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

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Abstract

Public-sector reforms often provoke political backlash. Can voters' experience of public service improvements shift beliefs and generate electoral benefits? We study this question in the context of Wisconsin's Act 10, which weakened teachers' unions and liberalized pay, prompting mass protests. Exploiting its staggered implementation across school districts, we show that exposure to the reform reduced union revenues, raised student test scores, and increased some teachers' pay. It also increased the incumbent governor's vote share and reduced contributions to his opponents. Gains were largest where more voters benefited, showing how initially controversial reforms can generate support once voters experience their effects.

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1 Introduction

Political constraints prevent the implementation of many potentially welfare-improving policies. Clear and concentrated costs often provoke strident opposition from losers, whereas diffuse and uncertain benefits for winners produce lukewarm support at best (Olson, 1965). These dynamics are particularly evident in education. School reforms often generate visible and immediate losses for politically organized groups — such as public-sector unions and tenured teachers — while the gains may accrue only slowly, and primarily to students who lack political voice. Due to this perceived asymmetry, many policymakers hesitate to adopt education proposals related to accountability, teacher performance pay, and school choice even in the presence of promising empirical evidence.[1]

However, when voters personally experience an effective policy, initial opposition can give way to acceptance or even support. For example, the Affordable Care Act became more popular in public opinion polls as people experienced its effects (Lerman and McCabe, 2017; Sances and Clinton, 2021). Teachers in India who took part in a performance-pay program became more supportive of it after participating in the program (Muralidharan and Sundararaman, 2011). These examples suggest that experiential learning can shape political attitudes and may even affect electoral outcomes.

Whether direct experience with a policy affects voters' choices is theoretically ambiguous and difficult to measure empirically. While theories of electoral accountability hold that voters reward or punish elected officials for the government's actions, identity-based voting models imply that partisan attachments and cultural cues can dominate policy responses. Testing these theories is complicated by the fact that reforms are frequently correlated with other factors that shape voters' political support (such as shifts in government budgets or broader economic conditions). These policies are also typically implemented statewide, leaving insufficient exogenous variation to isolate their causal impact on electoral outcomes, let alone the mechanisms through which such impacts may arise. Lastly, many education policies fail to achieve their intended effects or are never rigorously

[1] For example, Florida's Senate Bill 6 of 2010 proposed to tie teacher pay and job security to student test scores and eliminate tenure; it was met with enormous backlash and ultimately vetoed by the state's Republican governor (Hamacher, 2010). Michigan's Proposal 1 of 2000, which would have provided a $3,300 voucher to each student to attend private school, faced a similarly fierce campaign and was defeated in the polls (Keenon, 2001). Various large urban schools districts have given up significant federally funded programs due to the opposition of teachers' unions (Zubrzycki, 2012). Washington DC's IMPACT program is widely considered to have precipitated the electoral loss of its mayoral champion, despite evidence that it improved teacher selection (Dee and Wyckoff, 2015).

evaluated, limiting what can be inferred about downstream political responses.

In this paper, we address these challenges by studying the political effects of Act 10, a 2011 Wisconsin school reform — championed by a Republican governor — that significantly curtailed the powers and resources of teachers' unions. Two features of this setting make it particularly well suited to study the effects of voters' exposure to the reform on their political attitudes and the viability of the policy change. First, the reform's provisions could only be implemented in any given district after the expiration of its preexisting collective-bargaining agreements (CBA), creating plausibly exogenous variation in the timing of voters' experience of the consequences of the reform. This allows us to measure the effect of firsthand experience of the law as distinct from mere knowledge of it, which was common to all voters when the law passed. Second, the passage of the law 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 an opportunity to evaluate the electoral effects of exposure to the reform.

We find that that direct exposure to the reform increased the vote share of the incumbent governor by 2.6 percentage points (pp hereafter). These political gains were 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 exposure to concrete policy outcomes and institutional actors in shaping the political consequences of the reform.

Passed in March 2011, Act 10 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 substantial 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 some other public-sector employees, it was widely perceived as a direct attack on teachers. Large protests against the law broke out 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 exogenous variation in the timing of implementation 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. As voters across the state were widely exposed to media coverage of Act 10, our estimates capture the effect of being directly exposed to the reform's concrete effects, rather than simply being aware of the policy.

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 link these data to individual campaign contributions records, which provide another measure of political support; to district CBA expiration dates, which we hand-collected from union contracts, school board minutes, and newspaper articles; and to individual-level information on teachers and students in each district, from the Wisconsin Department of Public Instruction. Lastly, we 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 substantially weakened 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. Among teachers, the reform created clear sets of winners and losers. It flattened the relationship between salary and experience, leading to higher salaries for younger and less credentialed teachers while reducing the pay of older, more experienced teachers. 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.[2]

Turning to its political effects, we find that experiencing Act 10 made voters significantly more likely to vote for Walker. Event-study estimates indicate that one to four years of voter exposure to the consequences of the reform increased the share of votes for the GOP governor by 2.6 pp. These effects represent a large fraction (over 80%) of the margin of victory in the 2012 and 2014 elections.

[2] A possible explanation for the increase in test scores is that pay flexibility helped districts attract and retain high-quality teachers (Biasi, 2021).

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 2.9 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.

These effects are unlikely to be driven by the high-profile passage of the law in 2011, but rather by direct exposure to its practical implementation at the district level. We present three pieces of evidence to argue that our findings are consistent with most people holding negative priors of the reform's effects and then positively updating their beliefs upon exposure. 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 two distinct channels behind the political effects of Act 10. The first is a demobilization channel, operating through the decline in unions' power and ability to turn out democratic voters. The second is a persuasion channel, operating through the direct benefits that the reform generated for key constituencies such as teachers and the families of students. We assess the importance of the first channel by studying the effect of exposure to the law on voter turnout and GOP vote shares across school districts with varying union strength, and of the second by comparing Walker's electoral gains across school districts with varying numbers of winners and losers.

We find a precisely estimated zero effect on overall voter turnout, suggesting that changes in unions' ability to mobilize voters did not drive the vote share effects we observe.[3] These negligible effects on turnout were not significantly different in districts whose unions had contributed to

[3]In Wisconsin, voter registration files do not track party affiliation, preventing us from studying changes in the composition of voters. See https://badgervoters.wi.gov/Requests/NewRequest for more information.

electoral races pre-reform (a measure of political involvement), and they were only slightly smaller in districts whose unions charged higher fees pre-reform (an indicator of union strength). At the same time, GOP electoral gains were larger in districts with politically active and stronger unions, suggesting that local union strength mattered for the law's effects along dimensions other than voter mobilization.

By contrast, we find clear evidence that persuasion played an important role. GOP electoral gains were larger in districts with a higher share of young, less experienced teachers (who experienced salary increases after Act 10); in districts 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). 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. Taken together, these results suggest that Act 10 generated electoral support not only by weakening unions, but also by improving the experience of public education for a significant share of providers and users. 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 who benefited from the law.

Contribution to the literature Our paper contributes to the education literature on collective bargaining, teachers' labor markets, and teacher pay. Some studies on teacher unionization have 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 mixed conclusions (see Neal, 2011; Pham et al., 2021, for reviews). Unionization and performance pay tend to be politically contentious policies (Dee and Wyckoff, 2015); some implemented performance pay programs consist only of extra bonuses to avoid creating a set of losers (Leaver et al., 2021; Arechaga, 2025). We extend this literature by causally identifying the political effects of a large-scale change in collective bargaining rules and teacher compensation, highlighting how scalability depends not only on educational impacts but also on voters' responses to experiencing reform.

We also contribute to the literature on Act 10. Prior work has shown that the law changed teacher

labor markets (Biasi, 2021; Biasi and Sarsons, 2022; Biasi et al., 2021, 2025) and weakened unions (Foy, 2024a). We build on this evidence by demonstrating that the same reform improved student learning, particularly for disadvantaged students, and that these improvements, along with weakened union power, translated into meaningful electoral gains for the incumbent governor.[4]

Our study further informs the broader literature on the electoral consequences of public policies. Research on fiscal policy has found mixed effects on voter behavior (see, for example, Brender and Drazen, 2008; Carreri and Martinez, 2021), whereas studies of salient infrastructure or redistributive programs often document electoral rewards.[5] We contribute by showing that voters can notice and reward a policy that improves the quality and efficiency of public services, even without visible capital investments and even when the policy reduces spending. Our findings also underscore that the distribution of private benefits is a central determinant of political support (Méndez and Van Patten, 2022).

Finally, we contribute to a nascent literature on the electoral effects of education policies. Prior studies have focused primarily on developing-country settings or small-scale interventions (Sandholtz, 2023; Dias and Ferraz, 2019; Litschig and Morrison, 2013; Cox et al., 2024). A rare 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 by providing causal evidence on the political consequences of a large-scale education reform in the United States and by shedding light on why such a controversial policy generated political support. By studying a reform that directly targeted public-sector unions, we also contribute to research on how unions shape public policy and electoral outcomes (Carreri et al., 2025; Anzia and Moe, 2015; Hartney and Kogan, 2024), showing that reductions in union strength meaningfully shifted political behavior (Moe, 2011; Hartney, 2022).

