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# Right-to-Work Laws and Labor Market Dynamics: Insights from the Recent Adoption Wave

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## Abstract

*This thesis examines the impact of right-to-work (RTW) laws on unionization rate, wages, and employment by leveraging data from the resurgence of these laws in five states over the past fifteen years. Using an event study design with staggered adoption, we find that RTW laws lead to a significant and immediate decline in unionization rate and a gradual increase in employment levels. However, our analysis does not provide evidence of any significant impact on hourly wages. We further strengthen our assessment by employing heterogeneity-robust estimators and assessing the sensitivity of our findings to potential violations of the common trends assumption. Finally, we demonstrate that the impact of RTW laws is not homogeneous across all workers. Less educated workers are most likely to leave unions, while highly unionized industries experience the largest declines in union membership.*

**Keywords:** right-to-work laws; unionization; staggered differences-in-differences.

**JEL classification:** J51, J83, J21, J23, J31.

*I extend my heartfelt gratitude to David Margolis for his invaluable assistance and availability during the writing of this master's thesis. Any errors are my own.*

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# 1 Introduction

What lies ahead for unionism? While unions in Western countries still garner media attention occasionally, they are experiencing a sustained decline in both membership and influence. In contrast, academic interest in these institutions is burgeoning, spurred by the availability of detailed microdata, such as the Current Population Survey (CPS), which tracks union membership. This interest is also driven by the escalating issue of wage inequality, a concern closely linked to union dynamics. Numerous studies have sought to establish a causal connection between these phenomena by examining the *union wage premium*, which refers to the wage differential between unionized and non-unionized workers, all other things being equal. The prevailing view in the scholarly literature suggests that unionism correlates with higher wages that are less variable (Callaway and Collins, 2018; Farber et al., 2021; Freeman, 1980). Furthermore, there is a robust argument in the literature that the decline in unionization in the West is a key factor contributing to the rise in wage inequality since the latter half of the 20th century (Card, 2001; Card et al., 2003; DiNardo et al., 1996; Farber et al., 2021; Western and Rosenfeld, 2011). However, the endogenous nature of unionization—where individuals opt to join unions and unions choose which jobs to cover during collective bargaining—complicates the clear delineation of a causal impact.

The primary focus of this thesis is on right-to-work (RTW) laws in the United States. These state-specific laws prevent companies from mandating union dues from employees, thereby diminishing union funding and services, which in turn accelerates membership decline (Fortin et al., 2023). Originating from Republican victories in state legislatures, these laws ensure that no employee can be forced to pay dues as a condition of employment, while still requiring unions to represent all employees during collective negotiations, as mandated by 29 U.S. Code § 159(a). Notably, of the 26 states with RTW laws, many enacted them shortly after World War II, with a resurgence of adoptions in the 2000s and 2010s. This “new wave of adoption” includes Oklahoma (2001), Indiana (2012), Michigan (2013), Wisconsin (2015), West Virginia (2016), and Kentucky (2017). The recency of these laws, coupled with the availability of microdata, offers a unique opportunity to quantitatively study their impact on labor market dynamics. The state-level framework of these adoptions permits the application of Difference-in-Differences (DiD) methodologies.

This thesis examines the effects of RTW laws on labor market outcomes—unionization, wages, and employment—within a public policy evaluation framework. Additionally, this research contributes to the body of literature concerning labor unions, positioning RTW laws as a shock that diminishes union resources, degrades service quality and quantity, and weakens collective bargaining power. This analysis draws on Moore et al. (1986) framework, which delineates three interrelated but distinct effects of RTW laws on unions. First, Moore et al. (1986) posit that RTW laws may not directly affect unions but rather reflect prevailing negative societal views towards unions. Second, they discuss the intensification of the free-rider problem, wherein unions are compelled to offer services despite diminished resources. Finally, they suggest that RTW laws may undermine unions’ leverage in negotiations with employers, irrespective of the union’s resource base. In addition to being influenced by subjective sentiments that are not directly

related to the objective conditions of unions, RTW laws can also impact wages through various spillover effects, independently of their direct effect on unionization rates, as discussed by Fortin et al. (2023). This underscores the endogeneity of RTW laws, which are correlated with a range of confounding factors. Even within a DiD framework, it remains challenging to isolate the specific effects of RTW laws from broader political or societal changes, spillover effects, and other concurrent influences. Consequently, the primary objective of this study is to conduct an ex-post assessment of the adoption of RTW laws on labor market outcomes, without interpreting these effects as causal relationships.

To investigate the impact of RTW laws on labor market outcomes, we utilize data from the CPS. This dataset offers a substantial sample size, information on wages, and a comprehensive array of explanatory variables, including the union status of employees. Our analysis is grounded in a theoretical model of collective bargaining, which enables us to identify the channels through which RTW laws could influence labor market outcomes (Manning, 1987). Our empirical methodology involves an event study analysis, which aims to quantify the effect of RTW laws on unionization, wages, and employment. We further assess the validity of our findings by applying heterogeneity-robust estimators (de Chaisemartin and D’Haultfœuille, 2023; Roth et al., 2023). These estimators are specifically designed to accommodate the staggered implementation of RTW laws across states, thereby avoiding the risks of inappropriate comparisons arising from heterogeneous treatment effects. We conduct sensitivity analyses to evaluate the robustness of our results against potential violations of the parallel trends assumption (Rambachan and Roth, 2023). Finally, we examine whether the effects of RTW laws differ across workers based on their educational background, as well as between industries with varying levels of unionization. Throughout this thesis, we emphasize the importance of reproducibility and transparency in research. To facilitate this, we provide the complete R program utilized to generate the results presented in this paper on GitHub<sup>3</sup>.

We find that RTW laws result in an immediate and stable decrease in unionization. This decline in union membership coincides with a gradual increase in employment. We do not identify any direct effects of RTW laws on overall wage levels. Nonetheless, these laws temporarily reduce the wages of non-educated workers; however, doubts about this result arise due to violations of the common trends assumption. Overall, our findings align with the results obtained from heterogeneity-robust estimations.

We enhance the existing literature by analyzing the impact of a recent legislative change on the American labor market. We extend previous studies on RTW laws by broadening the analysis to include employment levels, employing recent methodologies that effectively handle heterogeneity in treatment effects, and quantitatively assessing the sensitivity of our findings to potential violations of the parallel trends assumption.

The document is structured as follows: Section 1 provides an overview of the foundational elements pertaining to collective bargaining and RTW laws, alongside a theoretical framework that elucidates their interconnections. This section also delves into a critical review of the relevant literature and delineates the specific contributions of this thesis. Section 2 details the methodology employed, including data used, two-way fixed effects models, and heterogeneity-robust estimators. Section 3 presents a comprehensive

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<sup>3</sup>[https://github.com/JulienPeignon/master\\_thesis](https://github.com/JulienPeignon/master_thesis)

exposition of our empirical findings. [Section 4](#) evaluates the robustness of these findings through sensitivity analyses, with a focus on potential violations of the common trends assumption and an examination of results across different industries and worker groups. Finally, [Section 5](#) concludes the thesis.

## 1.1 Unions, Collective Bargaining, and Right-to-Work Laws

The legal framework governing collective bargaining in the private sector within the United States is anchored by the National Labor Relations Act (NLRA), commonly known as the Wagner Act, which was enacted in 1935. This pivotal legislation guarantees employees the right to organize into unions, engage in collective bargaining, and exercise their right to strike as outlined in 29 U.S. Code § 157. Within a unit of employees, a majority vote elects a single representative who becomes the exclusive agent for the group, authorized to negotiate with the employer on behalf of all employees, regardless of their union membership status (29 U.S. Code § 159(a)). Both the designated employee representative and the employer are mandated to engage in collective bargaining, with specific regulations in place to deter unfair or abusive practices by either party (29 U.S. Code § 158).

Initially, the NLRA allowed for three types of union security agreements, permitting the union and employer to mutually set terms for employee recruitment ([Baird, 1998](#)). Under a closed shop agreement, employees were required to be union members as a condition of employment. A union shop agreement allowed for the hiring of non-union employees, provided that they joined the union within a prescribed period. An agency shop required non-union employees to pay fees equivalent to the costs incurred by the union in collective bargaining.

This legal landscape shifted in 1947 with the adoption of the Labor Management Relations (LMR) Act, also known as the Taft-Hartley Act. This amendment, responding to the substantial strike activities of 1945 and 1946, aimed to curb union powers by introducing new unfair labor practices specific to unions. Among its various provisions, the Taft-Hartley Act explicitly prohibited closed shop arrangements (29 U.S. Code § 158(a)). Notably, Section 14(b) of the Taft-Hartley Act (29 U.S. Code § 164(b)) empowers U.S. states and territories to enact laws that ban all forms of union security agreements, including union and agency shops. These laws are commonly referred to as RTW laws:

*“Nothing in this subchapter shall be construed as authorizing the execution or application of agreements requiring membership in a labor organization as a condition of employment in any State or Territory in which such execution or application is prohibited by State or Territorial law.”*

In the United States, RTW laws prohibit union-security agreements that would require employees to pay agency fees covering collective bargaining costs or to join a union. Despite these restrictions, unions in RTW states or territories are still mandated to represent all employees within the bargaining unit fairly and equitably. However, they must do so with significantly reduced financial and human resources following the enactment of RTW laws. In its *Pattern Makers v. NLRB* decision of 1985, the Supreme Court outlawed

union shops ([Austin and Lilley, 2021](#)). Consequently, RTW laws now focus solely on prohibiting the agency shop, they prevent unions from requiring employees to pay agency fees meant to cover the costs of collective bargaining.

The Taft-Hartley Act primarily addresses the rights and regulations applicable to workers in the private sector, explicitly excluding those employed within the railway and airline industries, who are subject instead to the provisions of the Railway Labor Act. This exclusion renders state right-to-work laws non-applicable to these groups. State and local government employees are subject to the regulatory frameworks established by individual states, whereas federal employees fall under the purview of Title VII of the Civil Service Reform Act. Notably, states that have adopted right-to-work laws for private sector employees typically extend these legal protections to encompass state and local government workers as well ([Baird, 1998](#)). Furthermore, in 2018, the US Supreme Court decided in *Janus v. AFSCME* that requiring agency fees in the public sector violates the First Amendment, thereby rendering the entire public sector right-to-work.

As of the latest data, 26 states along with the territory of Guam have implemented RTW laws (National Labor Relations Board), with the bulk of these laws being enacted in the late 1940s and early 1950s subsequent to the Taft-Hartley Act. States such as Texas, typically “*red states*” with conservative leanings, were among the early adopters. A new wave of RTW adoptions began in the 2000s, starting with Oklahoma in 2001. Subsequent adopters include Indiana (2012), Michigan (2013), Wisconsin (2015), West Virginia (2016), and Kentucky (2017). In February 2024, Michigan became the first state to repeal its RTW law, thereby reducing the number of right-to-work states from 27 to 26 (Michigan, Public Acts of 2023, Act No. 8). [Table 4](#) presents a summary of the years in which each state adopted the RTW law, and [Figure 13](#) shows the geographical repartition of these adoptions. The recent enactments have sparked a resurgence of academic interest, particularly as contemporary adoptions can be analyzed using microdata previously unavailable, offering new insights into the impact of RTW laws.

These laws, as [Fortin et al. \(2023\)](#) details, were enacted in the context of local electoral victories by the Republican Party, which influenced their implementation. In Michigan, Wisconsin, and Kentucky, the adoption of a RTW law was preceded by a political shift from Democratic to Republican control in gubernatorial elections. In these three states, Republicans achieved a trifecta, controlling the governorship as well as both state legislative chambers. Indiana did not experience a political switch but confirmed their Republican authorities in a gubernatorial election four years prior to the enactment of the RTW law. Finally, West Virginia is the only RTW state under Democratic control, although the Democrats do not hold a trifecta there. At the time of its adoption in February 2016, West Virginia Code § 21-1A-3 was enacted under a Republican majority in both legislative chambers. The Senate comprised 18 Republicans and 16 Democrats, mirroring the House of Delegates, where Republicans controlled 64 of the 100 seats (West Virginia Legislature). We draw on [Fortin et al. \(2023\)](#) to summarize these political events in [Table 5](#). Given this context, which introduces numerous potential confounding factors, we do not aim to assess a



causal effect of RTW laws on labor market dynamics. Instead, we admit that our evaluation of the law's impact may be influenced by unobserved factors, such as a negative shift in societal views towards unions or the spillover effects mentioned earlier.

## **1.2 Theoretical Framework of Collective Bargaining**

To model collective bargaining dynamics and highlight the potential impacts of RTW legislation on negotiation, we employ the sequential bargaining framework of [Manning \(1987\)](#). In this model, unions and employers first negotiate either over wages or employment, and then bargain over the remaining aspect, either employment or wages, respectively. This sequential setting enables unions and employers to have distinct bargaining powers over wages and employment. According to [Manning \(1987\)](#), negotiations in the United States typically commence with discussions on wages. Furthermore, wages are classified as mandatory bargaining issues, thereby obligating employers to negotiate them with unions ([Gospel, 1983](#)). Employment is categorized as a permissive issue and does not require mandatory bargaining. Therefore, as concluded by [Manning \(1987\)](#), in the United States we can apply a framework wherein unions begin by negotiating wages. In these negotiations, unions possess greater bargaining power than when they negotiate over employment in a second stage. Conversely, as employment is classified as a permissive issue, employers can negotiate more easily on it than on wages.

Within this framework, we can further elaborate on how RTW laws are expected to impact negotiations between unions and employers. RTW laws undermine union finances by exacerbating a free-rider problem, wherein unions are compelled to negotiate on behalf of individuals who no longer pay fees. Considering unions as service providers, they are required to maintain the same level of service with fewer resources, consequently diminishing their quality. Therefore, unions will lose bargaining power, affecting both wages and employment decisions. Unions already possess limited influence over employment matters, so we anticipate that this diminished bargaining power will lead to more significant consequences during wage negotiations. We expect hourly wages to decrease due to the shift in the power balance favoring employers. In the second stage of negotiations we anticipate an increase in employment levels in response to lower wages, because it benefits both employers and workers.

In summary, by adopting a sequential bargaining framework, we anticipate that the reduction in unionization caused by RTW laws will lead to a decrease in wages and an increase in employment.

## **1.3 Literature Review on Right-to-Work Laws**

The recent proliferation of RTW law enactments has ignited a fresh wave of academic studies. These studies predominantly consist of working papers or articles published in lesser-known journals due to the contemporaneity of the subject. No consensus has yet emerged regarding the methodologies or outcomes of these studies. Historical discourse on RTW laws suggests a divide: some assert a detrimental impact on wages ([Carroll, 1983](#); [Garofalo and Malhotra, 1992](#)), while others argue that these effects are marginal

(Moore, 1980; Wessels, 1981). A comprehensive review of this body of literature is provided by Moore and Newman (1985). This section aims to delineate the varied methodologies utilized in recent research, synthesize the emerging results—whether aligned or disparate—and discuss the inherent limitations of these studies.

