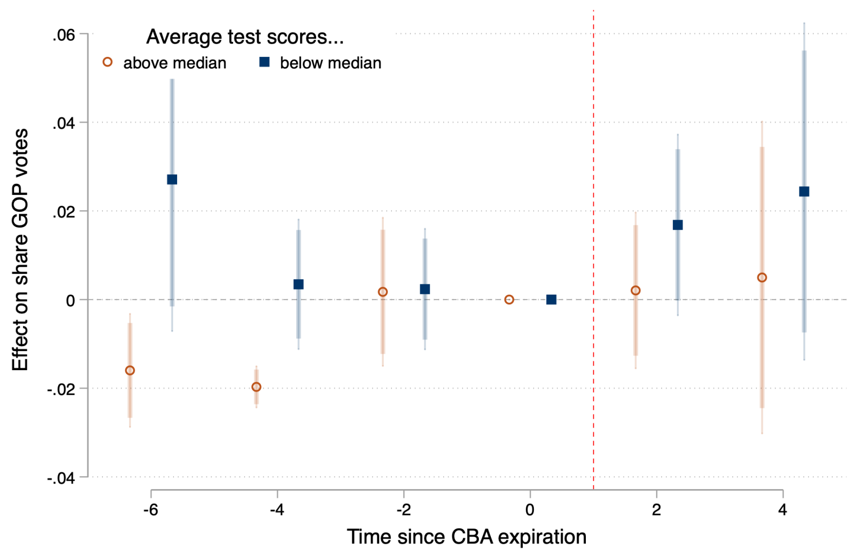
Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_k in equation (1), estimated using ward-level GOP vote shares for U.S. Presidential, House, and Senate elections. Confidence intervals are obtained using standard errors clustered at the district level.

Figure A11: Democratic Campaign Contributions, by Teachers and Petition Signers: Event-Study Estimates



Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_s in equation (1), obtained using the number of contributions to Democratic gubernatorial candidates, by people who did and did not sign the recall election petition, who were or were not teachers. Number of contributions for teachers is measured per 1,000 teachers in the district; number of contributions for non-teachers is measured per 1,000 people in the district. Observations are weighted by district population. Confidence intervals are obtained using standard errors clustered at the district level.

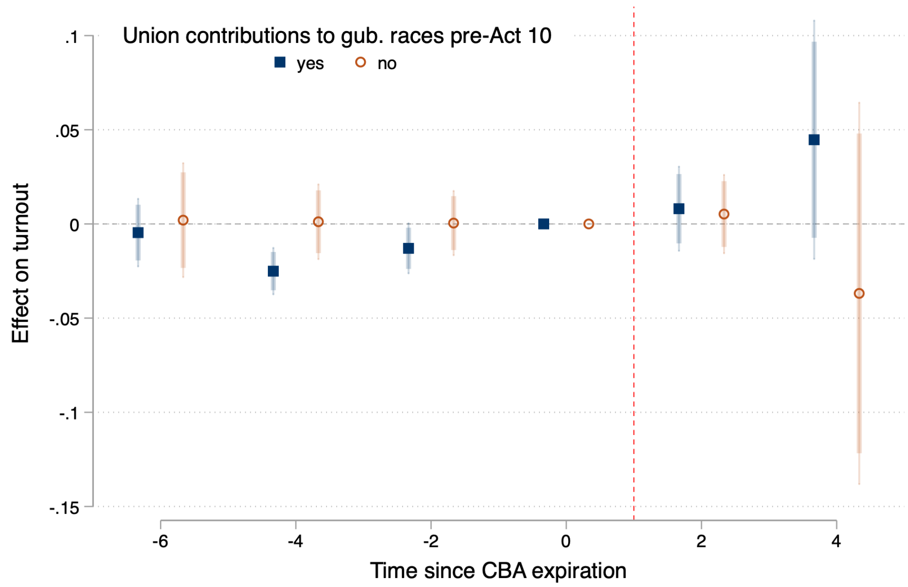
Figure A12: Winners and Losers of Wisconsin's Act 10: Event-Study Estimates by Ex Ante Student Test Scores



