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IZA DP No. 17836

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ISSN: 2365-9793

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ABSTRACT

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

Public service reforms often provoke political backlash. Can they also yield political benefits for the politicians who champion them? We study a Wisconsin law that weakened teachers' unions and liberalized pay, prompting mass protests. Exploiting its staggered implementation across school districts, we find that the reform cut union revenues, raised student test scores, and increased pay for some teachers. Exposure to the law increased the incumbent governor's vote share by about 20% of his margin of victory and reduced campaign contributions to his opponent. Gains were larger in districts with stronger unions ex ante and in those where more voters benefited from the reform. Our findings highlight how even politically risky reforms can generate electoral benefits under the right circumstances.

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

Keywords: education reform, political feasibility, collective bargaining, teacher salaries

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* We thank Jaime Arellano-Bover, Maria Carreri, Caroline Hoxby, Edoardo Teso, Seth Zimmerman, and seminar and conference participants at various institutions for useful comments. Diogo Conceição, João Dourado, Ariel Gelrud, and Francesca Rinaldi provided outstanding research assistance. This work was funded by Fundação para a Ciência e a Tecnologia (UIDB/00124/2020, UIDP/00124/2020 and Social Sciences - DataLab PINFRA/22209/2016), POR Lisboa and POR Norte (Social Sciences DataLab, PINFRA/22209/2016). We are also grateful for support from Yale University and The Broad Center at the Yale School of Management. All errors are our own.

1 Introduction

The political effects of education reforms 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 favor continuity. 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.[1] 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 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 in any given district after the expiration of its preexisting collective-bargaining agreements (CBA), creating quasi-random variation in voters' exposure to the consequences of the reform. Second, the reform was followed by a gubernatorial recall election—widely seen as a referendum on Act 10—as well as a regular election two years later, which provide us with an opportunity to evaluate the electoral effects of exposure

[1] For example, Florida's Senate Bill 6 of 2010 proposed to tie teacher pay and job security to student test scores and to eliminate tenure. The bill was met with enormous backlash and was ultimately vetoed by the state's Republican governor (Hamacher, 2010). Another example is Michigan's Proposal 1 of 2000, which would have provided a $3,300 voucher to each student to attend private school. The proposal generated a heated and expensive battle between pro- and anti-voucher groups. After being defeated at the polls, the initiative was removed from the policy agenda (Keenon, 2001).

to the reform.

We show that the reform was a political success for the incumbent governor, increasing his vote share by 1.4 percentage points (pp hereafter), or over 20% of his margin of victory. These political gains were larger in areas with ex ante stronger and more politically involved unions, whose power fell after the reform. Gains were also larger in districts where more people benefited from the reform: teachers with higher pay and students with stronger test score improvements. Our findings highlight the importance of both concrete policy outcomes and the role of institutional actors in shaping the political consequences of the reform.

Passed in March 2011, Act 10 weakened the collective bargaining power of public sector unions. Specifically, the law prohibited collective bargaining over teachers' salary schedules, giving school districts flexibility in setting individual teacher pay. In addition, Act 10 cut teachers' health care and pension benefits, generating important savings for school districts. The reform also prohibited unions from automatically collecting dues from employees' paychecks and required them to recertify every year. Although the law also affected other public-sector employees, it was widely perceived as a direct attack on teachers. Waves of protest against the law broke in Madison throughout the Spring of 2011; opponents gathered over 900,000 signatures to force a gubernatorial recall election in June of 2012 that many viewed as a referendum on Act 10 (Biskupic, 2013). The governor not only survived the recall—becoming the first incumbent to do so in U.S. history—but also won reelection in the regular gubernatorial election of 2014. These two elections serve as a clear measure of voter support for the elected official most closely 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' preexisting 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 those from difference-in-differences models, which compare districts exposed to Act 10 prior to the 2012 election with 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 gubernatorial 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. We further link these records to district-level information on students' demographics and test scores, teachers' demographics and salaries, and districts' turnover and retention rates, all constructed using data from the Wisconsin Department of Public Instruction. We also collect unions' financial 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. Unions were hurt by the law: Revenues per member fell by over 50% following its implementation, likely resulting from 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 allowing districts 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. Finally, students 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 two to four 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 (21-25%) of the margin of victory in the 2012 and 2014 elections. Reassuringly, trends in GOP vote share 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 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 voter turnout.

Since Act 10 was a high-profile statewide reform, the sizable political effects of district-level 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. By design, our estimates capture the effect of direct exposure to the consequences of the reform. 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 on 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 sections of the paper, we investigate the mechanisms at play behind the political effects of Act 10. We explore two sets of mechanisms. The first are the direct benefits from the reform's implementation on various constituencies of voters (such as teachers and students). If these benefits were visible or predictable, they could have boosted the share of GOP votes. The second is a decline in the power of unions, who were directly targeted by the law, and who play an important role in shaping both policy and the reactions to it. We assess the importance of these mechanisms by comparing Walker's electoral gains in the 2012 and 2014 elections across school districts with varying pre-reform characteristics.

We find empirical support for both sets of mechanisms: Gains were larger in districts with a larger constituency who gained from the reform and a stronger union presence ex ante. Specifically, the GOP vote share increased more in districts with a higher share of young, less experienced teachers (who experienced salary increases after Act 10); in those with a higher share of families with school-age children; and in areas with higher teacher turnover (which stood to benefit more from the introduction of flexible pay, as shown by Biasi, 2021). Electoral gains were also larger 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). 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 to identify the most significant predictors of treatment effects, we find that electoral gains were generally larger in districts with a high share of inexperienced teachers, a high share of poorer students, and ex ante politically active unions. This suggests that the reform won votes not only by weakening unions, but also by improving the experience of public education for a significant share of providers and users.

Although public service reforms such as Act 10 often carry salient political costs, our results indicate that these costs can be outweighed by electoral benefits for the politicians and parties that champion them under certain circumstances. In particular, our findings suggest that a reform can be a political success if it delivers meaningful benefits to a sufficiently large segment of the electorate. Our results also underscore the crucial role of unions in shaping the political responses to a policy change. 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. We also document a correlation between increased GOP support following Act 10 and the 2016 vote share for Donald Trump, highlighting the potential for persistent political consequences when governments improve voters' experience of public services.

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 positive or null effects on student outcomes (Matsudaira and Patterson, 2017; 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, a vital dimension of the feasibility of reform.

Additionally, our findings contribute to the literature on the effects of Act 10 (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 led to important increases in student learning, especially among disadvantaged students, and that these improvements had positive effects on the incumbent governor's re-election.

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 (see, for example, Brender and Drazen, 2008; Carreri and Martinez, 2021).[2] Studies on the electoral effects of specific policies show that large, salient infrastructure projects tend to win votes (Huet-Vaughn, 2019; Boudot-Reddy and Butler, 2024; Voigtländer and Voth, 2021; Harding, 2015; Leff Yaffe et al., 2025; 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. Recent studies have found well-identified evidence of electoral accountability for education quality, though none take place in a democracy as well-established as the U.S. (Sandholtz, 2023; Dias and Ferraz, 2019; Litschig and Morrison, 2013; Cox et al., 2024). A notable exception is Cook et al. (2020), who find that charter school entry in Ohio affected turnout—but not vote share—in school board elections. 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 draw a link between Act 10's electoral effects and its impact on

[2] 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. Fetzer (2019) shows that UK voters punished austerity.

union strength, highlighting unions' power to influence the adoption of education reforms (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 Walker, 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. While in principle affecting all public-sector employees, several large categories (such as local law enforcement and fire personnel) were exempt. As a result, the reform was perceived by most to be a direct attack to the teacher profession: A Google Term search of the term "Wisconsin Act 10" between January 1, 2011 and December 31, 2012 yields "teachers" as the most searched associated topic.

First, Act 10 imposed significant operational constraints on unions. It required 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 accounting 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 has been 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. 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.[3]

Efforts to overturn the legislation and counter Scott Walker's anti-union agenda persisted until 2012. A lawsuit challenging the bill's constitutionality, 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. Attempts to reverse Act 10 reached their climax with a gubernatorial recall election in June 2012, which saw Scott Walker oppose Democrat Tom Barrett; close to a million people signed the petition (in 2011, Wisconsin had an adult population of 4.2 million). Walker defeated Barrett by 6.8 pp, becoming the first governor in U.S. history to win a recall election. He then secured reelection in 2014 against Democrat Mary Burke with a 5.7-pp 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 the electoral performance of its most prominent proponent.[4]

3 Data

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

3.1 Voting Outcomes

Our primary outcomes are vote shares for the Republican candidate in gubernatorial and federal elections held between 2002 and 2014. We mainly focus on gubernatorial races and examine Presidential, U.S. House, and U.S. Senate races in robustness checks. We draw information on vote

[3]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.

[4]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."

counts from the Wisconsin Legislative Technology Services Bureau (2011) (WLTSB), hosted by the University of Wisconsin. We measure vote shares 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 meetings minutes, and local news reports.[5] 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 Wisconsin public-school teachers. Our teacher data come from the PI-1202 Fall Staff

[5] 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.

Report – All Staff Files maintained by the Wisconsin Department of Public Instruction (WDPI), spanning 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 (FRPL, our proxy for socioeconomic status), and math test scores on the state standardized exam.[6] 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 to calculate the share of households with children aged 18 and below. 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

[6] 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.

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, or WERC).

3.7 Recall Petition Signers

We link data on 2012 recall petition signers with campaign contribution records to measure contributors' ex ante opposition to Act 10 and Governor Walker. Over 900,000 Wisconsin residents signed the petition to initiate the 2012 special election to recall Walker, an amount equivalent to 46% of the total votes cast in the 2010 gubernatorial election and roughly one in every five voting-age residents. The Government Accountability Board (GAB), the state agency responsible for verifying signatures, made these records available online in PDF format. Conservative organizations then crowd-sourced an effort to digitize these documents, posting the resulting names on a searchable website (`iverifytherecall.com`). In March 2012, the GAB released its own searchable website of petition signers (Foy, 2024b). We designate campaign contributors as petition signers if and only if their names exactly match a name listed on either `iverifytherecall.com` or the GAB website. Using this method, we are able to link 4% of petition signers' names to a campaign contributor in the 2012 Wisconsin gubernatorial election and 29% of 2012 contributors' names to a petition signer. As expected, the vast majority of contributions from petition signers went 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 the WERC records. 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 dividing total revenues (primarily from membership dues) 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 were 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. Early-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 early-treated districts, with revenues per member equal to $483 in 2011 compared with $842 in late-treated districts.

4 Estimating The Impacts of Act 10: Empirical Strategy

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, which determined when each school district would become exposed to the consequences of the reform. By the time of the June 2012 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. 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 "treated" districts whose CBA had expired in 2011 and "control" districts with active CBAs (or extensions) in 2012. We implement this strategy by estimating the following equation 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 and was not extended; all other variables are defined as before. We consider all gubernatorial elections held between 2002 and 2014. 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):

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

Normalizing β_{2010} to zero, the parameters β_k estimate the difference in GOP gubernatorial votes between treated and control districts in year k, 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. While our identification requires merely that trends (not levels) be similar, Table 1 shows few meaningful observable differences in levels 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 teacher, student, and population 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, columns 1-2, for the pooled event study and Appendix Table A5, columns 1-2, for the difference-in-differences analysis).

5 Effects of Act 10 on Unions, Teachers, and Students

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

5.1 Teacher Unions: Revenues and Political Participation

Act 10 significantly curtailed union powers and made it harder for unions to operate. Specifically, the reform prohibited them from automatically collecting 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 Wisconsin.

To further document how Act 10 impacted unions, we use 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 using the natural logarithm of 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, panel (a), Figure 1, hollow circles, significant at 1%). However, not all unions were affected in the same way. Those with ex ante revenues per member above and below the state median saw a similar proportional decline (Figure 1, full markers), which implies that the loss in revenues was larger in absolute terms in ex ante richer (and stronger) unions (Appendix Figure A3).

Revenue losses curtailed unions' ability to participate in politics. We show this using information on contributions made by unions to gubernatorial races over time. Virtually all union contributions were to the Democratic party, both before and after Act 10. Estimates of β_k in equation (1), obtained using the number of campaign contributions made by a district's union in each year normalized by the number of people in the district, show that exposure to the reform dramatically lowered these contributions, by roughly 0.02 per 1,000 people three to four years after a CBA expiration (an 8.5-times decrease, Figure 1, panel (b), full markers).

5.2 Teacher Compensation

By prohibiting unions from engaging in collective bargaining with each school district over teachers' pay scales, Act 10 dramatically changed how teacher salaries are set and gave districts full discretion over pay. Under the pre-reform pay scale, teachers were exclusively rewarded based on 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 declined immediately and significantly, by approximately \$1000 in the first year after a CBA expiration (Appendix Figure A4, circle series). Teachers' fringe benefits declined even more, by around \$2000 (Appendix Figure A4, panel (b), square 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 2 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 with 3 or fewer years of experience or younger than 30. For example, after a CBA expiration 57-year-olds 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.[7]

These results indicate that Act 10 created clear winners and losers in terms of salaries, based

[7] Appendix Figure A5 shows the distribution of the post-CBA expiration change in salary across districts, separately for teachers aged 63 and older and for those aged 27 and below. In 155 districts (63%) the salary change for young teachers is larger than that for older teachers, and for 130 (53%) the change in salaries for older teachers is negative.

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 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 math scores of grade 3-8 students as the outcome variable, controlling for school and grade-by-year fixed effects and for 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 CBA expiration year, scores increased by 0.05 standard deviations (sd) two years after an expiration, and by 0.17 sd after five years (Figure 2, 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 2, panel (b), dashed lines). These findings align with those of Biasi (2021), who reports similar but smaller results using school-level average test scores. They are also consistent with those of Foy (2024a), who finds that test scores increased after Act 10 when unions failed to recertify.[8]

In theory, changes in students' learning environments may have led 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).

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 heterogeneity in the political consequences of Act 10.

[8] Baron (2018) finds negative short-run effects of Act 10's implementation on aggregate high-school test scores. Differently from Baron's, our analysis focuses on elementary and middle school students and examines a longer five-year time horizon after a CBA expiration.

6 The Political Effects of Wisconsin's Act 10

We now examine the effects of Act 10 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 3 (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 3 pp by three to four 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 45% of the winning margin in the 2014 election (equal to 6.7%, with a 53.3% vote share for Walker and a 46.6% share for his Democratic opponent Mary Burke). Reassuringly, estimates of β_k are close to zero and insignificant for $k < 0$, indicating the absence of differential pre-trends across wards 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 $k > 0$; we estimate the average effect to be 1.4 pp, or 21% of the 2014 margin of victory. 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 ours 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. We thus 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 3 as the triangles and squares, 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.[9]

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.[10] 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 election, 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).[11]

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 4 (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). While wards are comparable in terms of population size, districts are not; in these specifications we thus weight observations by district population (unweighted estimates are very similar).