Chava et al. (2020) analyzed data from collective bargaining agreements to examine the impact of RTW laws on nominal wage growth. Employing a Difference-in-Differences approach, their findings suggest a significant adverse effect on wage growth. However, their analysis stops short of exploring the implications on real wages, as collective agreements typically stipulate only the nominal wage adjustments over a triennial period. They empirically document the unions' loss of bargaining power with two observations: firstly, RTW laws substantially decrease the number of collective agreements, with some firms becoming de-unionized due to financial constraints; secondly, they exacerbate the free-rider problem, depicted by the disparity between the number of employees a union covers and its actual membership. Additionally, by utilizing CRSP-Compustat merged data, Chava et al. (2020) demonstrated an uptick in corporate investments and labor demand subsequent to the enactment of RTW laws. Nevertheless, the reliance on a DiD methodology, without considering recent methodological advances, introduces potential biases (de Chaisemartin and D'Haultfœuille, 2023; Roth et al., 2023). Recent scholarly work highlights possible incorrect estimations in studies where the treatment implementation varies temporally, casting doubt on the robustness of their conclusions.

Austin and Lilley (2021) utilize a border-pair differences approach at the county level, leveraging Local Area Unemployment Statistics from the Bureau of Labor Statistics. Their findings indicate that RTW laws positively impact manufacturing employment, labor force participation, and overall employment. However, the data do not enable them to discern the effect of RTW laws on wages, as they cannot differentiate between changes in wages and variations in hours worked. Furthermore, the border-pair difference methodology of Austin and Lilley (2021) lends strong support to the internal validity of their results. Nevertheless, the external validity of these findings may be subject to scrutiny due to the highly localized approach of the methodology used.

Finally, Fortin et al. (2023) aligns closely with our thesis objective: evaluating the impact of RTW laws on key labor market outcomes. We will frequently reference this study for two primary reasons. First, it provides a foundational framework from which we can further develop our research. Confirming similar results using analogous methodologies and data will substantiate the robustness of our findings. Second, we aim to address and rectify the methodological limitations identified in their study, which we will explore in further detail. Fortin et al. (2023) employ a DiD approach using CPS data and finds a negative impact of RTW laws on wages and unionization. Regarding unionization rate, they report a reduction of 1.7 percentage points in the first year, which widens to 4 percentage points after five years, cumulating in an average effect of -1.85 percentage points over the period from the treatment date to 2019. In terms of wages, while most of the estimated effects are statistically insignificant, exceptions occur in the third and fifth years after treatment, with an overall estimated decrease of 1.23%. We reproduce their results in Figure 14 to facilitate comparison with our findings in subsequent sections. They also



employ a differential exposure design method, yielding similar results. However, [Fortin et al. \(2023\)](#) do not explore the effects of RTW laws on employment. Moreover, while they acknowledge the recent developments in DiD methodology, they do not fully incorporate these insights into their analysis. They correctly identify two potential sources of bias within a staggered adoption framework: heterogeneity of effects over time and across states. They note that the event study framework, by estimating relative time-to-treatment effects and controlling for yearly fixed effects, is safeguarded against biases stemming from temporal heterogeneity. Nonetheless, they appear to disregard the potential for biases due to treatment heterogeneity between states. They merely state that the “forbidden comparisons” do not produce negative weights in their computation of the Average Treatment Effect—a problem highlighted by [de Chaisemartin and D’Haultfœuille \(2020\)](#)—yet in their robustness checks, they fail to incorporate any estimates using heterogeneity-robust estimators from recent literature, such as those proposed by [Callaway and Sant’Anna \(2021\)](#) or [Borusyak et al. \(2024\)](#). Finally, they do not quantitatively assess the extent to which their results are sensitive to violations of the common trends assumption. The importance of transparency regarding the sensitivity of findings to assumption failures is increasingly emphasized in the literature, and, for example, recent methodologies for such assessments are provided by [Rambachan and Roth \(2023\)](#).

## 1.4 Contributions to Existing Literature

This thesis contributes to the literature on public policy evaluation related to the labor market. Existing literature extensively explores the impacts of laws and policies on labor market adjustments ([Biasi and Sarsons, 2021](#); [Farber et al., 2021](#)). Our research extends this inquiry by analyzing the recent implementation of RTW laws.

Moreover, we enhance previous assessments of RTW laws by directly addressing the problem of staggered adoption within Two-Way Fixed Effects (TWFE) estimations. We employ heterogeneity-robust estimators that are specifically designed to avoid “forbidden comparisons”, *i.e.*, when newly treated states serve as control for later treated one within the DiD framework ([de Chaisemartin and D’Haultfœuille, 2023](#); [Roth et al., 2023](#)).

Finally, we use recent methodologies to assess the sensitivity of our findings to potential violations of the parallel trends assumption ([Rambachan and Roth, 2023](#); [Roth and Sant’Anna, 2023](#)). This approach allows us to transparently quantify the degree of confidence we can place in our findings, a step that has not been taken in previous studies on RTW laws.

## 2 Methodology

### 2.1 Reproducibility

This thesis contributes to the field of reproducible research by promoting and implementing transparency in scientific investigations. We provide the code and data that generate the findings of this thesis. The entire source code is publicly available on GitHub<sup>4</sup>. To ensure complete transparency and reproducibility, we follow the programming and data management standards recommended by the National Institute of Statistics and Economic Studies<sup>5</sup> (Insee). Our code adheres to the Tidyverse style guide for readability and uses `renv` to maintain a consistent programming environment. Our project is organized in a modular way, enhancing understanding, with detailed documentation available in the `README.md` on our GitHub page and in the [GitHub Repository Structure](#).

Additionally, researchers looking to replicate our results can access the exact dataset used in this study. We employ an Application Programming Interface (API) developed by [Flood et al. \(2023\)](#) to download our data. For elements not available through the API, we use Git Large File Storage, allowing for their download via GitHub to ensure all necessary data components are available for full reproducibility.

### 2.2 Data Sources and Descriptive Statistics

The data for this study are derived from the Current Population Survey (CPS), a monthly survey sponsored by the Bureau of Labor Statistics and conducted by the U.S. Census Bureau. This survey encompasses approximately 60,000 households each month. These households are surveyed over a period of four consecutive months, followed by an eight-month hiatus, and then surveyed again for another four months. The CPS collects extensive information pertaining to employment and socio-demographic characteristics of the respondents. Of particular relevance to our analysis is the collection of data on respondents' union status. In March, the basic monthly CPS is augmented with the Annual Social and Economic Supplements (ASEC), which include additional observations from individuals whose wages and hours worked are collected.

We utilize data from the CPS spanning from January 2007 to December 2022. It covers the period from five years before RTW law adoption in Indiana (2012) to five years after the last adoption in Kentucky (2017). Similar to [Fortin et al. \(2023\)](#), we do not study the effect of the RTW law in Oklahoma, which was enacted in 2001. Our analysis focuses on the following five states: Indiana, Michigan, Wisconsin, West Virginia, and Kentucky ([Table 4](#)).

Data retrieval is facilitated through an API provided by IPUMS ([Flood et al., 2023](#)), a project designed to harmonize individual-level population databases. The initial dataset, prior to any cleaning processes, comprises approximately 25,200,000 observations. Given our objective to assess the impacts of RTW laws on unionization, wages, and employment, three distinct datasets are required. The *employment dataset*

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<sup>4</sup>[https://github.com/JulienPeignon/master\\_thesis](https://github.com/JulienPeignon/master_thesis)

<sup>5</sup><https://inseefrlab.github.io/formation-bonnes-pratiques-git-R/>

includes both employed and unemployed respondents within the active population, as defined by the International Labour Organization<sup>6</sup>. The *union dataset*, slightly smaller, encompasses only employed individuals. Lastly, *wage dataset* is considerably smaller because it includes only individuals for whom we have labor income data. This section is dedicated to a comprehensive description of the selection and cleaning processes applied to our data, followed by a quantitative depiction through descriptive statistics. The methodology employed for data processing is based on that of DiNardo et al. (1996), which has been notably utilized in subsequent studies such as those by Fortin et al. (2021) and Fortin et al. (2023).

We define our study population as individuals within the working age bracket. Specifically, we include those aged 15 to 65 years. We calculate potential work experience as the maximum of  $age - 6 - years\ of\ education$  and zero, trimmed at 40 years. Following Lemieux (2006), we include only workers with strictly positive potential experience<sup>7</sup>. The adjustment of six years to the calculation of potential experience is predicated on the typical starting age for formal education. Conventionally, educational tracking begins from the first grade, at which most individuals are approximately six years old. Thus, the term *years of education* inherently accounts for the span beginning from this age. In the raw dataset, the variable *education* is defined as either the highest degree obtained or the last year of education completed by an individual. Following the methodology outlined by DiNardo et al. (1996), we transform this categorical variable into a numerical one by assigning values that represent the minimum number of years necessary to achieve the respective educational level or degree. For instance, a designation of 12 years of education implies that the individual discontinued formal schooling upon completion of the 12th grade. Conversely, the upper limit of 20 years of education corresponds to an individual who has attained a doctoral degree (PhD).

Next, we omit sectors under special labor law regimes as delineated in Subsection 1.1. These sectors, which are not directly influenced by the RTW laws under investigation, include public sector employees at federal, state, and local levels, as well as employees in the aviation and railway sectors. Following the methodology of DiNardo et al. (1996), we also exclude workers who are not typically affected by unionization and, by extension, RTW laws. This group comprises self-employed individuals, unpaid family workers, members of the armed forces, and employees in the agriculture, forestry, and fisheries sectors. Additionally, we exclude states that enacted RTW legislation prior to 2007. These states are considered “always treated” and thus ineligible to serve as control groups in our analysis. After applying these filters, the initial dataset, initially comprising approximately 25,200,000 observations, is reduced to around 4,750,000 (Table 6). It is the *employment dataset*, used to analyze the effects of RTW laws on employment as it encompasses both employed and unemployed workers. The *union dataset*, which only contains individuals who are currently employed, consists of approximately 4,400,000 observations.

<sup>6</sup> “According to the International Labour Organization (ILO) definition, the active population includes employed and unemployed persons, both concepts defined according to the ILO definitions. An unemployed person as defined by the ILO is a person aged 15 or over who simultaneously meets three conditions: being unemployed for a given week; being available to take a job within two weeks; having actively sought a job in the last four weeks or having found one starting in less than three months. A person in employment is a person aged 15 or over who has done at least one hour’s paid work in a given week, or who is absent from work for certain reasons (annual leave, sickness, maternity, etc.) and for a certain period of time.” (Insee)

<sup>7</sup> By applying the constraint  $0 < \text{potential experience} \leq 40$ , we exclude approximately 560,000 observations.

To construct the *wage dataset* from the *union dataset* several additional data processing steps are required. Initially, we exclude allocated wages<sup>8</sup> from the analysis. As demonstrated by [Hirsch and Schumacher \(2004\)](#), these wages are often imputed to workers irrespective of their union status, thereby introducing an attenuation bias. Moreover, to address the issue of top-coded wages, we adjust their values by multiplying them by a factor of 1.4, following [Lemieux \(2006\)](#). Additionally, we deflate the wages to express them in constant 1999 dollars.

Our dataset includes three types of labor income: annual salary, weekly salary, and hourly wage. Whenever hourly wage data is available, it is retained. In cases where only annual or weekly salary data is available, we compute the hourly wage by either dividing the annual salary by the product of the typical number of weeks worked per year and the average hours worked per week, or by dividing the weekly salary by the average number of hours worked per week. This methodology ensures that we derive a consistent measure of deflated hourly wages for all individuals with available labor income data. Finally, before applying the logarithmic transformation, we limit the scope of our analysis to deflated hourly wages ranging between 1\$<sub>1999</sub> and 200\$<sub>1999</sub><sup>9</sup>. Consequently, the *wage dataset* comprises approximately 600,000 observations ([Table 6](#)), which is significantly smaller than the *union dataset*. This reduction in size is primarily due to the unavailability of labor income data for all individuals.

Having detailed our preprocessing procedures and defined the properties of the three datasets (*employment*, *union*, and *wage datasets*), we proceed to quantitatively characterize our variables. Comprehensive quantitative descriptions of all variables employed in subsequent analyses are provided in the appendix. [Table 6](#) presents the statistical summaries of numerical variables. Additionally, the distribution of each label within the categorical variables across the three datasets is systematically documented. Specifically, [Table 8](#) delineates the racial demographics, [Table 10](#) details marital status, and [Table 12](#) presents citizenship statuses. Further, [Table 14](#) describes the birthplaces of parents, [Table 16](#) categorizes employment statuses, and [Table 18](#) details the distribution between full-time and part-time employment statuses. The industry sectors are outlined in [Table 20](#), education levels in [Table 22](#), and the levels of potential experience in [Table 24](#). Overall, the analysis of the results reveals no inconsistencies in the variables' details.

Each table presenting descriptive statistics is accompanied by a corresponding table of T-tests. These tests are performed to determine whether there are statistically significant differences in the sample compositions across our three datasets. For example, it evaluates whether there are significant variations in the proportions of women or unionized employees among the datasets. The T-tests conducted reveal that despite the qualitative similarities in the compositions of our datasets, the large sample sizes allows the identification of significant differences across nearly all variables. This finding substantiates our decision to incorporate a correction for sample selection in our models. The *union* and *wage datasets* are limited to employed individuals, thereby selecting a distinct segment of the population that has access to employment and possesses specific characteristics. In this context, the [Heckman \(1979\)](#) selection model, discussed in

<sup>8</sup>Allocated wages are estimated earnings assigned to individuals when they refuse to report their income or when household members reporting for them can't provide this information.

<sup>9</sup>This restriction excludes approximately 2,000 observations.

Subsection 2.5, yields robust estimates by addressing the non-random nature of sample selection, under the assumption of joint normality between the disturbance terms of the selection equation and those of the main equations.

Table 1: Union Coverage and Membership by Industry in Employment, Union, and Wage Datasets

Industry category	<i>Employment dataset</i>		<i>Union dataset</i>		<i>Wage dataset</i>	
	Union coverage	Union membership	Union coverage	Union membership	Union coverage	Union membership
Business and personal services	0.15%	0.96%	0.16%	1.05%	0.44%	3.19%
Construction	0.17%	4.23%	0.20%	4.83%	0.50%	14.75%
FIRE <sup>10</sup>	0.13%	0.50%	0.14%	0.52%	0.32%	1.22%
Manufacturing	0.17%	2.47%	0.18%	2.63%	0.45%	8.96%
Trade	0.12%	1.10%	0.13%	1.19%	0.38%	3.81%
Transportation and utilities	0.24%	4.10%	0.26%	4.36%	0.64%	13.35%
Welfare and education	0.27%	2.06%	0.28%	2.15%	0.58%	5.26%
Sample mean	0.18%	1.90%	0.20%	2.04%	0.48%	6.06%

*Note:* This table presents the average union coverage and union membership across each dataset and industry category. Union coverage refers to individuals whose employment contracts are protected by a union, although they are not members of the union themselves.

Next, we consider the union coverage and unionization rates across selected industry sectors, presented in Table 1. Union coverage refers to individuals whose employment contracts are protected by a union, although they are not members of the union themselves<sup>11</sup>. This analysis enables us to pinpoint sectors with notably high union presence, characterized by higher coverage and membership rates, specifically in construction, transportation, and education and welfare sectors. In contrast, the business and personal services, finance, insurance, real estate (FIRE), and trade sectors exhibit low union influence across all considered datasets. These observations suggest the possibility of heterogeneous impacts of RTW laws across sectors, with sectors exhibiting higher unionization rates potentially facing more pronounced effects.

Finally, the proportion of observations from treated states constitutes between 14.2% and 15.6% of the datasets, as detailed in Table 2. Despite these states not being among the most populous, the exclusion of “always treated states” allows to have a substantial share of treated units throughout the dataset. This will provide us with sufficient statistical power to identify significant estimates using regression methods.