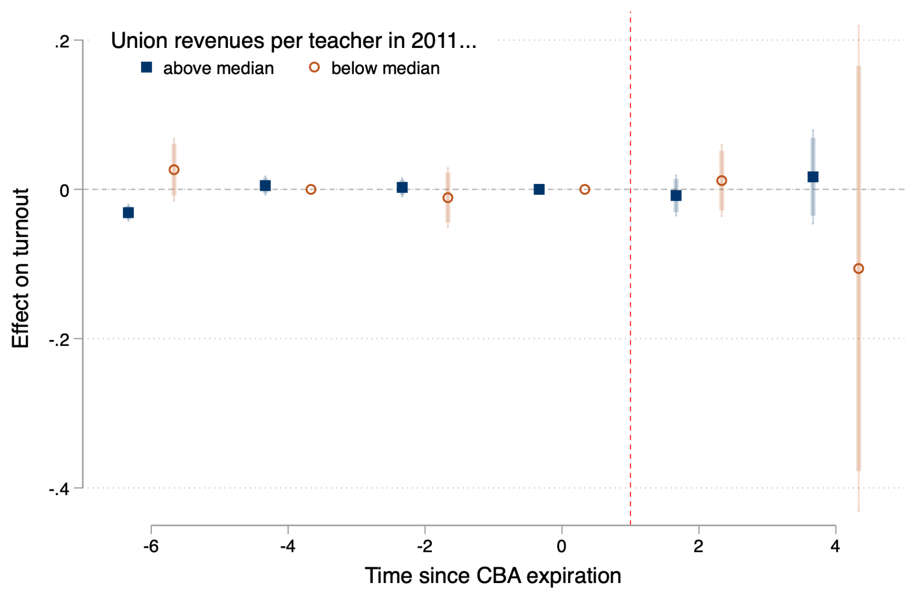
Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_k in equation (1), estimated using the GOP vote share in gubernatorial elections in each ward and year as the dependent variable and controlling for ward and year fixed effects. The squares show estimates for the subsample of districts in with 2010-11 average test scores below the state median and the circles show estimates for districts with scores above the median. Confidence intervals are obtained using standard errors clustered at the district level.

Figure A13: The Impact of Unions on Voter Turnout. Event-Study Estimates, by Ex Ante Union Campaign Contributions and Revenues