We find that exposure to the consequences of the reform significantly reduced support to the Democratic party and mildly increased support for the GOP. Estimates of β_k indicate that the Democratic party received 33.1 fewer contributions per 1,000 people 3 to 4 years after exposure (a ten-fold

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

[10] 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.

[11] Appendix Figure A8 shows estimates of the parameters β_s in equation (3). 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 (square series). The share of votes to Walker increased further in treated districts in 2012 relative to the control group and remained at a significantly higher level in the 2014 election.

decline given an average contribution rate of 3.2 contributions pre-reform), while the GOP party received 4.6 additional contributions (a 33% increase). These results confirm an increase in relative support to the GOP party after Act 10; they also reveal 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 4, 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 response to the passage of the law in all districts. Our empirical strategy, which leverages the staggered CBA expirations, is designed to capture the effect of *direct exposure* to the real consequences of Act 10; to the extent that information spillovers onto non-exposed districts exist, they would bias our estimates towards zero.

Additionally, we argue that directly experiencing Act 10 affected voters' behavior by shifting their beliefs relative to the baseline of knowing about and expecting the policy. Three pieces of evidence support this argument.

First, the media coverage of Act 10 was overwhelmingly negative, particularly in the aftermath of the law's passage. A sentiment analysis of national and local newspaper articles containing the words "Act 10" and "school" 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 5 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 voters 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 signers of the 2012 gubernatorial recall petition increased their relative support for 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 fell after exposure. Estimates of β_k in equation (1), obtained using partisan gubernatorial campaign contributions of petition signers and non-signers, show that the negative effect of Act 10 on Democratic contributions was even larger for signers (a 6-fold decline, compared with a 2-fold decrease for non-signers, Figure 6). This result further confirms that our main results are due to a shift in political attitudes towards the reform driven by the direct exposure to its real consequences.

Lastly, in Section 9 we assess the plausibility of the magnitudes of our effects. By comparing the number of extra votes implied by our estimates with counts of voters in each of the groups affected by the reform, we argue that a reasonable number of voters would have had to be persuaded by the policy to explain the effects we measure.

6.4 Voter Turnout

A potential driver of 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. To test this possibility, 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.

Our analysis suggests that changes in turnout are unlikely to drive our results. 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 examine changes in the composition of voters, these estimates do not support the notion that shifts in voter mobilization are a major factor behind the electoral effects of Act 10. Instead, our findings are consistent with previous research showing that campaign activity can change vote shares even when overall turnout remains unchanged (Spenkuch and Toniatti, 2018).

6.5 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 of exposure to Act 10 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 return to this finding in our conclusion.

6.6 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 our choice of considering extensions, we re-estimate our main models defining districts as exposed only after the expiration of the CBA, ignoring extensions. Our estimates remain unchanged in magnitude, but become less precise (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 columns 3-4 of Table 5 for the pooled event study and columns 3-4 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 columns 5-6 of Table 5 and columns 5-6 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 expected gains from policies implemented by the government. We thus examine how variations in the real-world impacts of Act 10 across districts relate to the law's differential political effects at the district level (whose distribution is shown in panel (b) of Appendix Figure A7). In particular, we focus on the reform'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 the most impacted by Act 10. Teachers are also a non-trivial voting bloc, accounting for roughly 2% of the electorate 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) on average experienced a salary increase, whereas older and more experienced teachers (rewarded by such a schedule) experienced a decline. These salary changes were evident already in the first year post-implementation (Appendix Figure A4).

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 (Figure 7, panel (a), square series), while districts with a share of winners below the median experienced no significant change (hollow circle series). This result remains robust when we define winners as teachers aged 27 or younger (Appendix Figure A11).

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 significantly 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 (Appendix Figure A14); 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 four years after a CBA expiration (Figure 7, 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, columns 5-8). 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, columns 7 and 8).

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. While test scores increased gradually over time, it is possible that parents may have been able to anticipate the positive impacts of the reform on students by observing the changes in teacher compensation documented above and the subsequent improvements in teacher selection and retention, which occurred immediately after the implementation of the law (Biasi, 2021).

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 children younger than 18. 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 households with school-age children above the state median, although there is evidence of differential pre-trends (Figure 8, panel (a), square series). By contrast, districts with a share below the median experienced a smaller increase, equal to 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 2, panel (b)). 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 8, panel (b), square series), whereas districts below the median experienced no significant change (circle series). In columns 5-8 of Table 7 we confirm that this result holds by allowing the impact of Act 10 to vary by quartile of the FRPL student share.

Lastly, 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 a reform of the public school 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 9 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.

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. 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 made contributions pre-reform (all of which went to the Democratic party). If the decline in unions' 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 test this hypothesis, we re-estimate equation (1) separately for districts with unions that contributed to gubernatorial races prior to 2011 and for districts whose 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 10, panel (a), square series). It instead did not change in districts without prior union contributions (circle series).

We also investigate whether these effects operate through unions' ability to mobilize voters, i.e., by influencing turnout. Appendix Figure A13 (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 (Appendix Table A3), 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, thus making it more difficult for them to generate revenue. As a result, union revenues per teacher fell following a CBA expiration (Figure 2, panel (a)). Moreover, this decline was larger in dollar terms in districts that initially had higher revenues per member (Appendix Figure A3).

Higher pre-reform revenues reflect, at least in part, unions that were better positioned to extract dues and thus likely more powerful. 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 pp four years after a CBA expiration (Figure 10, panel (b), square series), whereas it did not change in districts with weaker unions (circle series).

These findings suggest that the GOP's electoral gains can be traced back 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

This paper has studied the effects of voters' exposure to Act 10, a controversial reform of collective bargaining that primarily affected public school teachers, on the electoral fortunes of the reform's main proponent. We find sizable positive effects, indicating that the law was a political win. Heterogeneity analyses show that these electoral gains were most pronounced in areas where students and teachers stood to benefit the most from the law, suggesting that the reform garnered support from both users and providers of public education. We also show that the law's electoral effects were linked to its weakening of public sector unions, important political actors who influence both policy and the way people vote.

Plausibility of effect sizes While substantial, the magnitudes of our effects are plausible given the numbers of teachers and parents in Wisconsin districts. We calculate that exposure to Act 10 generated an additional 12,951 votes for Walker in 2012 in exposed districts relative to not-yet-exposed ones. Exposed districts employed 27,789 teachers in 2012; of these, 4,153 had 3 or fewer years of experience and thus gained the most from Act 10. If we assume that these teachers had a turnout rate equal to that of the electorate in exposed districts in the 2012 election (63%), the treatment effects we measure could be completely explained by each inexperienced teacher voting for Walker and persuading four others to do so. Exposed districts also contained 457,012 parents of school-age children in 2012. Assuming the same turnout rate of 63%, the treatment effects we measure could be completely explained by 3% of these parents voting for Walker.

Assessing the role of different potential explanations While various explanations seem to play a role in our context, understanding their relative importance is crucial for predicting whether an Act 10-like policy in a different state, with different teacher and student demographics and a different union landscape, could deliver similar electoral gains.

To do this, we first verify that the different dimensions of heterogeneity we consider represent independent channels, rather than capturing the same (or similar) underlying variation. Appendix Figure A14 shows pairwise relationships among our dimensions of heterogeneity, including the share of teachers with fewer than 3 and more than 21 years of experience; the fraction of households with children under 18; the share of FRPL students in the district; teacher turnover in the district; and whether the district's union made a political donation in the three gubernatorial elections prior to Act 10. The correlations are generally low (the strongest is a correlation of 0.5 between the share of FRPL students and the presence of a politically active union), suggesting that these variables capture different dimensions of heterogeneity.

Second, we quantify the extent to which the electoral gains from Act 10 correlate with these dimensions of heterogeneity when considered together. To do so, we include all these variables in our baseline pooled event-study model, interacting them with our treatment variable. To avoid overfitting, we apply LASSO techniques to select the most relevant predictors. We explain this procedure in detail in Appendix B.

Appendix Figure A15 summarizes the results of this exercise, showing how the electoral effects of Act 10 varied across groups of districts with common characteristics. The findings broadly confirm those from our analyses of individual dimensions of heterogeneity (although in both cases, these dimensions of heterogeneity are not randomly allocated, so the results should be interpreted with caution). The highest positive effects on the GOP vote share occur in districts with a high share of inexperienced teachers, a high share of FRPL students, and politically active unions prior to the reform. Districts with fewer of these characteristics saw systematically smaller effects—although no group showed significant negative effects. We take two broad lessons from these findings. First, although Act 10 generated a significant political backlash through protests and negative press, we find little evidence that actually experiencing Act 10 caused additional *electoral* backlash from any group. Second, our findings support a narrative in which Act 10's political success came both from its weakening of public sector unions and from its ability to build support among people who stood to benefit as public service users or providers.

Long-lasting political effects of education reform? Our analysis has primarily focused on the political consequences of Act 10 for the reforms' proponents, and in general we have not found spillovers of the law onto other federal offices. Yet, we close our analysis by pointing out a positive correlation between the magnitude of the 2010-2012 increase in the GOP vote share in gubernatorial elections and the share of votes for GOP presidential candidate Donald Trump in the 2016 presidential elections, in which Wisconsin was one of the decisive states (Appendix Figure A16). Although this relationship should not be interpreted as causal, it highlights how governing coalitions which fail to improve service provision create opportunities for anti-establishment challengers.

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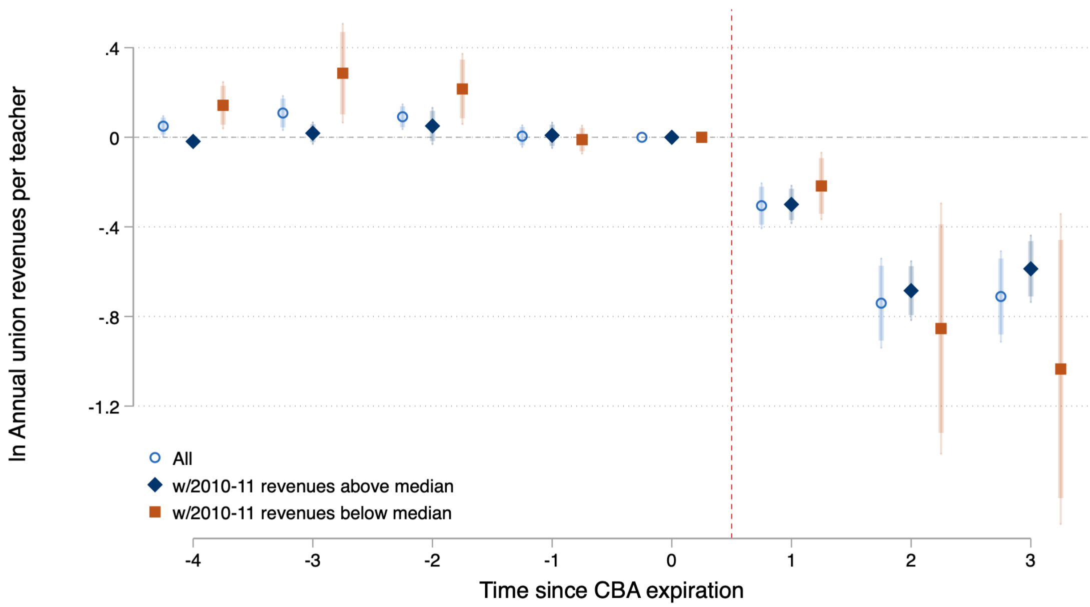
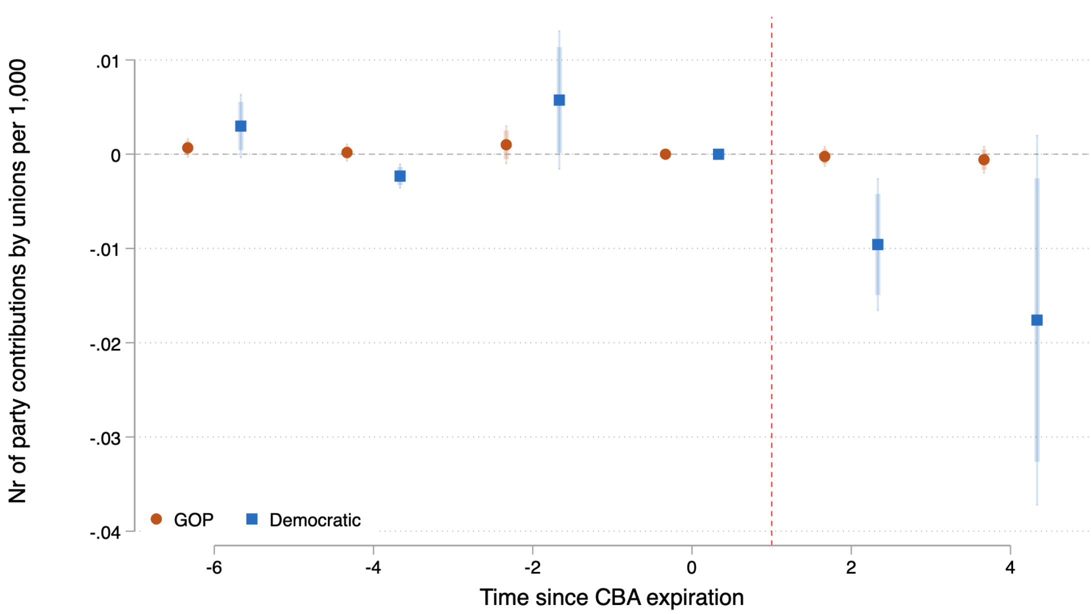
Figures

Figure 1: Effects of Act 10 on Union Revenues and Political Participation

(a) Union revenues



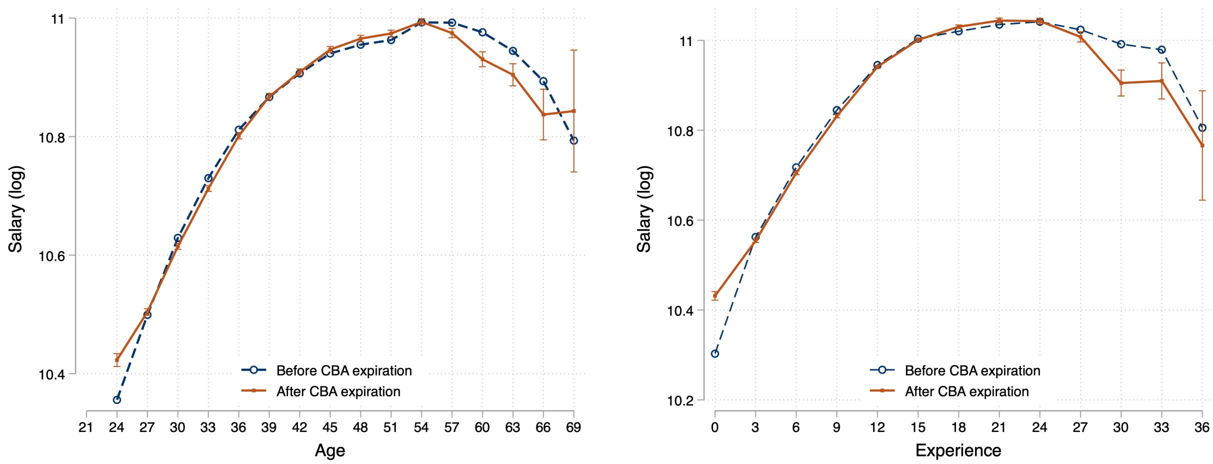
(b) Political participation: Campaign contributions