[4]Looking at a shorter time span than we examine and at focusing on high-school students, Baron (2018) finds negative effects of Act 10 on test scores.

[5]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. On the electoral effects of infrastructure, see Huet-Vaughn (2019); Boudot-Reddy and Butler (2024); Voigtländer and Voth (2021); Harding (2015); Leff Yaffe et al. (2025); Garfias et al. (2021). On the electoral effects of cash transfers, see Manacorda et al. (2011); De La O (2013).

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 teachers' 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 step-and-lane salary schedules, which determined pay based on seniority and academic credentials. This system ensured that teachers with comparable seniority earned 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 many to be a direct attack on the teacher profession: A Google Trends search of the

term "Wisconsin Act 10" between January 1, 2011 and December 31, 2012 yields "teachers" as the most searched associated topic.

Act 10 introduced a series of changes. First, it 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 made 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 was highly controversial. Protests erupted in Madison before the bill was voted on 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.[6]

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 percentage points (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.[7]

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 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

[6] 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.

[7] 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 fourth-most-searched related topic, preceded by "Recall election," "Trade union," and "Tony Evers."

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. In addition to vote shares, we use these data to calculate voter turnout. Because Wisconsin allows same-day voter registration, the state reports turnout as total vote count divided by the population aged 18 and over. We follow this convention, measuring ward population using the 2010 Census.

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 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, district employee handbooks, school board meeting minutes, and local news reports.[8] 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 levels), 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 unified school 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 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

[8] 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.

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.[9] 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 school-age children (aged 18 and below) and the number of people eligible to vote (aged 18 and above). Specifically, we aggregate population counts from the Census block level to the ward level by assigning each block to the ward in which its centroid is located.

3.6 Campaign Contributions

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

[9] 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 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.

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 crowdsourced 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 link 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 by 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

Table 1: Wisconsin Wards and School Districts: Summary Statistics

		Expiration in	
	All districts	2011	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.

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 do-

nations, 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. To ensure that these differences do not affect our estimates, in robustness checks we re-estimate our main specifications controlling for pre-existing observable differences interacted with linear year trends (Table 6, columns 1-2).

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 uses the variation in the timing of 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 in 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:

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

where V_{jk} denotes an outcome (for example, 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 outcome k years since an expiration, relative to the year of the expiration. Because gubernatorial elections occur only in selected years (2002, 2006, 2010, 2012, and 2014), we collapse the event-time dummies into two-year bins when vote shares or campaign donations are the dependent variable. 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. When outcomes are aggregated, we weight observations by the number of people in each cell (for example, we weight vote share and campaign contribution models by each ward's population of people aged 18 or older).[10]

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.

We also show estimates from pooled event study regressions:

$$V_{jt} = \beta^{\text{post}} \mathbb{1}\left(t \geq E_d(j)\right) + \theta_j + \tau_t + \varepsilon_{jt} \tag{2}$$

where everything is as before and and β^{post} captures an average post-expiration effect.[11]

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:

$$V_{jt} = \beta D_{d(j)} \mathbb{1}(t > 2011) + \theta_j + \tau_t + \varepsilon_{jt} \tag{3}$$

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 difference in the share of votes for the GOP governor between treated and control districts after 2011, relative to the pre-reform period.

[10] Our results are robust to omitting these weights.

[11] While equation (1) is limited by power to estimating cohort-specific coefficients $t \in [-4, 4]$, equation (2) includes data from all gubernatorial elections going back to 2002.

We also estimate a dynamic version of equation (3):

$$V_{jt} = \sum_{\substack{t \in \{2002,2006,\\ 2012,2014\}}} \beta_k D_{d(j)} \mathbb{1}(t = k) + \theta_j + \tau_t + \varepsilon_{jt} \tag{4}$$

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 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 later show that 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 6, columns 3-4, for the pooled event study and Appendix Table A3, columns 3-4, 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 Teachers' 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 member, 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 $k \in [-4, 3]$, obtained using the natural logarithm of each district's union revenues per member as the dependent variable, show that revenues had fallen by 55% two to three 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%). Unions 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).

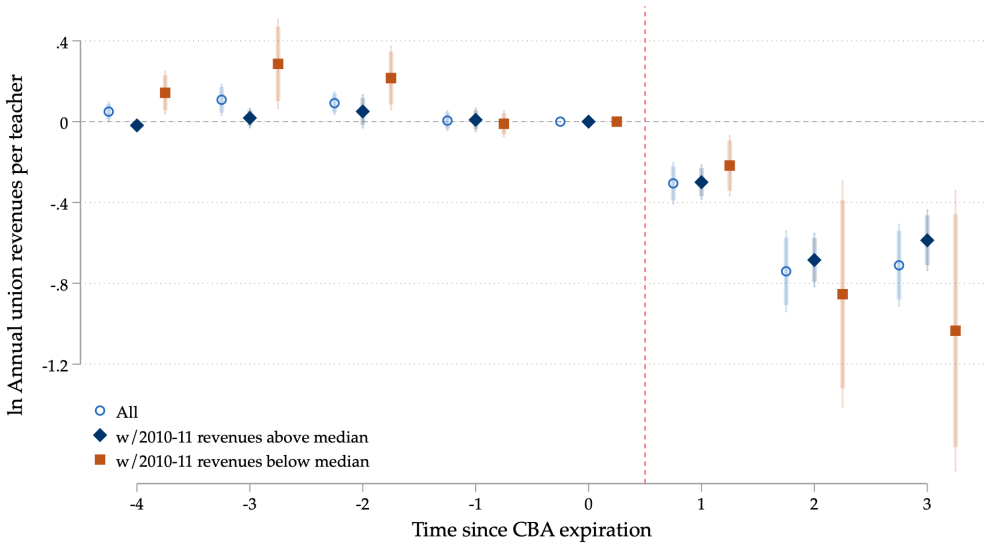
5.2 Teacher Compensation

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 new 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 considerably.

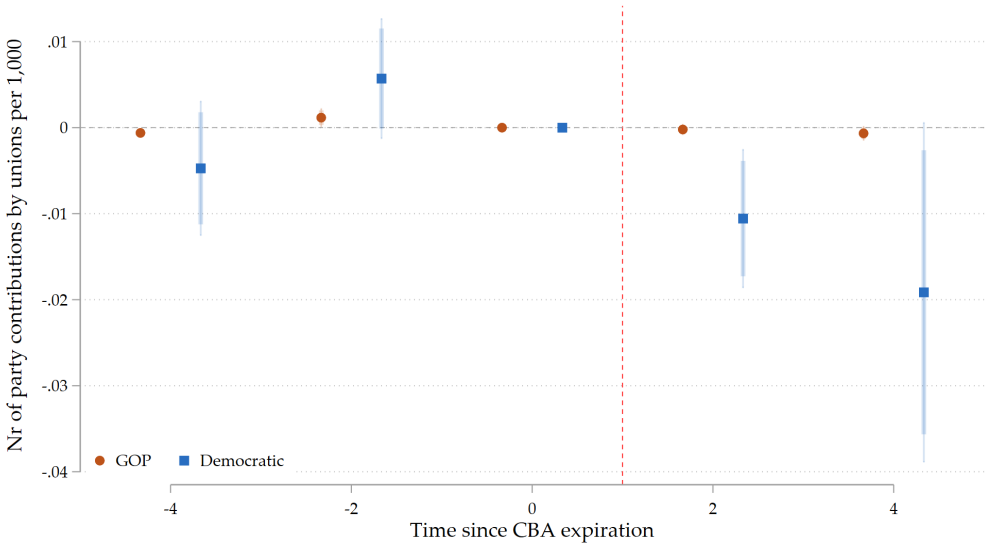
To study the average impacts of Act 10 on teacher compensation, we estimate equation (1) using individual teacher salaries and benefits as the outcome variable. Estimates of β_k indicate that average

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 (circle series) and separately for districts with revenues per member above and below the state median in 2010-11 (diamond and square series, respectively). 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.

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% more than teachers with 3 years of experience.[12]

These results indicate that Act 10 created clear winners and losers in terms of salaries, based on teacher age and experience. In Section 8, we link the distribution of benefits for teachers across Wisconsin districts to the political consequences of the reform.

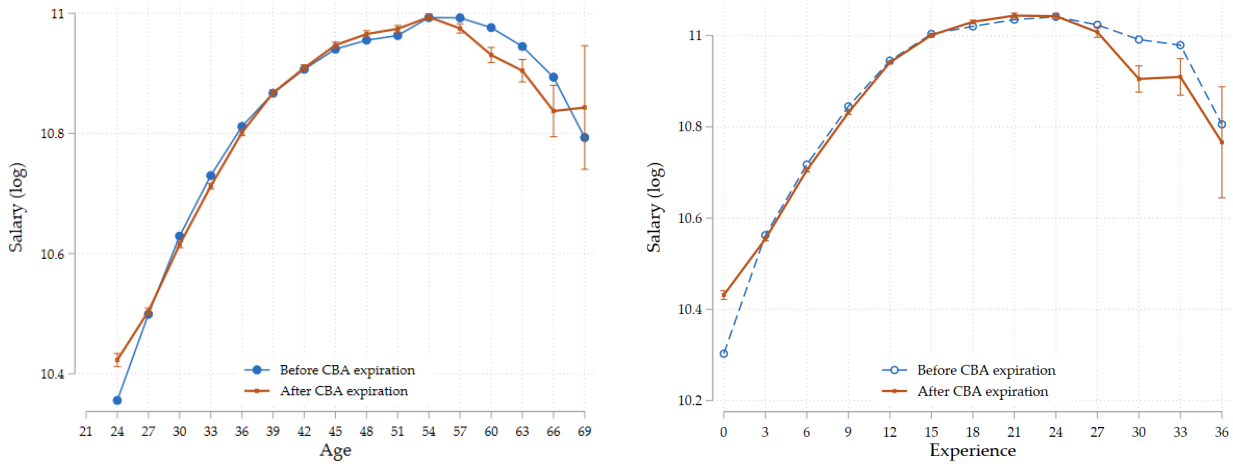
5.3 Student Test Scores

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.

