

Effects of Wisconsin Act 10 on Unions, Wages, and Benefits

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2025-01-12

Abstract

This paper examines the long-run effects of Wisconsin Act 10 (2011)-a law that restricted collective bargaining rights for most public sector workers-on union membership, wages, and fringe benefits. Using three decades of Current Population Survey data (1994-2023), the analysis employs several empirical strategies, including difference-in-differences (DD), difference-in-difference-in-differences (DDD), event studies, sensitivity checks ("honest" DD), and synthetic control methods.

The results consistently indicate a substantial decline in union membership, with estimated reductions ranging from 25% to 31%, underscoring the law's effectiveness in weakening public sector unions. Wages also showed significant negative effects, with hourly earnings declining by about 3% to 5% and weekly earnings declining by 5% to 8%. Non-wage benefits show a significant decline in employer-sponsored health insurance contributions of 41% to 59%. Robustness checks confirm these findings while revealing only modest or negligible shifts in the overall public sector employment share.

By integrating multiple identification strategies and rich microdata, this paper provides evidence that restrictions on collective bargaining can substantially reshape labor market structures. The results are relevant for policymakers considering similar reforms and for researchers studying the interaction between labor law, union strength, and employee compensation in the public sector.

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1 Introduction

Since the late 1950s, Wisconsin has played a pivotal role in shaping public sector labor relations in the United States, most notably as the first state to enact Duty-to-Bargain (DTB) legislation. This legislation established collective bargaining rights and laid the foundation for stable labor relations between public employers and employee unions. However, the political shift following the 2010 midterm elections led to the passage of Wisconsin Act 10 (2011). Commonly referred to as the "Budget Repair Bill", Act 10 significantly reshaped Wisconsin's public sector labor laws - limiting collective bargaining to base wages, tightening union certification requirements, imposing caps on wage increases, and increasing employee contributions to pension and health plans. In late 2024, a major court decision challenged the constitutionality of Wisconsin Act 10, invalidating significant portions of the law. This development has renewed debates over public sector labor reforms and highlighted the lasting political and economic impact of Act 10 since its enactment in 2011.

Understanding how these profound legal changes have affected union strength and worker well-being is essential for researchers and policymakers alike. Existing studies of collective bargaining laws have documented their effects on outcomes such as wages, employee turnover, and student achievement (Baron, 2018; Biasi, 2021; Brunner & Ju, 2019; Freeman & Valletta, 1988; Hirsch et al., 2011; Roth, 2017). Nevertheless, much of the early evidence on Act 10 has focused on Wisconsin public school teachers or relied on within-state empirical approaches. In contrast, this study provides a broader, long-run perspective using multiple identification strategies applied to a broader subset of public employees (excluding protective services).

By examining the effects of Act 10 over nearly three decades, this paper provides a comprehensive assessment of its impact on unionization, wages, weekly earnings, health insurance benefits, and pension participation. The empirical analysis uses the Current Population Survey (CPS), employing both the Outgoing Rotation Group (ORG) and the Annual Social and Economic Supplement (ASEC) microdata to capture union coverage, wages, and fringe benefits. The main research strategies consist of difference-in-differences (DD), difference-in-difference-in-differences (DDD), event studies, "honest" DD sensitivity checks, and synthetic control methods - each of which adds robustness to the identification of causal effects.

The remainder of this paper is organized as follows:

Section 2: Background. Provides a brief history of public sector collective bargaining in Wisconsin, highlighting the early adoption of Duty-to-Bargain laws in 1959 and explaining the key features of Act 10, including its legal and political context. A review of previous studies situates Act 10 within the broader literature on public sector labor law and provides a foundation for the current research.

Section 3: Data. Describes the construction and sources of the dataset. The analysis combines extracts from the CPS Outgoing Rotation Group (ORG) - covering union membership and wages - with the Annual Social and Economic Supplement (ASEC), which provides detailed information on health insurance and pension variables. Sampling decisions, variable definitions and methodological details (e.g. treatment of missing data and sample restrictions) are also detailed.

Section 4: Empirical Strategy. Presents the identification framework. First, I present the conventional difference-in-differences (DD) approach, extending it with event-study specifications and "honest" DD techniques to address potential violations of the parallel trends assumption. Second, I present a triple-difference (DDD) design that exploits variation across both states and sectors (public versus private). Finally, I describe the synthetic control methodology used to create a data-driven counterfactual for Wisconsin, thereby providing a complementary view of the effects of Act 10.

Section 5: Results. Summarizes the main findings on union membership, wages, weekly earnings, and fringe benefits (health insurance and pensions). The section highlights how the results from DD, DDD, event studies, and synthetic control analyses are consistent, the extent to which Act 10 has reshaped Wisconsin's public sector labor market, and the robustness of these conclusions to multiple sensitivity checks.

Section 6: Discussion. Interprets the empirical results in light of theoretical predictions and prior empirical literature. The discussion addresses data and identification limitations, as well as broader policy implications for governments considering reforms similar to Act 10.

Section 7: Conclusion. The final section concludes by discussing the long-term significance of Act 10. This final section also outlines directions for future research.

Using a variety of methodological techniques and extensive CPS data, this study offers new insights into how large-scale legislative reforms can reshape labor market structures, particularly in the public sector. As states continue to reconsider labor policies in times of fiscal stress, the Wisconsin experience remains an important case study of the long-term effects of restricting collective bargaining rights for public employees.

2 Background

2.1 Wisconsin Act 10

In 1959, Wisconsin was a pioneer in labor relations, becoming the first state to enact a Duty-to-Bargain (DTB) law requiring public employers to bargain in good faith with employee unions over wages, hours, and other terms and conditions of employment. This framework has created a robust collective bargaining environment for public employees, fostering stable labor relations and enabling unions to effectively represent workers' interests.

However, the 2010 Republican wave, marked by a shift toward conservative governance, set the stage for significant labor policy reforms. Governor Scott Walker, elected in 2010, pushed a legislative agenda aimed at reducing the influence of public sector unions, addressing budget constraints, and increasing government efficiency.

On June 29, 2011, Wisconsin enacted the Budget Repair Bill, commonly referred to as Act 10. This comprehensive legislation made several significant changes to the state's labor laws, fundamentally altering the dynamics of public sector employment. Key provisions of Act 10 include:

1. **Restricted Scope of Collective Bargaining:** Act 10 limited collective bargaining to base wages. This restriction excluded bargaining over benefits, pensions, and other nonwage compensation.
2. **Caps on Wage Increases:** The legislation imposed a cap on base wage increases, allowing increases only at or below the rate of inflation. This measure was intended to control public sector wage growth in line with economic conditions.
3. **Right-to-Work Measures:** Act 10 implemented Right-to-Work provisions, prohibiting unions from requiring the deduction of union dues from workers' paychecks. It also allowed workers to choose not to pay agency fees while remaining part of the bargaining unit, weakening the financial stability of unions.
4. **Shortened Collective Bargaining Agreements (CBAs):** The law mandated that CBAs be limited to one-year terms, significantly reducing the length of negotiated agreements and increasing the frequency of bargaining cycles.
5. **Certification Election Reform:** Prior to Act 10, once a union was certified as the bargaining representative, it kept that status unless a successful petition initiated a

decertification election. Act 10 required annual certification elections for unions to maintain their legal status. If a majority of bargaining unit members voted against retaining the union, it would be decertified.

6. **Increased Employee Contributions to Benefits:** Act 10 required state employees to contribute 50% of their annual pension payments and at least 12.6% of their average annual health insurance premiums. Prior to this mandate, state employees typically contributed about 6% to health insurance and minimal contributions to pensions.

These legislative changes effectively restricted the collective bargaining rights of public employee unions in Wisconsin, with the exception of certain public safety unions, such as those representing police officers, firefighters, and sheriff’s deputies, which were exempted. By limiting the scope of bargaining, imposing financial constraints on unions, and increasing employee contributions to benefits, Act 10 significantly reduced the bargaining power and influence of most public employee unions.

The implementation of Act 10 coincided with the expiration of existing CBAs for the majority of public employees, mostly in the summer of 2011. This timing ensured that the new restrictions were enforced uniformly across the state, as pre-existing agreements were not renewed under the new legal framework. Only a small fraction of districts with CBAs extending beyond mid-2011 were subject to delayed implementation (Roth, 2017).

2.2 Previous Research

A considerable amount of literature has examined the effects of public sector collective bargaining laws on unionization, wages, and related employment outcomes. A central theme is understanding how legal frameworks, which often vary across U.S. states, affect public sector unionization, bargaining power, compensation, and labor market dynamics. Several key papers are particularly relevant to this study.

Freeman and Valletta (1988) develops a theoretical model in which public sector labor laws influence union behavior by altering the allocation of union resources between wage bargaining and labor demand lobbying. Let $E = -qW + X + bRS$ be the log labor demand equation, where W is log wages, q is the (absolute) elasticity of labor demand, X measures the demand for services, and RS denotes the resources spent by the union on political activity. Union utility is modeled as $U = U(W, E)$, and bargaining outcomes

are characterized by $W = W(RB, L, S)$, where RB are resources allocated to bargaining, L is the legal environment, and S is supply side factors. The union's total resources satisfy $RB + RS = R$. By maximizing U subject to these constraints, the union allocates resources such that the marginal rate of substitution in utility equals the marginal costs and benefits of raising wages versus employment, satisfying the condition:

$$\frac{\partial U}{\partial W} \bigg/ \frac{\partial U}{\partial E} = q + bW'.$$

A more pro-union legal environment increases W' , allowing unions to secure wage gains at lower employment costs, thereby raising wages while reducing employment. Empirically, Freeman and Valletta (1988) find that state labor laws raise wages by about 6% and affect public sector employment by increasing unionized positions while reducing non-unionized ones.

Hirsch et al. (2011) use data from the Current Population Survey (CPS), the Schools and Staffing Survey (SASS), and historical censuses to estimate the effects of teacher CB laws and union coverage on wages and benefits. Their OLS estimates show a union wage gap of about 10%, with larger benefits advantages. However, instrumental variable (IV) estimates using historical state-level labor sentiment yield implausibly large effects. Further analysis of pre-law data suggests that about half of the measured gap predates the introduction of the CB, implying a modest causal union wage effect of about 5%.

Brunner and Ju (2019) using the 2005-2015 American Community Survey (ACS) data, they exploit policy discontinuities along state borders within a difference-in-differences (DD) framework. They find that mandatory CB laws raise public sector wages by about 5–8%.

Roth (2017) analyzes Wisconsin Department of Public Instruction (DPI) teacher-level panel data to assess the impact of Wisconsin's Act 10 on teacher turnover and student outcomes. The study documents a sharp increase in teacher turnover following Act 10 and finds that teachers who were encouraged to retire were relatively less effective, leading to small improvements in subsequent student outcomes.

Baron (2018) applies an event study design to Wisconsin high schools, taking advantage of the staggered timing of collective bargaining agreement expirations. The analysis finds significant declines in test scores of up to 20-30% of a standard deviation, particularly in math and science. These declines are attributed in part to reduced teacher pay (about 4% lower) and increased teacher turnover. In addition, he found no effect of the

policy on fringe benefits.

Biasi (2021) analyzes the adoption of merit pay systems in post-Act 10 flexible pay districts. Using a difference-in-differences approach with DPI microdata, the study finds that loosening salary rigidities attracted teachers with higher value-added and increased salary dispersion, particularly benefiting more effective teachers, which in turn led to improvements in student outcomes—contrary to the findings of Baron (2018). However, overall compensation declined relative to pre-reform policies.

This paper contributes to the literature by providing a long-run perspective from 1994 to 2023 on the effects of a major public sector collective bargaining reform, Wisconsin’s Act 10, using a broad sample of state and public employees (excluding protective service occupations) rather than focusing solely on teachers. Unlike prior work that relies primarily on within-state designs, this study implements difference-in-differences, difference-in-difference-in-differences, and synthetic control strategies with multiple donor states to ensure robust identification of changes in public sector unionization, wages, and benefits, including employer-sponsored health insurance contributions. The analysis provides a detailed description of how Act 10 affected unionization, wages, and fringe benefits such as employer-provided health insurance and pensions, as well as insights into public sector labor market employment. By including a broader range of public employees and employing a variety of empirical strategies, this study expands the evidence base on the long-term effects of changes in public sector labor law.

3 Data

This study uses microdata from the Current Population Survey (CPS), a monthly survey of U.S. households conducted jointly by the U.S. Census Bureau and the Bureau of Labor Statistics. Two key components of the CPS are used: the Outgoing Rotation Groups (ORG) extracts, accessed through the Economic Policy Institute’s (EPI) data service, and the Annual Social and Economic Supplement (ASEC) extracts, obtained from the Integrated Public Use Microdata Series (IPUMS). This combination provides comprehensive coverage of employment characteristics, wages, union representation, and fringe benefits over several decades. Importantly, both ORG and ASEC data are repeated cross-sections, meaning that the same individuals are not necessarily followed over time, but representative samples of the labor market are observed each year.

CPS ORG (Wage and Union Data) The ORG extracts from the EPI repository are used to analyze unionization and wages. These data provide variables on union membership and coverage, as well as detailed earnings information.

I use data from 1994 to 2023. Beginning in 1994, job classifications and hourly wage variables, including overtime, tips, and commissions, are consistently available, allowing for a consistent approach to identifying public employees and measuring compensation. The year 2023 is the last fully available year in the EPI CPS dataset, providing three decades of data before and after the implementation of Wisconsin Act 10 in 2011. Although the primary analyses use the entire 1994–2023 period, certain robustness checks restrict the sample to 2019 and 2015 to avoid confounding influences from the COVID-19 pandemic and particular confounding policies.

Key variables of interest in the CPS ORG data include

1. Union membership and representation: Indicators of whether a respondent is a union member or covered by a union contract, even if not a member.
2. Wages: Hourly earnings are measured both with and without overtime, tips, and commissions (OTC). Weekly earnings (with OTC) are also included to account for differences in employment patterns, such as shifts between part-time and full-time work or variations in hours worked.

CPS ASEC (Benefit Data) To examine the impact of Act 10 on benefits such as employer-sponsored health insurance and pension availability, I use CPS ASEC data from IPUMS. These data include annual measures of health insurance coverage and pension indicators. Employer contributions to health insurance are only available up to 2018, which sets the end point for the benefits analysis (1994–2018). Although pension plans are identified, direct measures of employer pension contributions are not available in the CPS or other open data sources at the individual or state level. Therefore, I focus primarily on employer health insurance contributions for fringe benefits.

Key variables of interest in the ASEC data include:

1. Employer-sponsored health insurance: Indicators of whether the individual was covered by an employment-based group health plan. Monetary amounts for employer contributions to health insurance are used to measure the generosity of fringe benefits.

2. Pension coverage: Indicators of whether a pension or retirement plan was available at the person’s longest job and whether the respondent was covered by the plan.

Both the ORG and ASEC datasets include demographic and socioeconomic characteristics such as age, sex, marital status, race, metropolitan residence, education level, and industry. These variables serve as controls and predictors to ensure that comparisons between treatment and control groups-and between pre- and post-periods-are not confounded by compositional changes in the workforce.

In addition, I restrict the sample in both datasets to employed individuals between the ages of 18 and 64. I exclude individuals who are working without pay. In the analysis and throughout the paper, individuals are classified as public employees if they work for state or local government, excluding protective service workers (e.g., police and firefighters) who were exempt from Act 10. The treatment group is defined as individuals in Wisconsin. The post period is defined as all years after 2011, when Act 10 was implemented. For all methods except DDD, I restrict the sample to public employees only.