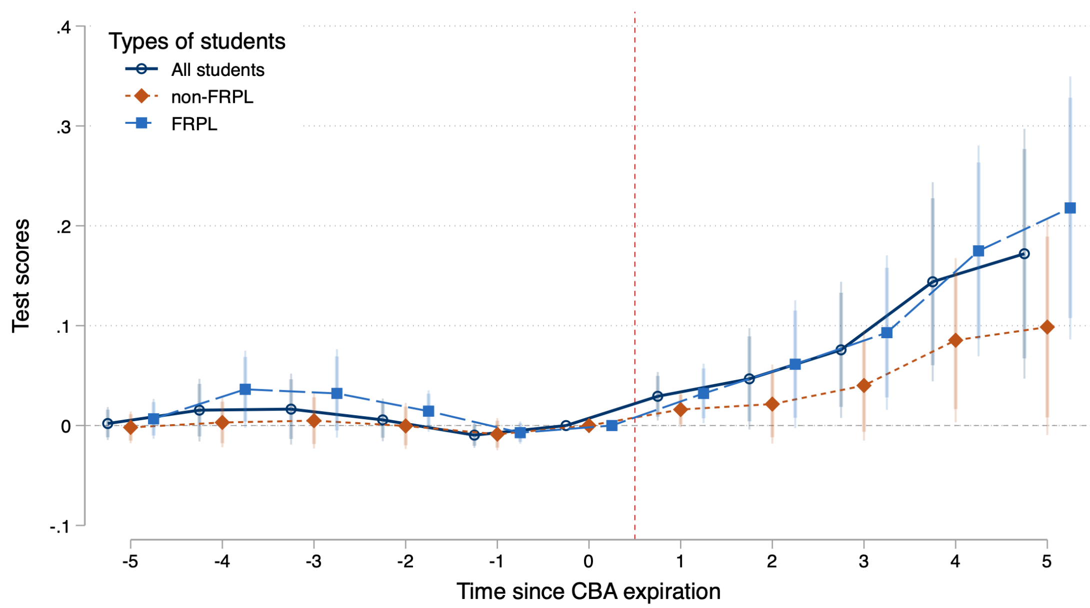
Notes: Panel (a) shows estimates and 90-95% confidence intervals of the coefficients β_k in equation (1), using the natural logarithm of district-year level union revenues per teacher as the dependent variable. Estimates are shown for all unions (solid line) and separately for districts with revenues per member above and below the state median in 2010-11 (dashed lines). Panel (b) shows estimates and 90-95% confidence intervals of the coefficients β_k in equation (1), using the number of gubernatorial race contributions made by unions per 1,000 people to the Democratic party (square series) and the GOP (circle series). In both panels, observations are weighted by the number of people in each district and standard errors are clustered at the district level.

Figure 2: Effects of Act 10 on Teachers and Students

(a) Teacher pay by age and experience

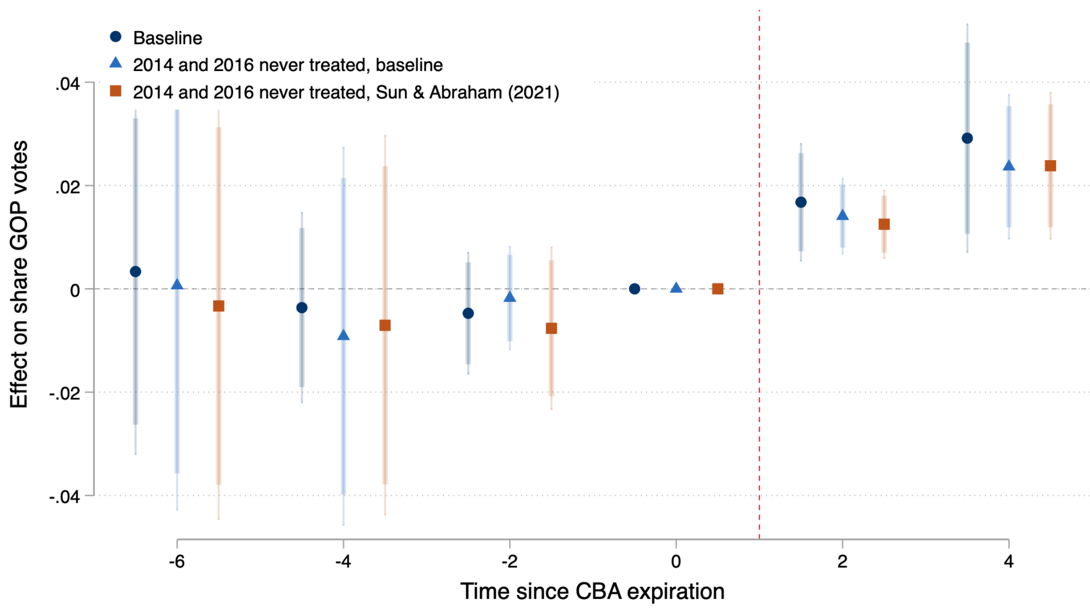


(b) Student test scores



Notes: Panel (a) shows estimates and 90-95% 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 (*Before CBA expiration*) and the two years following (*After CBA expiration*) the expiration of each district's CBA or extension. Panel (b) shows estimates and 90-95% confidence intervals of the coefficients β_k in equation (1), using individual-level math test scores of grade 3-8 students and controlling for student demographics, school, and grade-by-year fixed effects. The solid line shows estimates for the full student sample. Squared markers denote estimates for FRPL-eligible students, and diamond markers denote estimates for all other students. In all figures, standard errors are clustered at the district level.

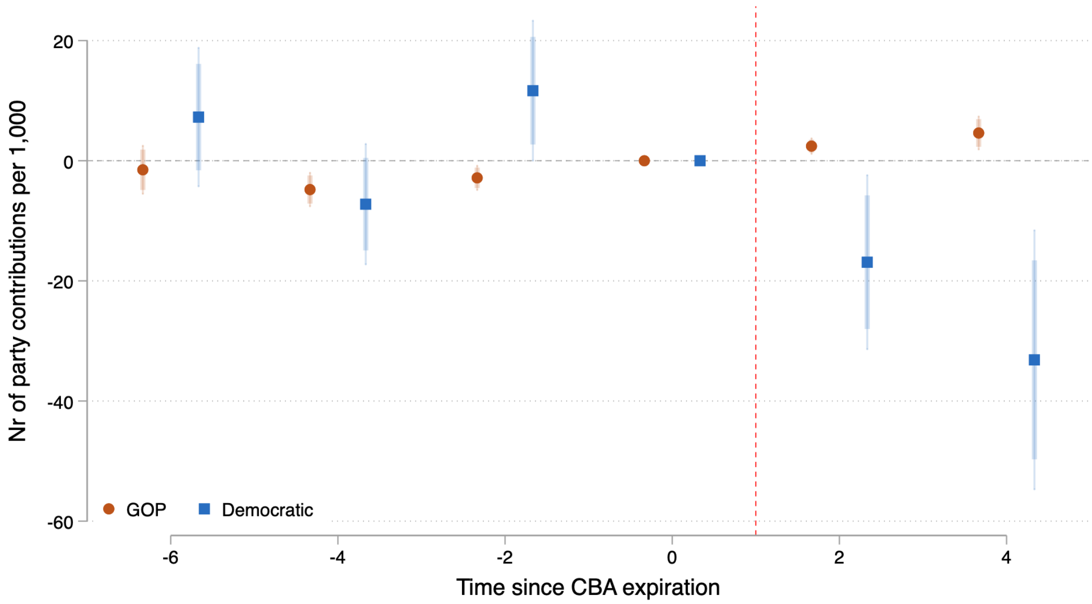
Figure 3: Political Effects of Wisconsin's Act 10: Event Study Estimates



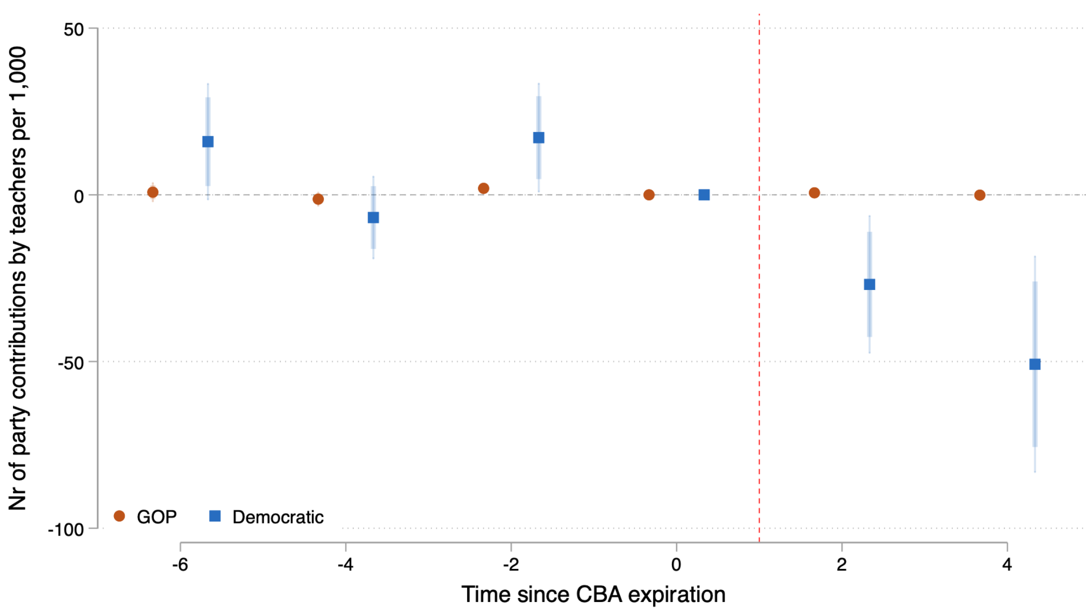
Notes: The circles show estimates and 90-95% confidence intervals of the coefficients β_k in equation (1), estimated using the GOP vote share in gubernatorial elections in each ward and year as the dependent variable and controlling for ward and year fixed effects. The triangles show the same estimates obtained 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. Standard errors are clustered at the district level.

Figure 4: Campaign Contributions to Political Parties, Total and for Teachers: Event-Study Estimates

(a) Total number of contributions

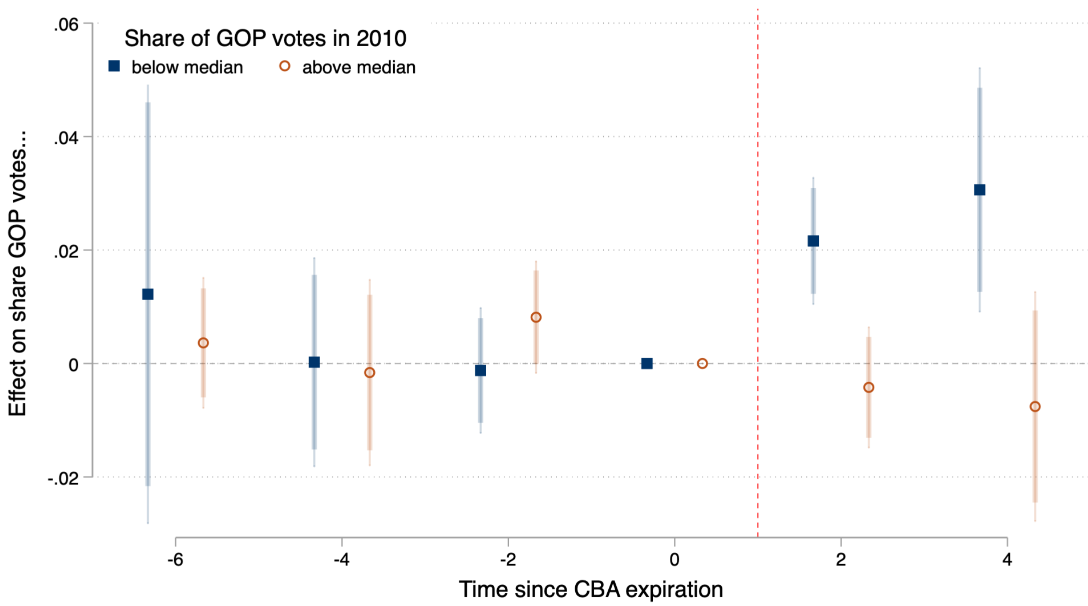


(b) Contributions by teachers



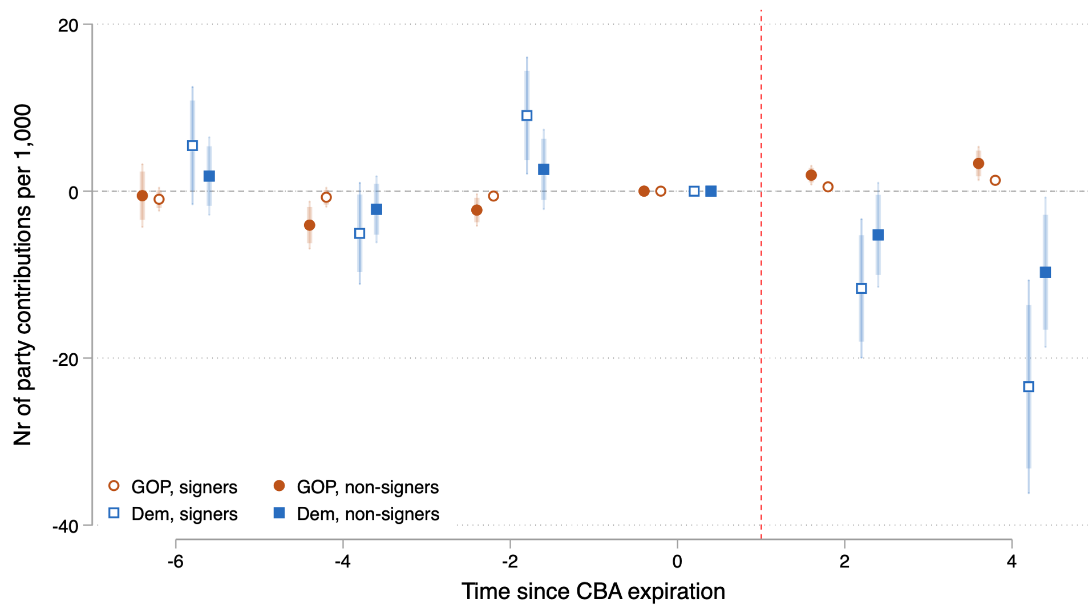
Notes: Panel (a) shows estimates and 90-95% confidence intervals of the coefficients β_k in equation (1), obtained using the number of gubernatorial race contributions per 1,000 people to the Democratic Party (square series) and the GOP (circle series). Panel (b) shows the same estimates using the number of gubernatorial race contributions made by teachers per 1,000 teachers. Observations are weighted by district population. Standard errors are clustered at the district level.