## 2.3 Two-Way Fixed Effects Models with Staggered Treatment Adoption

In this thesis, we employ an extended version of the canonical DiD model (Abadie, 2005). This section aims to describe the methodology of a DiD framework that accommodates multiple groups, multiple periods, and staggered adoption, *i.e.*, not all groups are treated at the same date. For a more detailed exploration of these extensions, the reader is referred to Roth et al. (2023).

<sup>10</sup>Finance, Insurance, and Real Estate

<sup>11</sup>The original data classified union status into three categories: “No union coverage”, “Member of a labor union”, and “Covered by a union but not a member”. From these categories, we created two variables: union membership (including only “Member of a labor union”) and union coverage (corresponding to “Covered by a union but not a member”).

Table 2: Number of Observations in Treated States Across Employment, Union, and Wage Datasets

States	<i>Employment dataset</i>			<i>Union dataset</i>			<i>Wage dataset</i>		
	Before RTW	After RTW	Total	Before RTW	After RTW	Total	Before RTW	After RTW	Total
Kentucky	71,934	29,294	101,228	65,672	27,857	93,529	10,708	3,604	14,312
Indiana	45,140	89,551	134,691	41,055	84,737	125,792	6,503	11,428	17,931
Michigan	79,111	109,744	188,855	70,679	103,254	173,933	11,267	13,308	24,575
Wisconsin	91,074	54,277	145,351	84,322	51,972	136,294	14,790	8,053	22,843
West Virginia	50,827	54,372	105,199	46,726	50,886	97,612	7,243	6,796	14,039
Total	338,086	337,238	675,324	308,454	318,706	627,160	50,511	43,189	93,700
% of Total Data set	—	—	14.2%	—	—	14.2%	—	—	15.6%

*Note:* This table presents the number of observations for treated states across each dataset. It details the number of observations before and after treatment, as well as the total sum of these observations.

Consider a finite set of periods,  $t \in \{1, \dots, \mathcal{T}\}$ . Each group (*e.g.*, state) may initiate treatment in any period  $t > 1$ . We define  $D_{i,t}$  as the indicator variable that equals 1 if individual  $i$  receives treatment in period  $t$ , and  $G_i = \min\{t \mid D_{i,t} = 1\}$  as the initial treatment period for individual  $i$ . For any control group  $\mathcal{C}$ , which never undergoes treatment, we set  $G_i = \infty$  for all  $i \in \mathcal{C}$ . Thus, we would have for Kentucky  $G_i = 2017$ , and for California,  $G_i = \infty$ .

We adopt the potential outcomes framework to articulate causal relationships (Robins, 1986; Rubin, 1974). Let  $Y_{i,t}(g)$  represent the potential outcome for individual  $i$  at period  $t$  assuming treatment adoption in period  $g \in \mathcal{G}$ . Implicitly,  $g$  defines a group, which in our case corresponds to a state, as each state adopts RTW laws at different periods. Given the absorbing nature of the treatment—whereby  $D_{i,t} = 1$  implies  $D_{i,t'} = 1$  for all  $t' > t$ —the potential outcome  $Y_{i,t}(g)$  implies that the individual would not receive treatment during the first  $g - 1$  periods, would be treated starting from period  $g$ , and would continue to be treated through the remainder of the periods up to  $\mathcal{T}$ . Furthermore, for a control group  $\mathcal{C}$  that never receives treatment, we define  $Y_{i,t}(\infty)$  for all members  $i \in \mathcal{C}$ . This notation specifies the potential outcomes for individuals who are perpetually untreated throughout the observed periods.

A natural extension of the Average Treatment Effect on the Treated (ATT) from the two-period, two-group scenario to cases involving multiple periods is to define group-time average treatment effects:

$$ATT(g, t) = \mathbb{E}[Y_{i,t}(g) - Y_{i,t}(\infty) \mid G_i = g] \quad (1)$$

It defines the average effect of the treatment on units within group  $g$  at time  $t$ . For instance,  $ATT(2017, 2020)$  is the average impact of the RTW law on the outcome variable  $Y$  in 2020 for individuals living in Kentucky. Researchers are also interested in aggregated forms of  $ATT(g, t)$ , such as  $ATT(t)$ , which represents the average cumulative effect of the treatment at time  $t$ , or simply  $ATT$ , which



denotes the average effect of the treatment on treated units across all groups and treatment periods<sup>12</sup>. In the following discussion, we will primarily focus on the latter aggregated  $ATT$ . Specific references to  $ATT(g, t)$  or  $ATT(t)$  will be explicitly stated.

The problem that arises from Equation 1 is that  $Y_{i,t}(\infty)$  is not an observable income for individuals in a treated state, *i.e.*, with  $G_i < \infty$ . Thus, direct identification of the  $ATT$  is impeded as we do not simultaneously observe  $Y_{i,t}(g)$  and  $Y_{i,t}(\infty)$ . We observe labor market outcomes in Kentucky under the influence of its RTW law. We cannot observe what these outcomes would have been if the state had never adopted this law. Consequently, assumptions are necessary to estimate the  $ATT$ , the foremost being the parallel trends assumption. This assumption posits that, in the absence of treatment, the outcome  $Y_{i,t}$  for both treated and untreated units would have evolved in parallel.

**Assumption 1** (Parallel Trends). For any  $t \neq t'$  and  $g \neq g'$ :

$$\mathbb{E}[Y_{i,t}(\infty) - Y_{i,t'}(\infty) \mid G_i = g] = \mathbb{E}[Y_{i,t}(\infty) - Y_{i,t'}(\infty) \mid G_i = g']$$

It stipulates that in a hypothetical scenario without treatment, the average outcomes across all groups would follow parallel trajectories. If RTW laws had not been implemented, labor market outcomes—unionization rate, wages, and employment—would have evolved similarly on average across all groups. This assumption applies to both states that adopted such laws and those that did not.

To identify the  $ATT(t)$ , it is also necessary to assume that individuals do not anticipate the impact of the treatment. The treatment should not exert any effects prior to its adoption:

**Assumption 2** (No Anticipatory Effects). For any  $t < g$ , it holds that  $Y_{i,t}(g) = Y_{i,t}(\infty)$ .

It hypothesizes that the RTW legislation in Kentucky influences labor market outcomes exclusively after its adoption. Thus, employers or employees do not modify their behaviors in anticipation of this law. If this assumption is not met, it becomes impossible to identify the causal effect of the treatment, since the estimated effect may be confounded by adjustments made in anticipation of the treatment.

Having established the conceptual framework and the assumptions required for causal inference, we now proceed to explore the conditions under which one can estimate an  $ATT$  using regression methods. We start with the static TWFE model outlined below:

$$Y_{i,t} = \alpha_s + \delta_t + D_{i,t} \cdot \beta_{\text{TWFE}} + X_{i,t}\phi + \varepsilon_{i,s,t} \quad (2)$$

<sup>12</sup>For example, Callaway and Sant’Anna (2021) suggests the following aggregations:

$$ATT(t) = \sum_{g \in \mathcal{G}} \mathbb{1}\{g + t \leq \mathcal{T}\} ATT(g, g + t) \mathbb{P}(G_i = g \mid G + t \leq \mathcal{T})$$

$$ATT = \sum_{g \in \mathcal{G}} \frac{1}{\mathcal{T} - g + 1} \sum_{t=1}^{\mathcal{T}} \mathbb{1}\{g \leq t\} ATT(g, t) \mathbb{P}(G_i = g)$$

where  $Y_{i,t}$  represents the dependent variable,  $\alpha_s$  is a group-level fixed effect (states in our case),  $\delta_t$  is a time fixed effect,  $D_{i,t}$  is the treatment indicator,  $X_{i,t}$  is a vector of individual-level covariates, and  $\varepsilon_{i,s,t}$  is the error term. Recent literature on the TWFE model indicates that the coefficient  $\beta_{\text{TWFE}}$  aligns with the *ATT* only under specific conditions (de Chaisemartin and D’Haultfœuille, 2020; Goodman-Bacon, 2021). In addition to [Assumption 1](#) and [2](#), the following assumptions are also required:

**Assumption 3** (Homogeneity of Treatment Effect across Units). For all units  $i$ ,

$$Y_{i,t}(g) - Y_{i,t}(\infty) = \tau(t)$$

**Assumption 4** (Homogeneity of Treatment Effect across Time). For all periods  $t \neq t'$  where  $t \geq g$  and  $t' \geq g$ ,

$$Y_{i,t}(g) - Y_{i,t}(\infty) = Y_{i,t'}(g) - Y_{i,t'}(\infty)$$

[Assumption 3](#) and [4](#) postulate that the treatment effect remains constant across all treated units and does not vary over time. They assume that the effect of the adoption of a RTW law is the same in Texas and Kentucky and that this effect, shared by Texas and Kentucky, must be constant over time. Should these assumptions fail to hold, it raises the question: what are the implications for the validity and interpretation of the coefficient  $\beta_{\text{TWFE}}$ ? If either [Assumption 3](#) or [4](#) is violated, then the coefficient  $\beta_{\text{TWFE}}$  no longer simply represents the *ATT* but becomes a potentially non-convex weighted sum of individual treatment effects. Specifically, it is expressed as  $\beta_{\text{TWFE}} = \sum_i \sum_t \omega_{i,t} \tau_{i,t}$ , where  $\tau_{i,t} = Y_{i,t}(g) - Y_{i,t}(\infty)$  denotes the treatment effect for individual  $i$  at time  $t$ , and  $\omega_{i,t}$  are weighting factors. These weights sum to 1 but can potentially include negative values. For more details, the reader can refer to [Goodman-Bacon \(2021\)](#).

The introduction of negative weights  $\omega_{i,t}$  poses a significant issue. As demonstrated by [de Chaisemartin and D’Haultfœuille \(2020\)](#), there are scenarios where all individual effects  $\tau_{i,t}$  are positive, yet the aggregation yields a negative  $\beta_{\text{TWFE}}$ . Beyond these extreme cases, the presence of negative weights also skews our interpretation of  $\beta_{\text{TWFE}}$ , detaching it from the intended measure of interest, the *ATT*. Intuitively, within the framework of staggered adoption, the computed  $\beta_{\text{TWFE}}$  incorporates negative weights due to the fact that it employs units treated earlier as controls for those treated later, a concept identified in the literature as “forbidden comparisons” ([de Chaisemartin and D’Haultfœuille, 2023](#)). If [Assumption 3](#) and [4](#) are satisfied, whereby  $\tau_{i,t} = \tau$  uniformly across all pairs  $(i, t)$ , then the negative and positive weights counterbalance each other. Under these conditions,  $\beta_{\text{TWFE}}$  effectively represents the *ATT*.

One method to circumvent [Assumption 4](#) involves adopting a dynamic specification of [Equation 2](#). This approach aims to accommodate temporal heterogeneity in treatment effects by associating a distinct coefficient with the treatment effect for each year subsequent to adoption. Formally, the model is specified as:

$$Y_{i,t} = \alpha_s + \delta_t + \sum_{k=-\kappa, k \neq -1}^{\kappa} \beta_k \cdot \mathbb{1}\{K_{i,t} = k\} + X_{i,t}\phi + \varepsilon_{i,s,t} \quad (3)$$

where  $K_{i,t}$  denotes the time relative to treatment adoption, defined as  $K_{i,t} = t - G_i + 1$ . In this configuration, assuming that parallel trends are satisfied, there are no anticipatory effects, and that the treatment effect is homogeneous across units, the coefficient  $\beta_k$  corresponds to the cumulative effect of  $k+1$  treatment periods, *i.e.*,  $\beta_k = \mathbb{E}[Y_{i,g+k}(g) - Y_{i,g+k}(\infty) \mid G_i = g]$ . However, as demonstrated by [Sun and Abraham \(2021\)](#), if the treatment effects vary across adoption cohorts—thus violating [Assumption 3](#)— $\beta_k$  is reinterpreted as a potentially non-convex weighted sum of treatment effects. This sum may include negative weights as a result of “forbidden comparisons”. Furthermore, as outlined by [Roth et al. \(2023\)](#), the  $\beta_k$  may also be biased by “cross-lag contamination”. This phenomenon occurs when the coefficient  $\beta_k$  assigns non-zero weights to lags  $k' \neq k$ , meaning its value is influenced by treatment effects at earlier or later lags.

Furthermore, [Sun and Abraham \(2021\)](#) demonstrates that the pre-treatment coefficients,  $\beta_k$  for all  $k \leq -2$ , exhibit bias when [Assumption 3](#) is violated. These coefficients are commonly employed to test the parallel trends assumption, yet this usage does not constitute empirical proof of the assumption’s validity. In scenarios where these coefficients are biased, it is possible to estimate statistically significant pre-treatment coefficients even when the parallel trends assumption holds, and conversely, to observe non significant coefficients despite the assumption being violated.

For the TWFE model with staggered adoption to yield valid estimates, whether in static or dynamic form, without assuming homogeneity of the treatment effect across cohorts  $g$ , it is necessary to employ heterogeneity-robust estimators. This is the focus of the following section.

## 2.4 Heterogeneity-Robust Two-Way Fixed Effects Estimators

In this section, we delineate the robust estimators employed in our analysis. These estimators allow the relaxation of [Assumption 3](#), which mandates homogeneity of treatment effect for deriving unbiased coefficients. A distinctive characteristic of these estimators is their restriction of comparisons to valid control groups only, thereby circumventing “forbidden comparisons”. In this discussion, we confine our analysis to estimators that rely exclusively on the parallel trends assumption. Estimators built on any form of randomization in treatment assignment or timing will not be considered as RTW laws adoption does not correspond to such designs ([Athey and Imbens, 2022](#); [Roth and Sant’Anna, 2023](#); [Shephard et al., 2021](#)). Furthermore, our focus is on estimators that enable the estimation of a dynamic equation, such as [Equation 3](#), to discern the dynamic effects of RTW laws on labor market outcome. Thus, [de Chaisemartin and D’Haultfœuille \(2020\)](#)’s static estimator will not be considered.

In the literature reviews ([de Chaisemartin and D’Haultfœuille, 2023](#); [Roth et al., 2023](#)), two estimators are particularly notable: the estimator proposed by [Callaway and Sant’Anna \(2021\)](#) and the one by [Borusyak et al. \(2024\)](#). In the following, we will refer to them respectively as the CS and BJS estimators. These two estimators rely on [Assumption 1](#) of parallel trends, and are robust to heterogenous treatment effects. In the following, we will describe each of these and explain their connections to other estimators in the literature.

**The estimator proposed by Callaway and Sant’Anna (2021)** They begin with the  $ATT(g, t)$  defined in the previous section (Equation 1). Under Assumption 1 and 2, it is readily identifiable as:

$$ATT(g, t) = \mathbb{E}[Y_{i,t} - Y_{i,g-1} \mid G_i = g] - \mathbb{E}[Y_{i,t} - Y_{i,g-1} \mid G_i \in \mathcal{J}] \quad (4)$$

The general expression  $G_i \in \mathcal{J}$  accommodates two types of control groups: the never treated and the not yet treated. In these cases,  $\mathcal{J}$  is defined respectively as  $\mathcal{J} = \{\infty\}$  for the never treated, and  $\mathcal{J} = \{g' : g' > t\}$  for the not yet treated. In our analysis, the choice of using the not yet treated group as a control seems pertinent. The recent RTW laws were enacted following local electoral victories by the Republican Party, suggesting that these states might exhibit similar characteristics, thereby supporting the parallel trends assumption. However, as detailed in Table 2, the treated states account for only 14.2% to 15.6% of our dataset. Consequently, restricting our analysis to these states would significantly diminish our statistical power. Therefore, in subsequent analyses we will prioritize the never treated group as the control group.