(a) By ex ante campaign contributions

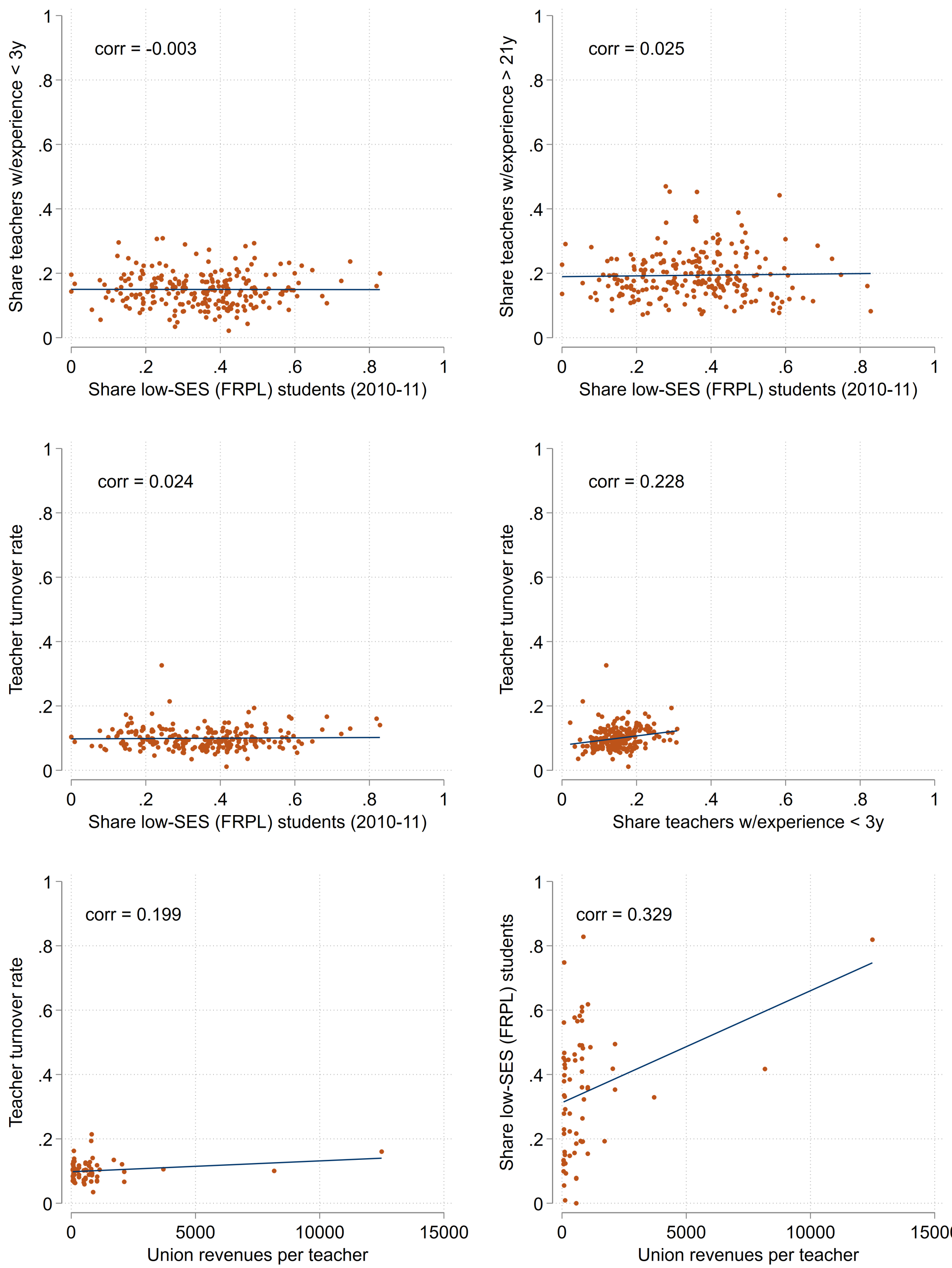


(b) By ex ante revenues



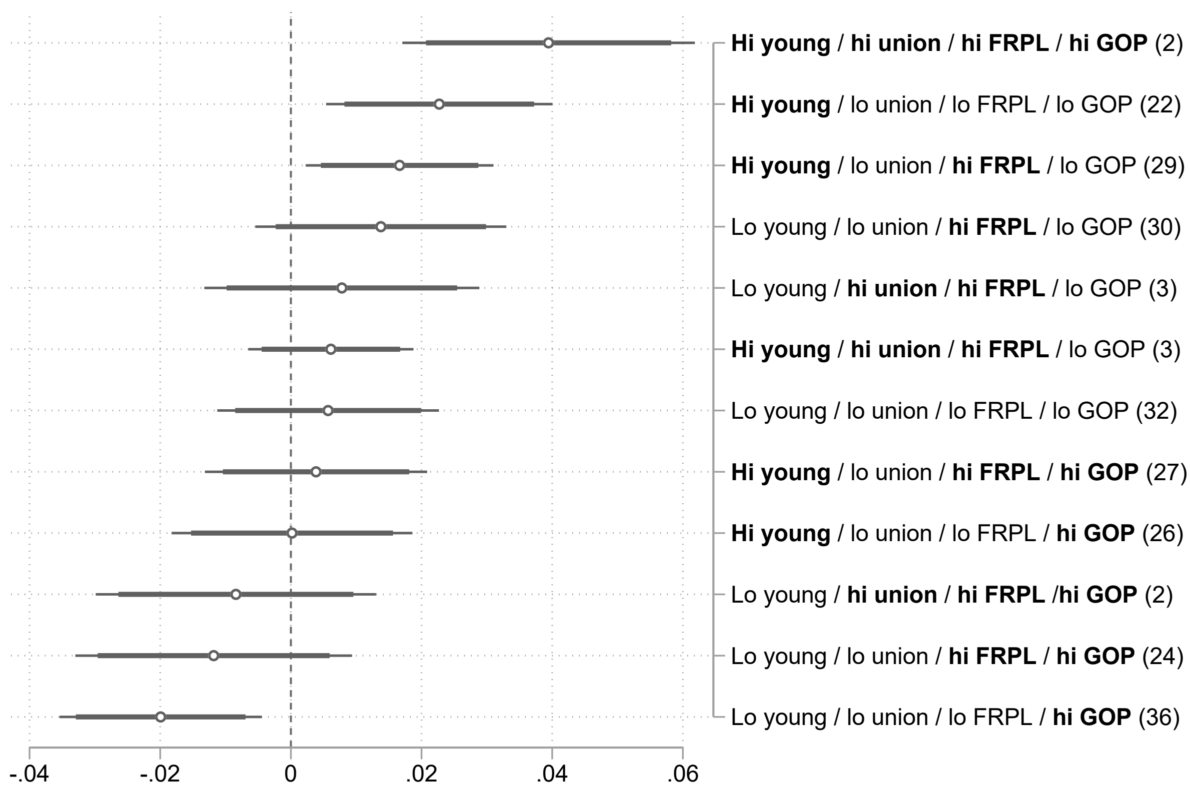
Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_k in equation (1), estimated using voter turnout (the ratio between the number of votes cast and the number of people aged 18 and older) in each ward and year as the dependent variable and controlling for ward and year fixed effects. In panel (a), the squares show estimates for wards in districts whose unions made campaign contributions to gubernatorial races prior to 2011 the circles show estimates for wards in districts with no contributions. In panel (b), the squares show estimates for the subsample of wards in districts with union revenues per teacher above the state median in 2011; the circles show estimates for wards with union revenues below the median. Confidence intervals are obtained using standard errors clustered at the district level.