Figure 5: Event-Study Estimates by 2010 GOP vote share



Notes: Estimates and 90-95% 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. Standard errors are clustered at the district level.

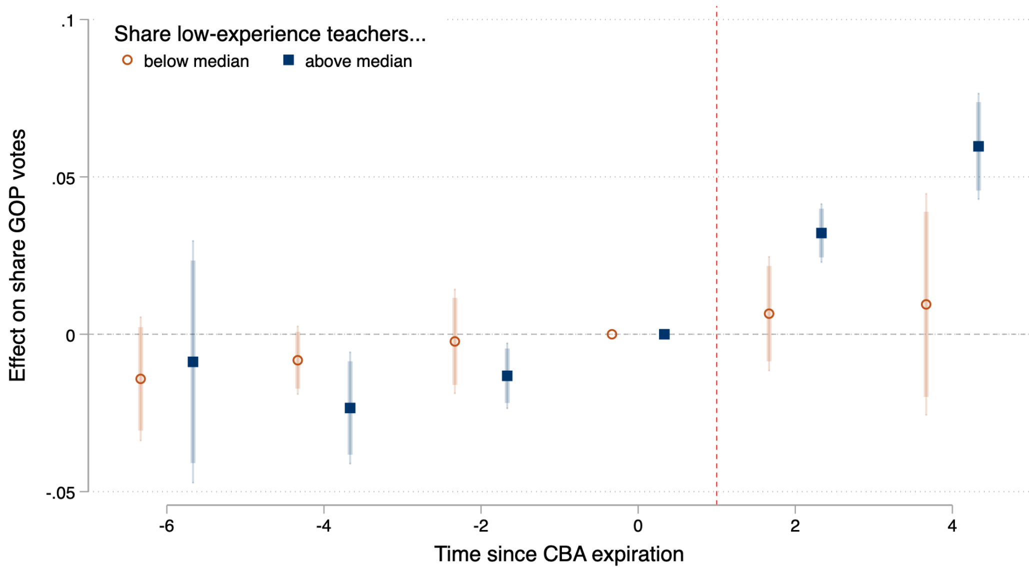
Figure 6: Campaign Contributions to Political Parties by Petition Signers: Event-Study Estimates



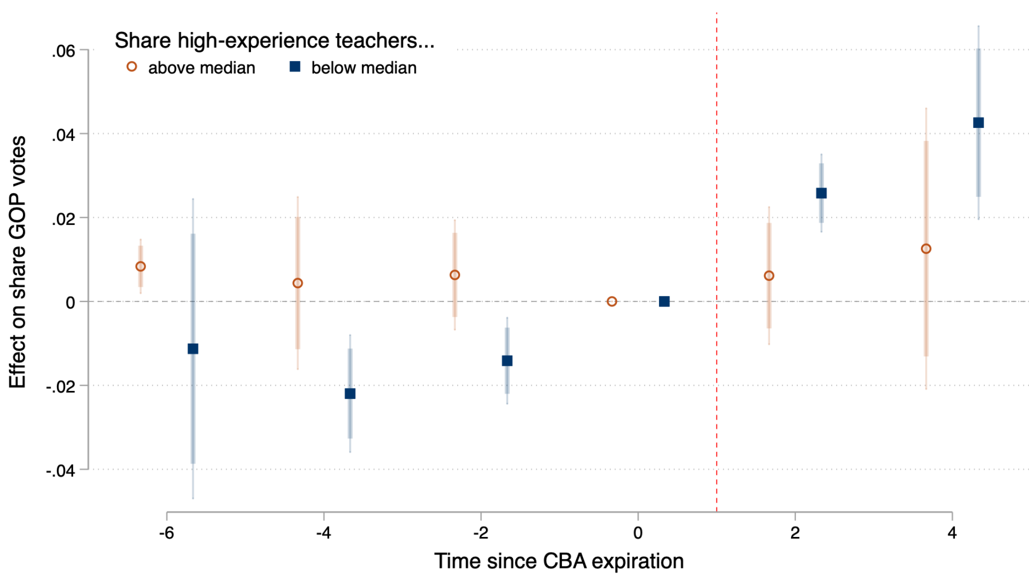
Notes: Estimates and 90-95% confidence intervals of the coefficients β_k in equation (1), obtained using the number of gubernatorial race contributions to the Democratic party (squares) and the GOP (circles), by people who signed the recall election petition (hollow marks) and those who did not sign (full marks), per 1,000 people in the district. Observations are weighted by district population. Confidence intervals are obtained using standard errors are clustered at the district level.

Figure 7: Winners and Losers: Event-Study Estimates by Share of Low- and High-Experience Teachers in District

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



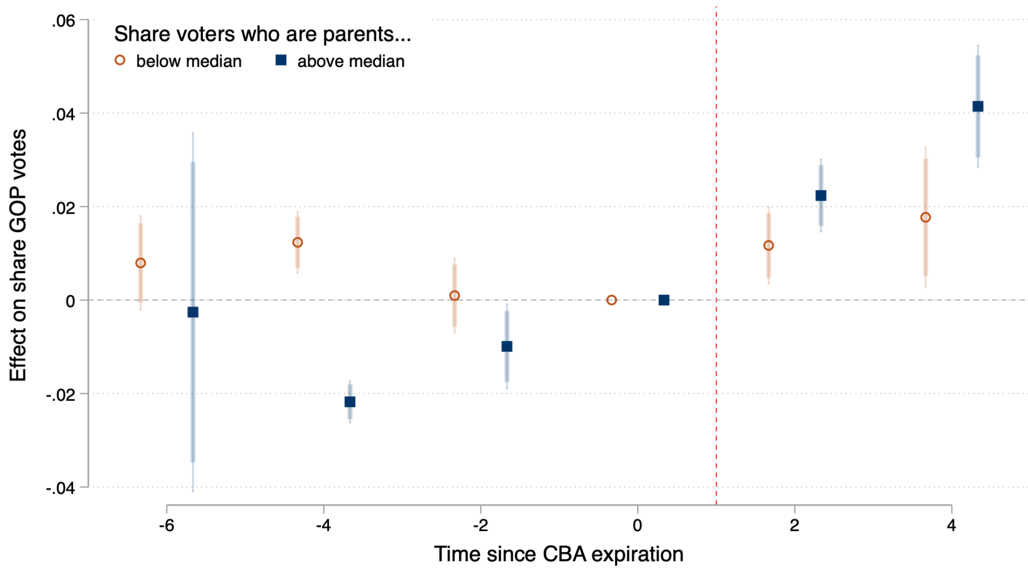
(b) By share of high-experience teachers (*losers*)



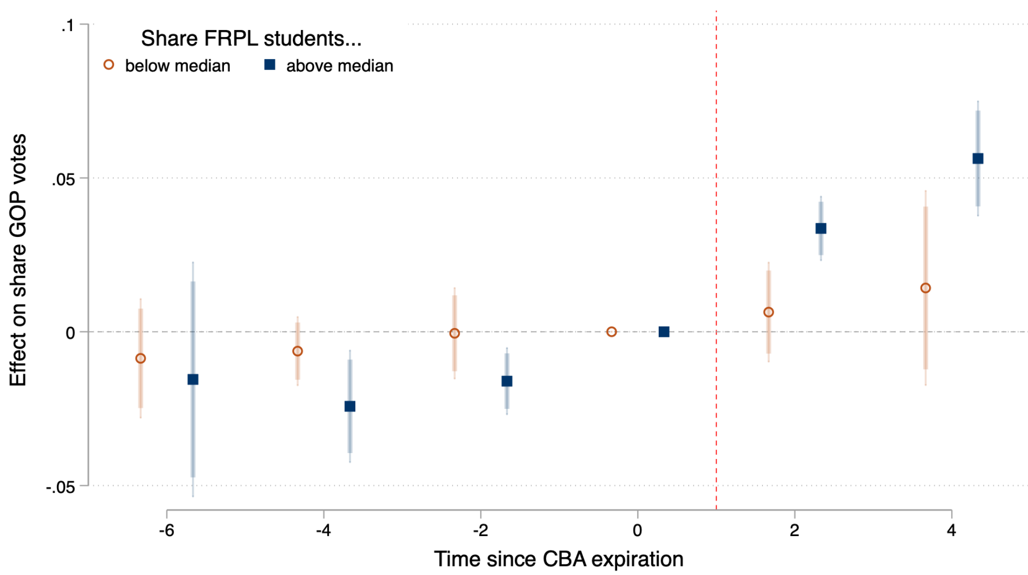
Notes: Estimates and 90-95% confidence intervals of the coefficients β_k in equation (1), estimated using the GOP vote share in gubernatorial elections in each ward and year as the dependent variable and controlling for ward and year fixed effects. In panel (a), the squares show estimates for the subsample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

Figure 8: Winners and Losers: 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)

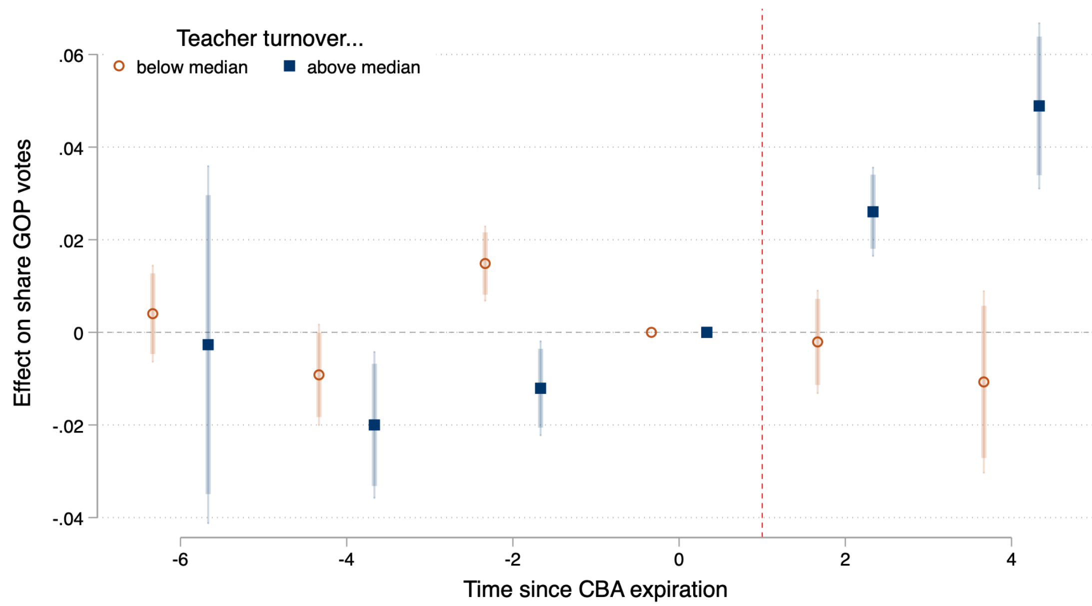


(b) By share of FRPL students



Notes: Estimates and 90-95% 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 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. Standard errors are clustered at the district level.

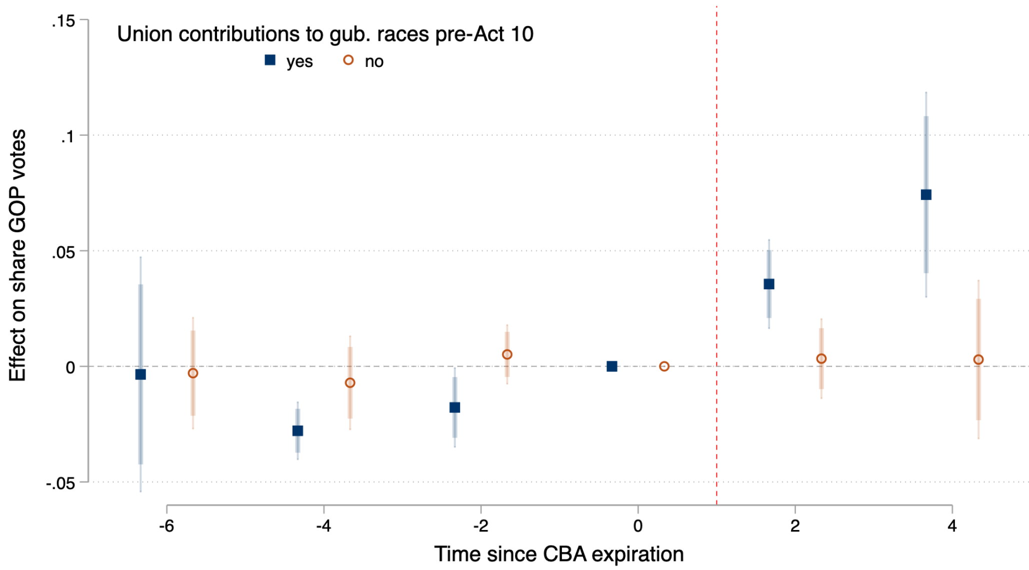
Figure 9: Winners and Losers of Wisconsin's Act 10. Event-Study Estimates, by Teacher Turnover in The District



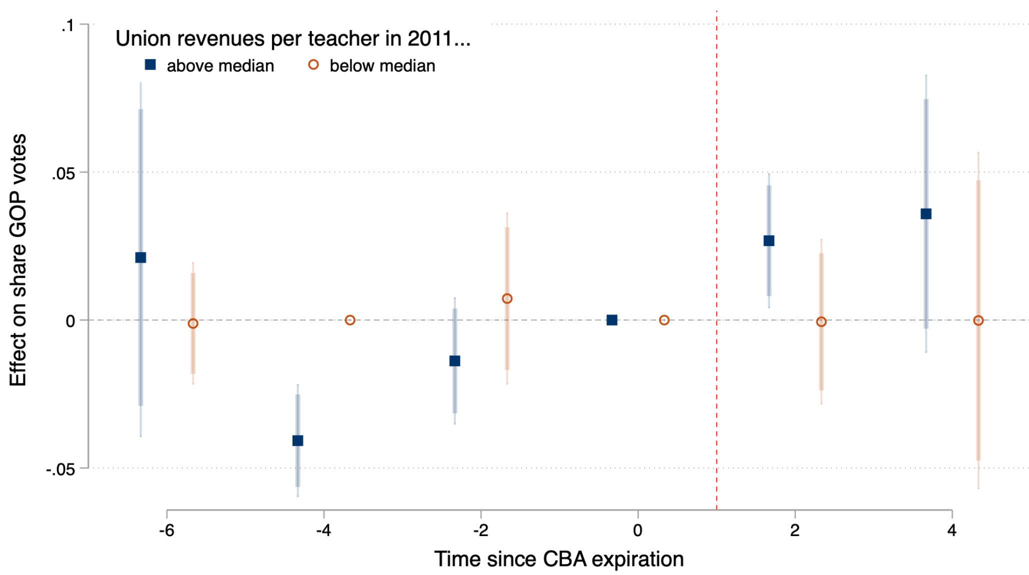
Notes: Estimates and 90-95% confidence intervals of the coefficients β_k in equation (1), estimated using the GOP vote share in gubernatorial elections in each ward and year as the dependent variable and controlling for ward and year fixed effects. The squares show estimates for the subsample of districts above the state median 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. Standard errors are 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 90-95% 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 and the circles show estimates for wards in districts with no contributions. In panel (b), the squares show estimates for wards in districts with union revenues per teacher above the state median in 2011 and the circles show estimates for wards with union revenues below the median. Standard errors are clustered at the district level.