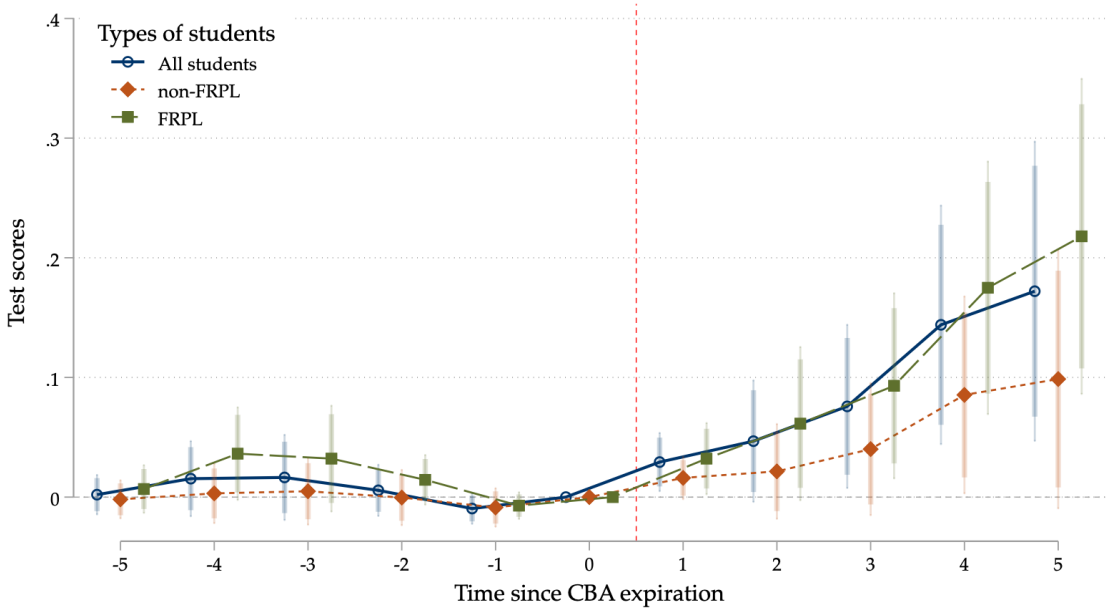
[12] Appendix Figure A5 shows the distribution of the post-CBA expiration change in salary across districts, separately for teachers aged 63 and older and for those aged 27 and below. In 155 districts (63%) the salary change for young teachers is larger than that for older teachers, and for 130 (53%)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

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. Square 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.

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.[13]

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 nor on the share of minority (Black or Hispanic) or FRPL 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 voters' responses to Act 10 in Section 8.

6 The Political Effects of Wisconsin's Act 10

We now examine the effects of Act 10 on vote shares and campaign donations to each party in gubernatorial elections. We present event-study estimates and probe the robustness of our findings to different empirical models and assumptions.

6.1 Effects on Gubernatorial GOP Vote Share

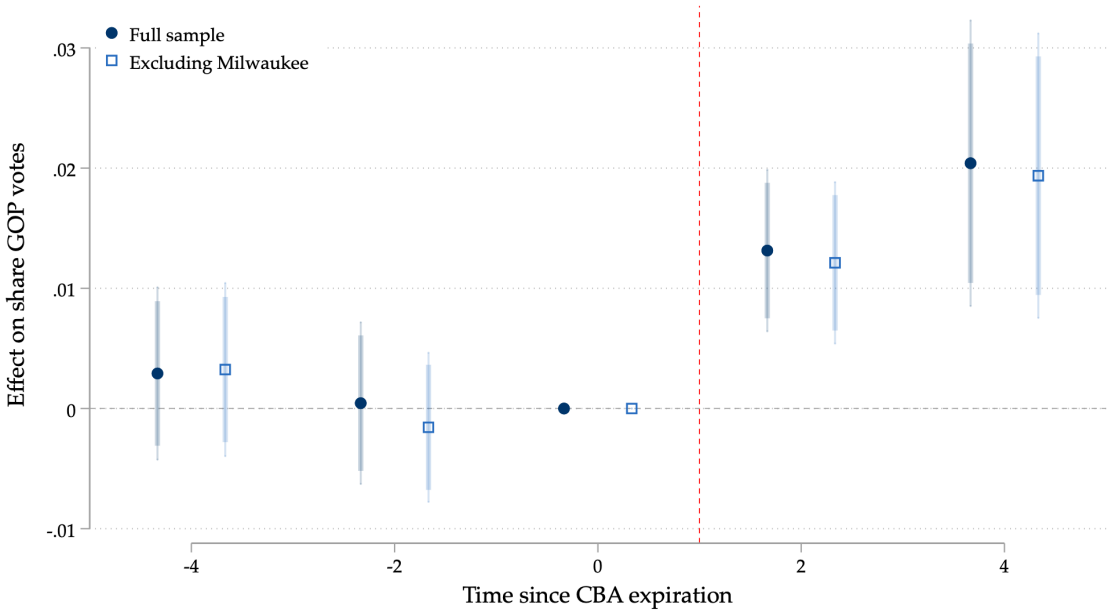
Exposure to Act 10 significantly increased the share of votes to the GOP in gubernatorial elections. Figure 3 (circle series) shows estimates of β_k in equation (1) with ward-level GOP vote shares as the dependent variable and controlling for ward fixed effects. The effect appears immediately after a CBA expiration and grows over time, reaching 2 pp by three to four years after an expiration. This change corresponds to a 3.9% increase in the GOP vote share relative to a pre-Act 10 mean of 51.3%. In two-party terms, this implies an increase in the GOP margin of 4 pp, or 60% of the actual margin of victory in the 2014 election.[14] 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

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

[14] Walker's margin of victory in 2014 was 6.7 pp, with 53.3% vote share vs. his Democratic opponent Mary Burke's 46.6% share. Reflecting the fact that increasing GOP vote share by one percentage point necessarily increases two-party margin by two percentage points, we calculate that the effect on GOP two-party margin is $2 \times 0.02 = 0.04$, which is 60% of the overall margin of victory of 0.067.

and after each expiration and estimate β^{post} in equation (2); the average effect is equal to 2.6 pp, corresponding to 84% of the 2012-2014 average margin of victory. Effects are robust to the inclusion of either district (column 1) or ward fixed effects (column 2).

Figure 3: Electoral 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 squares show the same estimates excluding the school district of Milwaukee. Observations are weighted by the number of people aged 18 and above in each district. Standard errors are clustered at the district level.

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 Appendix Figure A8 as the triangles and squares, respectively, are similar to our baseline estimates.[15]

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

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.[16] Results from this model confirm findings from the event studies. Estimates of the parameter β in equation (3), obtained including the 2012 recall election as the only post-reform election and controlling for ward fixed effects, indicate that wards in districts that had been exposed to the law by that time had a 2.9 pp (6.2%) higher GOP vote share compared with not-yet-treated districts (Table 3, column 2, significant at the 5% level). Estimates are similar if we also include data from the 2014 election, which Walker also won (columns 3 and 4). The estimates become larger (at 5.6 pp) when we include 2014 and limit the control group to wards in districts whose agreements expired in either 2014 or 2016 ("clean controls," columns 5 and 6).[17]

[16] 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.

[17] Appendix Figure A9 shows estimates of the parameters β_s in equation (4). 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.

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

	All districts		2014 and 2016 never treated	
	(1)	(2)	(3)	(4)
Exposed	0.026***	0.026***	0.026***	0.026***
	(0.007)	(0.007)	(0.007)	(0.007)
District FE	Yes	No	Yes	No
Ward FE	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
Mean dep. var. control	0.465	0.465	0.465	0.465
N	24542	24538	24542	24538
Clusters (districts)	236	236	236	236
R-squared	0.75	0.95	0.75	0.95

Notes: The dependent variable is the share of GOP votes in gubernatorial elections in each ward and year. We show estimates and standard errors of the parameter β^{post} in equation (2). 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. Observations are weighted by the number of people aged 18 and above in each district. Standard errors in parentheses are clustered at the district level. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.

6.2 Effects on Campaign Contributions

Next, we examine the impact of the law on campaign contributions to gubernatorial races made by individual donors. Campaign contributions provide a complementary measure of political support: most voters do not donate to campaigns, so contributions function as a measure of enthusiasm among the strongest partisans on either side. This therefore also allows us to separately identify changes in support for each party over time among the most engaged segment of the electorate. 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). We weight observations by district above-18 population.