The  $ATT(g, t)$  is estimated using the method of moments:

$$\hat{ATT}(g, t) = (\bar{Y}_{t=t, G=g} - \bar{Y}_{t=g-1, G=g}) - (\bar{Y}_{t=t, G \in \mathcal{J}} - \bar{Y}_{t=g-1, G \in \mathcal{J}}) \quad (5)$$

As described in Subsection 2.3, the  $ATT(g, t)$  can be further aggregated into dynamic effects, group effects, or an overall aggregated treatment effect. A notable advantage of the CS estimator is that it requires a weaker parallel trends assumption than Assumption 1. It necessitates that the parallel trends holds, but only after the first group receives treatment.

This straightforward approach effectively prevents “forbidden comparisons” and avoids potential negative weights. This methodology is similarly employed by the estimator proposed by Sun and Abraham (2021).

**The estimator proposed by Borusyak et al. (2024)** This approach involves estimating the potential outcome  $Y_{i,t}(\infty)$  for treated units. Specifically, Borusyak et al. (2024) suggest estimating the following TWFE regression:

$$Y_{i,t}(\infty) = \alpha_i + \lambda_t + \varepsilon_{i,t}$$

for the not yet treated observations. This is achieved by utilizing all observations for which  $G_i > t$ , across all  $G_i$ . In this manner, the estimate  $\hat{Y}_{i,t}(\infty)$  is used to compute a treatment effect specific to each unit, expressed as  $Y_{i,t} - \hat{Y}_{i,t}(\infty)$ . These individual effects are then aggregated to determine the  $ATT(g, t)$ . Similar to the CS estimator, the BJS estimator avoids biases arising from treatment effect heterogeneity, under Assumption 1 and 2. This methodology, commonly known as the imputation estimator (Roth et al., 2023), is also utilized by Gardner (2022), Liu et al. (2022), and Wooldridge (2021).

**Comparison of the two approaches** The distinction between these two approaches resembles a bias-variance tradeoff. The [Callaway and Sant’Anna \(2021\)](#) approach anchors all comparisons to the last pre-treatment period, while the imputation estimator method bases comparisons on the average of pre-treatment periods, thereby enhancing the precision of the estimator. Furthermore, [Borusyak et al. \(2024\)](#) demonstrate that, under conditions of homoskedasticity and serially uncorrelated errors, their estimator is efficient. However, the [Callaway and Sant’Anna \(2021\)](#) approach relies on a weaker—and potentially more credible—parallel trends assumption. For a more exhaustive discussion of the similarities and differences between these two approaches, the reader is referred to [de Chaisemartin and D’Haultfœuille \(2023\)](#).

## 2.5 Addressing Selection Bias

In the *union* or *wage* datasets, we restrict our observations to employed individuals. This situation is a classic example of non-randomly selected samples ([Gronau, 1974](#)). Individuals who are employed may have significantly different characteristics from those who are unemployed. Thus, not considering employment selection in our analysis could bias our results ([Heckman, 1979](#)).

In our regressions, we will address the sample selection bias by applying the method described by [Heckman \(1979\)](#), which involves modeling the selection into employment. To this end, we use the number of children under five years old in the household as an instrumental variable. We assume this variable is relevant and verify the exclusion restriction; that is, it correlates with employment but does not directly affect unionization or wages. This variable has often been used in this context, particularly in studies on female labor supply ([Lehrer and Nerlove, 1986](#); [Nakamura and Nakamura, 1991](#)).

The Heckman model, or generalized tobit model, involves estimating the probability of being employed through a probit model, assuming that the error term  $\varepsilon_{i,s,t}$  follows a standard normal distribution,  $\mathcal{N}(0, 1)$ :

$$\begin{aligned} \text{First stage: } \text{Employment}_{i,s,t} &= \Phi(X_{i,t}\phi + \text{Number of children under five} \times \psi) \\ &= \Phi(Z_{i,t}\gamma) \end{aligned}$$

where  $\Phi(\cdot)$  is the cumulative distribution function (CDF) of a standard normal distribution. In the second step, we use the predicted employment probability to compute the Inverse Mills’ Ratio (IMR), which is the ratio of the probability density function (PDF) to the CDF of a standard normal distribution at the estimated employment probability:

$$\lambda_{i,t} = \frac{\phi(Z_{i,t}\hat{\gamma})}{\Phi(Z_{i,t}\hat{\gamma})} \quad (6)$$

where  $\phi(\cdot)$  is the PDF of a standard normal.

Finally, we incorporate the IMR as a covariate in our regressions to correct for sample selection bias, under the assumption of joint normality between the disturbance terms of the selection equation and those of the main equations, namely, the unionization or wages equations. [Table 26](#) presents the results of the first stage of the two-step estimation procedure described above. We employ the same model

specification and set of individual-level covariates  $X_{i,t}$  as used in the static specification of employment in the subsequent section. The unique distinction lies in the model type; the first stage of the Heckman model employs a probit model, while the subsequent section utilizes linear models. Due to the probit functional form, we cannot interpret the magnitude of the estimated coefficients directly. However, we observe that the number of children under five years old in the household has a negative and statistically significant effect on the probability of being employed (Table 26). This observation reinforces our initial assumption that this variable is a relevant instrumental variable. As we cannot empirically verify that our instrument satisfies the exclusion restriction, we rely on the extensive literature supporting this assumption (Browning, 1992). In the following analyses, we will systematically include the IMR in our sets of covariates for all models that select observations based on employment status. This applies to models where the dependent variable is either the unionization rate or log hourly wages.

### 3 Results

#### 3.1 Event Study Findings

We now proceed to estimate the impact of RTW laws on labor market outcomes with static and dynamic TWFE models, as in Equation 2 and 3, and using Ordinary Least Squares. The labor market outcomes of interest include unionization rate, employment levels, and hourly wage. *A priori*, we hypothesize that RTW laws will negatively influence unionization rate by exacerbating the free-rider problem, thereby diminishing union funding. This financial constraint is expected to reduce the bargaining power of unions. Consequently, employers will have more leverage to negotiate lower wages during the bargaining of mandatory issues, leading to an expected decrease in wages. As a direct consequence, employment should increase.

We estimate three static and three dynamic models, one for each labor market outcomes. The specification of these models is given as follows:

$$Y_{i,s,t} = \alpha_s + \delta_t + D_{i,t} \cdot \beta_{\text{TWFE}} + X_{i,t}\phi + \varepsilon_{i,s,t} \quad (\text{static}) \quad (7)$$

$$Y_{i,s,t} = \alpha_s + \delta_t + \sum_{k=-5, k \neq -1}^5 \beta_k \cdot 1\{K_{i,t} = k\} + X_{i,t}\phi + \varepsilon_{i,s,t} \quad (\text{dynamic}) \quad (8)$$

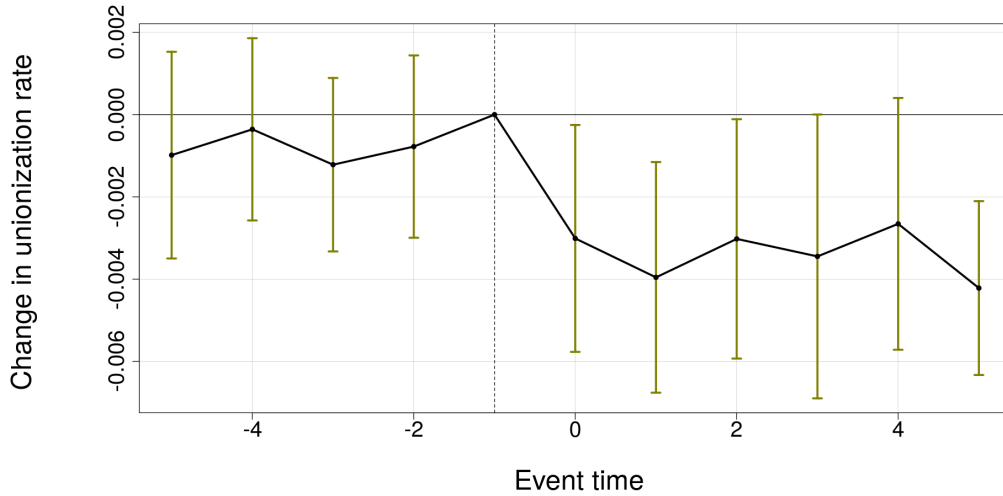
where the variables correspond to those described in Section 2. The individual-level covariates  $X_{i,t}$  are specifically tailored to each dependent variable under study and will be detailed individually for each model. Following the approach recommended by Abadie et al. (2023), standard errors  $\varepsilon_{i,s,t}$  will be clustered at the state level to account for potential intra-group correlation. We differ from Fortin et al. (2023) that cluster their standard errors at the state-year level. They argue that clustering at the state level provides low precision due to the limited number of treated clusters. In contrast, clustering at both



the state and year levels results in more clusters, a total of 85 in their case<sup>13</sup>. However, this number is still insufficient to ensure the convergence required by the central limit theorem for accurate inference. Additionally, there exists a substantial body of literature that provides methods for conducting robust inference with a limited number of clusters (Canay et al., 2021; Conley and Taber, 2011; Donald and Lang, 2007; Ferman and Pinto, 2019; Hagemann, 2020).

**Unionization rate** For the unionization rate, we consider the following explanatory variables: demographic factors (age, sex, race, citizenship status, birthplace of parents), educational background and experience variables (years of education, a quartic in potential experience, and experience-education interactions<sup>14</sup>), work-related characteristics (industry of employment, full-time status, union coverage), family context (marital status, number of children in the household), industry-specific interactions (each industry dummy interacted with year dummy variables to control for industry trends), and the IMR to address sample selection in employment. We use the same covariates as Fortin et al. (2023), with the addition of the IMR to account for sample selection issues, as detailed in Subsection 2.5. An exhaustive description of these variables is provided in Tables 6 to 25.

Figure 1: RTW Law on Unionization Rate



*Note:* The figure plot the estimated effects of RTW law on unionization rate, including 95% confidence intervals. Standard errors are clustered by state. Results are detailed in Table 27. These estimations are computed using the `fixest` package in R. See the main text for details.

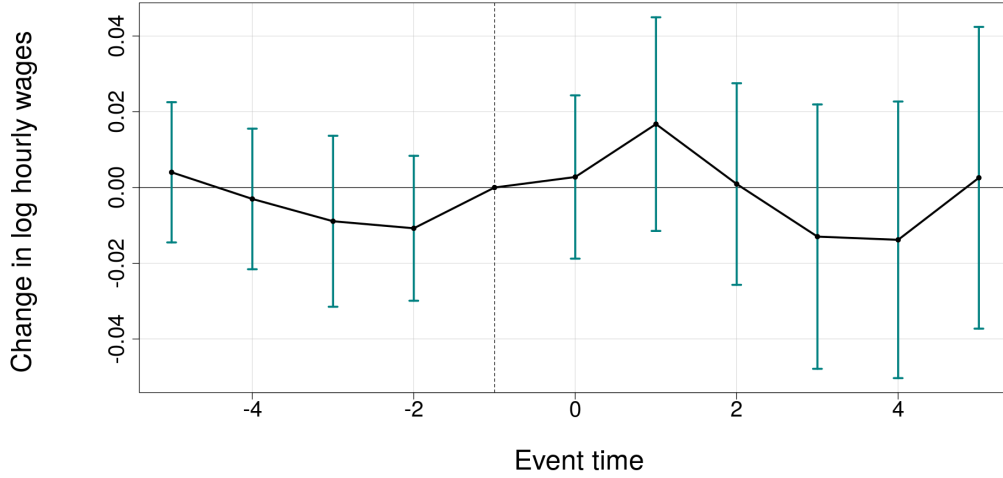
The results from the static and dynamic models are shown in Table 27, and Figure 1 illustrates the dynamic effects of the law. After a RTW law is passed, the unionization rate immediately drops by 0.3 percentage points. The largest decrease of 0.42 percentage points occurs 5 years later. All estimated effects are significant at the 5% level, except for the third and fourth years after treatment, which are significant at

<sup>13</sup>Their dataset spans from 2003 to 2019, covering 5 treated states:  $(2019 - 2003 + 1) \times 5 = 85$ .

<sup>14</sup>We follow Fortin et al. (2023), and use as covariates education multiplied by experience ( $education \times experience$ ) and interactions of education categories with experience categories. The latter are described in Table 22 and 24.

the 10% level. There is no evidence of a pre-trend; all coefficients estimated before the law’s adoption are not statistically different from zero. Across all periods, the law is estimated to reduce the unionization rate by 0.32 percentage points. This provides compelling evidence of an immediate and negative impact of the law on the unionization rate. These results are all the more credible as they are closely aligned with those reported by [Fortin et al. \(2023\)](#), as shown in [Figure 14](#).

Figure 2: RTW Law on Log Hourly Wages



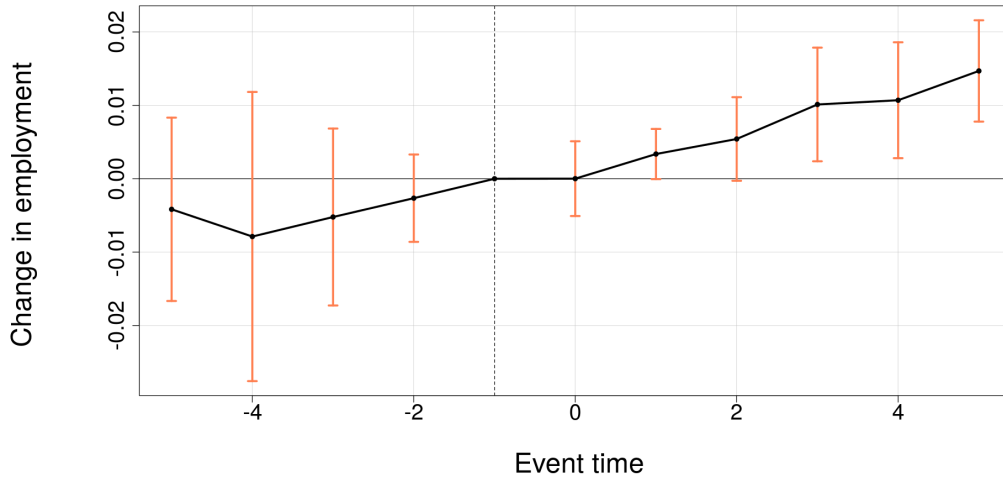
*Note:* The figure plot the estimated effects of RTW law on log hourly wages, including 95% confidence intervals. Standard errors are clustered by state. Results are detailed in [Table 27](#). These estimations are computed using the `fixest` package in R. See the main text for details.

**Log hourly wages** The set of covariates for analyzing wages is identical to that used for the unionization rate, except it includes the variable for union membership. As illustrated in [Table 27](#) and [Figure 2](#), we find no evidence of an impact of the RTW law on wages. Neither the static nor the dynamic models estimate statistically significant coefficients. This result contrasts with those of [Fortin et al. \(2023\)](#), who estimate a negative impact of the RTW law on wages. The discrepancy between our findings and theirs can be attributed to methodological differences; [Fortin et al. \(2023\)](#) does not account for selection into employment. Accounting for this sample selection affects the results, as indicated by the statistical significance of the Inverse Mills’ Ratio in both the static and dynamic models of log hourly wages ([Table 27](#)). Indeed, disregarding sample selection yields results comparable to those reported by [Fortin et al. \(2023\)](#). Specifically, we estimate a 2.8% decrease in wages for the third and fourth years after adoption, significant at the 10% level ([Figure 15](#)).