Tables

Table 1: Wisconsin Wards and School Districts: Summary Statistics

	All districts	Expiration in 2011	Expiration in After 2011
Teachers			
Share teachers w/experience < 3y (2010-11)	0.15	0.15	0.15
	(0.051)	(0.056)	(0.047)
Share teachers w/experience > 21y (2010-11)	0.18	0.18	0.17
	(0.067)	(0.067)	(0.067)
Teacher turnover rate (share who exits) (2010-11)	0.100	0.10	0.099
	(0.030)	(0.027)	(0.031)
Students			
Share low-SES (FRPL) students (2010-11)	0.38	0.32	0.41
	(0.19)	(0.16)	(0.19)
Std. test scores, Math (2010-11)	0.024	0.14	-0.034
	(0.33)	(0.29)	(0.34)
Share HHs with children < 18 yo (2010)	0.32	0.32	0.32
	(0.047)	(0.050)	(0.046)
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 (2010)	0.39	0.35	0.41
	(0.51)	(0.63)	(0.44)
100 * #Donations pp to GOP (2010)	1.43	1.87	1.19
	(1.28)	(1.79)	(0.79)
Unions			
Union made political donations (2002-201)	0.20	0.093	0.26
	(0.40)	(0.29)	(0.44)
Union revenues per teacher (2006-11)	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 $k \leq 0$ 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
Years	until 2012	until 2012	until 2014	until 2014	until 2014	until 2014

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 or extensions expiring in 2011. Columns 1-2 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 *Exposed* variable. Columns 3 and 4 are estimated on the sample of districts that excludes 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 1-3, 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 5-7, 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 Children Younger than 18 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 1-3, we split the sample by the quartile of the share of households with children (aged 18 and younger) in 2010. In columns 5-7, 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

	By quartile of teacher turnover			
	Q1	Q2-Q3	Q4	All
	(1)	(2)	(3)	(4)
Exposed	0.004	0.012**	0.034***	
	(0.006)	(0.006)	(0.011)	
Exposed * Q1				0.001
				(0.007)
Exposed * Q2				0.017
				(0.014)
Exposed * Q3				0.011
				(0.008)
Exposed * Q4				0.044***
				(0.015)
Ward FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No
Year * qtile FE	No	No	No	Yes
Mean dep. var. control	0.549	0.487	0.367	0.476
N	5381	10149	5703	21151
Clusters (districts)	66	105	65	235
R-squared	0.87	0.93	0.97	0.94

Notes: The dependent variable is the share of GOP votes in gubernatorial elections in each ward and year. We show estimates and standard deviations of the parameter β_k in equation (1), where we constrain $\beta_k = 0$ for $k < 0$ and β_k to be the same across all $k > 0$. In columns 1-3, 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.

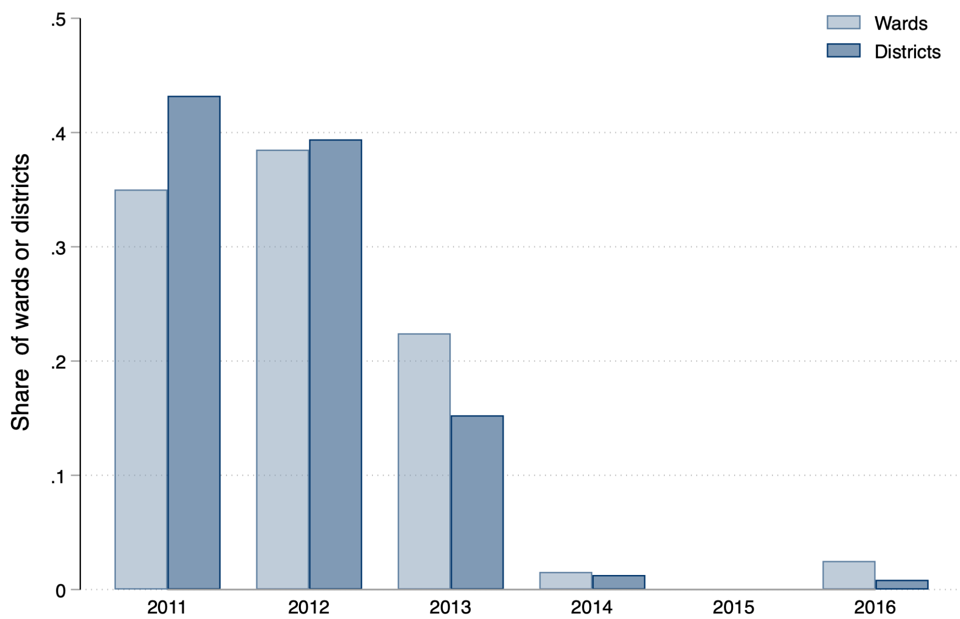
The Political Consequences of Controversial Education Reform: Lessons from Wisconsin's Act 10

Barbara Biasi and Wayne Aaron Sandholtz

Online Appendix

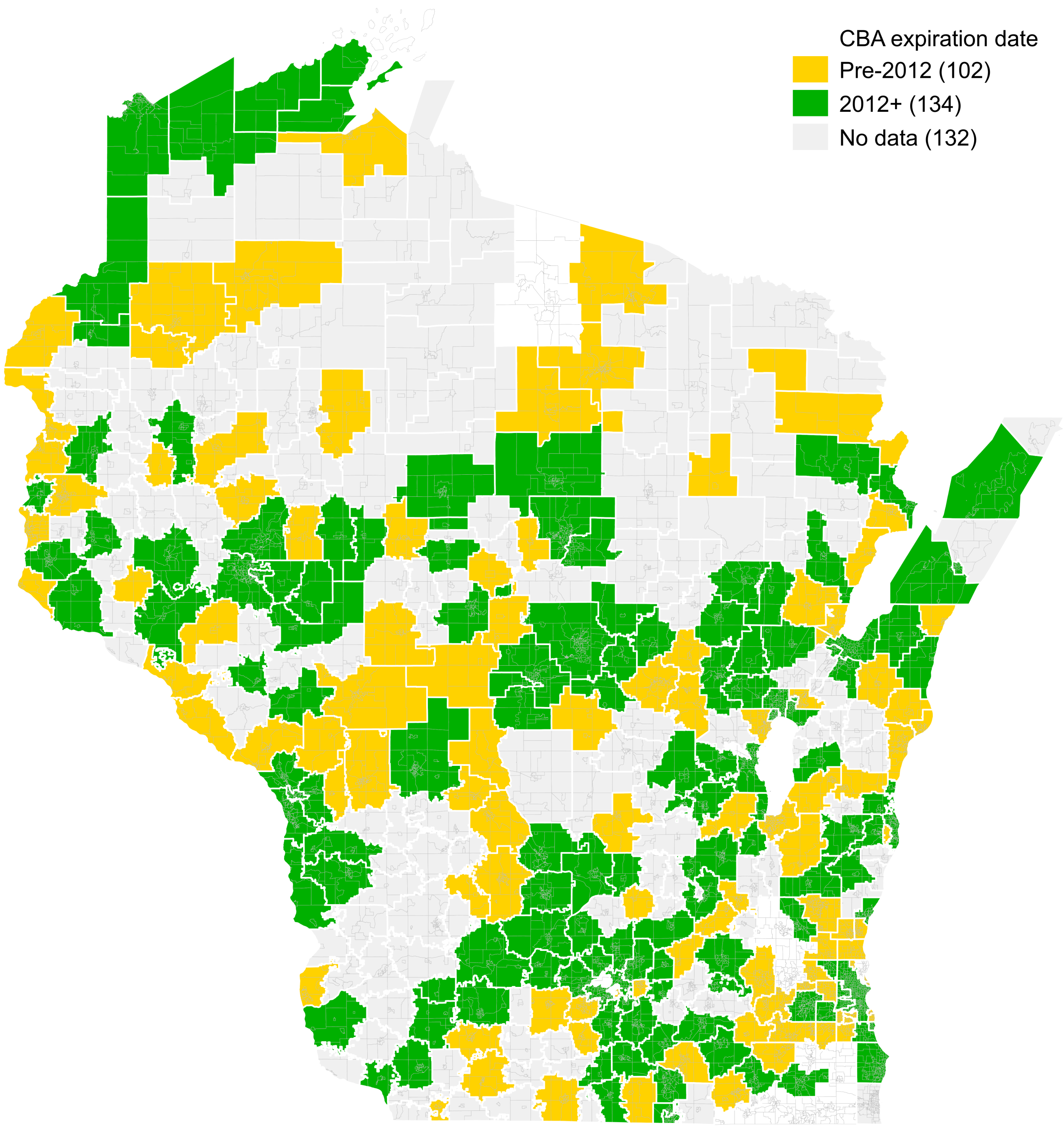
Appendix A Additional Tables and Figures

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



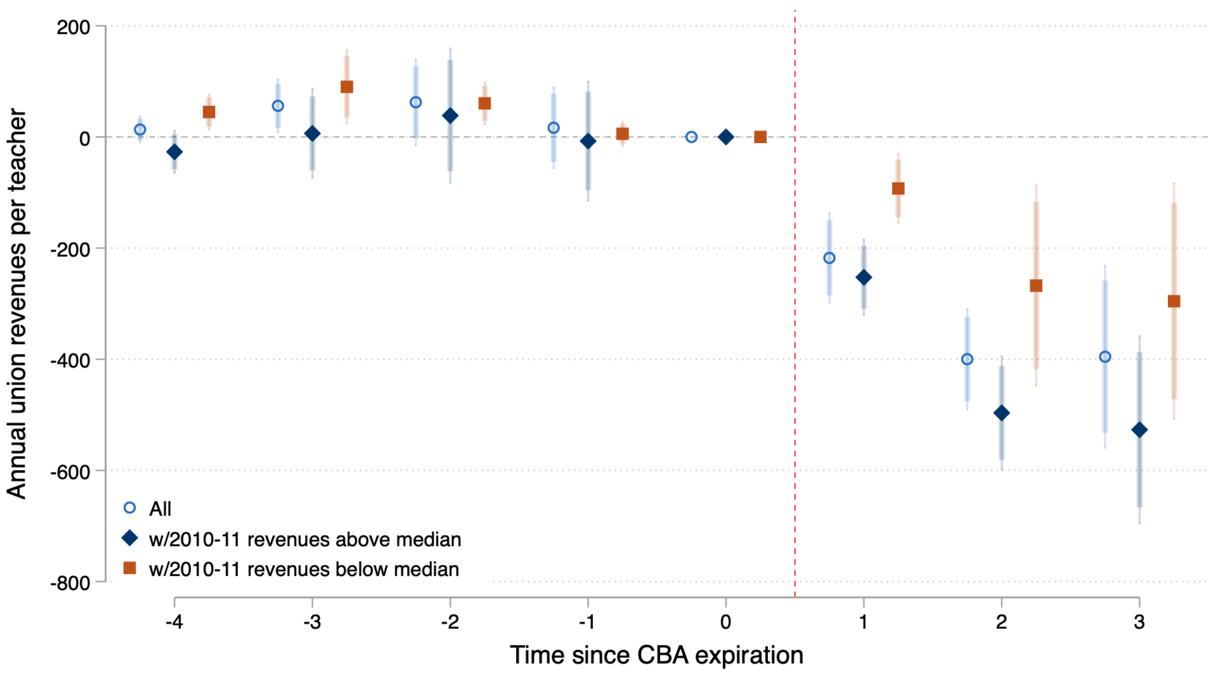
Notes: Share of wards and districts by date of expiration of the district's CBA or of its extension.

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



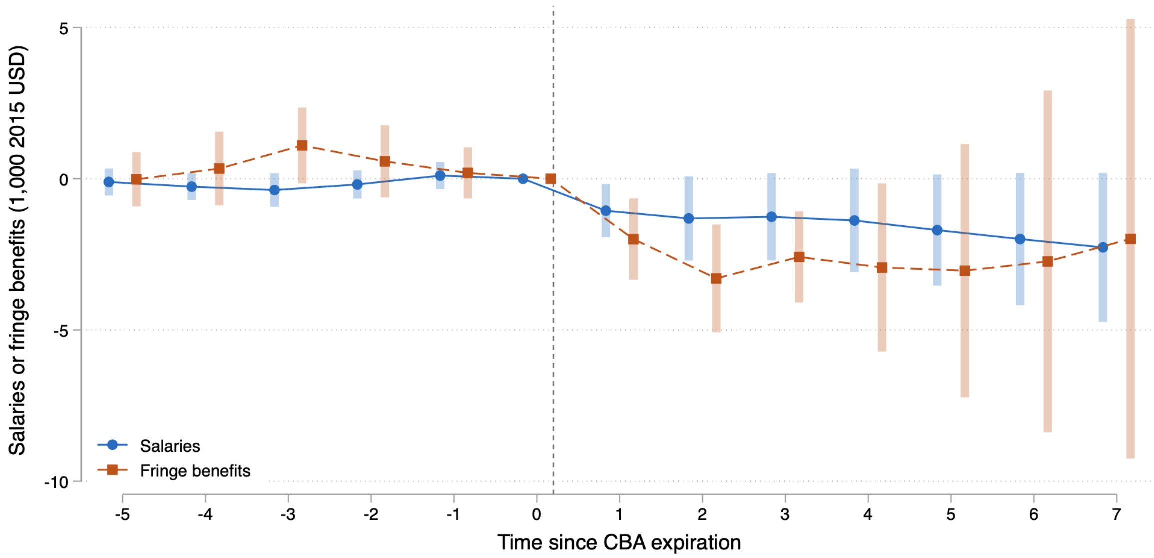
Notes: The map shows school districts by when their (extended) CBAs expired (pre-2012 vs 2012 or later). Non-unified school districts (elementary and secondary) not shown. School districts are delineated by thick white lines; wards are delineated by thin gray lines.