We find that exposure to the consequences of the reform significantly reduced support for the Democratic party and mildly increased support for the GOP. Estimates of β_k indicate that the Democratic party received 34.6 fewer contributions per 1,000 people 3 to 4 years after exposure (a seven-fold decline given an average contribution rate of 4.6 contributions pre-reform; Figure 4, panel(a)), while

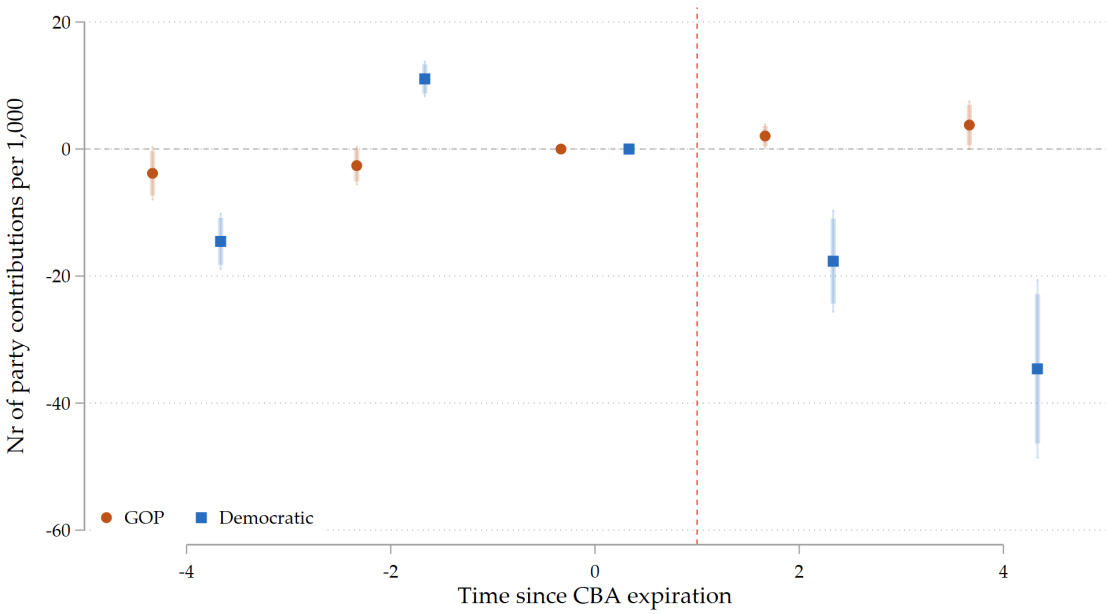
Table 3: Electoral Effects of Wisconsin's Act 10: Difference-in-Differences Estimates

	All districts				Only clean controls	
	(1)	(2)	(3)	(4)	(5)	(6)
CBA after 2011 * post 2011	0.029**	0.029**	0.026**	0.026**	0.056***	0.056***
	(0.012)	(0.012)	(0.011)	(0.011)	(0.010)	(0.010)
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	19640	19632	24542	24538	9596	9594
Clusters (districts)	236	236	236	236	107	107
R-squared	0.75	0.95	0.75	0.95	0.87	0.97
Latest year included	2012	2012	2014	2014	2014	2014

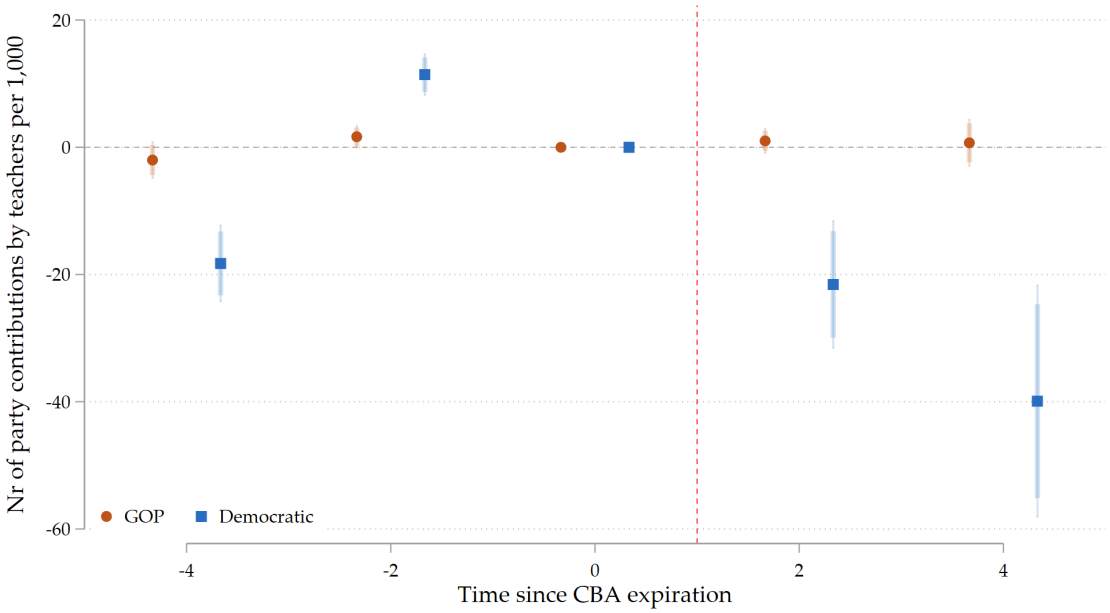
Notes: The dependent variable is the share of GOP votes in gubernatorial elections in each ward and year. We show estimates and standard errors of the parameter β in equation (3), using as the treatment variable an indicator for districts with CBAs or extensions expiring in 2011. Columns 1-2 are estimated 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 year and ward fixed effects. Columns 5-6 are estimated only on wards in districts with CBAs expiring in 2011, 2014, and 2016. Observations are weighted by the number of people aged 18 and above in each district. Standard errors in parentheses are clustered at the district level. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.

Figure 4: Effects on Campaign Contributions to Political Parties: 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 the number of people aged 18 and above in each district. Standard errors are clustered at the district level.

the GOP received 3.8 additional contributions (a 23% increase). These results confirm an increase in relative support for the GOP after Act 10; they also reveal that this change was largely driven by a decline in support for the Democratic party.

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	4.205***	-11.452	1.138	-11.779	0.306	-7.474	-0.144***
	(1.059)	(7.961)	(1.048)	(7.200)	(0.199)	(5.342)	(0.055)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var. control	16.825	4.637	3.775	8.747	2.401	2.305	0.062
N	1080	1080	1075	1075	1080	1080	1080
Clusters (districts)	216	216	215	215	216	216	216
R-squared	0.79	0.82	0.34	0.61	0.71	0.80	0.74

Notes: The dependent variable in columns 1-6 is the number of contributions to gubernatorial races per 1,000 people in each district to each party. The dependent variable in column 7 is a dummy for whether any union in the district made a contribution to a Democratic gubernatorial candidate. We show estimates and standard errors of the parameter β^{post} in equation (2). 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 1,000 people in the district; columns 3 and 4 use donations by teachers to each party per 1,000 teachers in the district; columns 5 and 6 use donations by 2012 gubernatorial recall petition signers per 1,000 people in the district; and column 7 uses a dummy for whether any donation was made by the teacher's union in the district. All specifications control for year and district fixed effects. Observations are weighted by the number of people aged 18 and above in each district. Standard errors in parentheses are clustered at the district level. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.

The drop in campaign contributions to the Democratic party was even larger among teachers, at 40.0 fewer contributions per 1,000 teachers (a nearly five-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 Findings Are Consistent With Changes in Voters' Beliefs

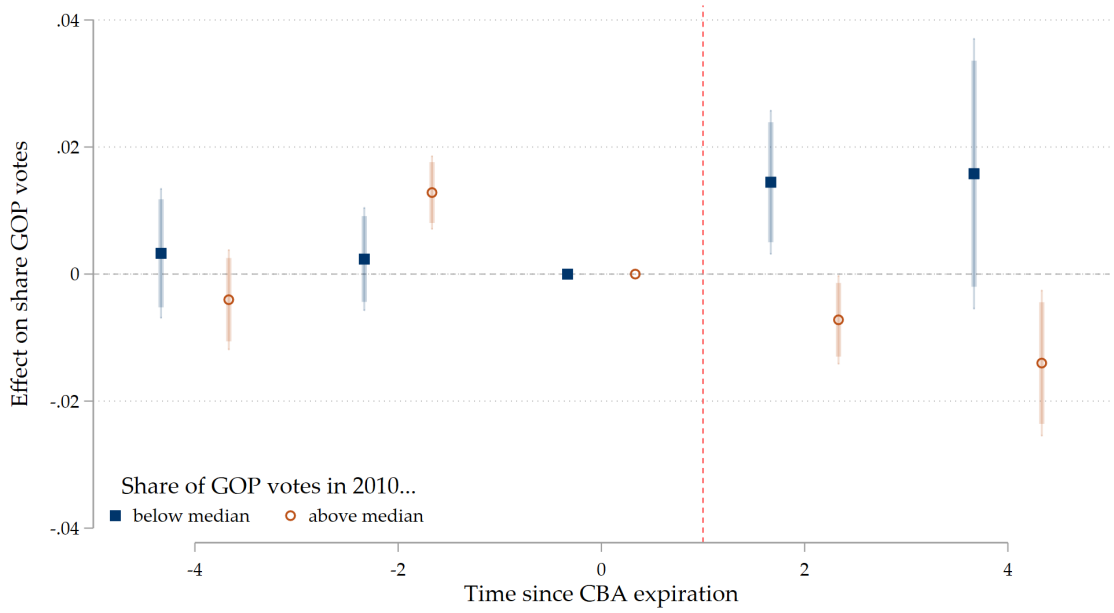
Our results so far indicate that exposure to Act 10's consequences triggered an increase in political support for the GOP governor. These results cannot be interpreted as identifying the overall electoral treatment effect of Act 10. All Wisconsin voters — including those in late-treated districts — could plausibly have known at the time of the law's passage that Act 10 would eventually be implemented.