**Employment** For employment, the covariate set mirrors that used for the unionization rate, with two distinctions: it excludes work-related characteristics (industry of employment, full-time status, union coverage), and it incorporates the number of children under 5 years old in the household, a variable

omitted in previous regressions as it is used to model selection into employment in the first stage of the Heckman model. Results are presented in [Table 27](#) and [Figure 3](#). Overall, the law is associated with a significant 1.3 percentage point increase in employment. Employment rose by 0.3 percentage points one year after the law’s adoption and continued to rise, reaching a 1.5 percentage point increase five years after treatment. From three years after adoption onwards, all estimated effects are statistically significant at the 5% level, the others being significant at the 10% level. Additionally, there is no evidence of pre-trends, as all coefficients estimated before the law’s adoption are not statistically significant. These results offer credible evidence of a positive impact of the RTW law on employment. We cannot compare these results with those in [Fortin et al. \(2023\)](#) because they did not investigate the impact of RTW law on employment.

Figure 3: RTW Law on Employment



*Note:* The figure plot the estimated effects of RTW law on employment, including 95% confidence intervals. Standard errors are clustered by state. Results are detailed in [Table 27](#). These estimations are computed using the `fixest` package in R. See the main text for details.

Overall, our results provide strong statistical evidence that RTW laws lead to an immediate reduction in the unionization rate, a gradual increase in employment over time, and no effect on wages. Nevertheless, as discussed in [Subsection 2.4](#), the TWFE estimator is not robust to heterogeneity in treatment effects across states. In the following section, we confirm the validity of our results using the estimators proposed by [Callaway and Sant’Anna \(2021\)](#) and [Borusyak et al. \(2024\)](#).

## 3.2 Heterogeneity-Robust Estimators

### 3.2.1 Diagnosis of “forbidden comparisons”

The TWFE results are derived by aggregating the  $ATT(g, t)$ , with weights that can be negative due to “forbidden comparisons”, where the outcome of a treated state is compared to that of an earlier treated state, which is considered as a control. These negative weights can distort and undermine the validity of

the results. [de Chaisemartin and D’Haultfœuille \(2020\)](#) developed a diagnostic method for identifying the number of  $ATT(g, t)$  with negative weights and quantifying the sum of these negative weights. We use this diagnostic to evaluate the importance of “forbidden comparisons” in the previous results. It shows that none of our findings are affected by negative weights ([Table 29](#)). Our results are based on the aggregation of 37  $ATT(g, t)$ , none of which have negative weights, thereby supporting the validity of our previous findings. To further substantiate our results, we will now estimate the effect of the RTW law using heterogeneity-robust estimators.

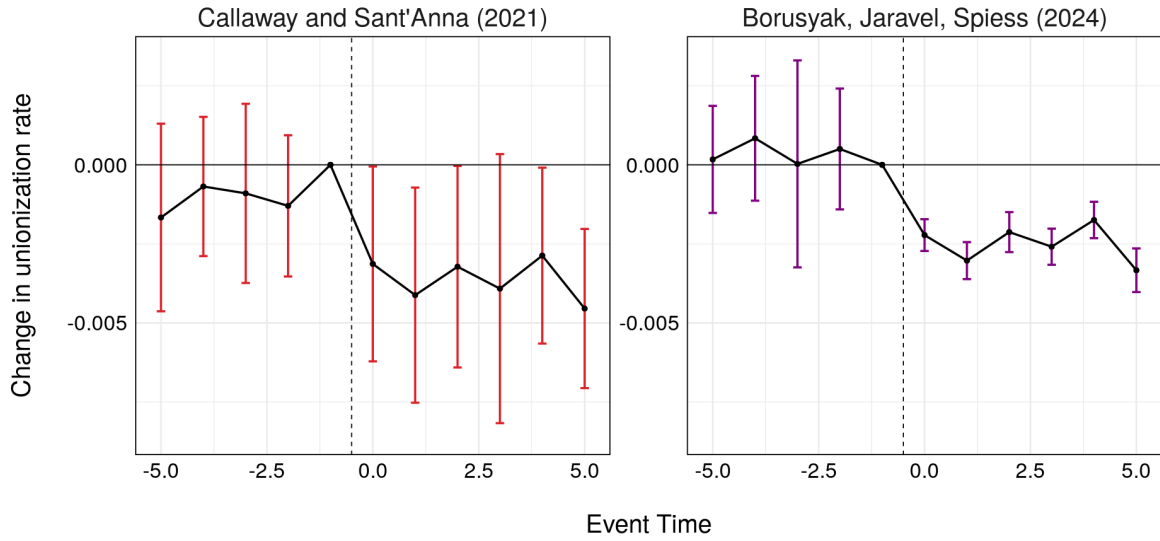
### 3.2.2 Heterogeneity-robust event studies

In this section, we reestimate the dynamic model from [Subsection 3.1](#) for unionization rate, log hourly wages, and employment, using the same sets of covariates, but employing the heterogeneity-robust estimators presented in [Subsection 2.4](#).

**Unionization rate** Using the CS and BJS estimators, we find results consistent with those obtained using TWFE model. The RTW law causes an immediate decrease in unionization that remains stable over time ([Figure 4](#)). The magnitude of the effect is similar to previous findings, with a drop in unionization ranging from 0.32 to 0.45, and 0.17 to 0.33 percentage points for the CS and BJS estimators respectively ([Table 30](#)). There is no evidence of pre-trends, and all estimated coefficients are statistically significant, with significance levels of either 5% or, in some cases, 10%. [Figure 4](#) illustrates the bias-variance trade-off between these two estimators. The BJS estimator is more precise, providing narrower confidence intervals but potentially biased, estimating slightly smaller coefficients. Finally, [Figure 16](#) shows how similar are the results from the CS and BJS estimators compared to those from the previous TWFE model, thereby supporting our findings.

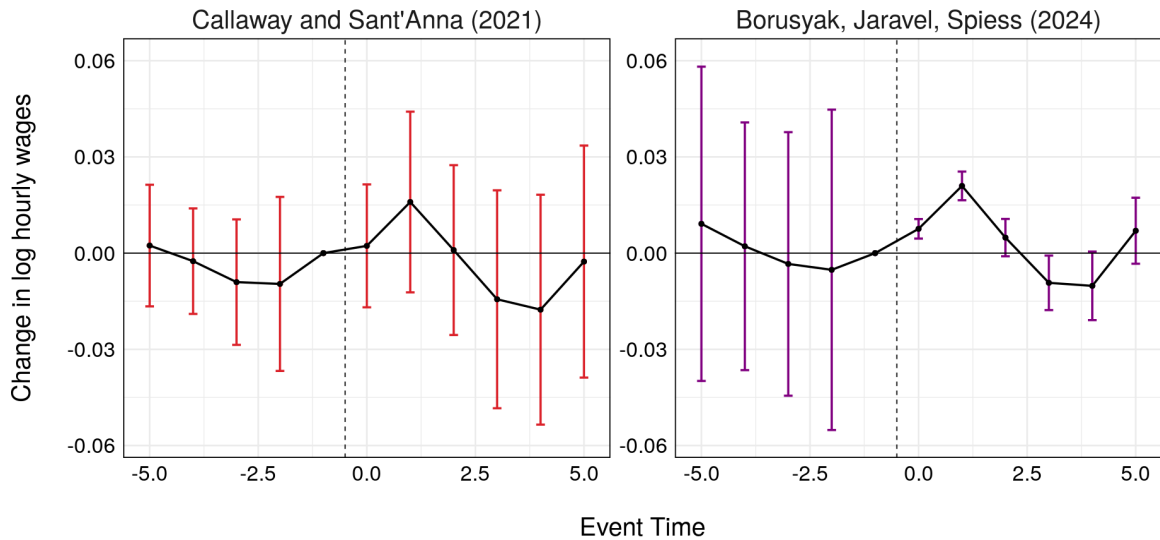
**Log hourly wages** Unlike for unionization rate, the CS and BJS estimators yield different results regarding the potential effect of the RTW law on wages. The CS estimator, consistent with previous estimations, provide no evidence of an effect of the RTW law on log hourly wages across any of the periods considered. Applying the BJS methodology, we observe a positive impact of RTW law on wages during the initial two periods post-treatment, and a significant decrease in the third and fourth years following adoption ([Figure 5](#)). Specifically, wages increase by 2.1% in the first year after treatment and then decline by 1.0% in the fourth year after adoption ([Table 30](#)). It is noteworthy that the estimated wage decline four years after treatment is consistent, both in terms of timing and magnitude, with the findings of [Fortin et al. \(2023\)](#). Given these differing results, we prefer to rely on the CS methodology. This estimator is based on a less restrictive parallel trends assumption, allowing us to place greater confidence in the validity of its estimations.

Figure 4: RTW Law on Unionization Rate: CS and BJS Estimators



*Note:* The figure plot the estimated effects of RTW law on unionization rate using [Callaway and Sant'Anna \(2021\)](#) and [Borusyak et al. \(2024\)](#) estimators, and includes 95% confidence intervals. Standard errors are clustered by state. Results are detailed in [Table 30](#). For the CS estimator, control states are never-treated ones. These estimations are computed using the `did` and `didimputation` packages in R. See the main text for details.

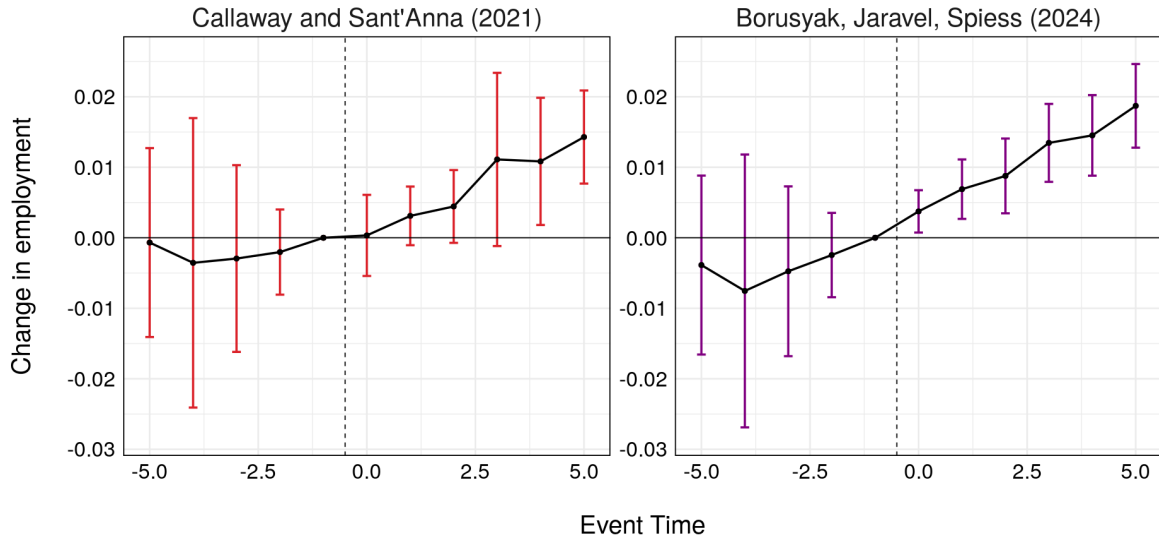
Figure 5: RTW Law on Log Hourly Wages: CS and BJS Estimators



*Note:* The figure plot the estimated effects of RTW law on log hourly wages using [Callaway and Sant'Anna \(2021\)](#) and [Borusyak et al. \(2024\)](#) estimators, and includes 95% confidence intervals. Standard errors are clustered by state. Results are detailed in [Table 30](#). For the CS estimator, control states are never-treated ones. These estimations are computed using the `did` and `didimputation` packages in R. See the main text for details.

**Employment** The CS and BJS estimators also reveal a progressive increase in employment following the adoption of the RTW law (Figure 6). Using the BJS methodology, we estimate an immediate significant effect on employment of 0.037 percentage points, which grows to 1.87 percentage points five years after treatment (Table 30). These estimated coefficients are higher, and more precise, than those obtained from TWFE or CS estimators. Using the CS estimator, only the estimated effects for the fourth and fifth years after adoption are significant at the 5% level. The estimated cumulative effects from one to three years after treatment are significant at the 10% level. As with the unionization rate, the three estimators yield very similar results for employment, as shown in Figure 18. Additionally, neither the CS nor the BJS estimators provide evidence of pre-trends.

Figure 6: RTW Law on Employment: CS and BJS Estimators



*Note:* The figure plot the estimated effects of RTW law on employment using Callaway and Sant'Anna (2021) and Borusyak et al. (2024) estimators, and includes 95% confidence intervals. Standard errors are clustered by state. Results are detailed in Table 30. For the CS estimator, control states are never-treated ones. These estimations are computed using the `did` and `didimputation` packages in R. See the main text for details.

Overall, heterogeneity-robust estimators offer additional evidence that supports the validity of our results. These estimators corroborate the initial findings that RTW laws prompt an immediate decrease in unionization, and a gradual increase in employment levels. They present mixed evidence regarding the absence of an effect of RTW laws on wages.



### 3.2.3 State-level heterogeneity-robust results

As previously discussed, the CS estimator provides a versatile framework for aggregating  $ATT(g, t)$  into various forms. One such form is  $ATT(g)$ , which represents the average effect of adopting a RTW law in state  $g$ <sup>15</sup>. We compute the  $ATT(g)$  for each treated state under study. The corresponding values for the unionization rate, log hourly wages, and employment levels are respectively depicted in Figure 19, 20, and 21.

For unionization rate and employment, the results are consistent across states. Each state exhibits an estimated  $ATT(g)$  with a uniform sign. All states experience a decrease in unionization due to RTW law, but the estimated effect is not significant in Indiana. The same pattern is observed with employment, where all states show a positive effect. However, in Indiana and Michigan the estimated coefficients are not significant, likely due to a lack of statistical power.

The impact of RTW laws on wages varies significantly among the states. Michigan and Indiana report an increase in wages, while West Virginia and Wisconsin experience a decrease. In Kentucky, the effect is not significant. These results are summarized in Table 3. Overall, West Virginia and Wisconsin exhibit outcomes that align with our initial guess: a reduction in unionization rate and bargaining power leads to a decline in wages alongside an augmentation in employment levels. In Kentucky, the effects observed are consistent with our findings at the aggregate level: RTW law results in a decrease in unionization rate, an increase in employment, and no significant effect on wages.

Table 3: Summary of State-Level Aggregated ATTs of RTW law on Labor Market Outcomes

States	Unionization rate	Wages	Employment
Kentucky (2017)	-	0 <sup>+</sup>	+
West Virginia (2016)	-	-	+
Wisconsin (2015)	-	-	+
Michigan (2013)	-	+	0 <sup>+</sup>
Indiana (2012)	0 <sup>-</sup>	+	0 <sup>+</sup>

*Note:* This table summarizes the results from Figure 19, 20, and 21. A “+” denotes a positive and statistically significant effect, while a “-” indicates a negative and statistically significant effect. A “0” signifies that the effect is not statistically significant, with the sign of the effect indicated by the superscript.