Figure A14: Correlations Between District Characteristics



Notes: Scatter plots and correlations between various district-level characteristics, measured prior to 2011. Information on teacher turnover and experience is available for 235 districts; on share of FRPL students for 213 districts; and union revenues for 68 districts.

Figure A15: Political Effects of Wisconsin's Act 10, by group



Notes: Estimates and 95% confidence intervals of the coefficients β_s in equation (1), for different groups of districts. Number of districts in each cell shown in parentheses

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

		Expiration in	
	All districts	No extension	W/extension
Population age			
Share pop < 18 yo	0.23	0.23	0.23
	(0.067)	(0.061)	(0.071)
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	0.024	0.12	-0.044
	(0.33)	(0.28)	(0.35)
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: Effects of Wisconsin's Act 10 and 2010 Share of GOP Votes: Event Study Estimates

	(1)	(2)
Exposed	0.056***	0.056***
	(0.019)	(0.018)
Exposed × 2010 GOP share	-0.086***	-0.086***
	(0.032)	(0.030)
District FE	Yes	No
Ward FE	No	Yes
Year FE	Yes	Yes
Mean dep. var. control	0.477	0.476
N	21222	21221
Clusters (districts)	236	236
R-squared	0.92	0.95

Notes:The dependent variable is the share of GOP votes in gubernatorial elections in each ward and year. The variable *Exposed* equals one in years following a CBA expiration in each district. The variable *2010 GOP share* is the GOP vote share in the 2010 gubernatorial election in each ward. 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. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.

Table A3: 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. The variable *Exposed* equals one in years following a CBA expiration in each district. 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. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.

Table A4: Spillover Effects of Wisconsin's Act 10 onto Other Races: 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). The variable *Exposed* equals one in years following a CBA expiration in each district. 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 A5: Political Effects of Wisconsin's Act 10: Difference-in-Differences Estimates, Robustness Checks

	Ignoring extensions	Excluding Milwaukee	Only fully aligned wards			
	(1)	(2)	(3)	(4)	(5)	(6)
CBA after 2011 * post 2011	0.038*	0.037*	0.011*	0.010*	0.020*	0.020*
	(0.020)	(0.020)	(0.006)	(0.006)	(0.010)	(0.010)
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	0.465	0.465	0.489	0.489	0.438	0.438
N	24550	24545	22935	22930	14344	14340
Clusters (districts)	236	236	235	235	231	231
R-squared	0.73	0.93	0.72	0.92	0.77	0.93