The CPS ORG extracts have minimal missing data. For Wisconsin public employees, the missing data rate remains very low ($<0.5\%$), while the private sector has higher missing values rates ($\sim 10\%$). This may only affect the triple difference (DDD) analysis, as only this method uses the private sector as an additional control.

To handle missing values in the CPS ORG data, the hot-deck imputation method recommended by the U.S. Census Bureau is used. This approach replaces missing values with observations from similar individuals matched on specific criteria such as state, year, and public sector status. Hot-deck imputation preserves the distributional characteristics of the sample and reduces bias due to systematic missingness.

For the CPS ASEC data, variables related to health insurance and pensions have no missing values, eliminating the need for imputation in the analysis of non-wage benefits.

The combination of the CPS ORG and CPS ASEC datasets provides a rich, repeated cross-sectional view of union representation, wages, and fringe benefits among public employees in Wisconsin, as well as appropriate comparison groups. By ensuring consistent definitions, controlling for important individual-level covariates, and comprehensively addressing missing data, I can credibly assess the impact of Wisconsin Act 10 on public sector labor market outcomes.

4 Empirical Strategy

In this section, I present the empirical methodology used to estimate the causal effects of Wisconsin Act 10 on key labor market outcomes, including unionization, wages, weekly earnings, and fringe benefits. Three main identification strategies are employed: (1) difference-in-differences (DD) and related methods, (2) difference-in-difference-in-differences (DDD), and (3) synthetic control.

First, a basic difference-in-differences (DD) approach is implemented that compares the outcomes of Wisconsin public employees (treatment group) before and after policy implementation with the outcomes of a control group over the same time periods. This methodology relies on the standard assumption of parallel trends: that in the absence of Act 10, Wisconsin's public employees would have had similar outcome trends as the control units. To further support the credibility of the DD strategy, I also estimate event study models, which allow for a more flexible time structure and help to visually and statistically assess the validity of the parallel trends assumption. In addition, I use "honest" difference-in-differences techniques, which refine the inference by allowing for the possibility that the parallel trends assumption may be imperfectly satisfied.

Second, I use difference-in-difference-in-differences (DDD) estimation to strengthen identification. The DDD approach introduces an additional level of differencing by including private-sector workers (or other unaffected groups) within the same states, who serve as a "within-state placebo." In this way, the DDD estimation can adjust for any unobserved state-specific shocks that might confound the simple DD estimates. As with DD, the DDD strategy is accompanied by event study analyses to visualize dynamic treatment effects and validate assumptions.

Finally, I implement a synthetic control technique. This technique constructs a "synthetic" control for Wisconsin by selecting a weighted combination of other states that best replicate Wisconsin's pre-Act 10 outcome trajectories and characteristics. The synthetic control acts as a counterfactual, representing what would have happened in Wisconsin had Act 10 not been implemented. The synthetic control method generalizes the usual difference-in-differences approach by allowing for time-varying unobserved confounders. In this respect, the estimates of the policy intervention obtained by the synthetic control method are robust not only to time-invariant unobservables, but also to unobservable confounders that vary over time, provided that these confounders vary in a similar way after the intervention. In addition, the synthetic control method uses aggregate data rather

than individual-level data. This approach provides a transparent way to select comparison units and can be viewed as a generalization of the difference-in-differences strategy. It reduces the researcher's discretion in selecting controls and uses a data-driven process to form the comparison group, which increases the robustness of causal inference.

Below, I detail the methods and specifications for the DD, event study, "honest" DD, DDD, and synthetic control approaches. All of these analyses include demographic and socioeconomic covariates as controls and typically employ robust standard errors clustered at the state level.

4.1 Difference-in-Differences

To identify the causal effects of Wisconsin Act 10 on labor market outcomes such as unionization, wages, and benefits, I begin with a difference-in-differences (DD) research design. The DD framework compares the evolution of outcomes in a treated group to that of a control group before and after the policy is implemented, assuming that both groups would have followed parallel trends over time in the absence of the treatment.

First, consider a simplified two-period, two-group setting. Let Y_{its} denote the outcome of interest for individual i , in period t , and state s . Define $WI = 1$ if the observation is from Wisconsin (the treated state) and 0 otherwise; define $Post = 1$ for all periods after the policy is implemented and 0 otherwise. Suppose we have only one pre-treatment period, one post-treatment period, and an untreated control group U . The simple DD estimator can be written as:

$$\begin{aligned}\hat{\delta}_{WIU}^{2 \times 2} &= (E[Y_{WI} | Post = 1] - E[Y_{WI} | Post = 0]) - (E[Y_U | Post = 1] - E[Y_U | Post = 0]) \\ &= (E[Y_{WI}^1 | Post = 1] - E[Y_{WI}^0 | Post = 1]) + (E[Y_{WI}^0 | Post = 1] - E[Y_{WI}^0 | Post = 0]) \\ &\quad - (E[Y_U^0 | Post = 1] - E[Y_U^0 | Post = 0]) = \underbrace{(E[Y_{WI}^1 | Post = 1] - E[Y_{WI}^0 | Post = 1])}_{ATT} \\ &\quad + \underbrace{[(E[Y_{WI}^0 | Post = 1] - E[Y_{WI}^0 | Post = 0]) - (E[Y_U^0 | Post = 1] - E[Y_U^0 | Post = 0])}_{Non-parallel trends bias},\end{aligned}$$

where Y_{WI}^1 denotes the treated outcome for Wisconsin in the post-treatment period and Y_{WI}^0 , Y_U^0 denote the counterfactual untreated outcomes for Wisconsin and the control units, respectively. The parameter of interest, $\hat{\delta}_{WIU}^{2 \times 2}$, identifies the average treatment effect on the treated (ATT) if the parallel trends assumption holds. In other words, we

require that:

$$E[Y_{WI}^0 \mid \text{Post} = 1] - E[Y_{WI}^0 \mid \text{Post} = 0] = E[Y_U^0 \mid \text{Post} = 1] - E[Y_U^0 \mid \text{Post} = 0].$$

This assumption assumes that in the absence of the treatment, the treated and control units would have experienced the same changes in Y^0 over time. Since we never observe Y_{WI}^0 directly and must rely on the control units as a counterfactual, this assumption is intrinsically untestable.

In practice, our analysis spans several years and includes several control states rather than a single before and after period. We therefore use a more flexible regression specification with state and time fixed effects, as well as demographic and socioeconomic controls, to capture persistent state-level differences and common time shocks. A generalized multi-period DD model is:

$$Y_{its} = \alpha_s + \lambda_t + \delta(WI_s \times \text{Post}_t) + X_{its}\beta + \varepsilon_{its}, \quad (1)$$

where WI_s is an indicator for Wisconsin (essentially a dummy for treated units), Post_t is an indicator for post-Act 10 periods, α_s are state fixed effects, λ_t are time fixed effects (one of which is dropped due to multicollinearity with WI_s and Post_t , but I include them in the equation for visualization), X_{its} is a vector of observed covariates (including age, sex, education, marital status, race, metropolitan residence, and industry), and ε_{its} is an error term. Standard errors are clustered at the state level to account for serial correlation within states.

4.2 Event Study

The key identifying assumption of the DD approach, parallel trends, is untestable because it concerns the counterfactual trajectory of the treated units. However, we can provide supporting evidence by examining the data in several ways. First, we plot the raw outcome trends for both treated and control states, providing a transparent and intuitive check on whether their paths were similar before the policy. Although such a visual inspection is not definitive, it provides a useful starting point.

To further assess parallel trends, we estimate event study models that include treatment leads and lags. By introducing leads, we are effectively conducting placebo tests: if the policy had no effect before it was actually implemented, then any significant pre-

treatment coefficients would suggest that the treated and control units were not evolving in parallel even before the intervention. Conversely, if these pre-treatment leads are statistically insignificant, this is consistent with (but not conclusive evidence of) parallel trends. I note that failure to reject the null hypothesis for pretreatment trends is neither necessary nor sufficient to confirm parallel trends. In addition, such tests often suffer from low power and fail to detect subtle deviations.

Event study models relax the assumption of a constant treatment effect after the intervention, allowing the effect to vary dynamically over time. Defining $t = 0$ as the year of treatment, we let $\tau < 0$ denote pre-treatment leads and $\tau \geq 0$ denote post-treatment lags. A standard specification for event studies is

$$Y_{its} = \gamma_s + \lambda_t + \sum_{\tau=-q}^{-1} \gamma_{\tau} D_{s\tau} + \sum_{\tau=0}^m \delta_{\tau} D_{s\tau} + X_{its}\beta + \varepsilon_{its}, \quad (2)$$

where $D_{s\tau}$ is an indicator for being τ years relative to the treatment event. By plotting γ_{τ} for $\tau < 0$ and δ_{τ} for $\tau \geq 0$, along with their confidence intervals, we can visualize whether the outcome paths diverged before the intervention and how the treatment effects evolve after the intervention. If the pre-treatment coefficients are close to zero and statistically insignificant, this supports the plausibility of parallel trends. The observation of clear dynamic post-intervention effects can also inform our understanding of the timing and persistence of policy effects.

4.3 "Honest" Difference-in-Differences

While event-study analyses and the inclusion of demographic controls can increase confidence in the parallel trends assumption, it remains an untestable and potentially imperfect assumption. To further refine inference under possible violations of parallel trends, I implement an "honest" difference-in-differences approach. This method, introduced in methodological paper (Rambachan and Roth, 2023), provides robust inference by imposing additional formal constraints informed by the observed pretreatment trends.

The key idea behind "honest" DD is that pre-treatment deviations from parallel trends can be used to constrain the magnitude of possible post-treatment violations. Intuitively, if we let δ_t^H represent the deviation from parallel trends at time t , then observing these deviations before the intervention provides information about how large the deviations

could be after the policy is implemented. Formally, consider the set $\Delta^{RM}(\bar{M})$, defined as

$$\Delta^{RM}(\bar{M}) = \left\{ \delta^H : \forall t \geq 0, |\delta_{t+1}^H - \delta_t^H| \leq \bar{M} \cdot \max_{s < 0} |\delta_{s+1}^H - \delta_s^H| \right\}.$$

This set limits post-treatment deviations from parallel trends to no more than a factor \bar{M} larger than the largest pre-treatment deviation. The parameter \bar{M} , known as the relative magnitude bound, can be set based on prior knowledge or sensitivity analysis. A natural benchmark is $\bar{M} = 1$, which means that post-treatment deviations cannot exceed the largest pre-treatment deviation in absolute magnitude.

Under these restrictions, the "honest" DD procedure constructs confidence intervals that remain valid even if parallel trends fail by a limited amount. If the resulting "honest" confidence intervals for the treatment effect still exclude zero, we gain confidence in the robustness of our conclusions. Conversely, if small relaxations of the parallel trends cause our inference to become inconclusive, we learn that the results are sensitive to the assumptions that have been made.

In practice, to implement "honest" DD, I first estimate the standard event study model (2), where the parameters $\{\gamma_\tau, \delta_\tau\}$ capture the estimated leads and lags relative to the treatment event. The pre-treatment parameters $\{\gamma_\tau : \tau < 0\}$ provide a measure of how strongly parallel trends are violated before the intervention. "Honest" DD then uses these estimates to construct sensitivity analyses. By choosing \bar{M} and quantifying how large post-treatment violations can be relative to pre-treatment violations, we obtain robust confidence intervals for the ATT. These intervals widen as pre-treatment uncertainty and deviations increase, reflecting the idea that weaker evidence for parallel pre-treatment trends necessitates more cautious inference regarding post-treatment effects.

4.4 Difference-in-Difference-in-Differences

While the DD framework relies on comparisons across states to capture the counterfactual trends of the treated state, they can still be confounded if there are state-specific shocks that differentially affect the treated group. To further strengthen identification, I employ a difference-in-difference-in-differences (DDD) approach. The DDD design introduces an additional level of differencing by allowing for variation not only across states and time, but also across groups within the same state.

Let WI_s be an indicator indicating whether state s is Wisconsin ($WI_s = 1$ if s is Wisconsin and 0 otherwise), and let $\text{Post}_t = 1$ for all post-Act 10 time periods and 0

otherwise, as before. In addition, define a group indicator Public_j that distinguishes between the treated group and an unaffected group within the same state. For example, $\text{Public}_j = 1$ for public employees covered by Act 10 (both in Wisconsin and in the control states), and $\text{Public}_j = 0$ for private sector employees or a subset of public employees not covered by the policy. The DDD estimator can be obtained from a regression model of the form:

$$Y_{ijst} = \alpha_s + \lambda_t + \beta_1 \text{Post}_t + \beta_2 \text{Public}_j + \beta_3 WI_s + \beta_4 (\text{Public}_j \times \text{Post}_t) + \beta_5 (WI_s \times \text{Post}_t) + \beta_6 (WI_s \times \text{Public}_j) + \beta_7 (WI_s \times \text{Post}_t \times \text{Public}_j) + X_{ijst}\beta + \varepsilon_{ijst}, \quad (3)$$

where Y_{ijst} is the outcome of interest for group j in state s at year t , X_{ijst} is a vector of observed covariates, α_s and λ_t are state and time fixed effects, respectively (again, some of the terms are dropped due to multicollinearity, I include them for explanation), and ε_{ijst} is an error term. The parameter of interest is β_7 , which captures the triple interaction of being in the treated state ($WI_s = 1$), in the affected group ($\text{Public}_j = 1$), and in the post-treatment period ($\text{Post}_t = 1$), and identifies ATT under its own parallel trend assumption, discussed below.

The intuition behind the DDD approach is as follows: any state-specific shocks that affect both groups similarly in the same state are differenced out when comparing the difference-in-differences of the main group with the difference-in-differences of the comparison group. By introducing this additional layer of differencing at the group level, DDD can remove confounding influences that would persist under a simple DD design. Thus, the DDD estimator identifies the policy effect if, in the absence of the treatment, the difference between the main and comparison groups in the treated state would have evolved in parallel with the corresponding difference in the control states.

Formally, for the DDD estimator to identify the ATT, the following triple-difference parallel trends assumption must hold (for a full proof, see Olden and Møen, 2022):

$$\begin{aligned} & (E[Y_0 \mid WI_i = 1, \text{Public}_j = 1, \text{Post}_t = 1] - E[Y_0 \mid WI_i = 1, \text{Public}_j = 1, \text{Post}_t = 0]) \\ & - (E[Y_0 \mid WI_i = 1, \text{Public}_j = 0, \text{Post}_t = 1] - E[Y_0 \mid WI_i = 1, \text{Public}_j = 0, \text{Post}_t = 0]) \\ & = (E[Y_0 \mid WI_i = 0, \text{Public}_j = 1, \text{Post}_t = 1] - E[Y_0 \mid WI_i = 0, \text{Public}_j = 1, \text{Post}_t = 0]) \\ & - (E[Y_0 \mid WI_i = 0, \text{Public}_j = 0, \text{Post}_t = 1] - E[Y_0 \mid WI_i = 0, \text{Public}_j = 0, \text{Post}_t = 0]). \end{aligned} \quad (4)$$

As in the DD case, we can complement the DDD approach with event-study specifications that include leads and lags of the triple interaction to visually inspect whether the triple-difference dimension followed parallel trends before the intervention. If the pre-treatment triple-difference coefficients are close to zero and statistically insignificant, this gives credibility to the DDD assumption.