Our empirical strategy, which leverages staggered CBA expirations, is designed to capture the effect of *direct exposure* to the real consequences of Act 10, as distinct from the information shock of the law's passage. To the extent that information spillovers from early- to late-exposed districts exist, they would bias our estimates toward zero.

We hypothesize that firsthand experience with a policy can change political attitudes by revealing its actual effects. When voters or affected groups directly observe a policy's consequences, initial opposition driven by uncertainty or perceived losses may weaken or reverse. Three pieces of evidence support this hypothesis.

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, indicated that 51% of these articles portrayed the law negatively, 24% neutrally, and 25% positively (Appendix Figure A10).

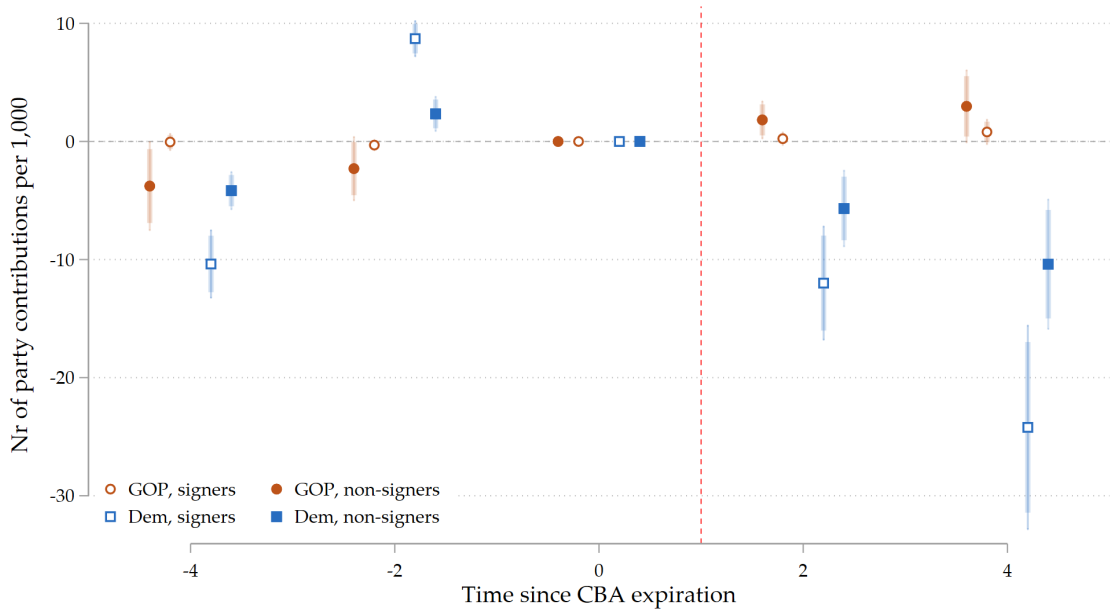
Figure 5: Electoral Effects of Wisconsin's Act 10 by 2010 GOP Vote Share: Event Study Estimates



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 wards below the median of the state distribution of the 2010 gubernatorial GOP vote share, and the circles show estimates for wards above the median. Observations are weighted by the number of people aged 18 and above in each district. Standard errors are clustered at the district level.

Second, the positive electoral impacts of exposure to Act 10 were most pronounced in districts least likely to support the legislation ex ante: those 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 pp in districts with a 2010 share below the median, while it did not change in the rest of the state. We confirm this result when we allow the effect of exposure to Act 10 to vary linearly with the 2010 GOP vote share (Appendix Table A2). We do not find evidence of significant effects on voter turnout, either overall or by ex ante partisan support (see Section 6.4 for additional details). This suggests that the reform persuaded voters who were not already strong supporters, rather than merely energizing existing supporters.

Figure 6: Effects on Campaign Contributions to Political Parties for Petition Signers and Non-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 the number of people aged 18 and above in each district. Standard errors are clustered at the district level.

Our third piece of evidence 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 strong 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 seven-fold decline, compared with a four-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 direct exposure to its real consequences.

6.4 Voter Turnout

A potential driver of the rise in GOP votes after Act 10 is a change in voter turnout due to a decrease in Democratic voter mobilization. The state of Wisconsin, which allows same-day voter registration, defines turnout as the number of votes cast divided by each ward's adult population (age 18 and over). We adopt this definition and estimate the effect of exposure to Act 10 on turnout using equation (2). Our findings suggest that turnout played little role in the political effects documented above.

Table 5: Effects of Wisconsin's Act 10 on Voter Turnout: Pooled Event-Study Estimates

		2010 GOP share		Pol. active union		Union strength	
	Full sample	Low	High	Yes	No	Strong	Weak
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Exposed	0.008	-0.007	0.001	0.003	0.005	-0.015*	0.012
	(0.006)	(0.010)	(0.006)	(0.014)	(0.008)	(0.008)	(0.008)
Ward FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var. control	0.444	0.431	0.464	0.420	0.453	0.425	0.450
N	24588	12296	12233	5064	19524	4882	19706
Clusters (districts)	236	175	199	16	220	24	212
R-squared	0.76	0.78	0.71	0.83	0.72	0.83	0.73

Notes: The dependent variable is voter turnout, defined as the number of votes cast divided by the population over 18 in each ward. We show estimates and standard errors of the parameter β^{post} in equation (2). Column 1 shows estimates on the full sample. Column 2 shows estimates for districts with a GOP share in the 2010 gubernatorial election below the state median; column 3 shows estimates for districts above the median. Column 4 shows estimates for districts with politically active unions, i.e., those that contributed to political races prior to 2011; column 5 shows estimates for districts with non-active unions. Column 6 shows estimates for districts with strong unions, defined as those having union revenues per member above the state median in 2010-2011; and column 7 shows estimates for districts with weak unions (with revenues per member below the state median or missing). All specifications control for ward and year fixed effects. Observations are weighted by the number of people aged 18 and above in each district. Standard errors in parentheses are clustered at the district level. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.

The estimated β^{post} is a statistically insignificant 0.8 pp (Table 5, column 1), and the 95% confidence interval rules out effects larger than 2 pp or smaller than –0.4 pp. Since Wisconsin voter registration files do not track party affiliation, we cannot directly examine changes in the composition of voters. However, the effects appear similar in wards with ex ante high vs. low support for the GOP (proxied by each ward's 2010 GOP vote share; Table 5, columns 2-3). These findings do not suggest that CBA expirations hindered unions' mobilization efforts, nor that they altered the composition of the electorate (Spenkuch and Toniatti, 2018). This pattern also suggests that policy reform may be more effective at political persuasion than campaigning, which has been found to have zero effect on vote choices in American elections in general (Kalla and Broockman, 2018) and in Wisconsin around the time of our study (Bailey et al., 2016).

6.5 Spillovers onto Federal 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 onto other political races, such as federal presidential, House, or Senate elections. We test this formally using the same event-study specification outlined in equations (1), using GOP vote shares in these races as the outcome. Although the results tend to be noisy, we generally do not observe the same positive effects of exposure to Act 10 as in gubernatorial races (Appendix Figure A11). These findings suggest that the political effects of Act 10 were mostly confined to gubernatorial races.

6.6 Robustness

We probe the robustness of our results to different empirical choices and potential violations of our identifying assumptions.

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. If anything, our estimates become slightly larger (see Table 6, columns 3-4, for the pooled event study and Appendix Table A3, columns 3-4, for the difference-in-differences).