Notes: The dependent variable is the share of GOP votes in gubernatorial elections in each ward and year. The variable *CBA after 2011* equals one for districts with CBAs expiring after 2011. All specifications control for year fixed effects; columns 1, 3, and 5 control for district fixed effects and columns 2, 4, and 6 control for ward fixed effects. Columns 1-2 are estimated considering only CBA expirations and ignoring extensions to construct the *CBA after 2011* variable. Columns 3-4 are estimated excluding Milwaukee. Columns 5-6 are 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 A6: Political Effects of Wisconsin's Act 10: Pooled Event Study, By Ex Ante Test Scores

	Q1	Q2-Q3	Q4	All
	(1)	(2)	(3)	(4)
Exposed	0.038***	0.006	0.012**	
	(0.011)	(0.005)	(0.006)	
Exposed * Q1				0.043**
				(0.017)
Exposed * Q2				0.005
				(0.008)
Exposed * Q3				0.008
				(0.009)
Exposed * Q4				0.008
				(0.008)
Ward FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No
Year * qtile FE	No	No	No	Yes
Mean dep. var. control	0.386	0.526	0.464	0.481
N	4894	9912	6427	19798
Clusters (districts)	44	121	71	214
R-squared	0.95	0.89	0.96	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 constant across all $k > 0$. We split the sample by the quartile of average district test scores, measured in 2010-11. Q1, Q2, Q3, and Q4 refer to the first, second, third, and fourth quartiles of each variable, respectively. Columns 1-3 and 5-7 control for ward and year fixed effects; columns 4 and 8 control for ward and quartile-year fixed effects. Standard errors in parentheses are clustered at the district level. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.

Table A7: Political Effects of Wisconsin's Act 10: Pooled Event Study, interacted with continuous measure of ex ante union revenues

	(1)	(2)
Exposed	-0.019	-0.021
	(0.025)	(0.025)
Union revenues per member (2011)	0.000	0.000
	(0.000)	(0.000)
Exposed $\times$ Union revenues per member (2011)	0.005	0.006
	(0.004)	(0.004)
District FE	Yes	No
Ward FE	No	Yes
Year FE	Yes	Yes
Mean dep. var. control	0.437	0.437
N	10098	10097
Clusters (districts)	69	69
R-squared	0.75	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 $k > 0$. We include 2011 district-level union revenues per member as a control, and interact it with a dummy for whether the district's CBA has already expired. Column 1 controls for district fixed effects; Column 2 controls for ward fixed effects. Standard errors in parentheses are clustered at the district level. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.

Table A8: Political Effects of Wisconsin's Act 10: Pooled Event Study, interacted with continuous measure of share of young ($\leq$ 27) or old ($\geq$ 55) teachers in district

	(1)	(2)	(3)	(4)
Exposed	0.010	0.009	-0.006	-0.007
	(0.011)	(0.010)	(0.015)	(0.015)
Exposed $\times$ Share age 27 and below	0.060	0.078		
	(0.133)	(0.132)		
Exposed $\times$ Share age 55 and above			0.104	0.107
			(0.073)	(0.073)
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 $k > 0$. We include a control for the share of teachers aged 27 and younger (or the share of teachers aged 55 or over) in 2010-2011, and interact this control with a dummy for whether the district's CBA has already expired. Column 1 controls for district fixed effects; Column 2 controls for ward fixed effects. Standard errors in parentheses are clustered at the district level. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.

Table A9: Political Effects of Wisconsin's Act 10: Pooled Event Study, interacted with continuous measure of share of inexperienced ($\leq$ 3 years) or experienced ($\geq$ 21 years) teachers in district

	(1)	(2)	(3)	(4)
Exposed	-0.017	-0.017	0.023	0.023
	(0.013)	(0.013)	(0.016)	(0.016)
Exposed $\times$ Share w/0-3 yrs of experience	0.201**	0.204**		
	(0.096)	(0.096)		
Exposed $\times$ Share w/more than 21 yrs of experience			-0.055	-0.057
			(0.079)	(0.078)
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	0.74	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 $k > 0$. We include a control for the share of teachers with 3 years or less of experience (or the share of teachers with 21 years or more of experience) in 2010-2011, and interact this control with a dummy for whether the district's CBA has already expired. Column 1 controls for district fixed effects; Column 2 controls for ward fixed effects. Standard errors in parentheses are clustered at the district level. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.