4.5 Synthetic Control

The synthetic control method, as introduced by Abadie et al., 2010, provides an alternative approach to evaluating policy interventions. Unlike standard DD or DDD analyses, which rely on researcher-chosen control groups and the assumption of parallel trends, the synthetic control technique uses a data-driven procedure to construct a "synthetic" counterfactual for the treated unit. This method is particularly well suited to comparative case studies where the unit of analysis is aggregated (states) and the number of available control units is limited.

In essence, the synthetic control approach identifies a weighted combination of untreated units that collectively reproduce the pre-intervention characteristics and outcome trajectory of the treated unit. By minimizing the distance between the predictors (covariates and pretreatment outcomes) of the treated unit and those of the weighted combination of control units, the method results in a synthetic unit that closely approximates what would have happened to the treated unit in the absence of the intervention. This process reduces the researcher's ability to choose comparison groups because the weights are chosen to optimize the pre-treatment match without extrapolating beyond the support of the observed data. In addition, by matching on pretreatment outcome trajectories and covariates, the synthetic control method implicitly controls for unobserved, time-varying confounders that affect the treated and control units similarly.

Formally, consider $J + 1$ aggregate units (e.g., states) observed over T periods, where unit 1 is treated. The intervention occurs at time $T_0 + 1$, and Y_{1t} denotes the outcome for the treated unit at time t . The goal is to estimate the treatment effect:

$$\tau_{1t} = Y_{1t} - Y_{1t}^0,$$

where Y_{1t}^0 is the counterfactual outcome in the absence of treatment. The synthetic control method constructs weights $W = (w_2, \dots, w_{J+1})^\top$ such that no weight is negative and the weights sum to one. These weights are chosen to minimize the difference between

the pre-treatment characteristics of the treated unit X_1 and a weighted average of the characteristics of the control units X_0W . Given a set of predictors X_1 for the treated unit and X_0 for the controls, along with an appropriate diagonal matrix V that assigns importance to each predictor, the optimal weights $W^*(V)$ are solved:

$$\min_W \|X_1 - X_0W\|_V = \sqrt{(X_1 - X_0W)^\top V (X_1 - X_0W)}.$$

These weights also produce the synthetic control's outcome in the post-treatment period:

$$\hat{Y}_{1t}^0 = \sum_{j=2}^{J+1} w_j^* Y_{jt}.$$

The estimated treatment effect for each post-treatment period is then:

$$\hat{\tau}_{1t} = Y_{1t} - \hat{Y}_{1t}^0.$$

In this analysis, I use aggregated state-level data rather than the individual-level microdata used in the DD and DDD analyses. This aggregation allows the synthetic control to find an appropriate counterfactual at the state level, so that Wisconsin's outcomes can be compared to those of a synthetic Wisconsin constructed from a combination of other states.

The choice of predictors is important for the synthetic control method to accurately replicate the pre-intervention outcomes of the treated unit.

I selected variables that are essentially the same demographic and socioeconomic controls as in the DD specifications, only aggregated to the state level: specifically, average age, percentage female, percentage white, percentage college educated, percentage married, and percentage living in metropolitan areas. These covariates ensure that the synthetic control closely reflects the demographic composition of Wisconsin. For models focusing on unionization and wages, predictor variables such as average union membership, average log wages, and average log weekly wages are included. These variables directly influence the outcomes of interest and help to accurately model the counterfactual. Fringe benefit analyses include additional predictors such as average pension plan participation and average log employer insurance contribution. These variables capture the nuances of benefit provision and ensure that the synthetic control adequately reflects the benefits situation in Wisconsin.

After constructing the synthetic control, it is important to assess the balance between the treated unit and its synthetic counterpart on the selected predictors and pretreatment outcomes. This is typically done by examining the predictor balance table and visualizing the fit of the synthetic control relative to the pre-treatment unit. A close fit in the pre-treatment period indicates that the synthetic control is a reasonable approximation of the counterfactual.

Inference relies on placebo testing and permutation inference as recommended by Abadie et al., 2010. The procedure includes:

1. Apply the synthetic control method to each control unit as if it were treated, generating a distribution of placebo treatment effects.
2. Calculate the RMSPE for each placebo for both the pre- and post-treatment periods:

$$\text{RMSPE} = \left(\frac{1}{T - T_0} \sum_{t=T_0+1}^T \left(Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt} \right)^2 \right)^{\frac{1}{2}}.$$

3. Compute the ratio of post-treatment to pre-treatment RMSPE for each placebo.
4. Rank the RMSPE ratio of the treated unit within the distribution of placebo ratios and calculate the p -value as the ratio of the rank of the treated state to the total number of states. This p -value indicates the extremity of the effect of the treated unit relative to the placebo distribution.

A low p -value indicates that the observed treatment effect for Wisconsin is statistically significant and unlikely to have occurred by chance. In addition, visual representations such as path plots and gap plots are used to illustrate the divergence between Wisconsin and its synthetic counterpart after treatment, as well as to compare the performance of the treated unit to that of the placebo units.

This strategy complements difference-in-differences methods. While DD and DDD rely on carefully selected comparison groups and require strong assumptions, the synthetic control method provides a data-driven way to construct the control group. It also emphasizes transparency, as the contribution of each donor unit is explicitly reported, and highlights the importance of achieving good pre-treatment matching. By considering the results from DD, event studies, "honest" DD, DDD, and synthetic control together, I obtain a more comprehensive and credible assessment of Act 10's impact on unionization, hourly wages, weekly earnings, and fringe benefits.

4.6 Choice of Control States

A critical part of implementing the DD, DDD, and synthetic control methods is the selection of appropriate control states. The credibility of the counterfactual, and by extension the causal inferences drawn from these methods, depends on comparing Wisconsin to states with broadly similar pre-Act 10 legal, institutional, and economic environments.

For this study, I focus on states with a history of public sector collective bargaining rights. By limiting attention to all states where employers are legally required to bargain over wages and working conditions with all public employees (including teachers, police, firefighters, and other local and state employees, as in pre-Act 10 Wisconsin), I aim to ensure that the baseline institutional context in Wisconsin and the control units prior to the passage of Act 10 is comparable. This approach uses the analysis in Morrissey and Sherer, 2024, which provides a comprehensive examination of public employee collective bargaining for each state.

The complete donor pool included the following states and districts: South Dakota (SD), New Mexico (NM), District of Columbia (DC), Missouri (MO), Kansas (KS), Nebraska (NE), Delaware (DE), Montana (MT), Nevada (NV), Alaska (AK), Ohio (OH), Michigan (MI), Illinois (IL), Vermont (VT), New Hampshire (NH), Massachusetts (MA), Oregon (OR), Minnesota (MN), Washington (WA), Maine (ME), California (CA), Pennsylvania (PA), Rhode Island (RI), Connecticut (CT), New Jersey (NJ), New York (NY), and Hawaii (HI).

From this initial set, I exclude the District of Columbia (DC) and Alaska (AK) to maintain a consistent sample of states with more similar demographic and economic structures to Wisconsin. In addition, while Nevada (NV) has a statutory requirement for collective bargaining for public employees, significant legislative changes in 2011 and 2015 weakened its scope and effectiveness (see Division, 2015). Because these changes make it difficult to assume a stable framework, I also exclude NV from the pool of potential donors.

I used two consistent control state specifications throughout the analyses. The first set included Illinois (IL), Kansas (KS), Michigan (MI), Minnesota (MN), South Dakota (SD), Massachusetts (MA), Pennsylvania (PA), and Washington (WA). This group was chosen for its mix of geographic proximity and institutional similarities to the Wisconsin labor market, ensuring robust comparisons for unionization and wage outcomes.

The second specification focused on Massachusetts (MA), Pennsylvania (PA), and Washington (WA). This smaller group proved particularly effective in ensuring parallel

pre-treatment trends for fringe benefit outcomes, especially for the DDD analysis (see figures 36, 37, and 38). In addition, it addressed Stable Unit Treatment Value Assumption (SUTVA) concerns because these states do not share a direct border with Wisconsin, reducing the likelihood of spillover effects. This specification also served as a robustness check, increasing the credibility of the results.

This manual selection of states, while guided by geographic and institutional considerations, is subjective and ad hoc. One of the main motivations for implementing the synthetic control method in this study is to reduce this discretion. Rather than pre-selecting a small number of comparison states, the synthetic control approach uses a data-driven algorithm to assign weights to all eligible donor states based on how well they replicate Wisconsin's pre-treatment characteristics and outcomes. By allowing the method to determine the relative importance of each donor state, the synthetic control technique mitigates subjective judgments and ensures that the resulting synthetic unit closely matches Wisconsin's pre-Act 10 conditions.

4.7 Potential Limitations

While the methods described above are designed to provide a credible estimate of the causal effects of Act 10, several limitations and potential sources of bias must be acknowledged.

Parallel Trends Assumption Violations A fundamental assumption underlying difference-in-differences (DD) and related approaches is the existence of parallel trends in the absence of treatment. While event study analyses help visualize pre-treatment trajectories and "honest" difference-in-differences methods quantify sensitivity to deviations from parallel trends, it is important to recognize that these tests are neither definitive nor fully robust to subtle violations. For some specifications, we observe strong evidence of parallel trends in the pre-treatment period; for others, the evidence is weaker. Although "honest" DD reduces reliance on the strict parallel trends assumption by imposing plausible limits on post-treatment deviations, both event studies and honest DD have limited statistical power and may fail to detect small but meaningful trend differences. Thus, although the methods used improve on standard DD inference, there remains some residual uncertainty about the validity of the parallel trends assumption. However, the synthetic control method helps to reduce this uncertainty by constructing a data-driven counterfactual that closely matches the pre-treatment trajectory of the treated unit, thereby increasing the

robustness of causal estimates.

Policy Exogeneity One known phenomenon is the possibility of an "Ashenfelter dip", where treated units experience a temporary decline in outcomes just before treatment for reasons unrelated to the intervention. Such a dip, if incorrectly attributed to the intervention, could bias the estimated treatment effect. In this context, if Wisconsin public employees had unusually low outcomes just before Act 10, this could inflate the estimated effect of the policy. However, preliminary trend checks and event studies show no strong evidence of such a pre-trend dip. Moreover, Act 10 itself can be seen as exogenous to union or worker behaviour in the state. It emerged from a unique alignment of political forces - the 2010 Republican sweep, Governor Walker's policy agenda, and national efforts to reduce union influence - rather than from specific economic or union-driven changes. This exogeneity gives credibility to the treatment of Act 10 as a quasi-random shock.

Sample size and representativeness The analysis is based on microdata from the Current Population Survey (CPS). Although the CPS is a highly respected and nationally representative survey, it samples only a small fraction of the labor force. For example, in Wisconsin's state and local government employment sector, even a large population (e.g., about 289,944 full-time equivalent employees in 2002) translates into a few hundred observations (e.g., 445 in 2002) in the CPS, or about 0.2% of the population. Such a small sample may limit our ability to detect subtle effects or to fully represent heterogeneity in outcomes. However, the CPS is the standard dataset for analyzing U.S. labor markets, and its representative design ensures that our inferences, while potentially imprecise, remain unbiased on average. By relying on a long period of data (1994–2023) and multiple identification strategies, I attempt to mitigate concerns about sampling variability.

Repeated cross sections and compositional changes The Current Population Survey (CPS) is a repeated cross-sectional survey, meaning that the same individuals are not interviewed over time. As a result, changes in the composition of the labor force may confound trend comparisons. To address this, I control for a comprehensive set of demographic and socioeconomic characteristics, which reduces the likelihood that compositional shifts are responsible for the observed results. However, unobserved factors may still influence the results. Therefore, while reasonable steps have been taken to account for compositional changes, the repeated cross-sectional nature of the CPS data inherently

allows for some unobserved shifts that cannot be fully controlled for.

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Anticipatory Effects Act 10 was announced in February 2011, passed in March and implemented in June of that year. In theory, unions or public employers could have anticipated the changes and adjusted their behavior in advance of formal implementation. However, most public sector collective agreements are at least annual, which limits the scope for immediate anticipatory action. Any anticipatory effects are therefore likely to be small and limited to a short window in mid-2011. By examining the leads in the event study framework, we confirm minimal evidence of pre-implementation responses. Thus, anticipatory effects do not appear to substantially bias our results.

Subsequent Policy Changes: Right-to-Work Law In 2015, Wisconsin enacted a Right-to-Work (RTW) law covering private-sector employees. RTW laws generally weaken unions by prohibiting employers from requiring union membership or the payment of union dues as a condition of employment. Specifically, Wisconsin's RTW law affects private-sector workers and affects collective bargaining agreements only when they are renewed, amended or extended. The primary goal is to give workers the freedom to choose whether to join or financially support a union, thereby reducing union membership and bargaining power in the private sector.

This policy change poses two potential challenges to the analysis. First, in the DDD framework, the RTW law only affects private sector outcomes, potentially leading to an underestimation of treatment effects on public sector outcomes if both Act 10 and the

RTW law affect labor market dynamics in the same way for public and private sector workers. However, union membership among private sector workers in Wisconsin was already low, declining from 10.3% in 2014 to 6.9% in 2015 (remaining roughly constant before and after treatment), limiting the magnitude of any bias in the DDD estimates. Second, the RTW law could induce general equilibrium changes, such as improvements in the state’s overall economic conditions, that could affect public sector employment outcomes through changes in government spending or labor market dynamics.

Preliminary empirical evidence from the raw data shows no significant shifts in trend patterns following the implementation of the RTW law in 2015. To further address this concern, robustness checks restricting the analysis to pre-2015 periods yield results consistent with the main findings, suggesting that the RTW law does not significantly distort the estimated effects of Law 10. Moreover, the slight decline in private sector union membership in 2015 and the stabilization of trends thereafter suggest that any underestimation of treatment effects in the DDD analysis is likely to be minimal. Thus, while the RTW law introduces a potential confound, the empirical evidence does not support a substantial bias in our estimates attributable to this subsequent policy change. Nevertheless, the potential impact of the RTW law requires further investigation to fully understand its effects on both private and public labor markets.

In summary, while this analysis employs robust empirical strategies to estimate the causal effects of Wisconsin Act 10, several limitations must be acknowledged. Potential violations of the parallel trends assumption, small sample sizes, compositional changes inherent in repeated cross-sectional data, concerns about policy exogeneity, anticipatory effects, and subsequent policy changes such as the RTW law present challenges that could affect the estimates. However, by employing multiple identification strategies - including difference-in-differences, difference-in-difference-in-differences, event studies, honest difference-in-differences and synthetic control methods - and conducting thorough robustness checks, this study mitigates many of these concerns. Despite the inherent limitations, the convergence of results across different methodologies increases the credibility and reliability of the findings. Future research could further explore these limitations, in particular the broader effects of RTW laws, to deepen our understanding of their impact on both private and public sector labor markets.

5 Results

In this section, I present the main results from the difference-in-differences (DD), event study, sensitivity analysis ("honest" DD), difference-in-difference-in-differences (DDD), and synthetic control (SC) methods. The comprehensive estimates are summarized in the Appendix Tables 1 and 2, while detailed visualizations are provided in the Appendix Figures.

5.1 Union Membership and Representation Rate

The analysis reveals a substantial and statistically significant decline in public sector unionization following the implementation of Wisconsin Act 10. Specifically, the DD and DDD estimates indicate that union membership rates declined by about 27% to 31% across different specifications, with all estimates significant at the 1% level (see Appendix Table 1, Panel A). Restricting the analysis to different time periods, the decline ranges from 25% to 26% for the 2019 sample and from 22% to 23% for the 2015 sample, all of which remain significant at the 1% level.