In Indiana and Michigan, we estimate a positive and statistically significant effect of RTW laws on wages. This particular outcome may be attributed to the specific timing of their untreated years, which coincide with the financial crisis of 2007-2008 and the subsequent recessionary period. Prior to the

<sup>15</sup>Equation (3.7) in Callaway and Sant’Anna (2021) provides a description of the aggregation process. Specifically, the state-level  $ATT$  is calculated as:

$$ATT(g) = \frac{1}{T - g + 1} \sum_{t=g}^T ATT(g, t),$$

where the variables retain the same definitions as provided in Subsection 2.3.

adoption of RTW laws, Indiana and Michigan exhibited the highest unemployment rates among the states recently treated, with pre-treatment means of 9.1% and 10.7% respectively. We suggest that their pre-RTW wages were already depressed, leading them to experience a more substantial post-recession recovery compared to the control states. This violation of the parallel trends assumption could explain why we estimate a positive effect of the RTW law on wages in these states.

## 4 Sensitivity Analysis

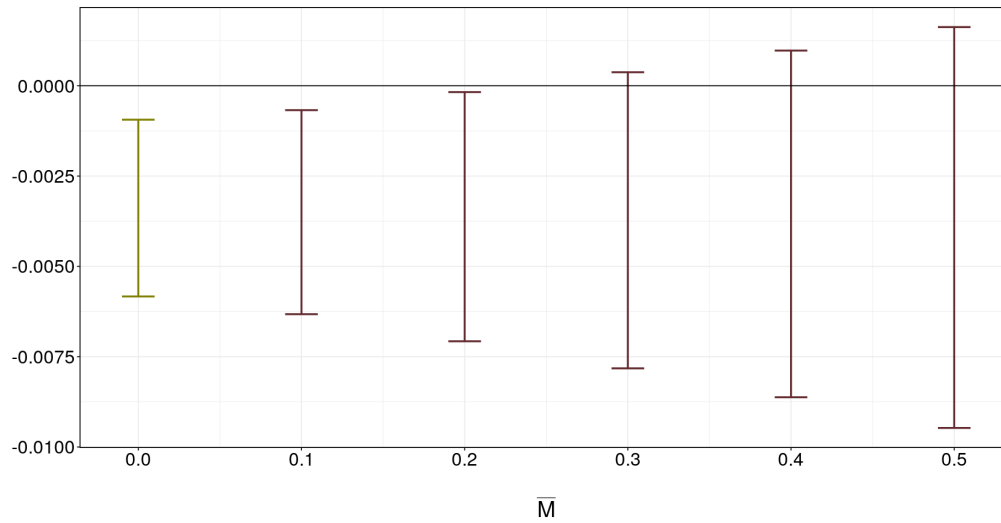
### 4.1 Sensitivity of Results to Violations of Parallel Trends

RTW laws often follow Republican electoral victories in gubernatorial elections. These adoptions are influenced by existing economic conditions and are not merely coincidental. For instance, it is conceivable that a state would enact a RTW law in response to a period of higher unemployment, wherein unions are held accountable. Additionally, these laws may arise from a shift in perceptions of unions that alters the bargaining dynamics within corporations prior to adoption. This suggests that states implementing RTW laws may have distinct characteristics compared to those that do not, and casts doubt on the validity of the parallel trends assumption.

Our analysis demonstrated that all coefficients associated with the treatment remained statistically insignificant prior to the law’s enactment, thus supporting the plausibility of the parallel trends assumption. This approach, while straightforward, has limitations that are discussed in recent literature (Bilinski and Hatfield, 2020; Freyaldenhoven et al., 2019; Kahn-Lang and Lang, 2020; Roth, 2022). Three concerns arise. First, despite the absence of a pre-treatment trend, the average outcome of states could diverge in the post-treatment period due to a confounding factor. Newly elected Republican authorities could explain our results through political, societal, and economic factors unrelated to RTW laws. Secondly, these tests could fail to find a violation of the parallel trends assumption due to low statistical power. This concern is particularly relevant in our context, where only a few states are treated and the focus is on dependent variables affecting only a limited sub-population, such as unionization or unemployment. Finally, failing to identify a pre-trend exposes our results to pre-test bias, as defined by Roth (2022), which can exacerbate the bias resulting from a violation of parallel trends.

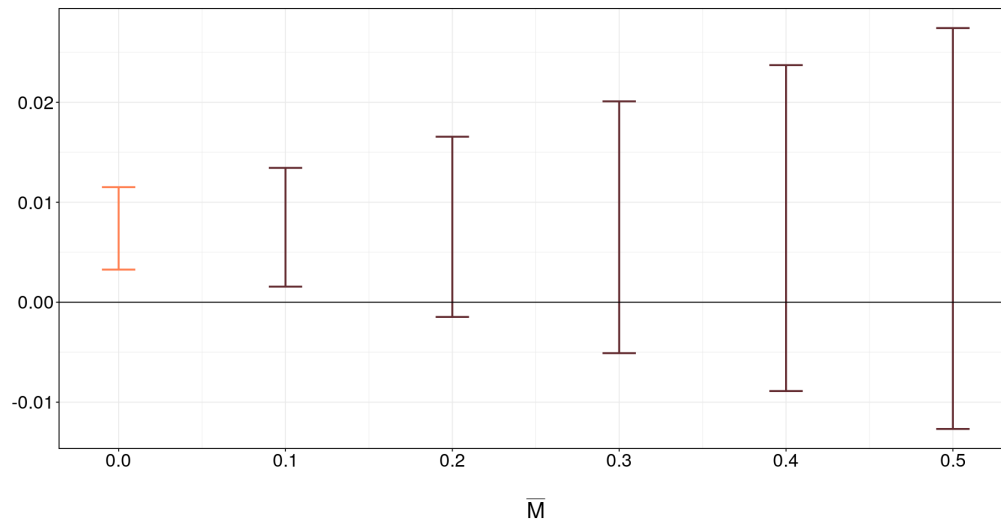
In this section, we aim to quantify the exposure of our results’ validity to violations of parallel trends using the methodology described in Rambachan and Roth (2023). Consider  $\delta_{pre}$ , the maximum pre-treatment violation of parallel trends, and  $\delta_{post}$ , the post-treatment estimated coefficient of the treatment effect. Rambachan and Roth (2023) quantify the exposure of  $\delta_{post}$  to parallel trends violations using a constant  $\bar{M}$ , such that:  $|\delta_{post}| \leq \bar{M} \times |\delta_{pre}|$ . For instance, if  $\bar{M} = 1$ , this implies that a post-treatment violation of parallel trends of the same magnitude as the pre-treatment one is sufficient to invalidate  $\delta_{post}$ . In other words, if post-treatment differences in trends are allowed to be as large as pre-treatment ones, the adoption of the treatment has no significant effect on the outcome.

Figure 7: Sensitivity of Unionization Rate Results to Violations of Parallel Trends Based on Relative Magnitude



*Note:* This figure plots the confidence intervals for the estimated aggregate effect of the RTW law on unionization rate as a function of  $\bar{M}$ , employing the methodology outlined by [Rambachan and Roth \(2023\)](#). Results are detailed in [Table 32](#). These estimations are computed using the `HonestDiD` package in R. See the main text for details.

Figure 8: Sensitivity of Employment Results to Violations of Parallel Trends Based on Relative Magnitude



*Note:* This figure plots the confidence intervals for the estimated aggregate effect of the RTW law on employment as a function of  $\bar{M}$ , employing the methodology outlined by [Rambachan and Roth \(2023\)](#). Results are detailed in [Table 32](#). These estimations are computed using the `HonestDiD` package in R. See the main text for details.

We aim to assess the reliability of our results regarding unionization rate and employment. We apply the presented methodology to the aggregated effects<sup>16</sup> of all six post-treatment estimated effects of our TWFE models plotted in [Figure 1](#) and [3](#). First, we find that our negative and significant effect of RTW law on unionization rate remains valid unless we are willing to accept that post-treatment differences in trend can exceed 0.3 times the pre-treatment differences ([Figure 7](#)). Second, our positive and significant effect of the RTW law on employment remains valid if  $\bar{M} < 0.2$ . In other words, for the estimated effect to remain significant, the post-treatment violation of parallel trends must be less than 0.2 times the maximum pre-treatment violation ([Figure 8](#)).

What we learn from these tests is that our results are highly sensitive to violations of parallel trends. To estimate significant effects, we must assume that there are almost no differences in trends between treated and untreated states. Indeed, our estimates are valid only when the values of  $\bar{M}$  are close to zero. The problem arises from the possibility that, due to the electoral context surrounding the adoption of RTW laws, post-treatment differences in trends could exceed pre-treatment ones. Our confidence in our findings would have been stronger if the breakdown values for  $\bar{M}$  exceeded one.

## 4.2 Comparative Impact of Right-to-Work Laws on Diverse Socioeconomic Groups

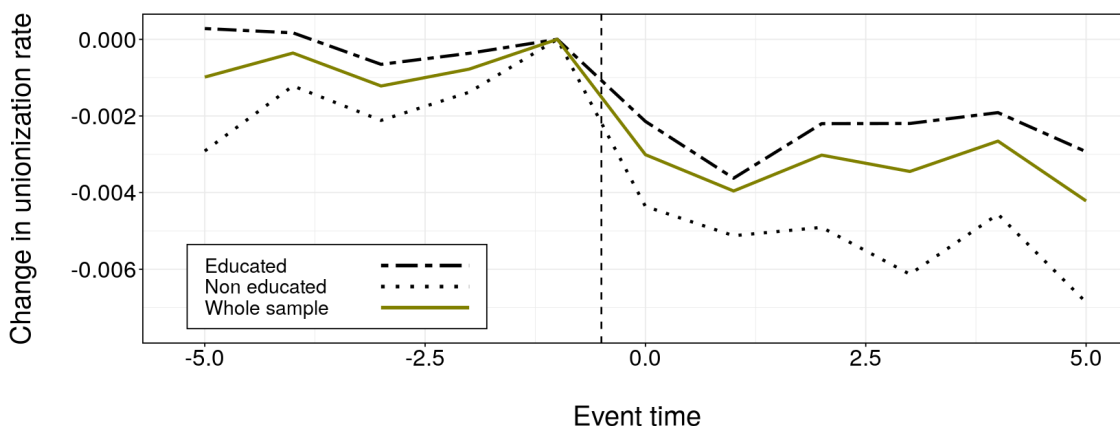
Our analysis provides evidence that RTW laws lead to a reduction in unionization rate, an increase in employment, and no discernible effect on wages. However, the impact of these laws may vary in strength based on socio-demographic characteristics. Union coverage is not uniform across all types of workers and industries. There is substantial evidence suggesting that blue-collar workers benefit more from unionization ([Hirsch and Schumacher, 1998](#)). Bargaining practices of unions may vary depending on the type of workers involved and can also differ across industries ([Aidt and Tzannatos, 2002](#)).

In this section, we aim to determine whether the effects of RTW law vary by worker education level or across different industries. To facilitate this analysis, we categorize workers into educated and non-educated groups. A worker is considered non-educated if they did not complete any formal education beyond high school, or if they dropped out before finishing high school; all others are classified as educated. Additionally, we categorize industries into highly unionized and low unionization groups based on their respective unionization rates prior to the adoption of a RTW law ([Table 34](#)). Sectors with high unionization include construction, manufacturing, transportation and utilities, as well as welfare and education. Sectors with low unionization are FIRE, trade, business and personal services. Welfare and education, the sector with the median unionization rate, is considered highly unionized.

Using a dynamic TWFE model, with the same covariates as in [Subsection 3.1](#), we observe that the decline in unionization rate following the implementation of RTW laws is more pronounced among non-educated workers ([Figure 9](#)). Five years after treatment, non-educated workers show a unionization rate decrease of 0.69 percentage points, compared to 0.42 for all workers, and 0.29 for educated ones

<sup>16</sup>The aggregated estimates are of the form  $\theta = l' \tau_{post}$ , where  $l = (\frac{1}{6}, \frac{1}{6}, \frac{1}{6}, \frac{1}{6}, \frac{1}{6}, \frac{1}{6})'$ , and  $\tau_{post}$  is the vector containing the six post-treatment estimated coefficients.

Figure 9: RTW on Unionization Rate: Comparison Between Educated and Non-Educated Workers



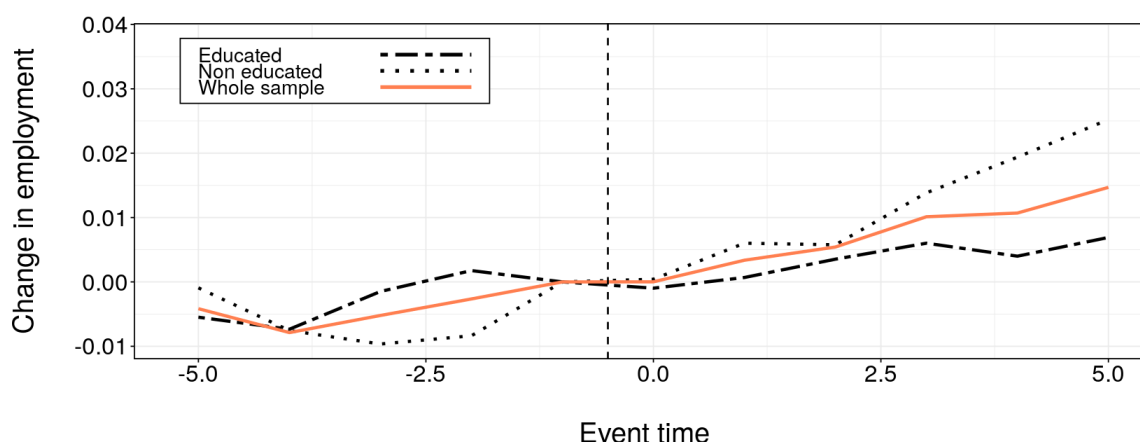
*Note:* The figure plot the estimated effects of RTW law on unionization rate, for educated, and non-educated workers. A worker is considered non-educated if they did not complete any formal education beyond high school, or if they dropped out before finishing high school; all others are classified as educated. Results are detailed in [Table 33](#). These estimations are computed using the `fixest` package in R. See the main text for details.

([Table 33](#)). More importantly, this greater reduction in union members correlates with a decline in wages. Four years after adopting a RTW law, non-educated workers experience a 3.2% decrease in hourly wages, with the estimated coefficient significant at the 1% level ([Table 33](#)). However, the presence of a negative and significant estimated coefficient prior to the adoption raises doubts about the validity of the parallel trends assumption. Finally, this larger decrease in unionization rate also translates into a more substantial increase in employment ([Figure 10](#)). Five years after the law's adoption, the employment increase for non-educated workers is almost four times that of educated ones, with gains of 2.5 and 0.7 percentage points, respectively.

Non-educated workers benefit most from unions but are also the most likely to leave following RTW law enactments. For these individuals, who typically earn lower hourly wages, union dues constitute a larger proportion of their income compared to more educated workers. Consequently, the decision to leave unions among non-educated workers may be driven by a wealth effect: without the obligation to pay fees, they choose to opt out to increase their disposable income. This effect is comparatively smaller for educated workers, which may explain their lower propensity to leave unions.