Table A10: Political Effects of Wisconsin's Act 10: Pooled Event Study, interacted with continuous measure of ex ante teacher turnover in district

	(1)	(2)
Exposed	-0.013	-0.014
	(0.017)	(0.017)
Exposed × Teacher turnover	0.271	0.277
	(0.180)	(0.180)
District FE	Yes	No
Ward FE	No	Yes
Year FE	Yes	Yes
Mean dep. var. control	0.476	0.476
N	21158	21151
Clusters (districts)	235	235
R-squared	0.74	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 $k > 0$. We include a control for the district's turnover rate in 2010-11 (defined as the share of teachers who leave the district in that year), and interact this control with a dummy for whether the district's CBA has already expired. Column 1 controls for district fixed effects; Column 2 controls for ward fixed effects. Standard errors in parentheses are clustered at the district level. * ≤ 0.1; ** ≤ 0.05; *** ≤ 0.01.

Table A11: Political Effects of Wisconsin's Act 10: Pooled Event Study, interacted with continuous measure of the share of FRPL students in district

	(1)	(2)
Exposed	-0.020*	-0.020*
	(0.010)	(0.010)
Exposed $\times$ Share FRPL students	0.088***	0.088***
	(0.027)	(0.027)
District FE	Yes	No
Ward FE	No	Yes
Year FE	Yes	Yes
Mean dep. var. control	0.476	0.476
N	21240	21233
Clusters (districts)	236	236
R-squared	0.74	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 $k > 0$. We include a control for the share of the district's students on free and reduced price lunch (FRPL) in 2010-11, and interact this control with a dummy for whether the district's CBA has already expired. Column 1 controls for district fixed effects; Column 2 controls for ward fixed effects. Standard errors in parentheses are clustered at the district level. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.

Table A12: Political Effects of Wisconsin's Act 10: Pooled Event Study, interacted with continuous measures of population age distribution

	(1)	(2)	(3)	(4)	(5)	(6)
Exposed	0.006	0.022**	-0.005	0.014	0.032***	0.020**
	(0.024)	(0.009)	(0.027)	(0.013)	(0.011)	(0.008)
Share of HHs with children	0.000					
	(0.000)					
Exposed × Share of HHs with children	0.032	-0.030				
	(0.112)	(0.046)				
Share under 18			0.000			
			(0.000)			
Exposed × Share under 18			0.082	0.001		
			(0.128)	(0.067)		
Share 65+					0.000	
					(0.000)	
Exposed × Share 65+					-0.130**	-0.048*
					(0.052)	(0.029)
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	0.476	0.476	0.476	0.476	0.476	0.476
N	21161	21155	21192	21186	21192	21186
Clusters (districts)	236	236	236	236	236	236
R-squared	0.74	0.94	0.74	0.94	0.74	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 $k > 0$. Odd columns control for district fixed effects; even columns control for ward fixed effects. In each regression using district fixed effects, we include a different control for age distribution of the population (measured at census block level and aggregated up to ward level): share of households with at least one child age 6-17; share of population under 18; and share of population 65 or over. In regressions with ward fixed effects these controls are absorbed and hence omitted. In all regressions, we also interact this control with a dummy for whether the district's CBA has already expired. Standard errors in parentheses are clustered at the district level. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.

Table A13: Political Effects of Wisconsin's Act 10: Pooled Event Study, interacted with continuous measure of 2010 GOP gubernatorial vote share

	(1)	(2)
Exposed	0.056***	0.056***
	(0.019)	(0.018)
Exposed × 2010 GOP share	-0.086***	-0.086***
	(0.032)	(0.030)
District FE	Yes	No
Ward FE	No	Yes
Year FE	Yes	Yes
Mean dep. var. control	0.477	0.476
N	21222	21221
Clusters (districts)	236	236
R-squared	0.92	0.95

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 $k > 0$. We include a control for the share of GOP vote share in the gubernatorial election of 2010, and interact this control with a dummy for whether the district's CBA has already expired. Column 1 controls for district fixed effects; Column 2 controls for ward fixed effects. Standard errors in parentheses are clustered at the district level. * ≤ 0.1; ** ≤ 0.05; *** ≤ 0.01.