- **DD and DDD:** The DD estimates consistently show a negative and significant impact of Act 10 on both union membership and representation rates. Similarly, the DDD approach, which includes additional control groups, confirms these findings with comparable effect sizes, reinforcing the robustness of the estimated treatment effects.
- **DD Event Study:** The event study results, shown in Figure 10, initially show some deviations from the parallel pre-treatment trends for some years prior to 2008. However, from 2008 to 2011, there are no significant coefficients, suggesting that there is no significant deviation from the pre-treatment trends for 4 years. After implementation, starting in 2012, unionization rates show a steady decline each year until stabilizing between 26% and 30% in 2017 (Figure 10). This pattern underscores the persistent negative impact of Act 10 on unionization over time.
- **Sensitivity Analysis:** The "honest" DD approach, which accounts for potential violations of the parallel trends assumption by imposing restrictions based on pre-treatment deviations, confirms the significant decline in union membership rates. Even when post-treatment deviations are allowed to be up to 1.5 times the magni-

tude of pre-treatment deviations ($\bar{M} = 1.5$), the estimated treatment effects remain statistically significant (Figure 19).

- **DDD Event Study:** The DDD event study, shown in Appendix Figures 28 and 29, shows improved adherence to the parallel trends assumption in the pre-treatment period, with almost all coefficients negligible and statistically insignificant. In the post-treatment period, unionization rates decline, consistent with the DD results.
- **SC:** The SC method constructs a synthetic Wisconsin that closely matches the pre-treatment unionization trends and covariate profiles of the actual Wisconsin (see Figure 39). The estimated treatment effect on union membership is a reduction of 31% in 2023, with a maximum possible p-value of 0.042, indicating that such a reduction is unlikely to have occurred by chance (Table 2). The placebo tests, shown in Figure 55, show that the observed reduction in Wisconsin is substantially larger than that observed in any of the placebo states, further strengthening the validity of the SC estimates. The placebo gaps for treated and control units (Appendix Figure 55) show that the decline in union membership rates in Wisconsin is significantly larger than in any other state in the donor pool, with the Post/Pre-Treatment Mean Squared Prediction Error (MSPE) ratio for Wisconsin standing out as the most extreme among all units (Figure 63).

Overall, the consistency of results across multiple identification strategies-DD, DDD, event studies, "honest" DD, and SC- indicates that Wisconsin Act 10 led to a significant reduction in both union membership and representation in the public sector. The robustness of these results is supported by the extensive sensitivity analyses and rigorous placebo tests employed, which together strengthen the credibility of the causal inferences drawn.

5.2 Earnings

The implementation of Wisconsin Act 10 had a significant negative impact on public sector earnings. Specifically, the DD and DDD estimates indicate that hourly earnings declined by about 3% to 5% (with and without OTC), while weekly earnings declined by about 5% to 8% under various specifications, most of which are statistically significant (see Table 1, Panel B).

DD and DDD: The DD estimates consistently indicate a significant negative impact of Act 10 on both hourly earnings (with and without OTC) and weekly earnings. Specifically, hourly earnings declined by about 3% to 5%, while weekly earnings declined by about 5% to 8% across specifications. The larger magnitude observed for weekly earnings compared to hourly earnings suggests a concurrent decline in hours worked.

The inclusion of individual controls in the DD models results in a slight attenuation of the estimated effects, suggesting the presence of compositional changes - particularly in education level - within the public sector workforce in Wisconsin. When education level is removed as a control variable, the magnitude of the treatment effect returns to its initial level, suggesting that shifts in educational composition are partially responsible for the observed changes. I did find a decline in the share of public employees with college degrees in Wisconsin that explains this attenuation, but it is both small and statistically insignificant (results not shown) and, as we see, does not change the sign of the estimated effects.

Some DD specifications for hourly earnings without OTC with individual controls yield insignificant estimates, likely due to low statistical power from small sample sizes. Conversely, the DDD approach that includes private sector workers as an additional control group has a larger sample size, which increases statistical power. As a result, the DDD estimates not only maintain the same negative estimate, but also achieve higher levels of statistical significance compared to the simpler DD estimates.

Furthermore, restricting the analysis to the period up to 2015 results in treatment effect estimates that are close to zero and lack statistical significance in almost all specifications. This finding is corroborated by the event study analysis, which shows insignificant coefficients for the earnings variables up to 2015. These results are consistent with the expectation that the wage bargaining effects of Act 10 develop gradually and become more evident several years after implementation, in contrast to the more immediate effects observed in unionization rates.

DD Event Study: The event study results (see Figures 12 and 13) show some pre-treatment violations of the parallel trends in 2008-2009 for hourly earnings. However, in the post-treatment, hourly earnings show several significant negative coefficients and a few significant positive coefficients, although the pattern is not consistent. For weekly earnings (see figure 14), the pre-treatment coefficients are statistically insignificant for 11 years before treatment, supporting the parallel trends assumption, and the post-treatment coefficients consistently show negative effects, with almost all coefficients negative after

2015, although only one coefficient is statistically significant, likely due to low power.

Sensitivity Analysis: The "honest" DD approach finds that the confidence intervals for all earnings variables remain insignificant even when allowing for post-treatment deviations up to 0.5 times the magnitude of the pre-treatment deviations ($\bar{M} = 0.5$). This suggests that while the estimated effects are negative, they are not robust to these assumptions, possibly due to low power.

DDD Event Study: Consistent with DD, weekly earnings have insignificant coefficients 11 years before treatment, supporting the common trend assumption. After treatment, weekly earnings have significant negative coefficients in 2016-2018, and most coefficients remain negative after 2015, suggesting that the effect of Act 10 on earnings was strongest a few years after the implementation of Act 10 and gradually diminished over time, but still remained negative.

SC: The synthetic control estimates corroborate the DD and DDD results, showing significant declines in both hourly and weekly earnings (see table 2). The treatment effects are statistically significant, with the lowest possible p-values for hourly and weekly earnings with OTC, indicating strong evidence against the null hypothesis of no effect. The Post/Pre-Treatment MSPE ratio (see Figure 66 and 67) shows that the earnings decline in Wisconsin is substantially larger than in any of the placebo states, further validating the synthetic control results. In addition, the outcome trajectories (see Appendix Figure 41) show that the synthetic Wisconsin closely matches the pre-treatment earnings trends of the actual Wisconsin, and that there is a noticeable post-treatment earnings decline that remains negative but diminishes in magnitude over time.

Overall, the consistent results across multiple identification strategies suggest that Wisconsin Act 10 led to a significant reduction in public sector earnings.

5.3 Share of Public Sector Employees

To assess the impact of Act 10 on public sector employment, I use the difference-in-differences (DD) approach to estimate the probability of being a public sector employee among all employed individuals in the sample. Across all specifications, Act 10 is associated with a statistically significant 1% increase in the share of public sector employees at the 5% significance level (see Appendix Table 1, Panel B). This marginal increase suggests a slight shift toward public sector employment after policy implementation, which may be due to the weakening of bargaining rights. By reducing bargaining constraints, Act

10 effectively increases labor demand, making it easier for employers to hire public sector workers.

However, the event study analysis shows violations of the pre-treatment parallel trends (see Figure 15), raising concerns about the validity of the DD estimates. To address this, I conduct a sensitivity analysis using the "honest" DD method. The robust 95% confidence interval with a relative size limit of $\bar{M} = 1$ for the treatment effect ranges from -0.4% to 2.5% (see figure 24). This range implies that, even with possible deviations from parallel trends, any negative effect of Act 10 on the share of public employees is negligible in magnitude.

Thus, the evidence does not support a substantial effect of Act 10 on the share of public employees and we cannot make convincing claims about employment likely due to low power.

5.4 Fringe Benefits

To assess the impact of Act 10 on fringe benefits, I focus primarily on employer-sponsored health insurance due to data limitations. Specifically, I analyze two main outcomes: the log of employer health insurance contributions and group health insurance coverage. I also examine participation in workplace pensions, but the binary nature of this variable and the lack of data on employer pension contributions limit the analysis for pensions.

DD and DDD: The DD and DDD estimates show no significant effect of Act 10 on workplace pension participation across all specifications (see Appendix Table 1, Panel C). This suggests that Act 10 did not significantly alter the likelihood that public employees participate in pension plans. In contrast, Act 10 is associated with highly significant negative coefficients on the log of employer health insurance contributions in all specifications, ranging from -0.53 to 0.90 . Translating these coefficients ($e^{\text{coeff}} - 1$), we observe a decline in employer health insurance contributions of about 41% to 59% . In addition, group health insurance coverage experiences a statistically significant decline of 3% to 7% across specifications. The inclusion of individual controls slightly attenuates the magnitude of these effects (see discussion in the Earnings subsection).

DD Event Study: For participation in a workplace pension plan, the pre-treatment coefficients show violations of the parallel trends assumption in 2008-2009 (see Figure 16), undermining the credibility of the DD estimates for this variable. Conversely, for the log of employer health insurance contributions and group health insurance coverage,

almost all pre-treatment coefficients are statistically insignificant, with only two exceptions occurring 8 and 13 years before treatment (see Figures 17 and 18). After treatment, the log of employer health insurance contributions consistently shows significantly negative coefficients, reaching its maximum decline in 2014 before diminishing in subsequent years. Similarly, group health insurance coverage has significantly negative coefficients in three post-treatment periods.

Sensitivity Analysis: Sensitivity analysis using the "honest" DD methodology with a relative size limit of $\bar{M} = 1$ yields robust 95% confidence intervals for all fringe benefit outcomes that include zero (see figures 25, 26, 27). This suggests that under possible deviations from the parallel trends assumption, any negative effects of Act 10 on fringe benefits are negligible in magnitude. The lack of significant effects in this sensitivity analysis is likely due to low statistical power.

DDD Event Study: Initial DDD event study specifications using a broader set of control states (IL, KS, MI, MN, SD, MA, PA, WA) did not adequately satisfy the pre-treatment assumption of parallel trends (see figures 33 and 34). To address this, an alternative specification using only Massachusetts (MA), Pennsylvania (PA), and Washington (WA) as control states was used (see Figures 36, 37, 38). This control group shows better compliance with the parallel trends in the pre-treatment period, resulting in event study coefficients for the log of employer health insurance contributions and group health insurance coverage that are consistent with the DD results, with significant negative post-treatment effects observed.

SC: The synthetic control analysis for fringe benefits shows mixed results. For workplace pension participation and group health insurance coverage, the synthetic control estimates are statistically insignificant, indicating no detectable effect of Act 10 on these outcomes (see Table 2 and Figures 44, 46). However, the analysis shows a significant decrease in employer-sponsored health insurance contributions, with estimates showing a reduction of about 60% (see Table 2 and Figure 69), with only California (CA) having a marginally higher ratio of post- to pre-treatment MSPE, which does not substantially undermine the significance of the treatment effect on health insurance contributions.

Overall, the consistent results across specifications for employer health insurance contributions suggest a substantial reduction of about 50%. In addition, all methods suggest no significant impact on participation in occupational pensions, despite some violations of the parallel pre-treatment trends. The results for group health insurance coverage remain mixed, with no clear consensus between the different approaches.

6 Discussion

The empirical analysis presented in this study highlights the diverse effects of Wisconsin Act 10 on public sector labor market outcomes, including unionization rates, wages, and fringe benefits. The results reveal a significant decline in both union membership and representation rates, substantial reductions in hourly and weekly earnings, and a nuanced effect on fringe benefits, particularly employer-sponsored health insurance contributions. In addition, the study finds a marginal and statistically insignificant increase in the share of public sector employees. These results are interpreted in light of theoretical frameworks and existing empirical literature, highlighting both consistencies and divergences.

The most notable finding relates to the significant decline in public sector unionization following the implementation of Act 10. The difference-in-differences (DD) and difference-in-difference-in-differences (DDD) estimates indicate a decline in union membership and representation rates of approximately 27% to 31%, a result that is both statistically and economically significant. This finding is consistent with the theoretical model proposed by Freeman and Valletta (1988), which suggests that restrictive labor laws divert union resources from wage bargaining to political activities, and therefore reduce bargaining power and, as a consequence, union membership. Act 10's provisions - such as annual recertification requirements, limiting collective bargaining to base wages, and banning agency fees - are designed to increase the cost of union participation and reduce the security of union membership. These legislative changes are consistent with Freeman's model and effectively limit the ability of unions to attract and retain members.

In addition, the synthetic control method corroborates the DD and DDD results, estimating a 31% reduction in union membership with a statistically significant p-value of 0.042. This convergence across methods strengthens the validity of the conclusion that Act 10 significantly weakened public sector unions in Wisconsin.

The study documents significant negative effects on both hourly and weekly earnings for public employees. Hourly earnings declined by 3% to 5%, while weekly earnings declined by 5% to 8%. These findings are consistent with Brunner and Ju (2019), who found that mandatory collective bargaining laws can suppress wage growth in the public sector. This finding is also directly consistent with the theoretical model of the Freeman and Valletta (1988), which suggests that restrictive labor laws reduce union bargaining power, leading to lower earnings. The wage declines are due to Act 10's limitation of collective bargaining to base wages and its requirement that wage increases be capped

at the rate of inflation. By limiting the scope of bargaining, Act 10 effectively limited the ability of unions to secure higher wages for their members, leading to the observed declines in earnings.

The decline in weekly earnings, which exceeds the decline in hourly wages, suggests a simultaneous decline in hours worked. The synthetic control estimates further strengthen the negative impact on earnings, with significant declines in both hourly and weekly earnings observed after the implementation of Act 10.

The analysis of fringe benefits reveals a significant reduction in employer-sponsored health insurance contributions, which declined by about 41% to 59%. This finding is consistent with Act 10's focus on limiting collective bargaining to base wages, thereby excluding fringe benefits such as health insurance and pensions from bargaining.

Conversely, participation in workplace pension plans remained statistically insignificant in all specifications, suggesting that Act 10 did not have a measurable impact on pension participation rates. Again, however, it is important to note the data limitations in this area; the CPS does not provide direct measures of employer pension contributions, which limits a comprehensive assessment of pension-related outcomes. Thus, while the analysis suggests stability in pension participation, it remains uncertain whether employer pension contributions were affected by Act 10.

The study finds a marginal 1% increase in the share of public employees, which is not statistically significant using the "honest" DD approach, which accounts for potential deviations from the parallel trends assumption. Theoretically, using the Freeman and Valletta (1988) model, Act 10's restrictions on collective bargaining could lead to an increase in the demand for public sector labor by reducing wage rigidity and lowering labor costs, thereby making public sector employment more attractive. However, the empirical evidence does not support a substantial shift in employment shares, suggesting that the policy's impact on public sector employment dynamics may be limited or offset by other factors, such as spending cuts.

The results of this study are highly consistent with the existing literature. Similarly, Brunner and Ju (2019), Hirsch et al. (2011), Baron (2018) documented wage suppression under mandatory collective bargaining laws, consistent with the observed declines in hourly and weekly earnings. Unlike Baron (2018), however, I find significant effects on fringe benefits, particularly employer contributions to health insurance.

While the study employs robust empirical strategies, several methodological challenges warrant consideration. Reliance on the assumption of parallel trends in DD and DDD

analyses remains a potential source of bias if pre-treatment trends were not appropriately parallel. Although event studies and the "honest" DD approach provide partial mitigation, they do not fully address concerns about unobserved confounding. The synthetic control method addresses some of these concerns by constructing a data-driven counterfactual; however, its effectiveness depends on the availability of appropriate donor states that closely match Wisconsin's pretreatment characteristics.