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



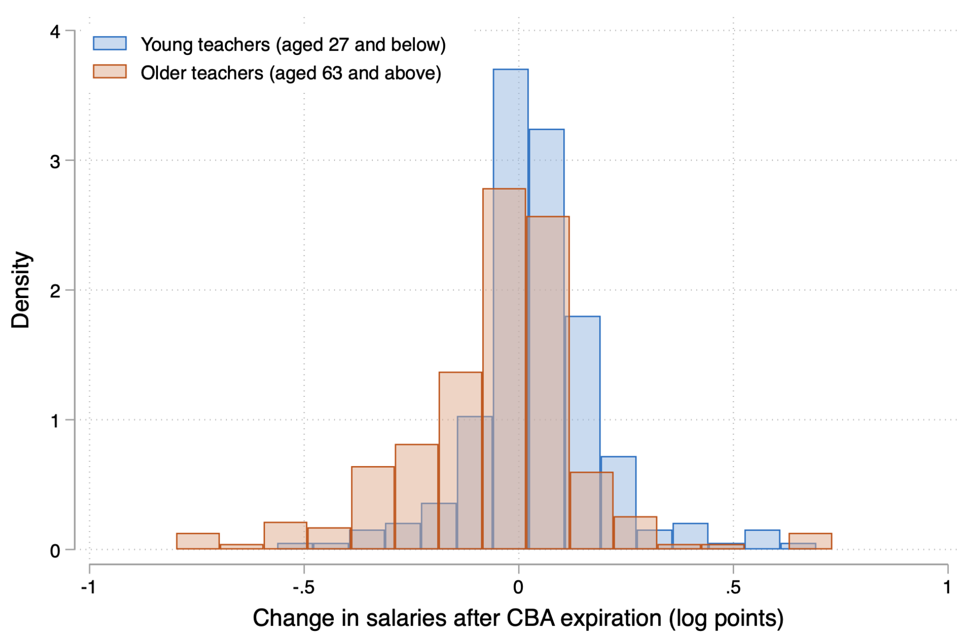
Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_k in equation (1), using district-year level union revenues per member as the dependent variable, for all unions (hollow circle markers) and separately for districts with revenues per member above (diamond markers) and below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

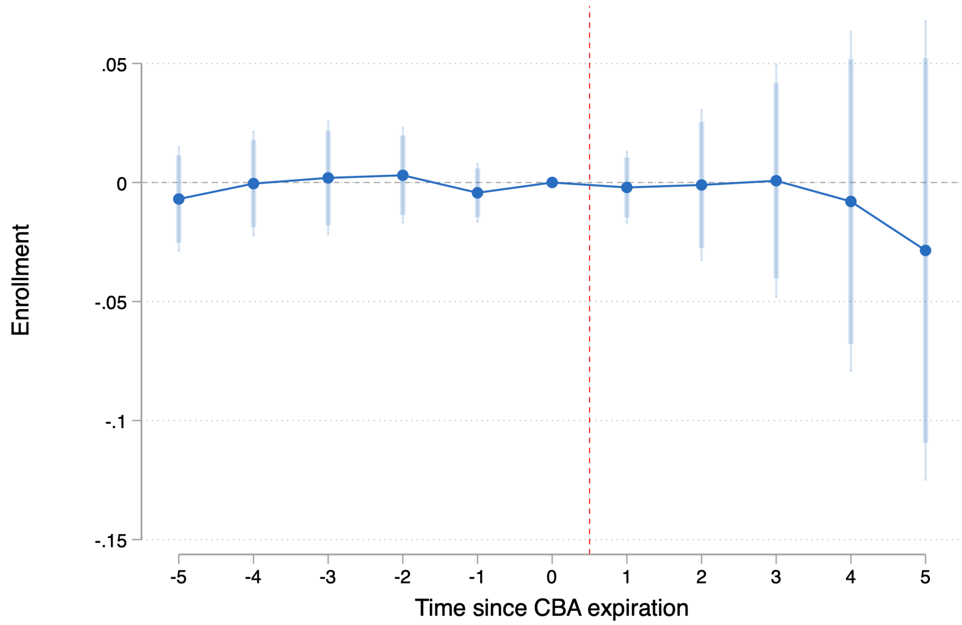
Figure A5: Distribution of Changes in Salaries, Before vs After a District's CBA Expiration



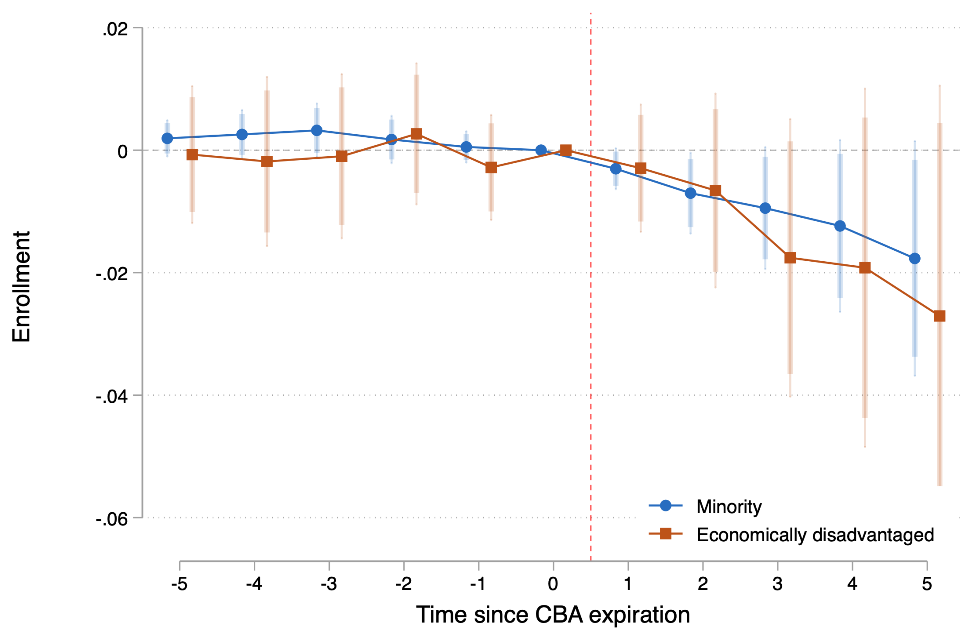
Notes: Distribution of district-level average changes in the logarithm of teacher salaries for teachers aged 63 and older (orange series) and those aged 27 and below (blue series), between the two years preceding and the two years following the expiration of each district's CBA or its extension.

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

(a) Enrollment



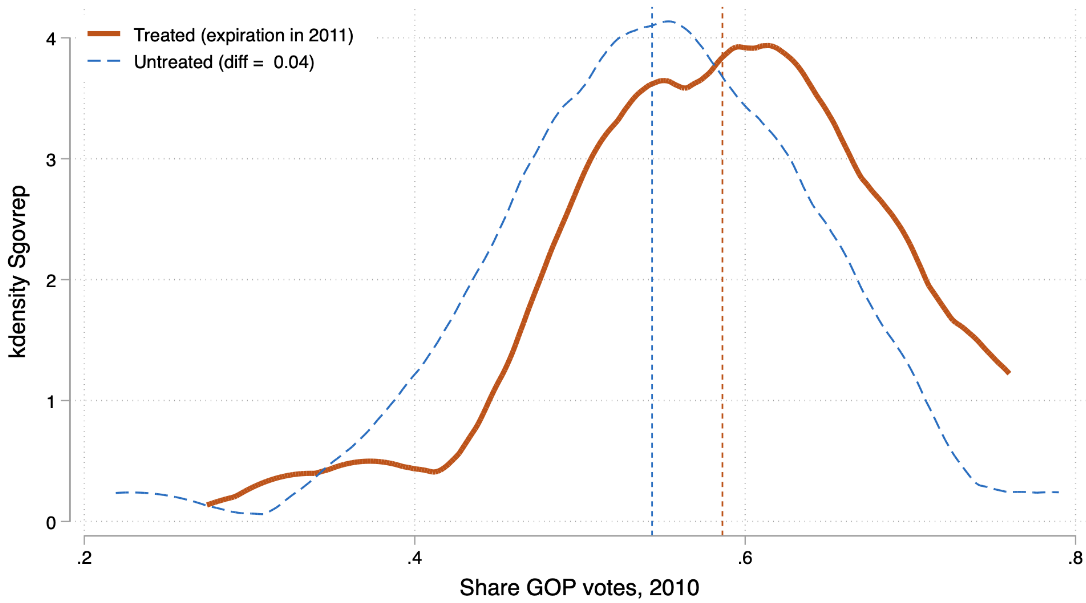
(b) Enrollment shares



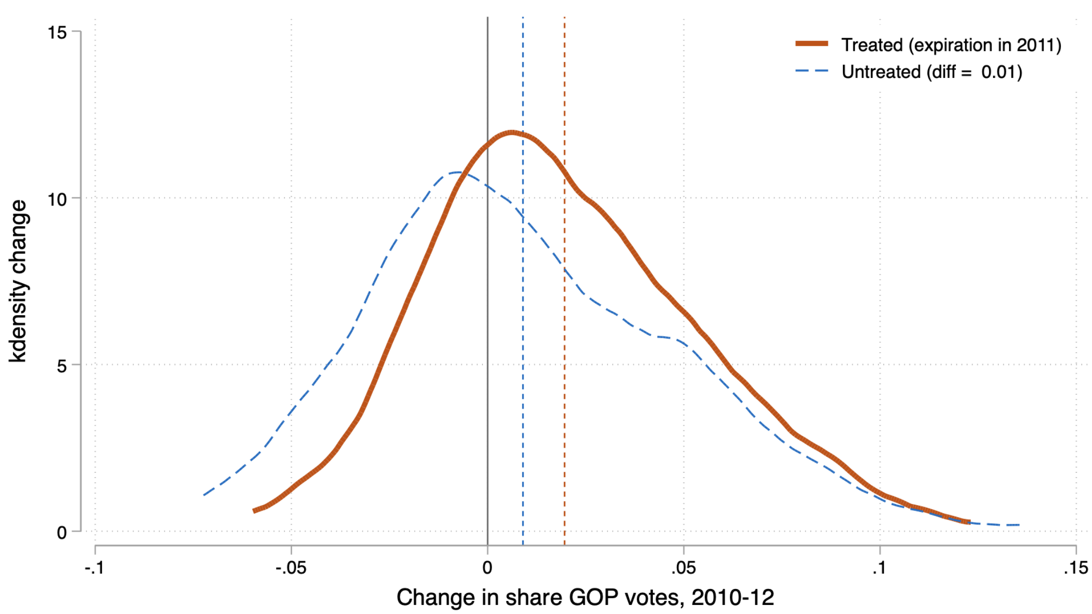
Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_k in equation (1), using as dependent variables the natural logarithm of district enrollment (panel (a)) and district enrollment shares of FRPL (*Economically disadvantaged*) and Black or Hispanic students (*minority*, panel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

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

(a) GOP vote shares in 2010 gubernatorial election

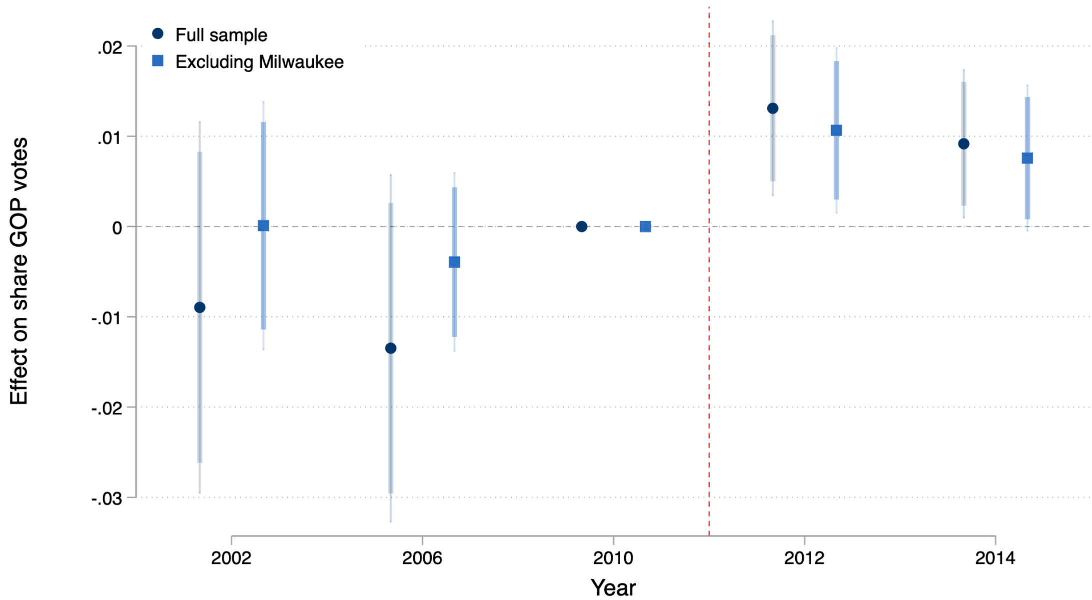


(b) Change in GOP vote shares, 2012 vs 2010



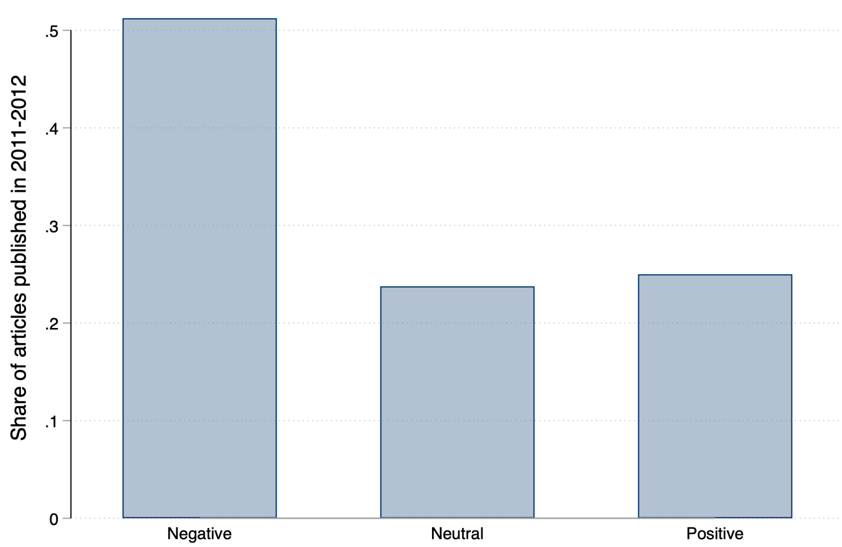
Notes: Panel (a) shows a distribution of the share of GOP votes in the 2010 gubernatorial election, separately for wards located in school districts with CBAs that expired in 2011 and were not extended (treated, thick solid line) and wards in districts with CBAs (or extensions) that expired after 2011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