Table A14: Political Effects of Wisconsin's Act 10: Pooled Event Study — dimensions of heterogeneity

	All dimensions		LASSO-selected	
	(1)	(2)	(3)	(4)
Exposed	-0.001	-0.001	0.004	0.005
	(0.011)	(0.011)	(0.008)	(0.008)
Exposed × High Union Donations	0.031***	0.030***	0.004	0.002
	(0.009)	(0.009)	(0.012)	(0.012)
Exposed × High Young Teachers	0.017**	0.018**	0.014*	0.014**
	(0.007)	(0.007)	(0.007)	(0.007)
Exposed × High FRPL	0.011	0.012	0.005	0.006
	(0.009)	(0.009)	(0.009)	(0.008)
Exposed × High GOP 2010	-0.022**	-0.023**	-0.020**	-0.021***
	(0.009)	(0.009)	(0.008)	(0.008)
Exposed × High Teacher Turnover	-0.005	-0.004		
	(0.007)	(0.007)		
Exposed × Low Test Scores	0.004	0.005		
	(0.008)	(0.008)		
Exposed × High HHs w/ kids	0.003	0.002		
	(0.009)	(0.009)		
Exposed × High Old Teachers	0.005	0.006		
	(0.008)	(0.008)		
District FE	Yes	No	Yes	No
Ward FE	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
Mean dep. var. control	0.482	0.482	0.474	0.474
N	21567	21561	23250	23244
Clusters (districts)	213	213	236	236
R-squared	0.72	0.94	0.74	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 2 include interactions of the 'Exposed' dummy with the four dimensions of heterogeneity selected by a LASSO regression including the interaction of 'Exposed' with all eight dimensions present in this table. Columns 3 and 4 include all eight of these interactions. Odd columns control for district fixed effects; even columns control for ward fixed effects. All regressions include controls for the interaction of year dummies with each of the dimensions of heterogeneity included in the regression. Standard errors in parentheses are clustered at the district level. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.

Table A15: Political Effects of Wisconsin's Act 10: Pooled Event Study, by district characteristics

	All dimensions		LASSO-selected	
Lo young / lo union / lo FRPL / lo GOP	0.006	0.007	0.012	0.012*
	(0.009)	(0.009)	(0.007)	(0.007)
Hi young / lo union / lo FRPL / lo GOP	0.023**	0.024***	0.012	0.012*
	(0.009)	(0.009)	(0.007)	(0.007)
Lo young / ***hi union*** / lo FRPL / lo GOP	-0.000	0.000	0.008	0.008
	(0.015)	(0.015)	(0.011)	(0.011)
Lo young / lo union / ***hi FRPL*** / lo GOP	0.014	0.015	0.018**	0.019**
	(0.010)	(0.010)	(0.008)	(0.008)
Lo young / lo union / lo FRPL / ***hi GOP***	-0.020**	-0.021***	-0.016**	-0.017**
	(0.008)	(0.008)	(0.007)	(0.007)
Hi young / ***hi union*** / lo FRPL / lo GOP	0.012	0.012	0.008	0.008
	(0.012)	(0.012)	(0.011)	(0.011)
Hi young / lo union / ***hi FRPL*** / lo GOP	0.017**	0.018**	0.013*	0.014**
	(0.007)	(0.007)	(0.007)	(0.007)
Hi young / lo union / lo FRPL / ***hi GOP***	0.000	-0.000	-0.002	-0.002
	(0.009)	(0.009)	(0.008)	(0.008)
Lo young / ***hi union*** / ***hi FRPL*** / lo GOP	0.008	0.008	0.015*	0.015*
	(0.011)	(0.011)	(0.009)	(0.009)
Lo young / ***hi union*** / lo FRPL / ***hi GOP***	-0.017	-0.018	-0.019*	-0.022*
	(0.015)	(0.015)	(0.012)	(0.012)
Lo young / lo union / ***hi FRPL*** / ***hi GOP***	-0.012	-0.013	-0.009	-0.010
	(0.011)	(0.011)	(0.010)	(0.010)
Hi young / ***hi union*** / ***hi FRPL*** / lo GOP	0.006	0.006	0.009	0.009
	(0.006)	(0.007)	(0.007)	(0.007)
Hi young / ***hi union*** / lo FRPL / ***hi GOP***	0.036**	0.033**	-0.006	-0.007
	(0.015)	(0.015)	(0.012)	(0.012)
Hi young / lo union / ***hi FRPL*** / ***hi GOP***	0.004	0.005	-0.001	-0.001
	(0.009)	(0.009)	(0.008)	(0.008)
Lo young / ***hi union*** / ***hi FRPL*** / ***hi GOP***	-0.008	-0.010	-0.013	-0.015
	(0.011)	(0.011)	(0.011)	(0.011)
Hi young / ***hi union*** / ***hi FRPL*** / ***hi GOP***	0.039***	0.038***	0.038***	0.037***
	(0.011)	(0.011)	(0.011)	(0.011)
Mean dep. var. control	0.474	0.474	0.474	0.474
N	23250	23244	23250	23244
Clusters (districts)	236	236	236	236
FE	District	Ward	District	Ward