Sectors with high unionization rates also experience a larger loss of members following the adoption of a RTW law ([Figure 11](#)), comparable in magnitude to the loss observed among non-educated workers. Notably, the decline in unionization among sectors with low unionization rates is almost insignificant. Only two coefficients are statistically significant, at the 10% level, and the effects are modest, with reductions of 0.17 and 0.19 percentage points ([Table 35](#)). However, this decline in unionization does not lead to a corresponding decrease in wages. This contrasts with previous findings between educated and non-educated workers. Those who experience a loss in wages following the enactment of RTW laws

Figure 10: RTW on Employment: Comparison Between Educated and Non-Educated Workers



*Note:* The figure plot the estimated effects of RTW law on employment, for educated, and non-educated workers. A worker is considered non-educated if they did not complete any formal education beyond high school, or if they dropped out before finishing high school; all others are classified as educated. Results are detailed in [Table 33](#). These estimations are computed using the `fixest` package in R. See the main text for details.

are workers vulnerable to the diminished bargaining power of unions. By analyzing sectors as wholes, whether highly unionized or not, we group together blue-collar and white-collar workers. Consequently, the negative impact on wages experienced by non-educated workers is mitigated. As a result, we do not observe any significant effect of the RTW law on wages.

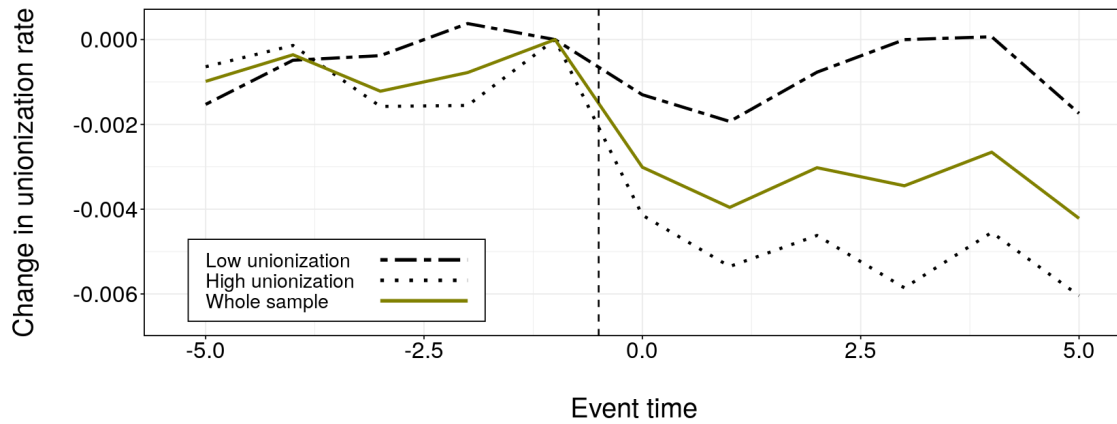
If we narrow our analysis to non-educated workers in highly unionized sectors, we observe a significant effect of the RTW law on wages ([Table 36](#)). Four years after treatment, non-educated workers in highly unionized sectors experience a 3.2% decrease in hourly wages, with the estimated coefficient significant at the 1% level. Additionally, this wage reduction coincides with a decline in unionization that is more than twice as large as that estimated for the entire sample. Five years after the adoption of the RTW law, the estimated decrease in unionization is 1.06 percentage point. However, for wages, we estimate a negative and statistically significant coefficient four years prior to treatment, which compromise the validity of the results ([Figure 12b](#)).

## 5 Conclusions, Caveats, and Extensions

In this thesis, we investigate the effects of RTW laws in the United States, focusing on the recent wave of adoptions. These laws prohibit the agency shop arrangement, wherein unions and employers agree that employees must pay fees to the union representing them to cover the costs associated with collective bargaining. *A priori*, we anticipate that these laws will adversely affect union finances and consequently diminish their bargaining power. Employing a sequential bargaining framework, we predict that this reallocation of decision-making power to employers will result in decreased wages and increased

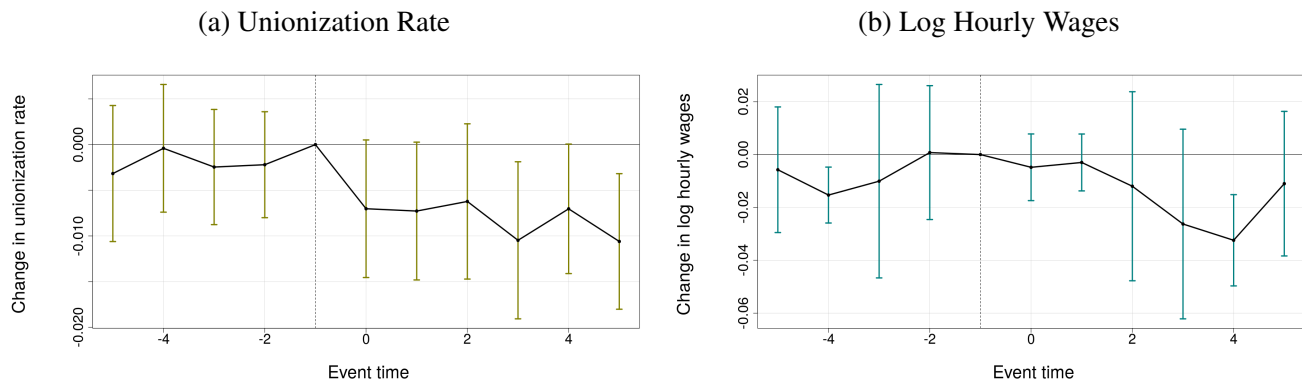


Figure 11: RTW on Unionization Rate: Comparison Between Sectors with High and Low Unionization



*Note:* The figure plot the estimated effects of RTW law on unionization rate, for sectors with high and low unionization. Sectors with high unionization include construction, manufacturing, transportation and utilities, as well as welfare and education. Sectors with low unionization are FIRE, trade, business and personal services. Results are detailed in [Table 35](#). These estimations are computed using the `fixest` package in R. See the main text for details.

Figure 12: RTW on Unionization Rate and Log Hourly Wages for Non-Educated Workers in Highly Unionized Sectors



*Note:* The figure plot the estimated effects of RTW law on unionization rate and log hourly wages, for non-educated workers in highly unionized sectors. A worker is considered non-educated if they did not complete any formal education beyond high school, or if they dropped out before finishing high school. Sectors with high unionization include construction, manufacturing, transportation and utilities, as well as welfare and education. The figure includes 95% confidence intervals. Standard errors are clustered by state. Results are detailed in [Table 36](#). These estimations are computed using the `fixest` package in R. See the main text for details.

employment levels. We employ CPS data within an event study framework, utilizing both TWFE and heterogeneity-robust estimators, and estimate that RTW laws lead to an immediate decrease in unionization, accompanied by a gradual increase in employment levels. Nonetheless, we do not find any statistical evidence of an effect of RTW laws on wages. We demonstrate that these effects are more pronounced in industries with high unionization rates and among low-educated workers. Finally, we identify a significant negative impact of RTW laws on wages in several states, and for non-educated workers, but evidence of pre-trends raises concerns regarding the robustness of this result.

Our thesis extends the previous work of [Fortin et al. \(2023\)](#) in several key ways. Firstly, we employ a theoretical framework of collective bargaining to elucidate how RTW laws might influence labor market outcomes through their impact on unions. Secondly, we expand their analysis to include employment, whereas their study was limited to unionization rate and wages. Third, we account for selection bias in estimating the effects of RTW laws on unionization and wages. Finally, we address potential biases associated with TWFE estimators with staggered adoption by employing robust estimators, and we utilize new methodologies to evaluate how sensitive our results are to violations of the parallel trends assumption.

Our results offer robust evidence of the effects of RTW laws on the labor market. We clearly highlight the underlying assumptions, employ robust estimators, and quantify the extent to which our findings are exposed to failures of initial assumption. However, important *caveat* to our results is that, despite utilizing large datasets in our analysis, we only study five treated states. Inference is based on the central limit theorem, which requires that the number of treated and untreated clusters increases, even when the number of observations within each cluster is large. With a limited number of clusters, the central limit theorem may yield poor approximations. In this thesis, we do not employ methods that allows to conduct correct inference within a framework characterized by few clusters ([Canay et al., 2021](#); [Conley and Taber, 2011](#); [Donald and Lang, 2007](#); [Ferman and Pinto, 2019](#); [Hagemann, 2020](#)).

It should also be noted that this work does not evaluate the causal impacts of RTW laws. We are unable to distinguish the direct effects of these laws from indirect effects, such as spillovers, political shifts, or changing societal views towards unions. To further extend this thesis we could employ a Regression Discontinuity Design ([Hahn et al., 2001](#)). This methodology would allow us to evaluate the causal effects of political shifts, specifically analyzing the impact of transitioning from Democratic to Republican governors during closely contested elections, by comparing states where Republicans narrowly won to those where they narrowly lost. This would permit us to assess whether these political shifts influenced the effects attributed solely to RTW laws, although that is somewhat a different question from the one posed here.

Additionally, we would have benefited from a more detailed analysis of how RTW laws affect union finances, like [Chava et al. \(2020\)](#). By utilizing union-level microdata, we could more precisely quantify the impact of these laws on unions and explore how this translates into a diminished capacity to negotiate with employers. The perspective adopted in this thesis, which treats unions as homogeneous entities, is somewhat limiting. The response of unions to RTW laws may vary depending on the industry in which

they operate, their prior bargaining experiences, or their initial membership size. For instance, a more detailed examination of how unions function could provide additional insights into the varying effects of RTW laws observed across different states, as discussed in [Subsubsection 3.2.3](#).

Finally, our analysis could also have included nearly-adopted RTW laws, such as in Missouri, where the legislature passed a RTW law in 2017<sup>17</sup> that was subsequently repealed by a referendum the following year<sup>18</sup> before it could take effect. We could have utilized this near-adoption to investigate potential anticipatory effects in collective bargaining due to the expected implementation of the RTW law. Additionally, the cases of New Mexico and Michigan provide an opportunity to estimate the specific effects of retracting from RTW laws. In New Mexico, several counties implemented local RTW laws, which were subsequently prohibited by state legislation<sup>19</sup>. Similarly, in February 2024, Michigan repealed its RTW law<sup>20</sup>. These natural experiments could have been utilized to assess the impact of retracting RTW laws on labor market outcomes. Finally, we could have adopted estimators that are capable of adapting to scenarios where treatment is not an absorbing state, meaning that units can transition from receiving treatment to being untreated once more ([de Chaisemartin and D'Haultfœuille, 2020](#); [Imai and Kim, 2021](#)), in which case all of these scenarios could have been addressed in the same estimation.

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<sup>17</sup>Missouri Senate Bill 19, 99th General Assembly, 2017 Regular Session.

<sup>18</sup>2018 Missouri Proposition A.

<sup>19</sup>New Mexico House Bill 85, 2019 Regular Session.

<sup>20</sup>Michigan, Public Acts of 2023, Act No. 8.

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Table 4: Adoption Years of Right-to-Work Laws by State and Territory

State	Adoption year of right-to-work laws
Alabama	1953
Arizona	1946
Arkansas	1947
Florida	1944
Georgia	1947
Guam	2000
Idaho	1985
Indiana	2012
Iowa	1947
Kansas	1958
Kentucky	2017
Louisiana	1976
Michigan	2013 <sup>21</sup>
Mississippi	1954
Nebraska	1946
Nevada	1951
North Carolina	1947
North Dakota	1947
Oklahoma	2001
South Carolina	1954
South Dakota	1946
Tennessee	1947
Texas	1947
Utah	1955
Virginia	1947
West Virginia	2016
Wisconsin	2015
Wyoming	1963

*Note:* This table summarizes all treated states and the year they adopted the RTW law. The states analyzed in this thesis are highlighted in gray. See the main text for details.

<sup>21</sup>Michigan repealed its right-to-work law in February 2024.

Table 5: The Role of Political Events in the Adoption of RTW Laws

State	Adoption year of RTW law	Gubernatorial election year	Incumbent	Winner	Trifecta status
Indiana	2012	2008	R	R	Republicans
Michigan	2013	2010	D	R	Republicans
Wisconsin	2015	2010	D	R	Republicans
West Virginia	2016	2012	D	D	Divided
Kentucky	2017	2015	D	R	Republicans

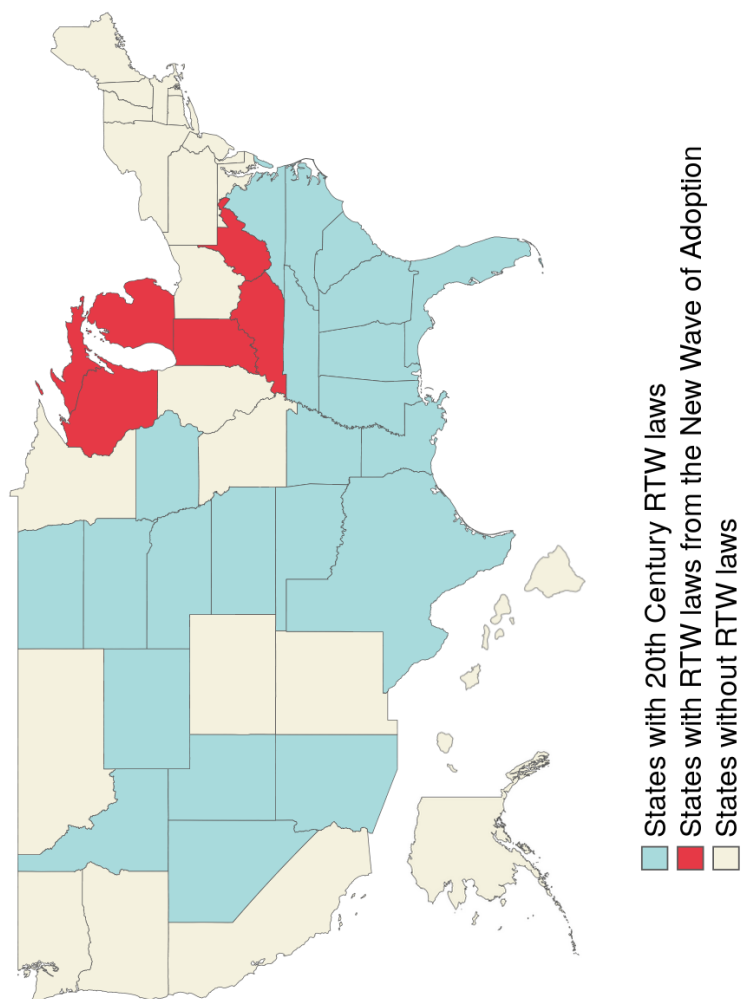
*Note:* This table presents the political context surrounding the adoption of recent RTW laws, as detailed in [Fortin et al. \(2023\)](#). The term “trifecta” refers to a situation where one political party controls the governorship and both chambers of the state legislature. See the main text for details.