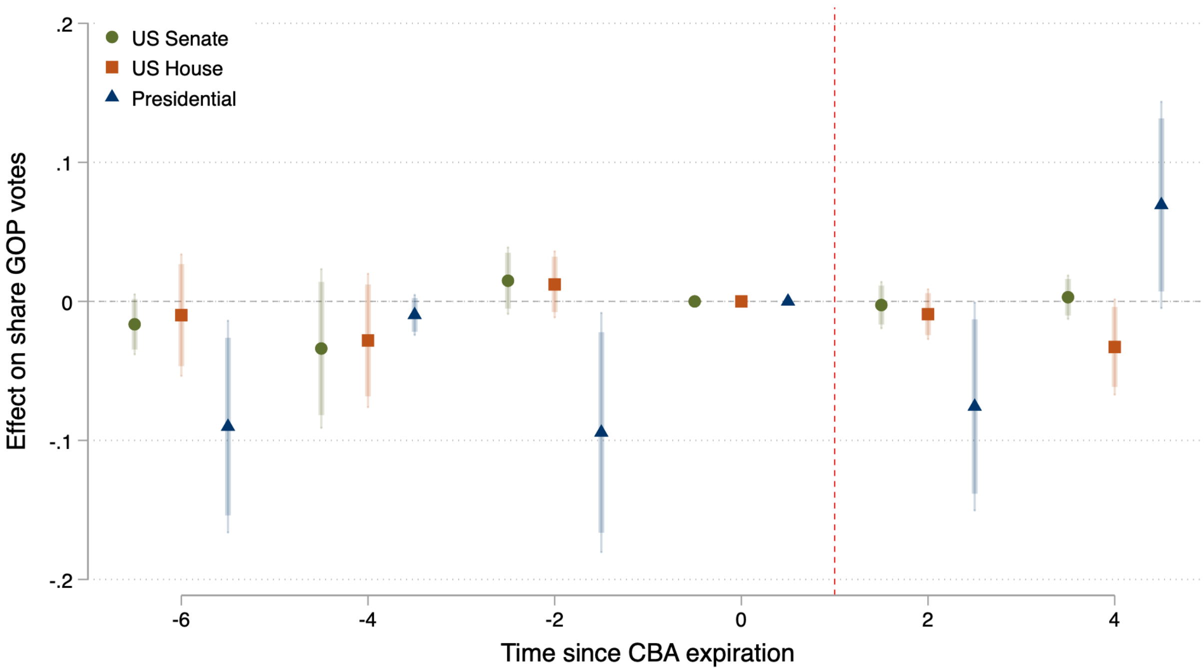
Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_k in equation (3), estimated on the full sample of districts (circles), and on the sample of districts that excludes Milwaukee Public Schools (squares). The outcome variable is the GOP vote share in gubernatorial races. Confidence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

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



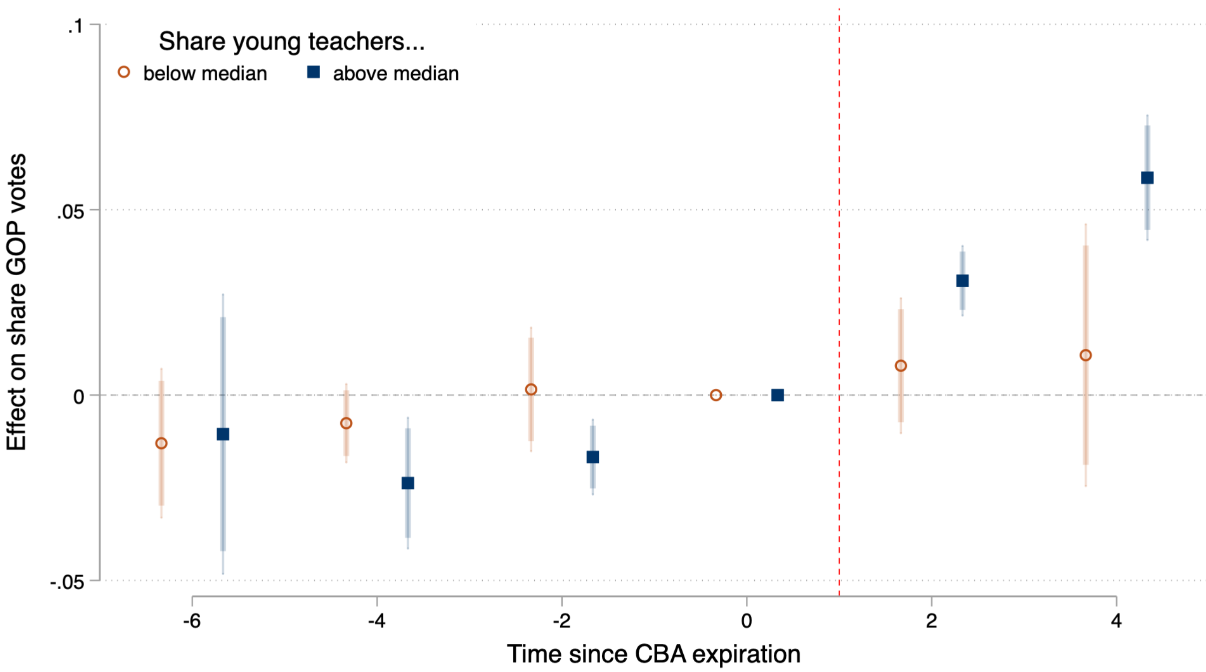
Notes: Share of articles in national and local newspapers published in 2011 and 2012 and containing the keywords "Act 10" and"school," by news sentiment. Sentiment analysis performed using the large-language model ChatGPT 4o.

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



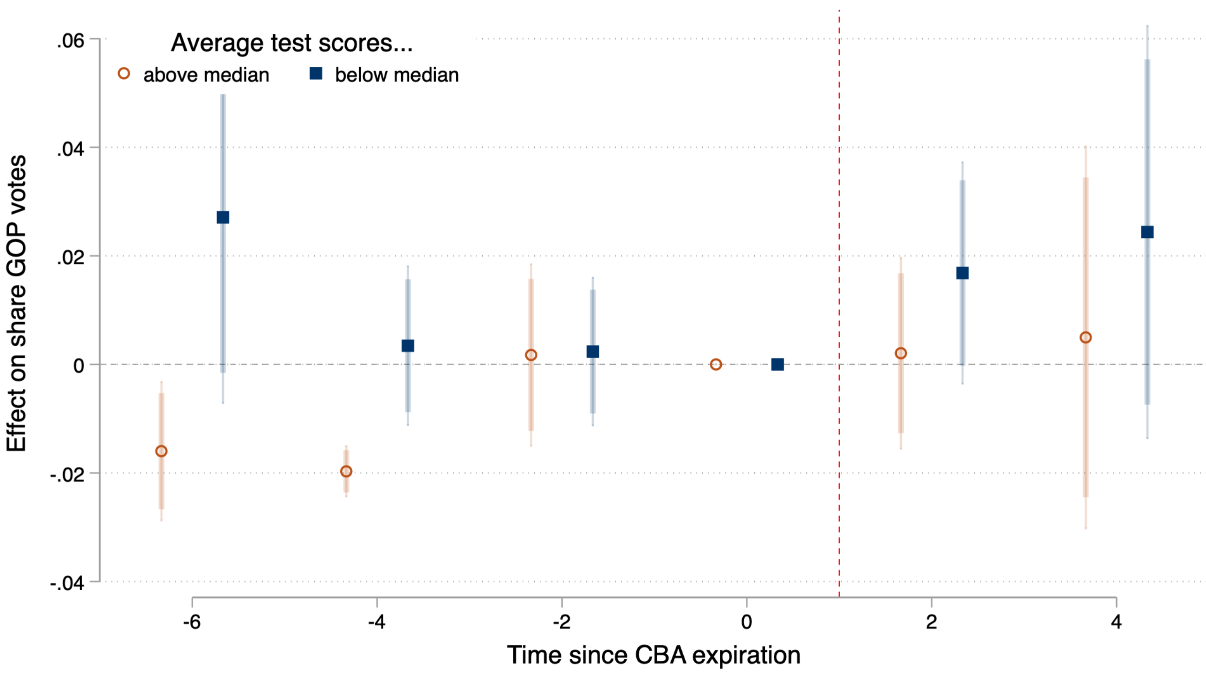
Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_k in equation (1), estimated using ward-level GOP vote shares for U.S. Presidential, House, and Senate elections and controlling for ward and year fixed effects. Standard errors are clustered at the district level.

Figure A11: Winners and Losers: Event-Study Estimates by Share of Teachers Younger than 27 in District



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 a 2010-11 share of teachers aged 27 or younger above the state median and the circles show estimates for districts with a share below the median. Standard errors are clustered at the district level.

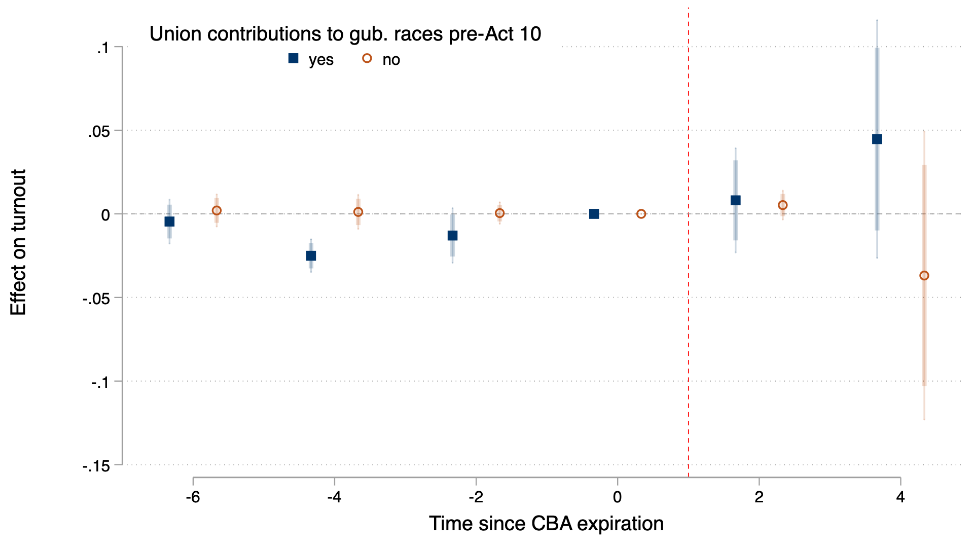
Figure A12: Winners and Losers: Event-Study Estimates by Ex Ante Student Test Scores



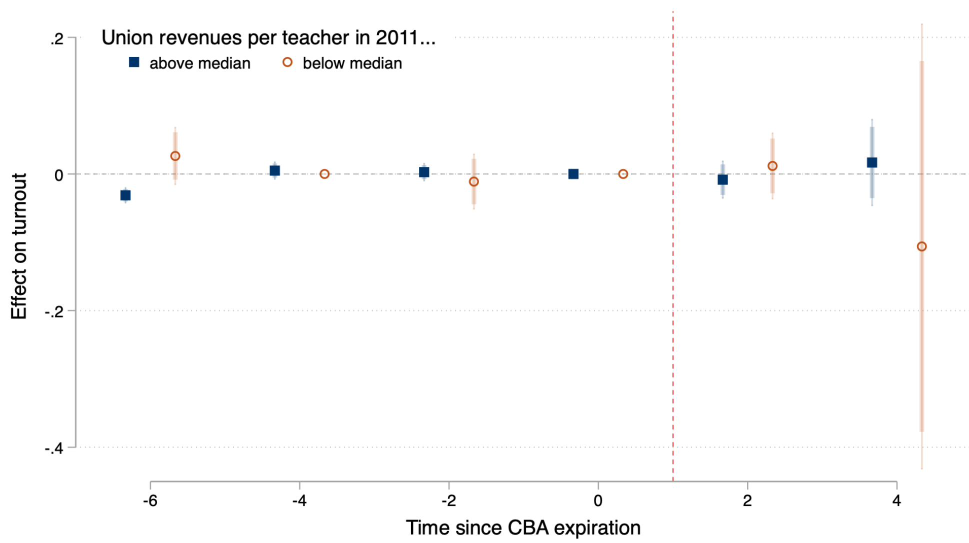
Notes: Estimates and 90% and 95% confidence intervals of the coefficients β_k in equation (1), estimated using the GOP vote share in gubernatorial elections in each ward and year as the dependent variable and controlling for ward and year fixed effects. The squares show estimates for the subsample of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