The use of repeated cross-sectional data from the Current Population Survey (CPS) introduces limitations related to compositional changes over time. Despite extensive demographic controls, unobserved changes in the composition of the labor force could confound trend comparisons. In addition, the subsequent implementation of Right-to-Work (RTW) legislation in 2015 represents an exogenous policy change that could affect labor market dynamics. Although preliminary analysis suggests minimal impact from the RTW law, its potential to induce broader economic shifts cannot be completely discounted. Moreover, the COVID-19 pandemic introduced various policy interventions that may have affected labor market outcomes, further complicating the isolation of Act 10's effects. These overlapping policy changes and external shocks highlight the challenges of attributing observed labor market trends only to Act 10 and emphasize the need for cautious interpretation of the results.

7 Conclusion

This study provides a comprehensive analysis of the long-run effects of Wisconsin Act 10 on public sector unions, wages, and fringe benefits. Using a robust methodological framework that includes difference-in-differences (DD), difference-in-difference-in-differences (DDD), event studies, "honest" DD, and synthetic control methods, the results present a consistent and credible assessment of the policy's impact.

The empirical results show that Act 10 significantly undermined public sector unionization, reducing union membership and representation rates by about 27% to 31%. These declines underscore the effectiveness of the legislation in weakening collective bargaining power and limiting union influence in the public sector. At the same time, the analysis reveals significant declines in both hourly and weekly earnings, ranging from 3% to 8%, suggesting that the policy has been successful in constraining wage growth and increasing labor market flexibility.

Fringe benefits, in particular employer-sponsored health insurance contributions, ex-

perienced substantial declines of about 41% to 59% across specifications, highlighting the restrictive scope of collective bargaining after Act 10. However, participation in workplace pension plans remained largely unaffected, suggesting that certain fringe benefits were insensitive to the direct effects of the policy, although data limitations limit a comprehensive assessment of pension contributions.

The marginal and statistically insignificant increase in the share of public sector employees suggests that Act 10 did not substantially alter employment shares, possibly due to offsetting factors such as government spending constraints or economic conditions. This nuanced finding suggests that while Act 10 effectively reduced union strength and wage levels, its impact on overall public sector employment dynamics was limited.

Despite its methodological rigor, the study acknowledges potential limitations, including its reliance on the assumption of parallel trends, the small sample size limitations and cross-sectional nature of the CPS data, and the confounding effects of subsequent policies, such as the Right-to-Work law enacted in 2015. These factors require cautious interpretation of the results, although the consistency of the findings across multiple empirical strategies increases the robustness of the conclusions.

Future research could improve the understanding of the impact of Act 10 by employing a combination of difference-in-differences (DD) and matching techniques, as outlined by Abadie ([2005](#)). This semiparametric approach allows for the control of observable differences between treated and control groups through nonparametric matching, while retaining the DD framework to address unobservable differences. Given the presence of some compositional changes observed in this study, integrating DD with matching can help mitigate potential biases in the magnitude of estimated treatment effects. In addition, this methodology allows for heterogeneous treatment effects, providing a more nuanced analysis of how Act 10 may have differentially affected different subgroups within the public sector workforce. By accounting for both observable and unobservable confounders, future studies can provide more precise estimates and deepen understanding of the policy's long-term effects.

In sum, Wisconsin Act 10 has had profound and lasting effects on public sector labor market outcomes, effectively reducing unionization, wages, and fringe benefits, while having mixed effects on employment shares. These findings contribute to a broader understanding of labor policy reforms and their effects on public sector workers.

Appendix

A. Treatment Effect Estimates for Union Membership, Earnings, and Fringe Benefits

Table 1: ATT Estimates for Union Membership, Wages, and Benefits Using DD and DDD

	Difference-in-Differences (DD)								Difference-in-Difference-in-Differences (DDD)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Panel A: Union Outcomes																
Union Membership Rate	-0.29*** (0.02)	-0.29*** (0.02)	-0.29*** (0.02)	-0.29*** (0.02)	-0.29*** (0.02)	-0.31*** (0.01)	-0.26*** (0.02)	-0.23*** (0.01)	-0.28*** (0.02)	-0.28*** (0.02)	-0.27*** (0.02)	-0.27*** (0.02)	-0.28*** (0.01)	-0.28*** (0.01)	-0.25*** (0.01)	-0.22*** (0.01)
Union Representation Rate	-0.28*** (0.02)	-0.28*** (0.02)	-0.28*** (0.02)	-0.28*** (0.02)	-0.28*** (0.01)	-0.29*** (0.01)	-0.25*** (0.01)	-0.22*** (0.01)	-0.27*** (0.01)	-0.27*** (0.01)	-0.26*** (0.01)	-0.26*** (0.01)	-0.27*** (0.01)	-0.27*** (0.01)	-0.24*** (0.01)	-0.21*** (0.01)
Panel B: Earnings and Employment																
Log of Hourly Earnings	-0.05*** (0.01)	-0.05*** (0.01)	-0.02 (0.02)	-0.02 (0.01)	-0.02 (0.01)	-0.05* (0.02)	-0.01 (0.01)	0.00 (0.01)	-0.03*** (0.01)	-0.03*** (0.01)	-0.02*** (0.01)	-0.02*** (0.01)	-0.02** (0.01)	-0.03* (0.01)	-0.01 (0.01)	0.01 (0.01)
Log of Hourly Earnings (OTC)	-0.05*** (0.01)	-0.05*** (0.01)	-0.03* (0.01)	-0.03* (0.01)	-0.03 (0.01)	-0.05* (0.02)	-0.01 (0.01)	0.00 (0.01)	-0.03*** (0.00)	-0.03*** (0.00)	-0.02*** (0.00)	-0.02*** (0.00)	-0.02** (0.00)	-0.02* (0.01)	-0.01 (0.01)	0.01** (0.00)
Log of Weekly Earnings (OTC)	-0.08*** (0.01)	-0.08*** (0.01)	-0.04** (0.02)	-0.04** (0.02)	-0.04* (0.02)	-0.07* (0.02)	-0.03 (0.01)	-0.00 (0.01)	-0.05*** (0.00)	-0.05*** (0.00)	-0.04*** (0.00)	-0.04*** (0.00)	-0.03*** (0.01)	-0.03* (0.01)	-0.03*** (0.01)	0.01 (0.00)
Share of Public Sector Employees	0.01** (0.00)	0.01*** (0.00)	0.01*** (0.00)	0.01*** (0.00)	0.01*** (0.00)	0.01** (0.00)	0.01*** (0.00)	0.01*** (0.00)	- (0.00)	- (0.00)	- (0.00)	- (0.00)	- (0.00)	- (0.00)	- (0.00)	- (0.00)
Panel C: Fringe Benefits																
Participation in Workplace Pension	0.00 (0.01)	0.00 (0.01)	0.01 (0.01)	0.01 (0.01)	0.00 (0.01)	0.01 (0.02)	0.00 (0.01)	-0.01 (0.01)	0.00 (0.01)	0.00 (0.01)	0.01 (0.01)	0.01 (0.01)	0.00 (0.01)	0.01 (0.01)	0.00 (0.01)	-0.00 (0.01)
Log of Employer Health Insurance Contribution	-0.89*** (0.08)	-0.89*** (0.08)	-0.69*** (0.09)	-0.70*** (0.08)	-0.68*** (0.09)	-0.78** (0.18)	-0.68*** (0.09)	-0.90*** (0.14)	-0.71*** (0.06)	-0.71*** (0.06)	-0.53*** (0.08)	-0.54*** (0.07)	-0.55*** (0.07)	-0.55** (0.14)	-0.55*** (0.07)	-0.73*** (0.13)
Group Health Insurance Coverage	-0.07*** (0.01)	-0.07*** (0.01)	-0.05*** (0.01)	-0.05*** (0.01)	-0.05*** (0.01)	-0.05** (0.01)	-0.05*** (0.01)	-0.07*** (0.01)	-0.05*** (0.01)	-0.05*** (0.01)	-0.03*** (0.01)	-0.04*** (0.01)	-0.04*** (0.01)	-0.03* (0.01)	-0.04*** (0.01)	-0.05*** (0.01)
Control States: IL, MI, MN, KS, SD	Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control States: MA, PA, WA	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period: 1994-2023	Yes	Yes	Yes	Yes	Yes	Yes			Yes	Yes	Yes	Yes	Yes	Yes		
Period: 1994-2019							Yes								Yes	
Period: 1994-2015								Yes								Yes
Imputed missing values		Yes	Yes	Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls			Yes	Yes	Yes	Yes	Yes	Yes			Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects				Yes	Yes	Yes	Yes	Yes				Yes	Yes	Yes	Yes	Yes
Year Fixed Effects					Yes	Yes	Yes	Yes					Yes	Yes	Yes	Yes

Notes: Each column in each panel represents a separate regression. Standard errors are clustered at the state level. Individual controls include age, sex, marital status, race, metropolitan status, education level, and industry. OTC refers to earnings including overtime, tips, and commissions. Fringe benefit analysis is limited to data up to 2018. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table 2: Synthetic Control Estimates for Union Membership, Earnings, and Fringe Benefits

Outcome	Treatment Effect	p-value
Union Membership Rate	−0.31**	0.042
Union Representation Rate	−0.29**	0.042
Log of Hourly Earnings	−0.06*	0.083
Log of Hourly Earnings (OTC)	−0.06**	0.042
Log of Weekly Earnings (OTC)	−0.07**	0.042
Participation in Workplace Pension	0.07	0.125
Log of Employer Health Insurance Contribution	−0.92*	0.083
Group Health Insurance Coverage	−0.07	0.167

Notes: Treatment effects are presented for the most recent year available: 2023 for union outcomes and wages, and 2018 for fringe benefits. OTC refers to earnings including overtime, tips, and commissions. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Notes: For Sections B–E, the control states are IL, KS, MI, MN, SD, MA, PA, and WA. For Section F, the control states are MA, PA, and WA. The additional DDD event study for fringe benefits in Section F is included to ensure better pre-treatment parallel trend assumptions.

B. Raw Trends

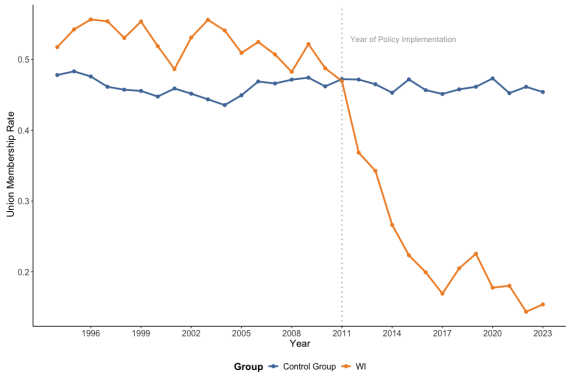


Figure 1: Union Membership Rate

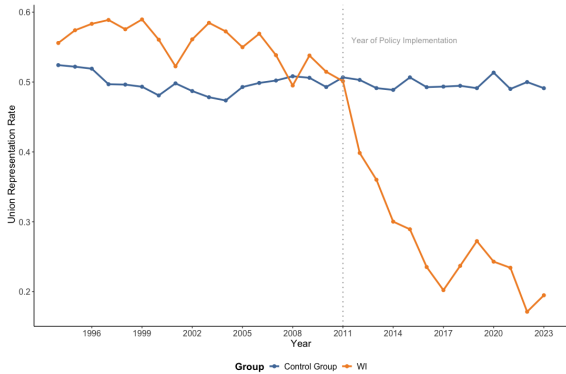


Figure 2: Union Representation Rate

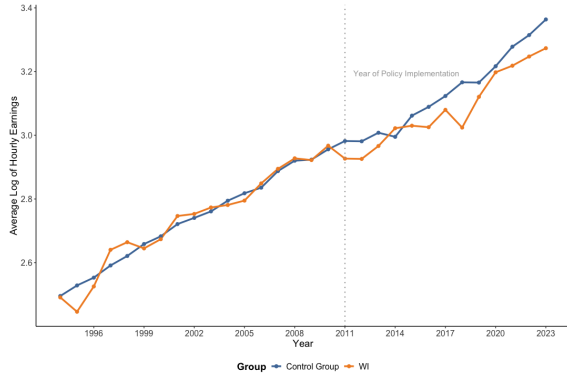


Figure 3: Log of Hourly Earnings

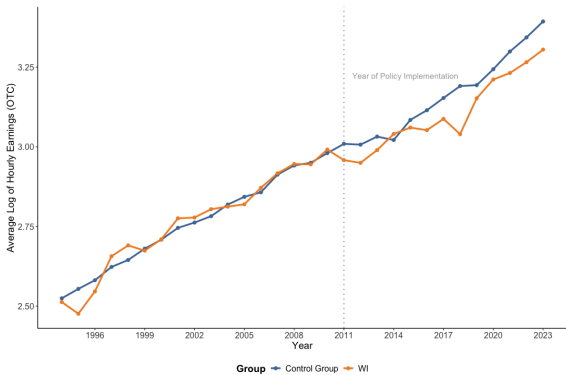


Figure 4: Log of Hourly Earnings (OTC)



Figure 5: Log of Weekly Earnings (OTC)