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

	Controlling for time trends		Ignoring extensions		Excluding Milwaukee		Only fully aligned wards	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exposed	0.012**	0.009*	0.038***	0.038***	0.020**	0.020**	0.026***	0.026***
	(0.005)	(0.005)	(0.013)	(0.013)	(0.008)	(0.008)	(0.007)	(0.007)
District FE	Yes	No	Yes	No	No	No	No	No
Ward FE	No	Yes	No	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cov. time trends	Yes	Yes	No	No	No	No	No	No
Mean dep. var. ctrl	0.465	0.465	0.465	0.465	0.489	0.489	0.438	0.438
rk F-stat	24439	24438	24542	24538	22927	22923	14336	14333
N	235	235	236	236	235	235	231	231
Clusters (districts)	0.96	0.97	0.75	0.95	0.78	0.95	0.78	0.95

Notes: The dependent variable is the share of GOP votes in gubernatorial elections in each ward and year. We show estimates and standard errors of the parameter β^{post} in equation (2). 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 control for pre-existing level of covariates (shares of teachers with 3 or fewer and with more than 20 years of experience; teacher turnover; share of FRPL students; average standardized test scores in 2009-10; the share of households with children younger than 18; vote shares for GOP candidates in the 2008 Presidential election and in the 2010 gubernatorial election; number of political contributions to the Democratic party and to the GOP prior to 2011 per 1,000 people; an indicator for whether the district's union made political contributions prior to 2011; and union revenues per member prior to 2011) interacted with linear year trends. Columns 3 and 4 are estimated considering only original CBA expiration dates and ignoring extensions to construct the *Exposed* variable. Columns 4 and 6 are estimated on the sample of districts that excludes Milwaukee. Columns 7 and 8 are estimated on the subsample of wards that do not contain district boundaries. Observations are weighted by the number of people aged 18 and above in each district. Standard errors in parentheses are clustered at the district level. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.

Excluding Milwaukee Estimates also remain robust when we exclude Milwaukee Public Schools, the largest district in the state and an outlier in terms of enrollment (see Figure 3, square series; columns 5-6 of Table 6 for the pooled event study; and columns 5-6 of Appendix Table A3 for the difference-in-differences).

Focusing on wards fully aligned with districts In our main specification, we assign each ward to a district (and hence to a treatment status) based on the district in which its centroid falls. Wards

that span district boundaries would then be assigned a treatment status that does not correspond precisely to what all the voters living in the ward experienced. This could add measurement error to our explanatory variable of interest, biasing its estimate toward zero. To assess this possibility, in columns 7-8 of Table 6 and columns 7-8 of Appendix Table A3 we re-estimate our main event-study and difference-in-differences models on the subsample of 2,938 wards (56% of the total) that are completely circumscribed within a district. Estimates are unchanged.

Non-linear differences in pre-trends To assess the importance of violations to the parallel pre-trends assumption, in Appendix Figure A12 (panel (a)) we implement the test proposed by Rambachan and Roth (2023). We compare 95% confidence intervals of OLS estimates of β_k in equation (1), averaged for $k \in [1, 4]$, with estimates that allow for differential trends that evolve smoothly over time with a change in slope equal to M times the maximum change in trend slopes observed in the pre-treatment period. Estimates remain statistically significant for $M \in [0, 1]$.

7 Demobilization: The Role of Teacher Unions

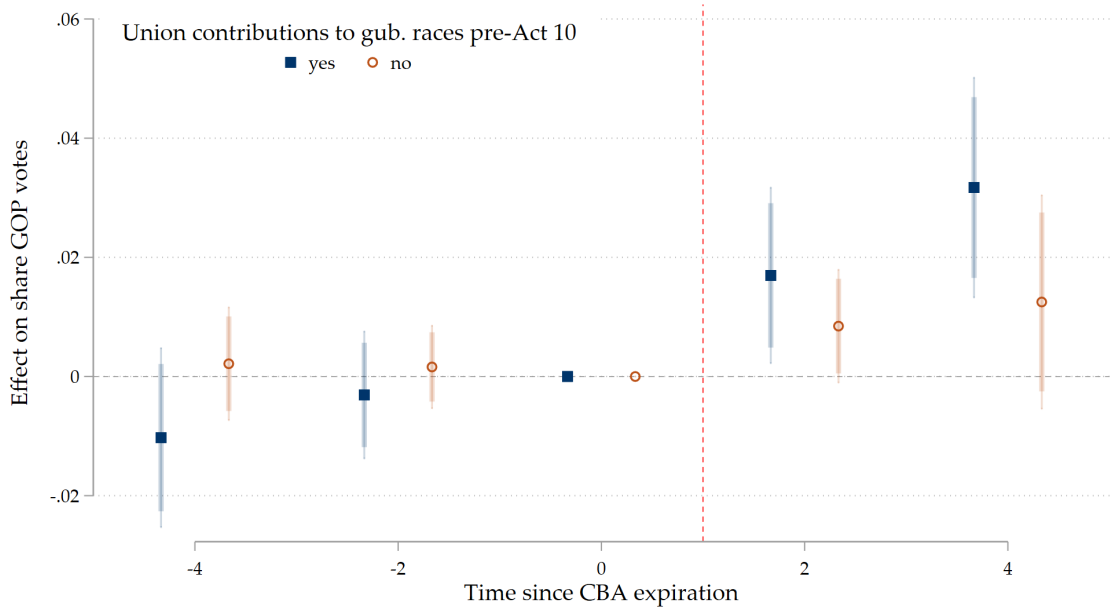
Our results thus far indicate that Act 10, although controversial, was a political win for its proponent. We now turn to the potential channels driving these positive effects. The first we consider is a demobilization channel, driven by the decline in union power. Unions have played a key role in shaping both policy and political outcomes, particularly in 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). It is therefore plausible that a reform such as Act 10, which curtailed unions' power, reduced unions' ability to mobilize voters and shape political preferences, thereby benefiting the GOP. We test this hypothesis here.

7.1 Unions' Political Participation and GOP Votes

In Section 5, we showed that unions' political participation — proxied by campaign contributions — declined following the passage of Act 10. However, only 11% of districts in our sample had unions that made contributions pre-reform (all of which went to the Democratic Party). To understand whether the decline in unions' political activity contributed to the GOP's electoral gains after Act 10, we test for differential effects of exposure to Act 10 on voter turnout and GOP vote share be-

tween districts where unions were politically active pre-reform and in those where unions were not active. We find no differential effects on voter turnout. Estimates of equation (2) with voter turnout

Figure 7: Electoral Effects by Ex Ante Union Campaign Contributions



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 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. Observations are weighted by the number of people aged 18 and above in each district. Standard errors are clustered at the district level.

as the outcome variable yields small and statistically indistinguishable effects of exposure to Act 10, both in districts with politically active unions (estimate of β^{post} equal to 0.003, p-value = 0.84, Table 5, column 4) and in those with non-active unions (estimate of β^{post} equal to 0.005, p-value = 0.54, column 5).

However, we find that the effects of exposure to Act 10 on GOP vote shares were larger in places with politically active unions. Estimates of equation (1) indicate that, in these districts, the GOP vote share increased by 3.2 pp four years after a CBA expiration (Figure 7, square series), whereas it increased by a smaller 1.3 pp in districts with non-active unions (circle series). This pattern suggests that unions may have played a mediating role in the effects of Act 10 on electoral outcomes through channels other than voter participation.

Table 7: Electoral Effects of Wisconsin's Act 10: Pooled Event-Study Estimates, by Ex Ante Union Revenues

	Union strength			
	Strong		Weak	
	(1)	(2)	(3)	(4)
Exposed	0.022**	0.022**	0.011**	0.011**
	(0.009)	(0.009)	(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	0.341	0.341	0.505	0.505
N	4882	4882	19660	19656
Clusters (districts)	24	24	212	212
R-squared	0.51	0.95	0.76	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 errors of the parameter β^{post} in equation (2). Columns 1 and 2 show estimates for wards in districts whose unions had revenues above the state median in 2011; columns 3 and 4 show estimates for wards in districts with union revenues below the state median or missing. Odd columns control for district and year fixed effects, and even columns control for ward and year fixed effects. Observations are weighted by the number of people aged 18 and above in each district. Standard errors in parentheses are clustered at the district level. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.

7.2 Union Revenues and GOP Votes

One of the ways in which Act 10 weakened unions was by prohibiting the automatic collection of dues from employees' paychecks, thereby limiting their ability to raise revenue. As a result, union revenues per teacher fell following a CBA expiration, particularly 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 revenue decline in districts with ex ante higher revenues suggests a larger loss of union power in these districts. To further assess how the decline in union power influenced the political effects of the reform, we split our sample based on whether a district had an ex ante strong union, defined as having 2011 revenues per member above the state median, or a weak union, defined symmetrically. We consider districts with missing revenues to have weak unions; all of our results are robust to this choice.

Our results reveal small differences in the effects on turnout across these two groups of districts.

Estimates of equation (2) indicate that turnout decreased slightly in districts with strong unions (with an estimate of β^{post} equal to -0.015, or 3.5% of the mean; Table 5, column 6, significant at 10%) whereas it remained stable in districts with weak unions (estimate of β^{post} equal to 0.012, column 7, p-value = 0.15). As before, we find starker differences in the effects on GOP vote share. Districts with strong unions saw a 2.2 pp increase in the GOP vote share following the implementation of Act 10 (significant at 5%, Table 7, columns 1-2); districts with weak unions experienced a smaller increase of 1.1 pp (significant at 5%, columns 3-4).

Taken together, these results indicate that the decline in union power contributed to the electoral effects of Act 10 primarily by shifting voters' partisan choices, rather than by demobilizing turnout. Notably, the decline in turnout in districts with strong unions is too small to fully account for the increase in the GOP vote share. Considering an average ward population size of 750 and making the (extreme) assumption that the entire decline in turnout were driven by Democrats, the implied GOP vote share increase would be at most 2 pp.[18] In districts with weaker unions, there was no reduction in turnout, yet the GOP vote share increased. Thus, while reduced union power appears to have affected vote choice, reduced mobilization alone cannot explain the magnitude of the GOP's electoral gains. We explore an additional mechanism next.