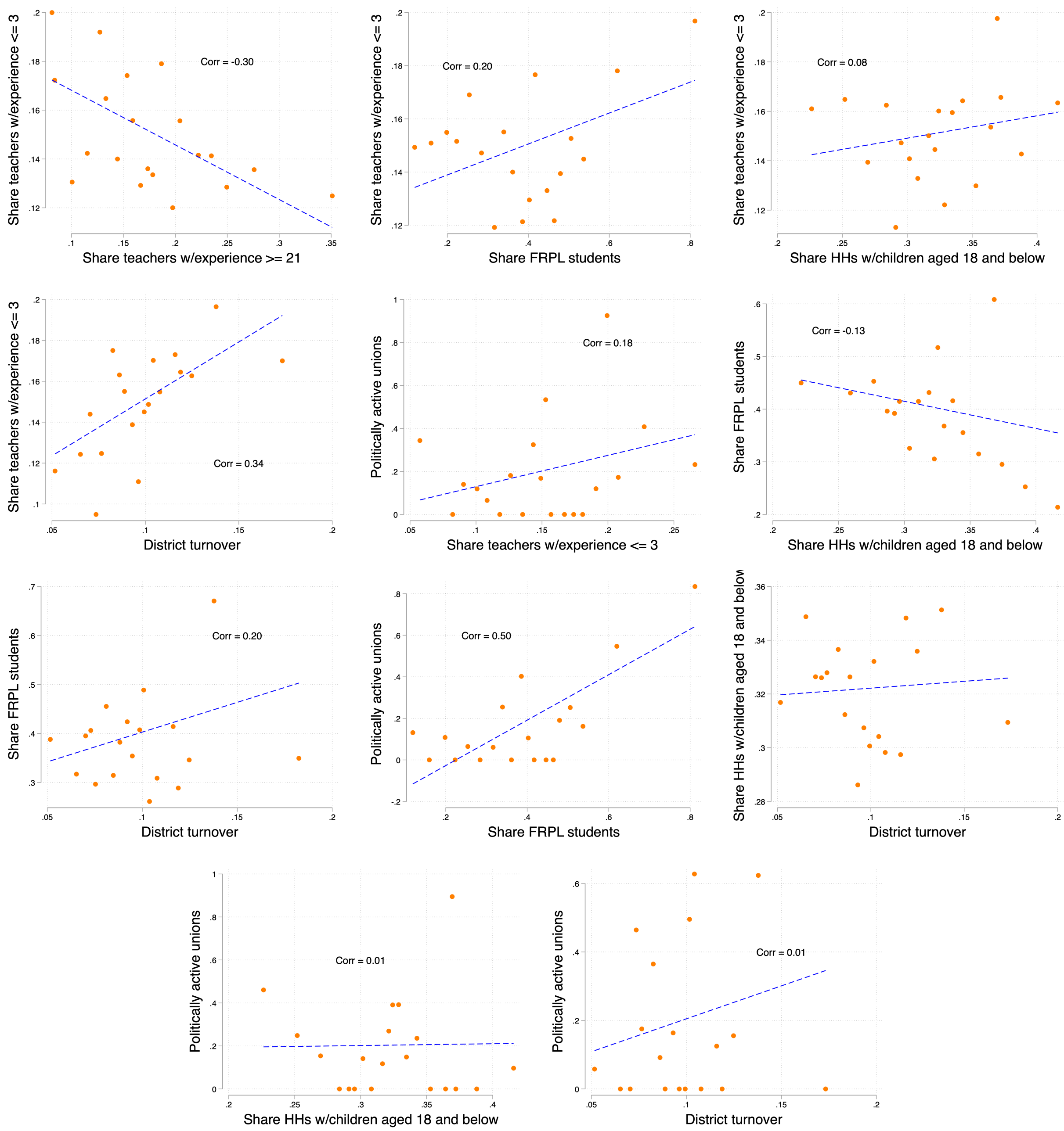


(b) By ex ante revenues



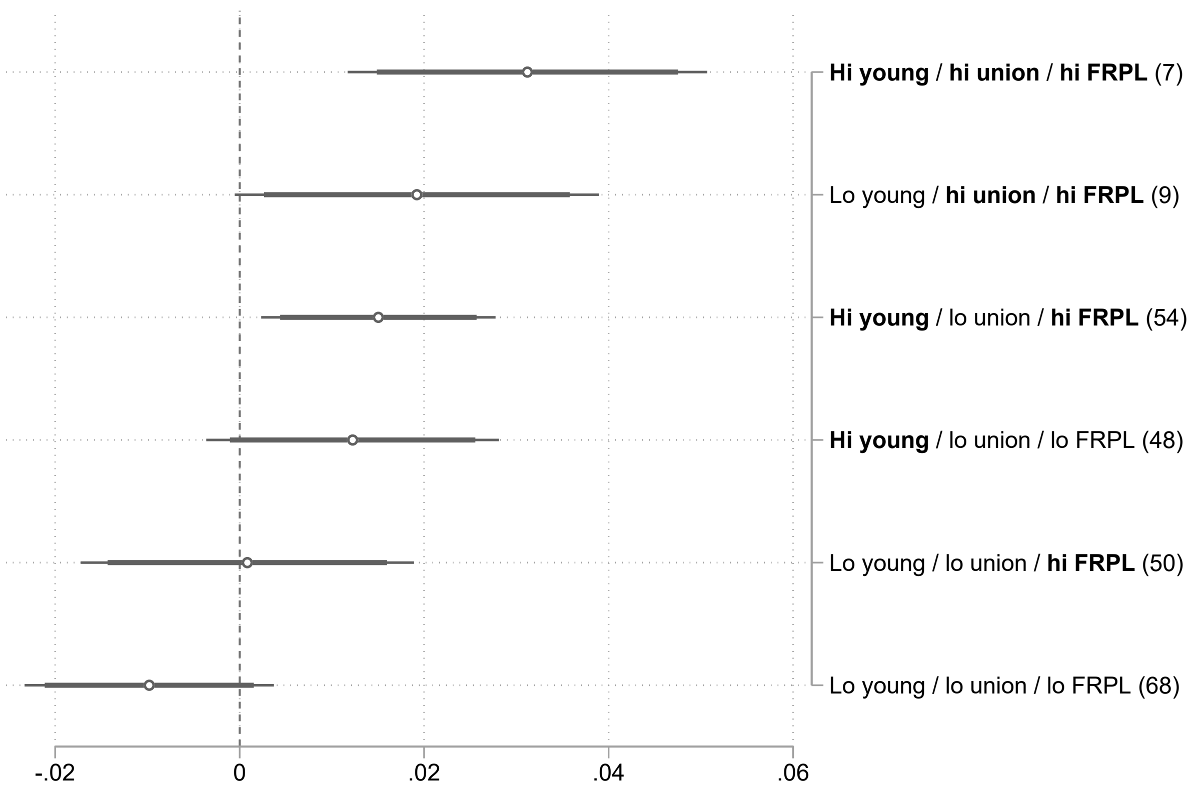
Notes: Estimates and 90-95% confidence intervals of the coefficients β_k in equation (1), estimated using voter turnout (the ratio between the number of votes cast and the number of people aged 18 and older) in each ward and year as the dependent variable and controlling for ward and year fixed effects. 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 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. Standard errors are clustered at the district level.

Figure A14: Correlations Between Dimensions of Heterogeneity



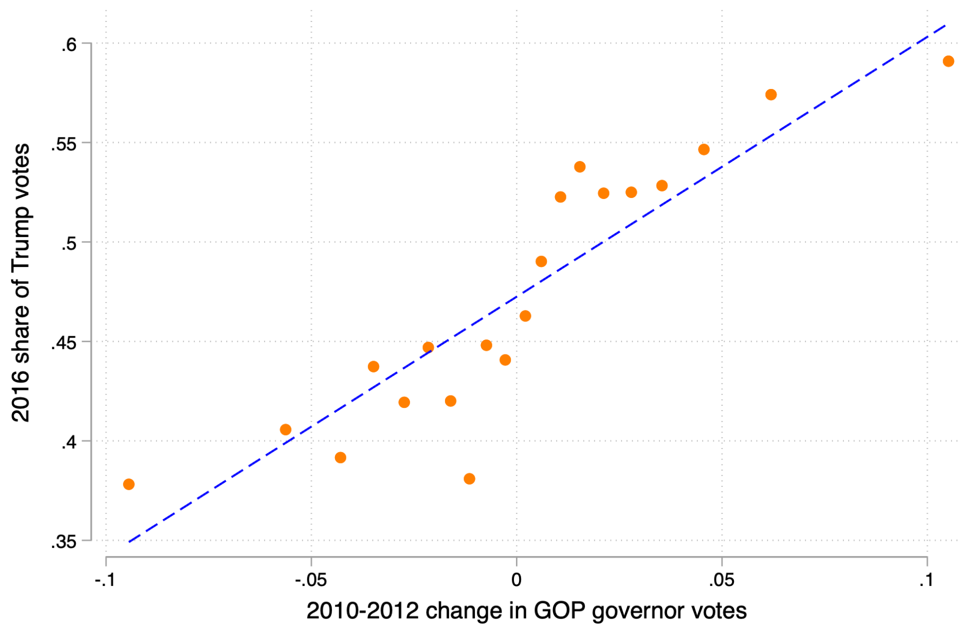
Notes: Scatter plots and correlations between various district-level characteristics, measured prior to 2011.

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



Notes: Estimates and 95% confidence intervals of the coefficients β_k in equation (1), for different groups of districts. 'Hi young' indicates districts with an above-median share of teachers with three years of experience or less. "Hi union" indicates districts represented by a union which was made at least one campaign donation to a Wisconsin gubernatorial campaign in the period 2002-2010. "Hi FRPL" indicates districts with an above-median share of students receiving free and reduced-price lunch. Number of districts in each cell shown in parentheses. Because no districts with politically active unions had a below-median share of FRPL students, the "Hi young / hi union / lo FRPL" and "Lo young / hi union / lo FRPL" cells are empty; this explains why the interaction of these three dimensions yields six rather than eight categories.

Figure A16: 2010-2012 Increase in Share of GOP Gubernatorial Votes and 2016 Share of GOP Presidential Votes



Notes: Binned scatterplot between the increase in the share of votes to the GOP gubernatorial candidate between 2010 and 2012 in each district (x-axis) and the share of votes for the GOP presidential candidate Donald Trump in 2016.

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

	All districts	No extension	W/extension
Teachers			
Share teachers w/experience < 3y (2010-11)	0.15	0.15	0.15
	(0.051)	(0.052)	(0.049)
Share teachers w/experience > 21y (2010-11)	0.18	0.19	0.17
	(0.067)	(0.069)	(0.065)
Teacher turnover rate (share who exits) (2010-11)	0.100	0.10	0.099
	(0.030)	(0.027)	(0.032)
Students			
Share low-SES (FRPL) students (2010-11)	0.38	0.32	0.43
	(0.19)	(0.16)	(0.19)
Std. test scores, Math (2010-11)	0.024	0.12	-0.044
	(0.33)	(0.28)	(0.35)
Share HHs with children < 18 yo (2010)	0.32	0.32	0.32
	(0.047)	(0.048)	(0.047)
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 (2010)	0.39	0.32	0.43
	(0.51)	(0.57)	(0.46)
100 * #Donations pp to GOP (2010)	1.43	1.82	1.13
	(1.28)	(1.68)	(0.74)
Unions			
Union made political donations (2002-201)	0.20	0.094	0.29
	(0.40)	(0.29)	(0.45)
Union revenues per teacher (2006-11)	733.9	448.4	879.6
	(1169.0)	(466.2)	(1375.2)
Number of wards	4,989	2,122	2,867
Number of districts	236	127	109

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 focuses on to districts without a CBA extension; and the third column focuses on 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; columns 2-3 controls for ward and year fixed effects. In column 3, we exclude the school district of Milwaukee. 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. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 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. * ≤ 0.1; ** ≤ 0.05; *** ≤ 0.01.

Appendix B Understanding The Importance of Various Dimensions of Heterogeneity

In this section, we detail our procedure to assess the importance of various dimensions of heterogeneity in explaining the overall electoral effects of Act 10. Our analysis proceeds in steps:

1. We augment the pooled version of equation (1) (where we assume $\beta_k = 0$ for $k \leq 0$ and constant β_k for $k > 0$), by interacting the treatment variable with six district-level dimensions of heterogeneity, measured in 2010-11 unless specified (for continuous variables, we use an indicator for the district being above the median): share of teachers with three or fewer years of experience; share of teachers with 21 or more years of experience; rate of teacher turnover; fraction of students on free and reduced-price lunch (FRPL); fraction of households with children under 18; and whether the union representing the district contributed to any Wisconsin gubernatorial race in the period 2002-2010. The results of this regression can be seen in columns 1 and 2 of Appendix Table B1.

2. To avoid overfitting the model, we apply LASSO to this regression model to select the most relevant interactions. LASSO identifies three interaction terms with non-zero coefficients: the share of low-experience teachers, the share of FRPL students, and the presence of politically active unions. Columns 3 and 4 of Table B1 show the results from the same regression described above, but including only those three LASSO-selected dimensions of interactions.

3. To allow for the possibility of interactions in the effects of each dimension of heterogeneity, we estimate a regression model which interacts the treatment indicator with each of the three LASSO-selected covariates, both individually and in groups of two. (Since there are no districts which feature both politically active unions and low FRPL, the interactions *Exposed* $\times$ *Hi FRPL* $\times$ *Hi Union* and *Exposed* $\times$ *Hi Young* $\times$ *Hi FRPL* $\times$ *Hi Union* are automatically excluded from the regression.) Columns 1 and 2 of Table B2 report the full results from this regression.

4. For ease of exposition, we report the linear combinations of interaction effects in Table B2 for each of the six cells characterized by the combination of the dimensions of heterogeneity selected by LASSO (again omitting the two empty cells). These linear combinations correspond to the full effect of Act 10 on districts in each cell. The cell-by-cell effects of Act 10 are reported in Table B3. The results are broadly consistent with those from the previous heterogeneity analyses. Positive effects on GOP vote share appear in districts with some combination of a high share of young teachers, a high share of FRPL students, and ex ante politically active unions. However, effects are largest and most significant in districts where all three measures are high.

5. Figure A15 plots these cell-by-cell effects (from the regression employing district fixed effects, column 1 in Table B3), displaying the number of districts in each cell to the right of the estimate in parentheses. (Empty cells are excluded from the graph.)

Table B1: Political Effects of Wisconsin's Act 10: Pooled Event Study — dimensions of heterogeneity

	All dimensions		LASSO-selected	
	(1)	(2)	(3)	(4)
Exposed	-0.007	-0.007	-0.008	-0.008
	(0.009)	(0.009)	(0.007)	(0.007)
Exposed × High Young Teachers	0.018**	0.019**	0.018**	0.018**
	(0.008)	(0.008)	(0.008)	(0.008)
Exposed × High Old Teachers	0.004	0.005		
	(0.008)	(0.008)		
Exposed × High Teacher Turnover	-0.003	-0.002		
	(0.008)	(0.008)		
Exposed × High FRPL	0.009	0.009	0.007	0.008
	(0.009)	(0.009)	(0.009)	(0.009)
Exposed × High HHs w/ kids	-0.001	-0.001		
	(0.008)	(0.008)		
Exposed × Politically Active Union	0.015	0.015	0.016*	0.016
	(0.010)	(0.011)	(0.010)	(0.010)
District FE	Yes	No	Yes	No
Ward FE	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
Mean dep. var. control	0.474	0.474	0.474	0.474
N	23168	23162	23250	23244
Clusters (districts)	235	235	236	236
R-squared	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 $k > 0$. Columns 1 and 2 include interactions of the 'Exposed' dummy with all six district-level dimensions of heterogeneity. Columns 3 and 4 include only the three interactions selected by LASSO. 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 B2: Political Effects of Wisconsin's Act 10: Pooled Event Study, with interactions among LASSO-selected dimensions of heterogeneity

	(1)	(2)
Exposed	-0.010	-0.010
	(0.007)	(0.007)
Exposed × Hi Young Teachers	0.022**	0.022**
	(0.009)	(0.009)
Exposed × Hi FRPL	0.011	0.011
	(0.010)	(0.010)
Exposed × Politically Active Union	0.018	0.018
	(0.012)	(0.012)
Exposed × Hi Young × Hi FRPL	-0.008	-0.008
	(0.009)	(0.009)
Exposed × Hi Young × Hi Union	-0.002	-0.002
	(0.016)	(0.017)
Mean dep. var. control	0.474	0.474
N	23250	23244
Clusters (districts)	236	236
FE	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. Each regression includes interactions of the 'Exposed' dummy with the fully saturated interactions of the three dimensions of heterogeneity selected by a LASSO reported in Table B1. Column 1 controls for district fixed effects; Column 2 controls for ward fixed effects. All regressions include controls for the interaction of year dummies with each of the dimensions of heterogeneity selected by LASSO. Standard errors in parentheses are clustered at the district level. * ≤ 0.1; ** ≤ 0.05; *** ≤ 0.01.

Table B3: Political Effects of Wisconsin's Act 10: Pooled Event Study, by district characteristics

	(1)	(2)
Lo young / lo union / lo FRPL	-0.010	-0.010
	(0.007)	(0.007)
Hi young / lo union / lo FRPL	0.012	0.012
	(0.008)	(0.008)
Lo young / lo union / ***hi FRPL***	0.001	0.001
	(0.009)	(0.009)
Hi young / lo union / ***hi FRPL***	0.015**	0.016**
	(0.006)	(0.006)
Lo young / ***hi union*** / ***hi FRPL***	0.019*	0.020*
	(0.010)	(0.010)
Hi young / ***hi union*** / ***hi FRPL***	0.031***	0.032***
	(0.010)	(0.011)
Mean dep. var. control	0.474	0.474
N	23250	23244
Clusters (districts)	236	236
FE	District	Ward

Notes: The dependent variable is the share of GOP votes in gubernatorial elections in each ward and year. The values shown in this table correspond to linear combinations of the interaction terms reported in Table B2 to recover the full effect of Act 10 on the different groups of districts represented by the combination of characteristics articulated in each row. "Lo young / hi union / lo FRPL" and "Hi young / hi union / lo FRPL" are excluded as no districts in the sample match these descriptions. Standard errors in parentheses are clustered at the district level. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.