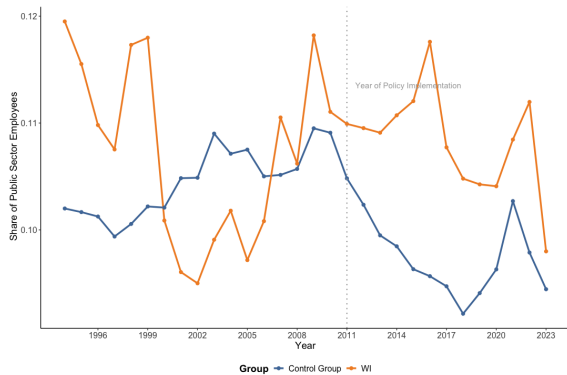


Figure 6: Share of Public Sector Employees

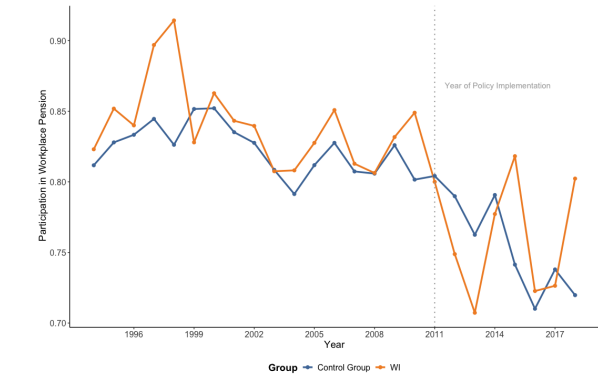


Figure 7: Participation in Workplace Pension

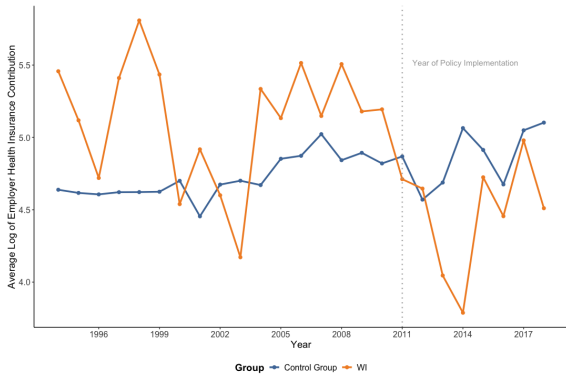


Figure 8: Log of Employer Health Insurance Contribution

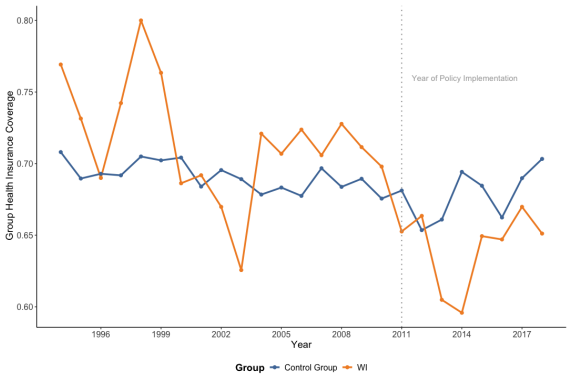


Figure 9: Group Health Insurance Coverage

C. DD: Event Study

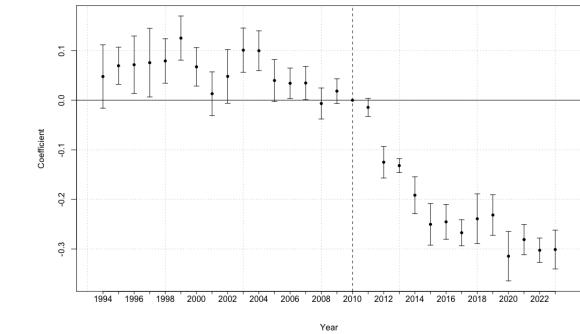


Figure 10: Union Membership Rate

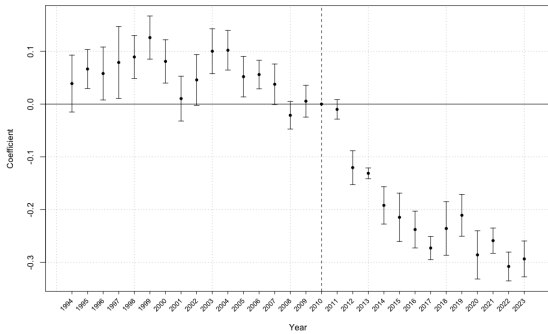


Figure 11: Union Representation Rate

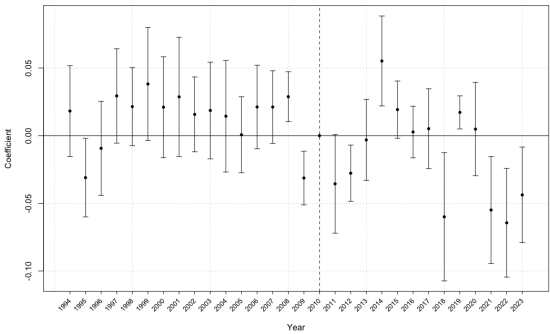


Figure 12: Log of Hourly Earnings

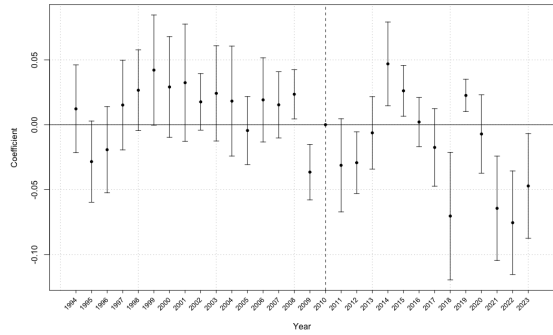


Figure 13: Log of Hourly Earnings (OTC)

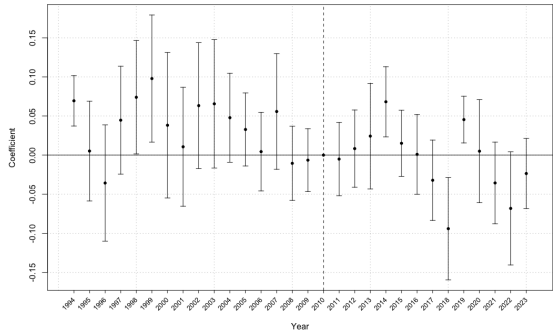


Figure 14: Log of Weekly Earnings (OTC)

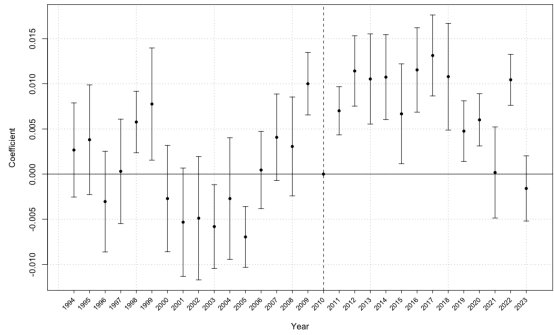


Figure 15: Share of Public Sector Employees

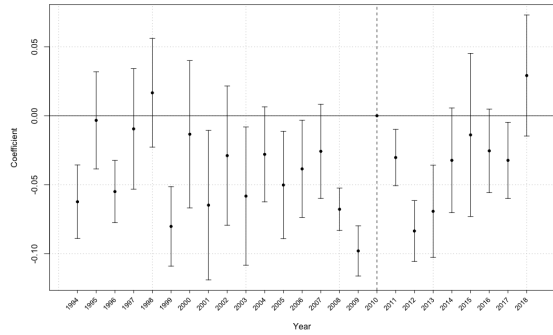


Figure 16: Participation in Workplace Pension

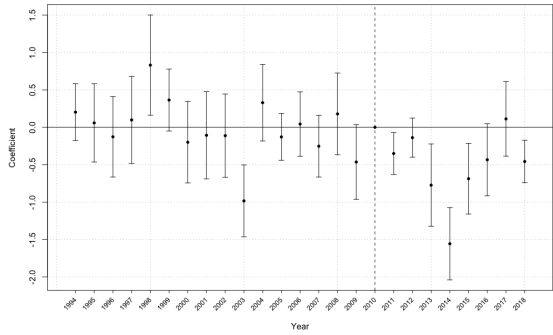


Figure 17: Log of Employer Health Insurance Contribution

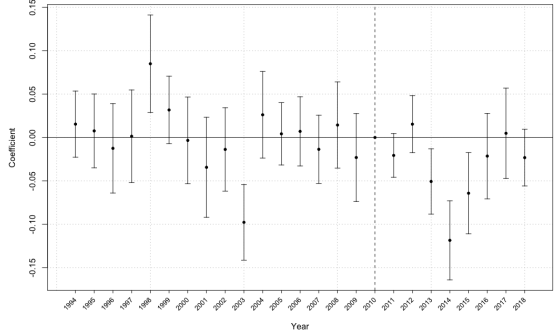


Figure 18: Group Health Insurance Coverage

D. DD: Sensetivity Analysis ("Honest" DD)

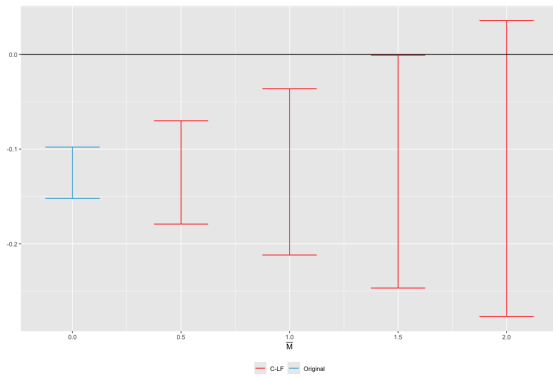


Figure 19: Union Membership Rate

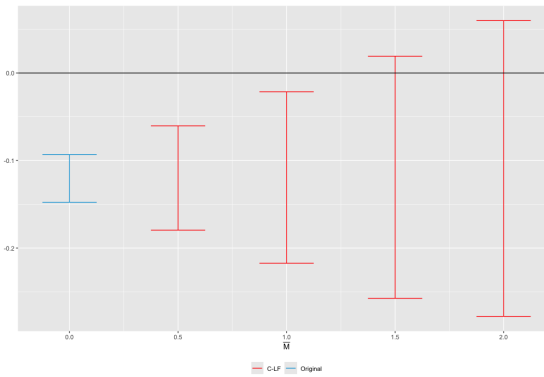


Figure 20: Union Representation Rate

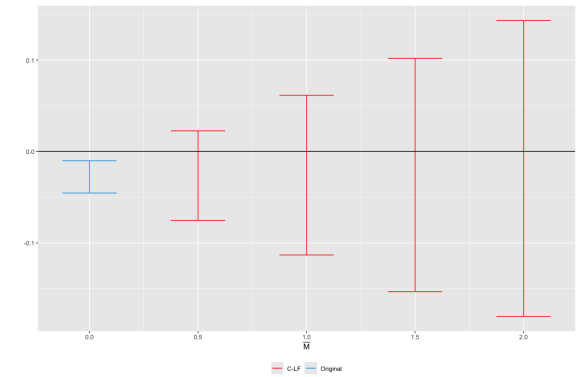


Figure 21: Log of Hourly Earnings

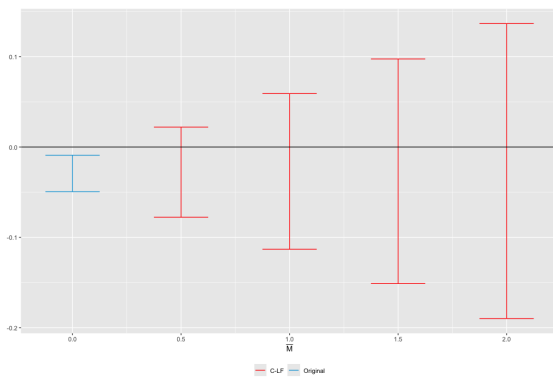


Figure 22: Log of Hourly Earnings (OTC)

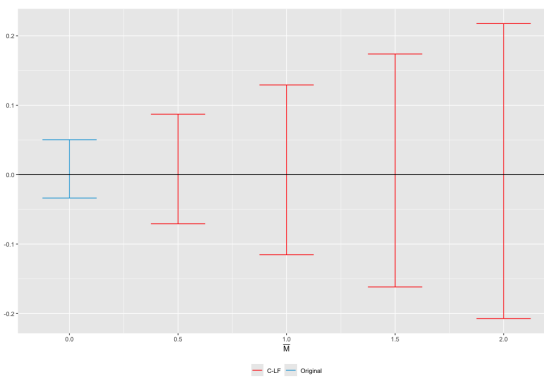


Figure 23: Log of Weekly Earnings (OTC)

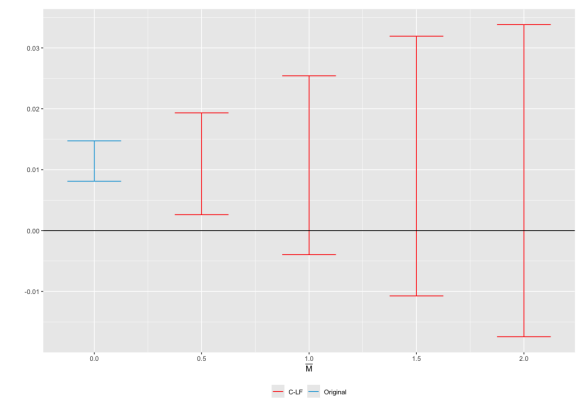


Figure 24: Share of Public Sector Employees

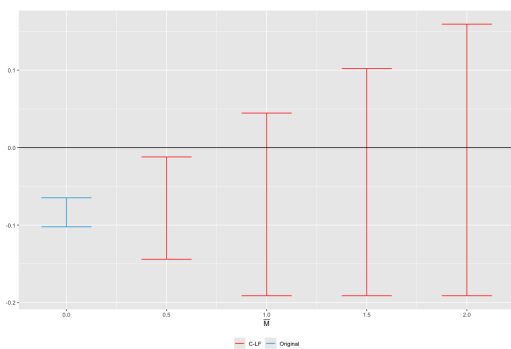


Figure 25: Participation in Workplace Pension

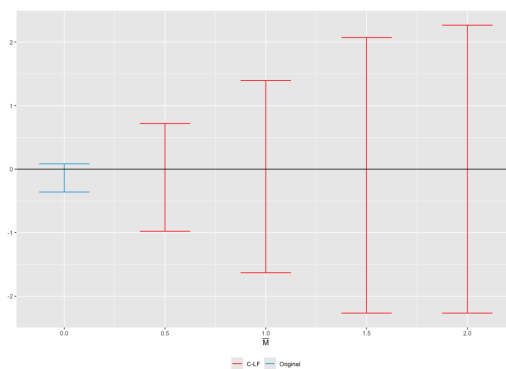


Figure 26: Log of Employer Health Insurance Contribution

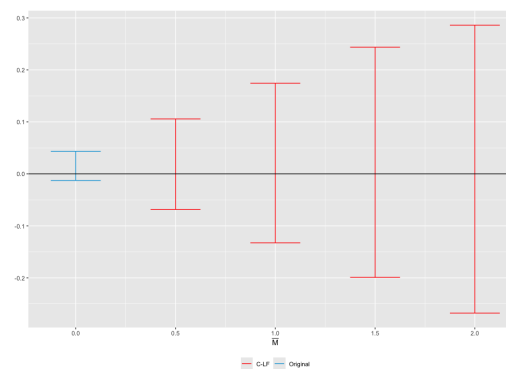


Figure 27: Group Health Insurance Coverage

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E. DDD: Event Study

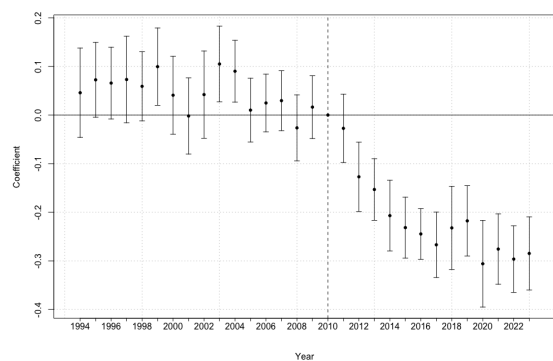


Figure 28: Union Membership Rate

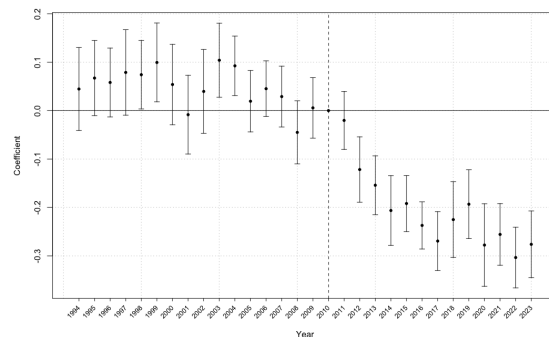


Figure 29: Union Representation Rate

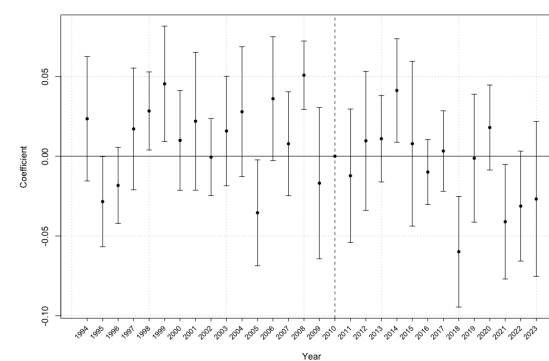


Figure 30: Log of Hourly Earnings

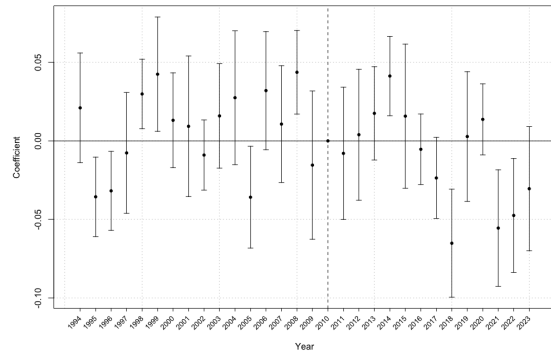


Figure 31: Log of Hourly Earnings (OTC)