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 $k > 0$. All regressions include year fixed effects. Columns 1 and 2 include interactions of the 'Exposed' dummy with the four dimensions of heterogeneity selected by a LASSO regression including the interaction of 'Exposed' with all eight dimensions present in this table. Columns 3 and 4 include all eight of these interactions. Odd columns control for district fixed effects; even columns control for ward fixed effects. All regressions include controls for the interaction of year dummies with each of the dimensions of heterogeneity included in the regression. Standard errors in parentheses are clustered at the district level. * ≤ 0.1; ** ≤ 0.05; *** ≤ 0.01.

Table A16: Political Effects of Wisconsin's Act 10: Pooled Event Study, with interactions

	All dimensions		LASSO-selected	
Exposed	0.006	0.007	0.012	0.012*
	(0.009)	(0.009)	(0.007)	(0.007)
Exposed × Hi Young Teachers	0.017*	0.017*		
	(0.009)	(0.009)		
Exposed × Hi Union Donations	-0.006	-0.006	-0.003	-0.004
	(0.012)	(0.012)	(0.009)	(0.009)
Exposed × Hi FRPL	0.008	0.008	0.007	0.007
	(0.012)	(0.011)	(0.010)	(0.010)
Exposed × Hi GOP 2010	-0.026**	-0.028***	-0.028***	-0.030***
	(0.010)	(0.010)	(0.009)	(0.009)
Exposed × Hi Young × Hi Union	-0.005	-0.005		
	(0.010)	(0.010)		
Exposed × Hi Young × Hi FRPL	-0.014	-0.014	-0.006	-0.005
	(0.010)	(0.010)	(0.007)	(0.007)
Exposed × Hi Young × Hi GOP	0.003	0.004	0.014*	0.015**
	(0.011)	(0.011)	(0.007)	(0.007)
Exposed × Hi Union × Hi FRPL	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)
Exposed × Hi Union × Hi GOP	0.009	0.009		
	(0.011)	(0.012)		
Exposed × Hi FRPL × Hi GOP	0.000	0.000		
	(0.013)	(0.013)		
Exposed × Hi Young × Hi Union × Hi FRPL	0.000	0.000		
	(.)	(.)		
Exposed × Hi Young × Hi Union × Hi GOP	0.037**	0.036**		
	(0.016)	(0.016)		
Exposed × Hi Young × Hi FRPL × Hi GOP	0.010	0.011		
	(0.016)	(0.016)		
Exposed × Hi Union × Hi FRPL × Hi GOP	0.000	0.000		
	(.)	(.)		
Exposed × Hi Young × Hi Union × Hi FRPL × Hi GOP	0.000	0.000	0.043***	0.042***
	(.)	(.)	(0.009)	(0.009)
Mean dep. var. control	0.474	0.474	0.474	0.474
N	23250	23244	23250	23244
Clusters (districts)	236	236	236	236
FE	District	Ward	District	Ward

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 $k > 0$. All regressions include district and year fixed effects. Columns 1 and 2 include interactions of the 'Exposed' dummy with the four dimensions of heterogeneity selected by a LASSO regression including the interaction of 'Exposed' with all eight dimensions present in this table. Columns 3 and 4 include all eight of these interactions. Odd columns control for district fixed effects; even columns control for ward fixed effects. All regressions include controls for the interaction of year dummies with each of the dimensions of heterogeneity included in the regression. Standard errors