8 Persuasion: Benefits of Act 10

The evidence so far shows limited support for a demobilization channel. We now test for an alternative explanation for the positive effects of exposure to Act 10 on GOP votes: the persuasion channel. This hypothesis, rooted in retrospective voting theory (Fiorina, 1978; Healy and Malhotra, 2013), posits that citizens shift their political attitudes — and thus cast votes — based on their perceived and expected gains from policies implemented by the government. We thus examine how the law's electoral effects in each district relate to the real-world impacts of Act 10 on the district's constituents. In particular, we focus on the reform's consequences for teachers, students and their families, and school districts.

[18] In our data, pre-reform turnout is 0.44 on average and the pre-reform GOP vote share was 0.56. Assuming an average ward population of 750, these numbers imply an average GOP vote count of $0.440\times\ 0.56\times 750=185$. Assuming this vote count did not change post-reform, a turnout of $0.44-0.015=0.425$ post-reform implies a GOP vote share of $185/(750\times 0.425)=0.58$, or 2 pp higher than in the pre-reform period.

8.1 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 were related to the gains and losses experienced by teachers, we test whether districts with varying shares of teacher "winners" and "losers" exhibited 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.

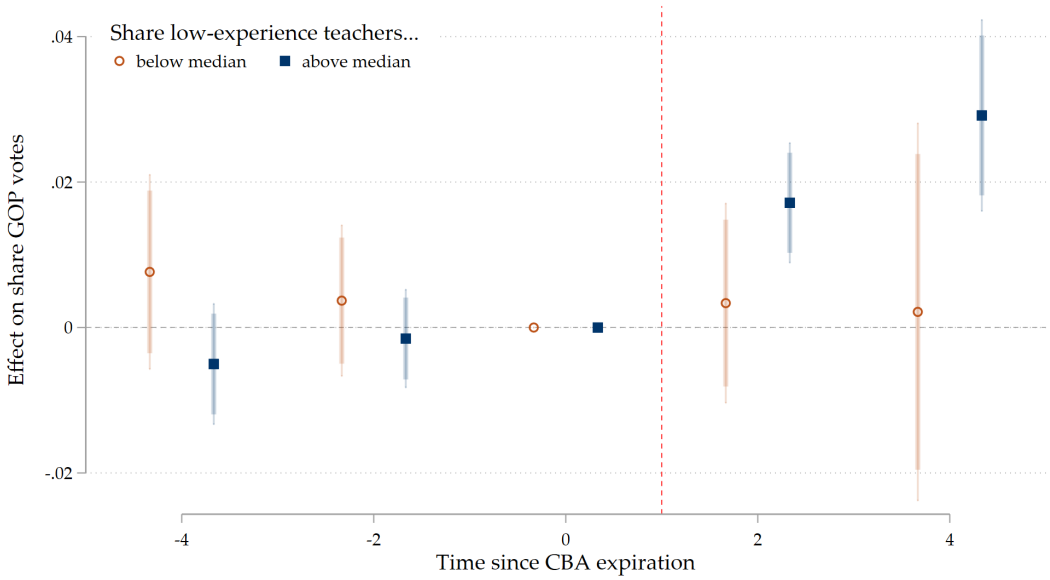
Our estimates indicate that the increase in the GOP vote share in gubernatorial elections was driven entirely by districts with a high share of winners. Four years after a CBA expiration, districts with a share of winners above the state median saw the GOP vote share rise by 3 pp (Figure 8, panel (a), square series), while districts with a share of winners below the median experienced no significant change (hollow circle series). This finding remains robust when we define winners as teachers aged 27 or younger (Appendix Figure A13). The result is also robust to non-linear deviations from parallel pre-trends (Appendix Figure A12, panels (b) and (c)).

Panel (a) of Table 8 (columns 1-3) further confirms this result by estimating equation (2) by quartile of the share of winners. 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.

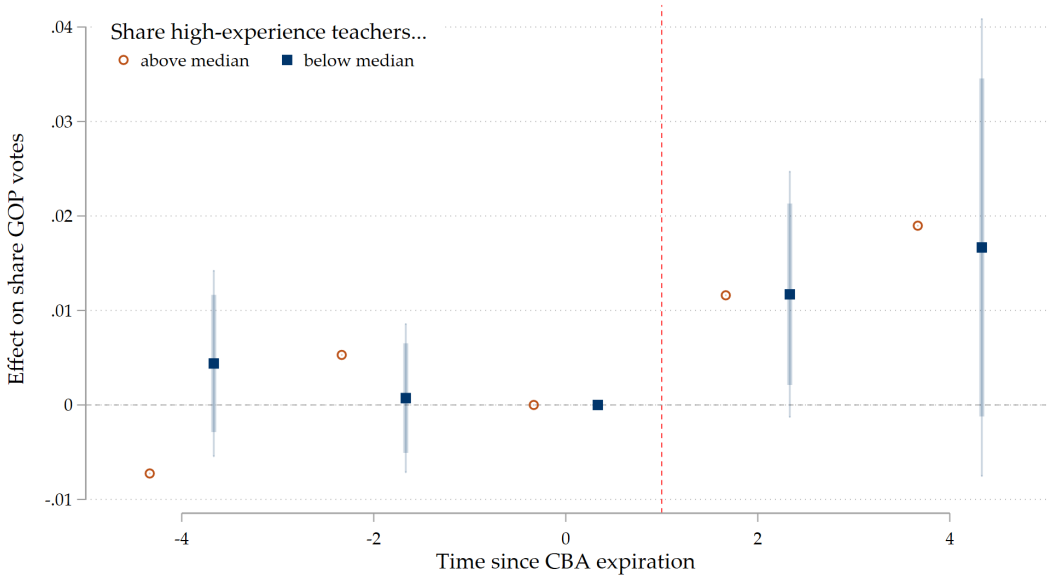
Next, we test whether the effects of the reform were also concentrated in districts with a low share of losers. Unsurprisingly, the shares of winners and losers are negatively correlated (Appendix

Figure 8: Electoral Effects by Share of Low- and High-Experience Teachers in the 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 (≤ 3 years) teachers above the state median and the circles show estimates for districts with a share below the median. In panel (b), the squares show estimates for the subsample of districts in with a 2010-11 share of high-experience (≥ 21 years) teachers below the state median and the circles show estimates for districts with a share above the median. Observations are weighted by the number of people aged 18 and above in each district. Standard errors are clustered at the district level.

Table 8: Electoral Effects of Wisconsin's Act 10: Pooled Event-Study Estimates, 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 errors of the parameter β^{post} in equation (2). 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. Observations are weighted by the number of people aged 18 and above in each district. Standard errors in parentheses are clustered at the district level. * ≤ 0.1; ** ≤ 0.05; *** ≤ 0.01.

Figure A14); however, the correlation is quite small at -0.3. Event-study estimates indicate that the increase in the GOP vote share was similar in districts with a share of losers below and above the median (Figure 8, panel (b)). Splitting the sample by quartile of the share of losers reveals slightly larger positive effects in the bottom quartile (Table 8, 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^{post} remain positive even in districts in the top quartile (Table 8, columns 7 and 8).

8.2 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 have been larger in areas where voters had 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 3.1 pp in districts with a share of households with school-age children above the state median (Figure 9, panel (a), square series). By contrast, districts with a share below the median experienced a smaller increase, equal to 1.6 pp (circle series). We confirm this result in columns 1-4 of Table 9, where we allow the impact of exposure to Act 10 to vary by quartile of the share of households with school-age children.[19]

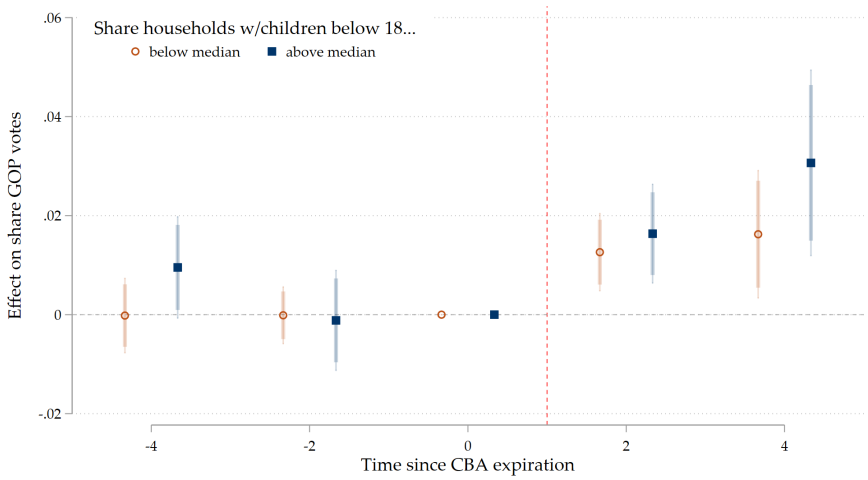
Next, we examine whether the GOP's political gains were larger in districts with a higher proportion of FRPL students, who benefited more from the reform (as shown in Figure 2, panel (b)). Four years following a CBA expiration, districts with a share of FRPL students above the state median saw the GOP vote share rise by 2.0 pp (Figure 9, panel (b), square series), whereas districts below the median experienced a smaller and imprecisely estimated 1.2 pp increase (circle series). In columns 5-8 of Table 9 we confirm this result by allowing the impact of Act 10 to vary by quartile of the FRPL student share.[20]

[19]The effect is robust to non-linear deviations from parallel pre-trends with slopes up to 0.2 times the maximum slope change in the pre-treatment period (Appendix Figure A12, panels (d) and (e)).