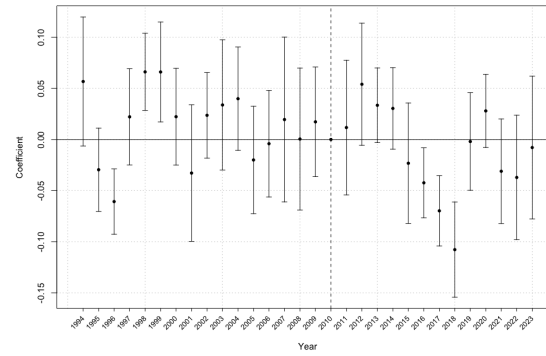


Figure 32: Log of Weekly Earnings (OTC)

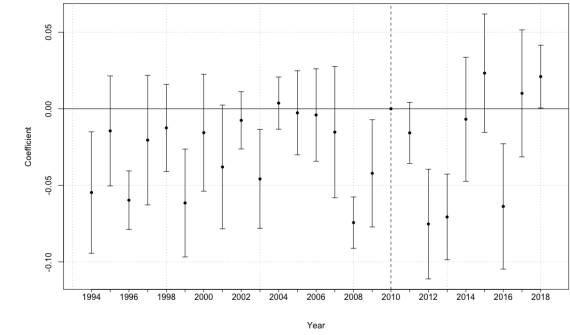


Figure 33: Participation in Workplace Pension

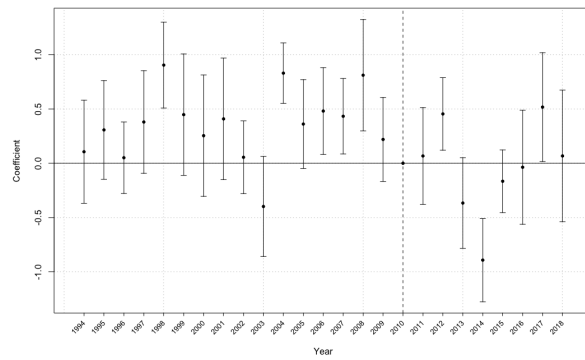


Figure 34: Log of Employer Health Insurance Contribution

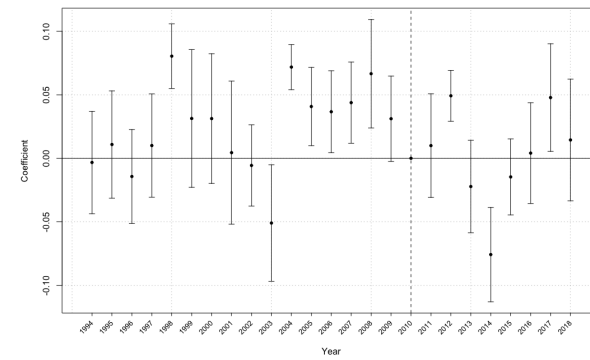


Figure 35: Group Health Insurance Coverage

F. DDD: Additional Event Study for Fringe Benefits

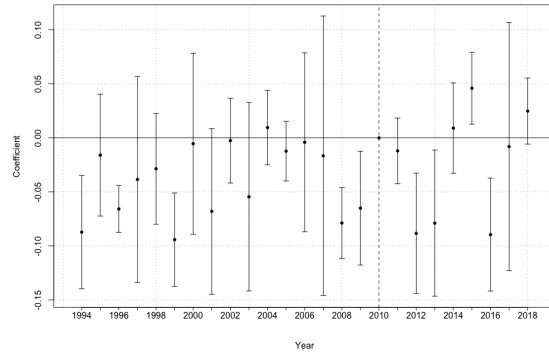


Figure 36: Union Membership Rate

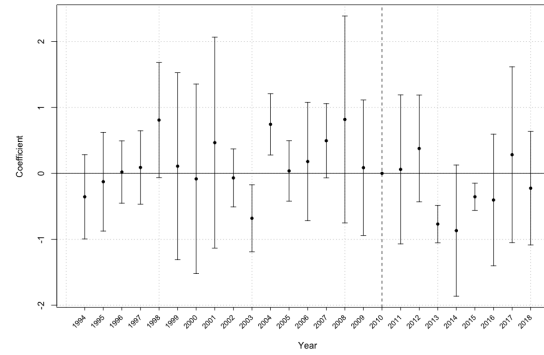


Figure 37: Union Representation Rate

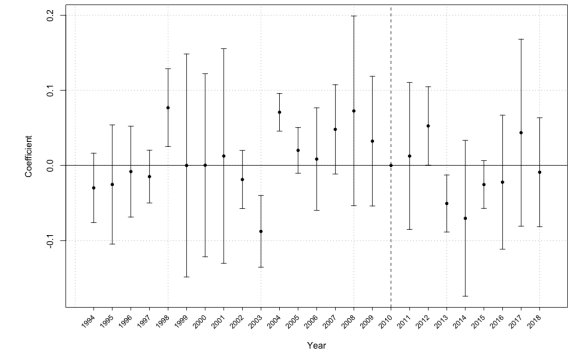


Figure 38: Log of Hourly Earnings

G. SC: Outcome Trajectories for Treated Unit and Synthetic Control Unit

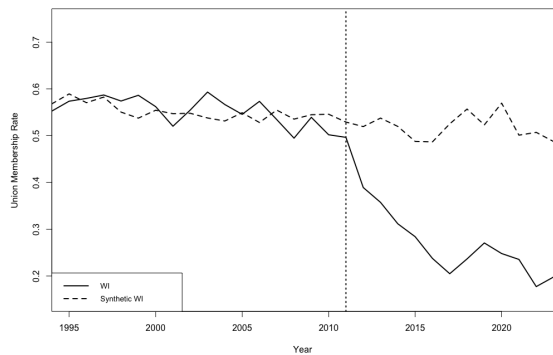


Figure 39: Union Membership Rate

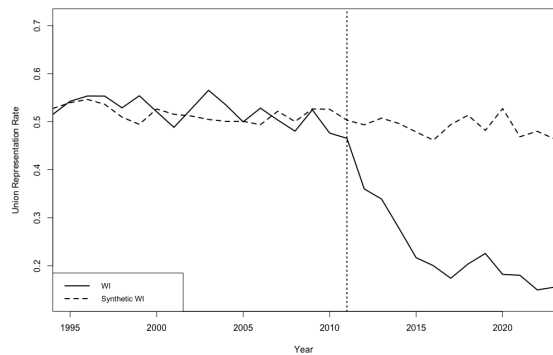


Figure 40: Union Representation Rate

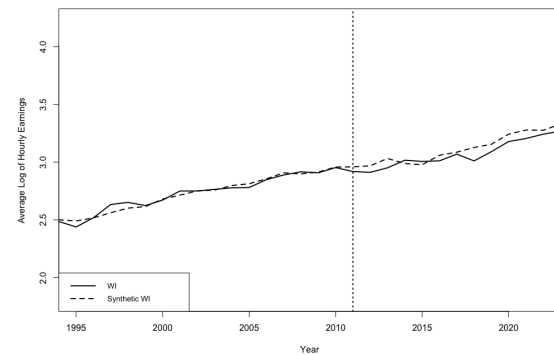


Figure 41: Log of Hourly Earnings

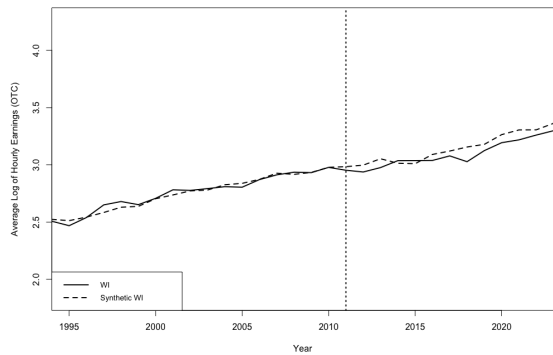


Figure 42: Log of Hourly Earnings (OTC)

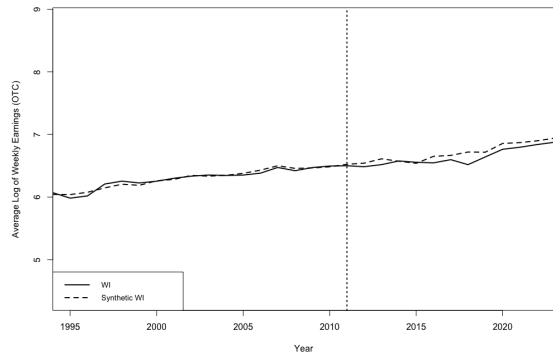


Figure 43: Log of Weekly Earnings (OTC)

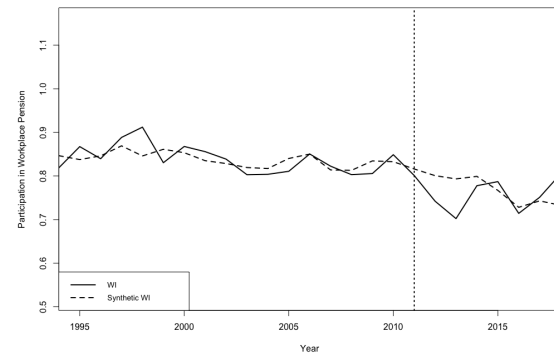


Figure 44: Participation in Workplace Pension

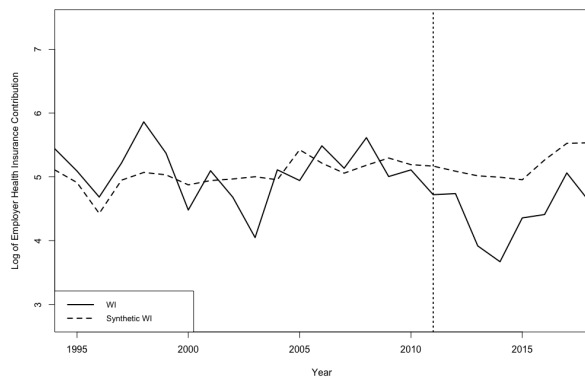


Figure 45: Log of Employer Health Insurance Contribution

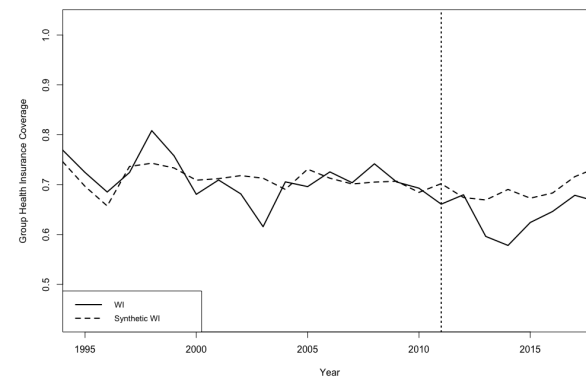


Figure 46: Group Health Insurance Coverage

H. SC: Gaps in Outcome Trajectories

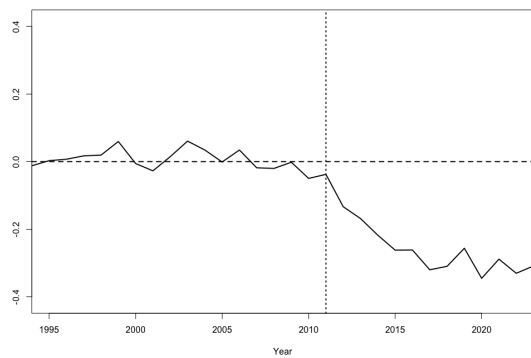


Figure 47: Union Membership Rate

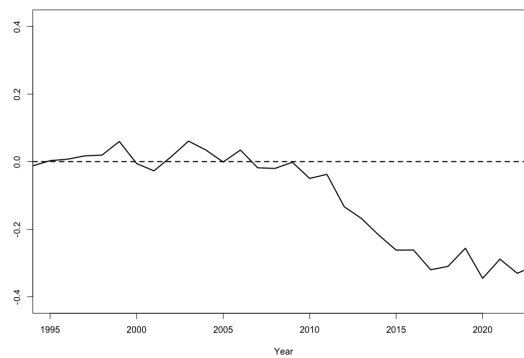


Figure 48: Union Representation Rate

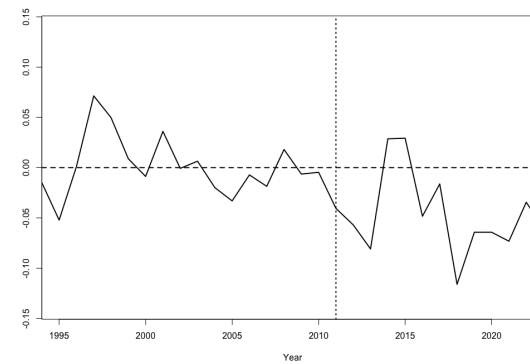


Figure 49: Log of Hourly Earnings

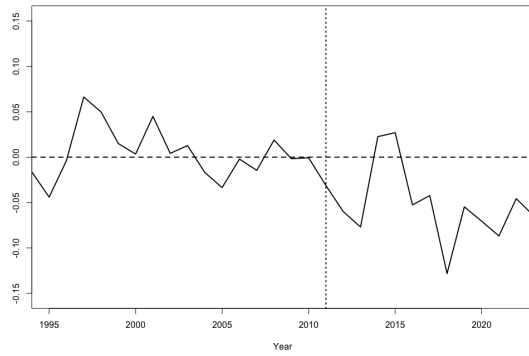


Figure 50: Log of Hourly Earnings (OTC)

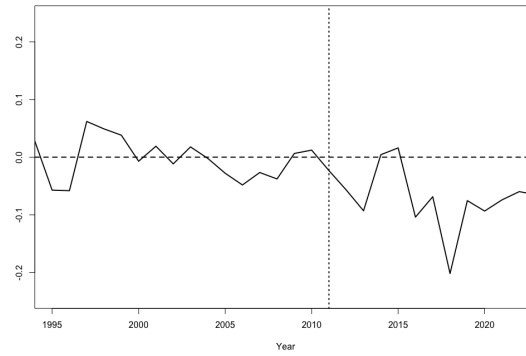


Figure 51: Log of Weekly Earnings (OTC)