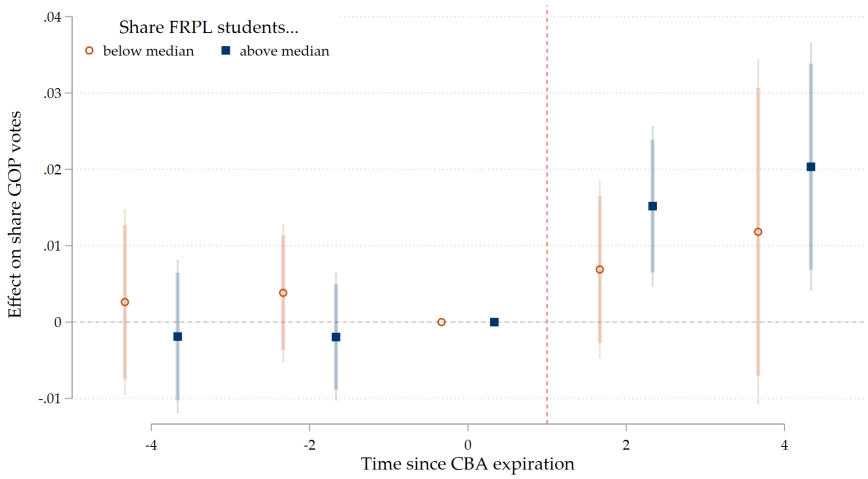
[20]The effect is also robust to deviations from parallel pre-trends (Appendix Figure A12, panels (f) and (g)).

Figure 9: Electoral Effects by Share of Households with School-Age Children and Share of FRPL Students in the District

(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. Observations are weighted by the number of people aged 18 and above in each district. Standard errors are clustered at the district level.

Lastly, Table A4 shows 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

Table 9: Electoral Effects of Wisconsin's Act 10: Pooled Event Study Estimates, 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	0.027***	0.022**	0.033**		0.008	0.013*	0.029***	
	(0.004)	(0.009)	(0.013)		(0.006)	(0.008)	(0.007)	
Exposed * Q1				0.024***				0.008
				(0.008)				(0.007)
Exposed * Q2				0.016**				0.008
				(0.008)				(0.015)
Exposed * Q3				0.021**				0.013
				(0.009)				(0.012)
Exposed * Q4				0.043**				0.051***
				(0.021)				(0.016)
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. ctrl	0.444	0.489	0.439	0.465	0.536	0.501	0.364	0.465
N	6088	12279	6171	24455	6104	12280	6154	24538
Clusters (districts)	204	234	208	236	62	131	43	236
R-squared	0.95	0.94	0.96	0.96	0.96	0.90	0.95	0.96

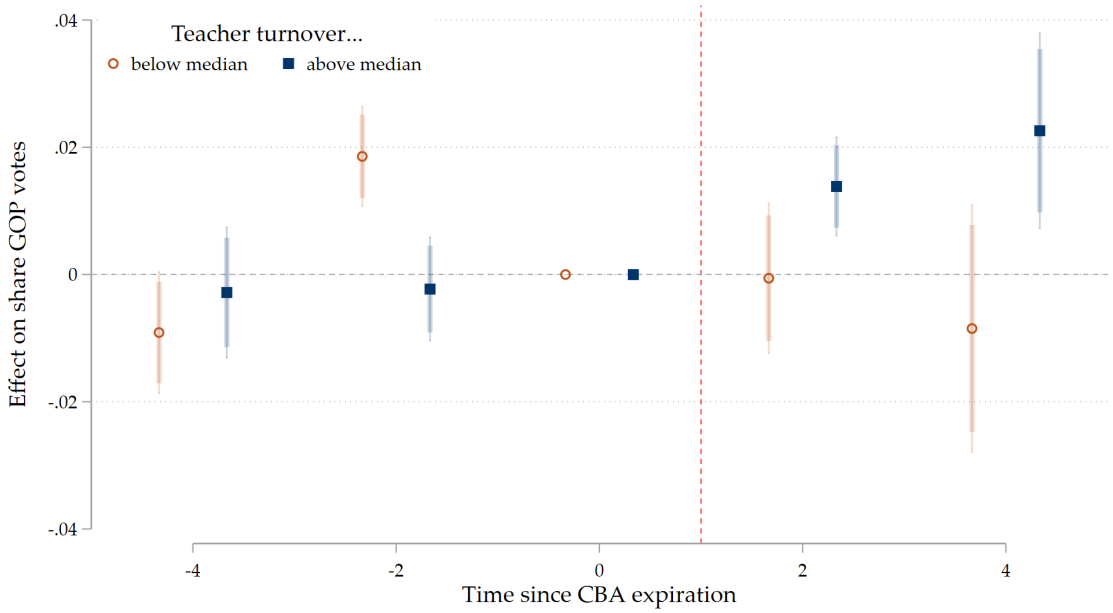
Notes: The dependent variable is the share of GOP votes in gubernatorial elections in each ward and year. We show estimates and standard errors of the parameter β^{post} in equation (2). In columns 1-3, we split the sample by the quartile of the share of households with children (aged 18 and younger) 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. Observations are weighted by the number of people aged 18 and above in each district. Standard errors in parentheses are clustered at the district level. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.

the GOP governor's electoral success following Act 10.

8.3 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 par-

Figure 10: Electoral Effects 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. Observations are weighted by the number of people aged 18 and above in each district. Standard errors are clustered at the district level.

ticularly 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 10 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 2.3 pp increase in the GOP vote share (square series), whereas districts below the median showed no significant change (circle series).[21] We confirm this finding in Table 10, 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.

[21] This finding is robust to non-linear deviations from parallel pre-trends (Appendix Figure A12, panels (h) and (i)).

Table 10: Electoral Effects of Wisconsin's Act 10: Pooled Event Study Estimates, by Ex Ante Teacher Turnover

	By quartile of teacher turnover			
	Q1	Q2-Q3	Q4	All
	(1)	(2)	(3)	(4)
Exposed	-0.002	0.029***	0.043***	
	(0.010)	(0.009)	(0.014)	
Exposed * Q1				-0.005
				(0.008)
Exposed * Q2				0.016
				(0.014)
Exposed * Q3				0.020*
				(0.011)
Exposed * Q4				0.052***
				(0.019)
Ward FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No
Year * qtile FE	No	No	No	Yes
Mean dep. var. control	0.548	0.468	0.365	0.465
N	6065	12514	5959	24463
Clusters (districts)	62	112	62	235
R-squared	0.91	0.95	0.96	0.93

Notes: The dependent variable is the share of GOP votes in gubernatorial elections in each ward and year. We show estimates and standard errors of the parameter β^{post} in equation (2). 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. 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 4 controls for ward and quartile-year fixed effects. Observations are weighted by the number of people aged 18 and above in each district. Standard errors in parentheses are clustered at the district level. * $\leq$ 0.1; ** $\leq$ 0.05; *** $\leq$ 0.01.

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. Although the passage of the law prompted protests and negative press coverage, experiencing the law made voters more likely to vote for its champion, Republican governor Scott Walker. It also reduced individual campaign contributions to Democratic gubernatorial candidates,

especially among teachers and signers of the petition to recall the governor, indicating that this relative movement toward the GOP was evident even among highly engaged segments of the electorate who were likely to oppose Act 10 ex ante. Heterogeneity analyses show that the 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. The law's electoral effects were linked to its weakening of public sector unions, though not through the channel of voter mobilization.

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 a turnout rate equal to that of the electorate in exposed districts in the 2012 election (63%), the vote share effects we measure could be completely explained by the treatment moving the averaged inexperienced teacher to persuade five people (potentially including herself) to vote for Walker. Exposed districts also contained 457,012 parents of school-age children in 2012. Assuming the same turnout rate of 63%, the vote share effects we measure could be completely explained by the treatment inducing 3% of these parents to vote for Walker.

Overall, our findings highlight how even politically risky policies can become electoral assets when they succeed in improving ordinary people's experience of public services. They also contravene some of the conventional wisdom surrounding the political economy of public service reform. Exposure to Act 10 increased votes for the GOP even though it was part of an effort to dramatically *reduce* public spending to bridge a budget gap — consistent with a certain degree of voter farsightedness (Carreri and Martinez, 2021). The reform had significant positive electoral effects even though it featured less direct visibility or inherent attributability than other vote-winning government actions such as capital infrastructure, direct cash transfers, and punishing corruption (Mani and Mukand, 2007; Manacorda et al., 2011; Ferraz et al., 2012). This shows that school reform can be a salient issue for voters and a winning issue for politicians (Dias and Ferraz, 2019; Cox et al., 2024).

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 low, 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 (seeAppendix B for details).

Appendix Figure A15 shows 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. 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.

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