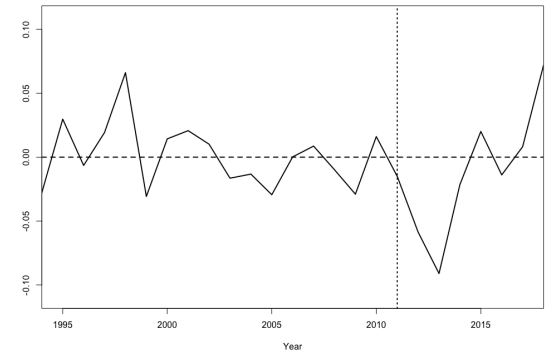


Figure 52: Participation in Workplace Pension

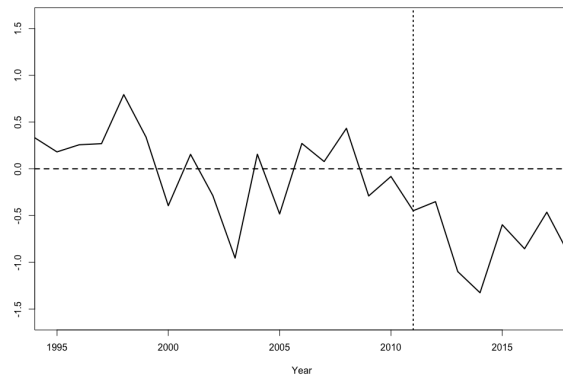


Figure 53: Log of Employer Health Insurance Contribution

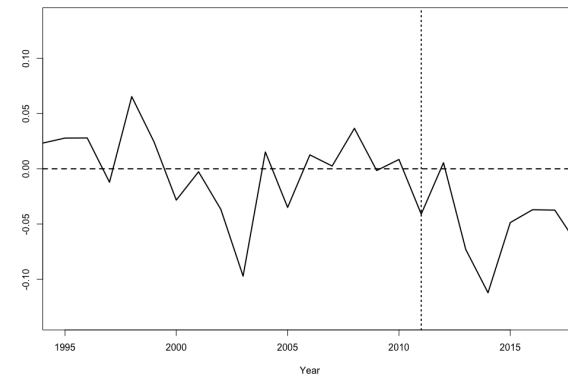


Figure 54: Group Health Insurance Coverage

I. SC: Placebo Gaps for Treated and Control Units

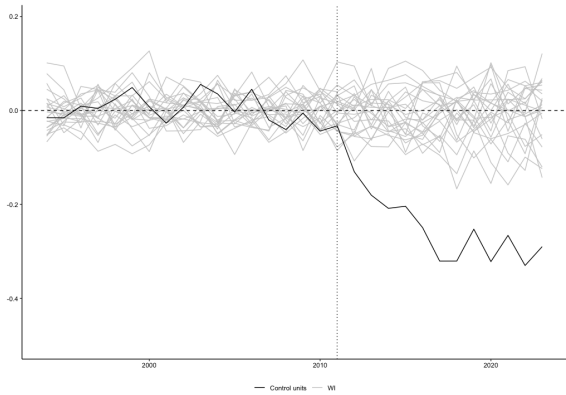


Figure 55: Union Membership Rate

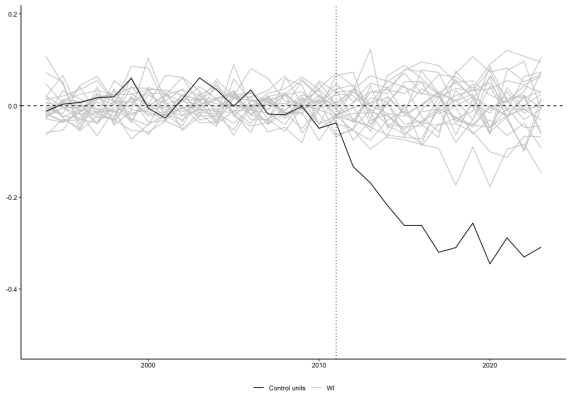


Figure 56: Union Representation Rate

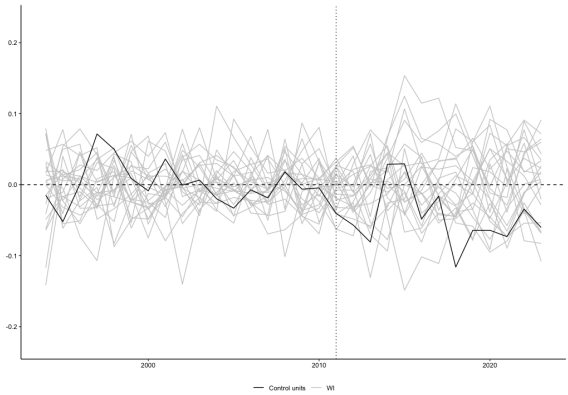


Figure 57: Log of Hourly Earnings

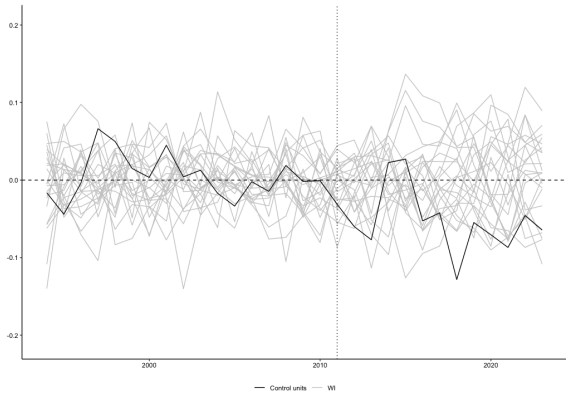


Figure 58: Log of Hourly Earnings (OTC)

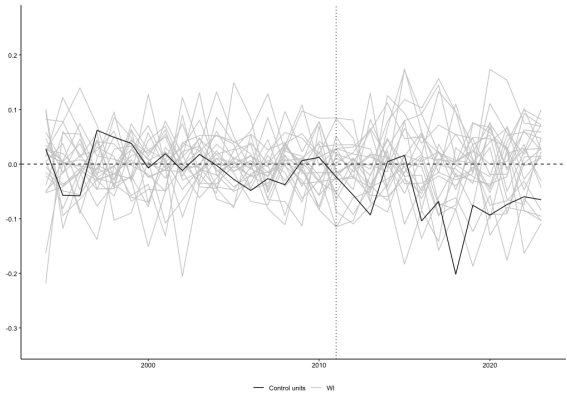


Figure 59: Log of Weekly Earnings (OTC)

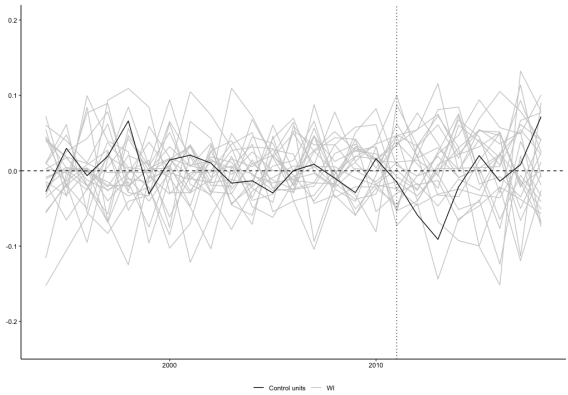


Figure 60: Participation in Workplace Pension

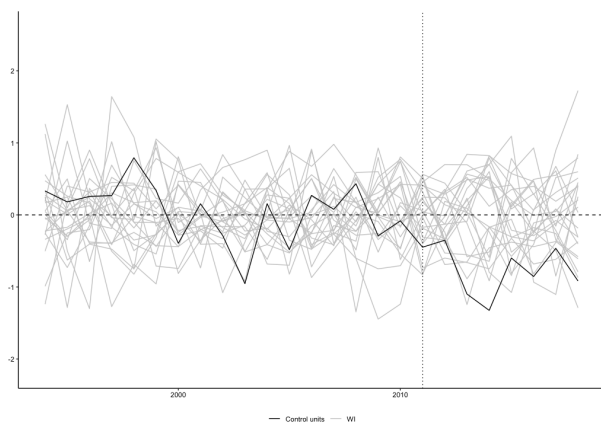


Figure 61: Log of Employer Health Insurance Contribution

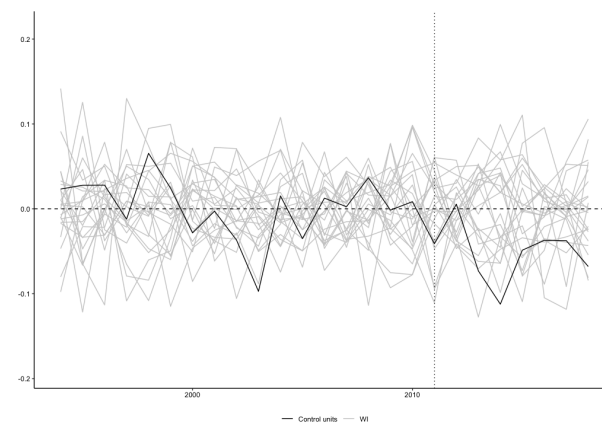


Figure 62: Group Health Insurance Coverage

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J. SC: Post/Pre-Treatment MSPE Ratio Plot

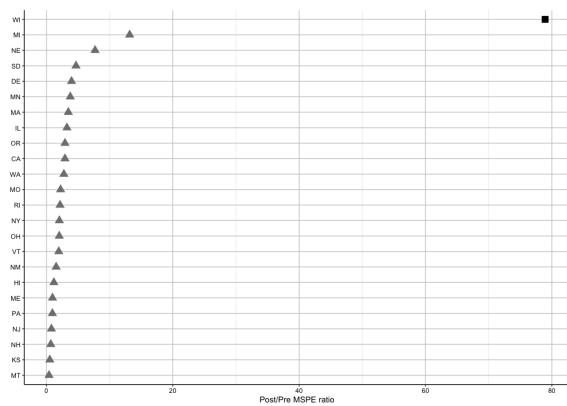


Figure 63: Union Membership Rate

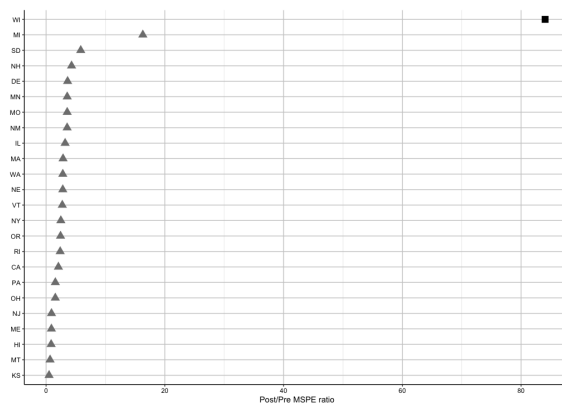


Figure 64: Union Representation Rate

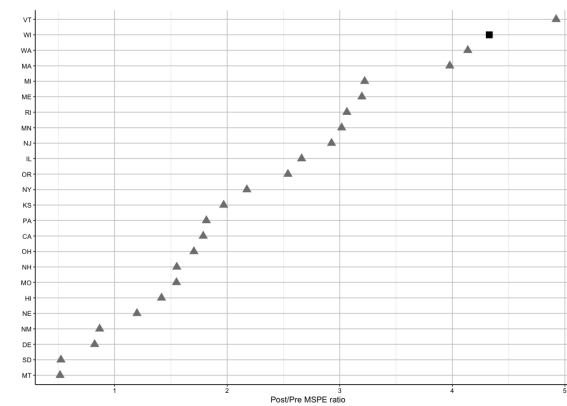


Figure 65: Log of Hourly Earnings

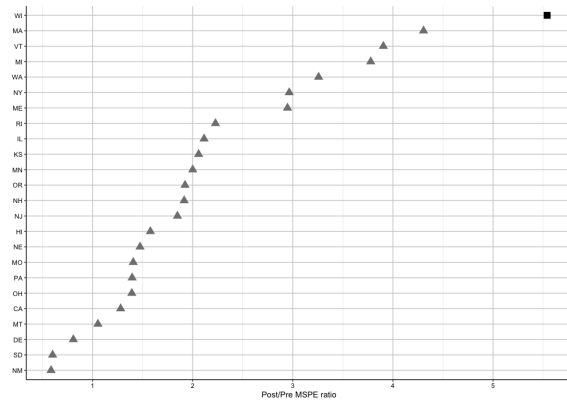


Figure 66: Log of Hourly Earnings (OTC)

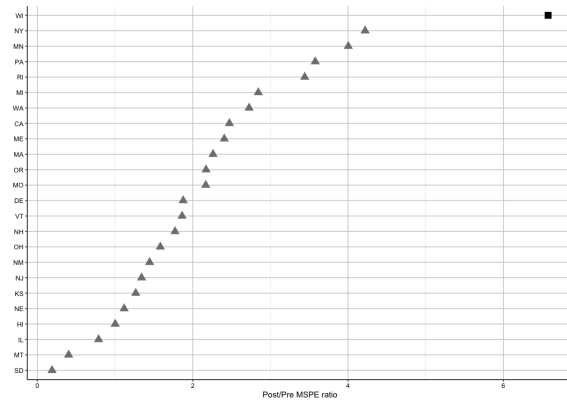


Figure 67: Log of Weekly Earnings (OTC)

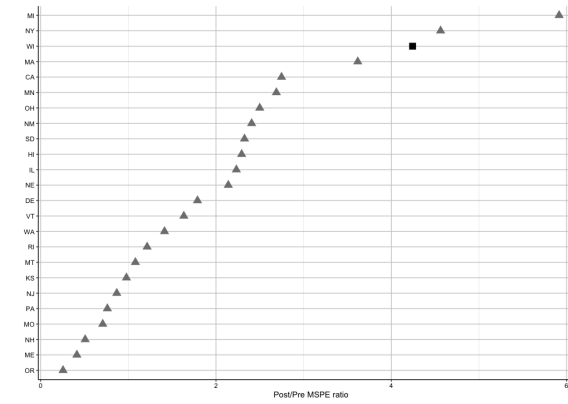


Figure 68: Participation in Workplace Pension

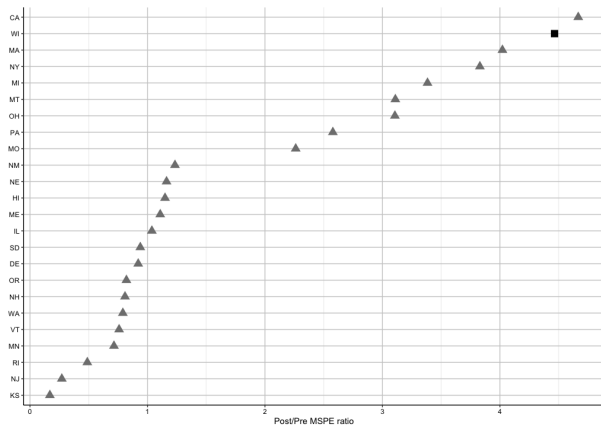


Figure 69: Log of Employer Health Insurance Contribution

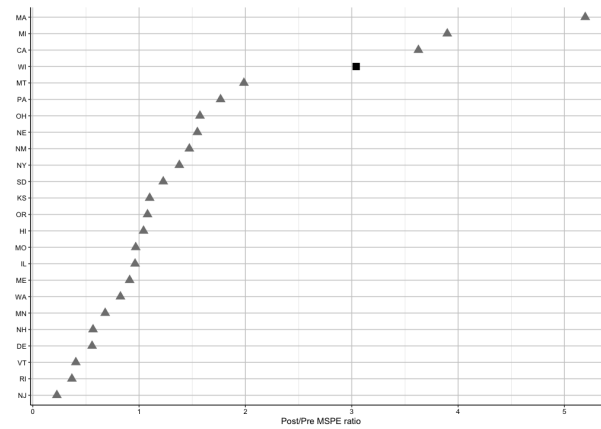


Figure 70: Group Health Insurance Coverage

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