Table 6: Summary Statistics of Numeric Variables for the Employment, Union, and Wage Datasets

Variable	<i>Employment dataset</i>			<i>Union dataset</i>			<i>Wage dataset</i>		
	Mean	Min	Max	Mean	Min	Max	Mean	Min	Max
Age	39.12	15	65	39.37	15	65	38.38	15	65
Sex	0.48	0	1	0.48	0	1	0.50	0	1
Education (years of)	13.78	0	20	13.86	0	20	13.48	0	20
Deflated hourly wage	–	–	–	–	–	–	15.53	1.00	199.80
Log (deflated) hourly wage	–	–	–	–	–	–	2.53	0.001	5.30
Union membership	0.02	0	1	0.02	0	1	0.06	0	1
Union coverage	0.002	0	1	0.002	0	1	0.005	0	1
Potential experience	19.34	1	40	19.51	1	40	18.90	1	40
Employment	0.93	0	1	1.00	1	1	1.00	1	1
Number of children	0.89	0	9	0.90	0	9	0.88	0	9
Number of children under 5	0.18	0	5	0.18	0	5	0.18	0	5
Observations	4,749,830			4,418,333			602,095		

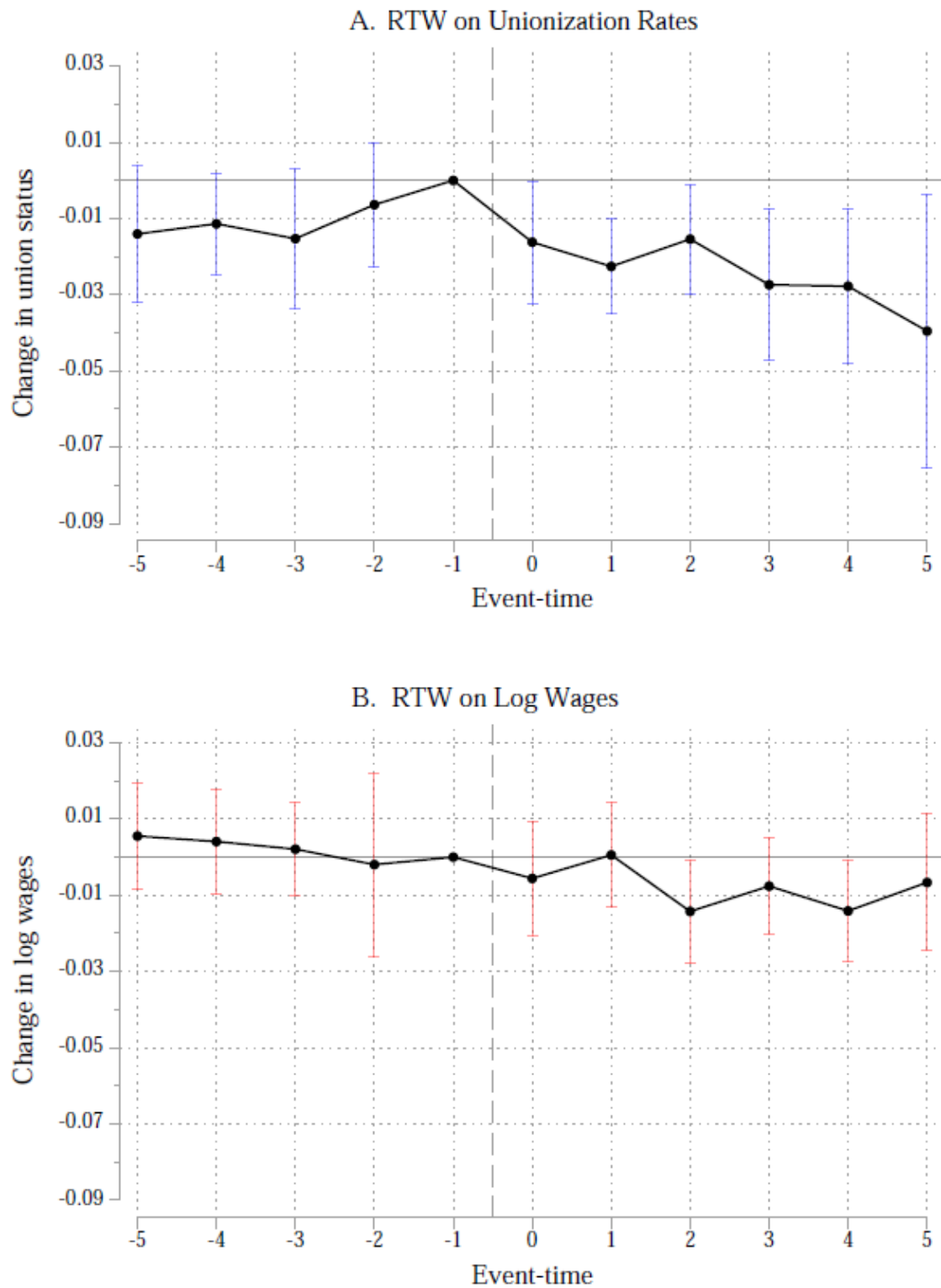
*Note:* This table presents the descriptive statistics of all numerical variables used in our analysis across our three datasets. See the main text for details.

Figure 13: Mapping Right-to-Work Laws in the U.S. by Adoption Period



*Note:* This figure plots the geographical repartition of treated states, differentiating them into states that newly adopted RTW laws and those that became RTW during the 20th century. See the main text for details.

Figure 14: Impact of Right-to-Work Laws on Unionization Rate and Wages: Event Study Results from Fortin et al. (2023)



Note: This figure plots the main findings of Fortin et al. (2023). They find a negative effect of RTW laws on both unionization rate and log hourly wages. We reproduce their findings to further compare our results to theirs. See the main text for details.

Table 7: p-values for T-Tests Comparing Numerical Variables Across Employment, Union, and Wage Datasets (see Table 6)

Variable	<i>Employment vs. Union</i>	<i>Employment vs. Wage</i>	<i>Union vs. Wage</i>
Age	< 2.2e-16***	< 2.2e-16***	< 2.2e-16***
Sex	4.0e-12***	< 2.2e-16***	< 2.2e-16***
Education (years of)	< 2.2e-16***	< 2.2e-16***	< 2.2e-16***
Union membership	< 2.2e-16***	< 2.2e-16***	< 2.2e-16***
Union coverage	1.8e-6***	< 2.2e-16***	< 2.2e-16***
Potential experience	< 2.2e-16***	< 2.2e-16***	< 2.2e-16***
Number of children	< 2.2e-16***	1.9e-10***	< 2.2e-16***
Number of children under 5	0.15	0.003	0.03

*Note:* This table presents the t-tests p-values for identifying statistically significant differences in the means of numerical variables across our three datasets. Significance levels are denoted as follows: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . See the main text for details.

Table 8: Racial Composition of Employment, Union, and Wage Datasets

Label	<i>Employment dataset</i>	<i>Union dataset</i>	<i>Wage dataset</i>
White	80.92%	81.43%	82.83%
Black	8.42%	7.89%	7.48%
American Indian/Aleut/Eskimo	0.90%	0.83%	0.91%
Asian only	7.19%	7.34%	6.12%
Hawaiian/Pacific Islander only	0.62%	0.61%	0.65%
White-Black	0.44%	0.42%	0.45%
White-American Indian	0.58%	0.56%	0.64%
White-Asian	0.38%	0.38%	0.37%
White-Hawaiian/Pacific Islander	0.11%	0.11%	0.11%
Black-American Indian	0.08%	0.07%	0.07%
Black-Asian	0.03%	0.03%	0.03%
Black-Hawaiian/Pacific Islander	0.01%	0.01%	0.01%
American Indian-Asian	0.01%	0.01%	0.01%
Asian-Hawaiian/Pacific Islander	0.12%	0.12%	0.12%
White-Black-American Indian	0.06%	0.05%	0.05%
White-Black-Asian	0.01%	0.01%	0.01%
White-American Indian-Asian	0.01%	0.01%	0.01%
White-Asian-Hawaiian/Pacific Islander	0.10%	0.10%	0.11%
White-Black-American Indian-Asian	0.00%	0.00%	0.00%
American Indian-Hawaiian/Pacific Islander	0.00%	0.00%	0.00%
White-Black-Hawaiian/Pacific Islander	0.00%	0.00%	0.00%
White-American Indian-Hawaiian/Pacific Islander	0.00%	0.00%	0.00%
Black-American Indian-Asian	0.00%	0.00%	0.00%
White-American Indian-Asian-Hawaiian/Pacific Islander	0.00%	0.00%	0.00%
Two or three races, unspecified	0.01%	0.01%	0.01%
Four or five races, unspecified	0.01%	0.01%	0.01%
Total	100%		

*Note:* This table presents the descriptive statistics of race distribution across our three datasets. See the main text for details.

Table 9: p-values for T-Tests Comparing Racial Composition Across Employment, Union, and Wage Datasets (see Table 8)

Label	<i>Employment vs. Union</i>	<i>Employment vs. Wage</i>	<i>Union vs. Wage</i>
White	< 2.2e-16***	< 2.2e-16***	< 2.2e-16***
Black	< 2.2e-16***	< 2.2e-16***	< 2.2e-16***
American Indian/Aleut/Eskimo	< 2.2e-16***	0.426	3.7e-8***
Asian only	< 2.2e-16***	< 2.2e-16***	< 2.2e-16***
Hawaiian/Pacific Islander only	0.16	0.003***	3.1e-4***
White-Black	4.3e-5***	0.23	0.002***
White-American Indian	8.3e-8***	7.1e-7***	1.3e-13***
White-Asian	0.79	0.08*	0.06*
White-Hawaiian/Pacific Islander	0.74	0.81	0.94
Black-American Indian	3.1e-4***	0.38	0.39
Black-Asian	0.54	0.21	0.35
Black-Hawaiian/Pacific Islander	0.60	0.47	0.64
American Indian-Asian	0.62	0.25	0.17
Asian-Hawaiian/Pacific Islander	0.86	0.19	0.16
White-Black-American Indian	0.02**	0.24	0.98
White-Black-Asian	0.69	0.59	0.47
White-American Indian-Asian	0.51	0.71	0.96
White-Asian-Hawaiian/Pacific Islander	0.87	0.02**	0.02**
White-Black-American Indian-Asian	0.92	0.72	0.69
American Indian-Hawaiian/Pacific Islander	0.68	0.60	0.74
White-Black-Hawaiian/Pacific Islander	0.90	0.45	0.49
White-American Indian-Hawaiian/Pacific Islander	0.68	0.56	0.69
Black-American Indian-Asian	0.75	0.04**	0.07*
White-American Indian-Asian-Hawaiian/Pacific Islander	0.92	0.24	0.26
Two or three races, unspecified	0.18	0.78	0.37
Four or five races, unspecified	0.38	0.32	0.17

*Note:* This table presents the t-tests p-values for identifying statistically significant differences in the means of racial composition across our three datasets. Significance levels are denoted as follows: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . See the main text for details.

Table 10: Marital Status Distribution across Employment, Union, and Wage Datasets

Label	<i>Employment dataset</i>	<i>Union dataset</i>	<i>Wage dataset</i>
Married, spouse present	51.24%	52.60%	50.31%
Married, spouse absent	1.41%	1.39%	1.38%
Separated	2.06%	1.99%	2.15%
Divorced	9.78%	9.71%	10.22%
Widowed	1.03%	1.03%	1.09%
Never married/single	34.47%	33.28%	34.85%
Total	100%		

*Note:* This table presents the descriptive statistics of marital status distribution across our three datasets. See the main text for details.



Table 11: p-values for T-Tests Comparing Marital Status Distribution Across Employment, Union, and Wage Datasets (see Table 10)

Label	<i>Employment vs. Union</i>	<i>Employment vs. Wage</i>	<i>Union vs. Wage</i>
Married, spouse present	< 2.2e-16***	< 2.2e-16***	< 2.2e-16***
Married, spouse absent	0.13	0.10*	0.37
Separated	< 2.2e-16***	1.3e-5***	< 2.2e-16***
Divorced	2.7e-4***	< 2.2e-16***	< 2.2e-16***
Widowed	0.21	2.6e-5***	1.7e-6***
Never married/single	< 2.2e-16***	1.1e-8***	< 2.2e-16***

*Note:* This table presents the t-tests p-values for identifying statistically significant differences in the means of marital status distribution across our three datasets. Significance levels are denoted as follows: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . See the main text for details.

Table 12: Citizenship and Nativity Status across Employment, Union, and Wage Datasets

Label	<i>Employment dataset</i>	<i>Union dataset</i>	<i>Wage dataset</i>
Born in U.S	82.12%	82.05%	82.91%
Born in U.S. outlying	0.51%	0.49%	0.49%
Born abroad of American parents	0.95%	0.96%	0.88%
Naturalized citizen	7.51%	7.61%	6.82%
Not a citizen	8.90%	8.90%	8.90%
Total	100%		

*Note:* This table presents the descriptive statistics of citizenship and nativity status distribution across our three datasets. See the main text for details.

Table 13: p-values for T-Tests Comparing Citizenship and Nativity Status Across Employment, Union, and Wage Datasets (see Table 12)

Label	<i>Employment vs. Union</i>	<i>Employment vs. Wage</i>	<i>Union vs. Wage</i>
Born in U.S	0.004***	< 2.2e-16***	< 2.2e-16***
Born in U.S. outlying	1.5e-9***	0.05*	0.32
Born abroad of American parents	0.13	3.0e-8***	3.8e-10***
Naturalized citizen	8.8e-09***	< 2.2e-16***	< 2.2e-16***
Not a citizen	0.61	0.84	0.97

*Note:* This table presents the t-tests p-values for identifying statistically significant differences in the means of citizenship and nativity status across our three datasets. Significance levels are denoted as follows: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . See the main text for details.

Table 14: Parental Nativity Status in Employment, Union, and Wage Datasets

Label	<i>Employment dataset</i>	<i>Union dataset</i>	<i>Wage dataset</i>
Both parents native-born	73.35%	73.38%	74.66%
Father foreign, mother native	1.89%	1.86%	1.88%
Mother foreign, father native	1.84%	1.84%	1.79%
Both parents foreign	5.04%	4.96%	4.56%
Foreign born	17.89%	17.96%	17.10%
Total	100%		

*Note:* This table presents the descriptive statistics of citizenship status distribution across our three datasets. See the main text for details.

Table 15: p-values for T-Tests Comparing Parental Nativity Status Across Employment, Union, and Wage Datasets (see Table 14)

Label	<i>Employment vs. Union</i>	<i>Employment vs. Wage</i>	<i>Union vs. Wage</i>
Both parents native-born	0.31	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$
Father foreign, mother native	0.006 <sup>***</sup>	0.83	0.27
Mother foreign, father native	0.91	0.16	0.14
Both parents foreign	5.6e-8 <sup>***</sup>	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$
Foreign born	0.004 <sup>***</sup>	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$

*Note:* This table presents the t-tests p-values for identifying statistically significant differences in the means of nativity status across our three datasets. Significance levels are denoted as follows: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . See the main text for details.

Table 16: Employment Status Distribution across Employment, Union, and Wage Datasets

Label	<i>Employment dataset</i>	<i>Union dataset</i>	<i>Wage dataset</i>
At work	90.21%	96.98%	97.07%
Has job, not at work last week	2.81%	3.02%	2.93%
Unemployed, experienced worker	6.44%	—	—
Unemployed, new worker	0.54%	—	—
Total	100%		

*Note:* This table presents the descriptive statistics of employment status distribution across our three datasets. See the main text for details.

Table 17: p-values for T-Tests Comparing Employment Status Across Employment, Union, and Wage Datasets (see Table 16)

Label	<i>Employment vs. Union</i>	<i>Employment vs. Wage</i>	<i>Union vs. Wage</i>
At work	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$	$5.2\text{e-}5^{***}$
Has job, not at work last week	$< 2.2\text{e-}16^{***}$	$3.8\text{e-}7^{***}$	$5.1\text{e-}5^{***}$

*Note:* This table presents the t-tests p-values for identifying statistically significant differences in the means of employment status across our three datasets. Significance levels are denoted as follows:  $*** p < 0.01$ ,  $** p < 0.05$ ,  $* p < 0.1$ . See the main text for details.

Table 18: Full-Time and Part-Time Employment Status Distribution across Employment, Union, and Wage Datasets

Label	<i>Employment dataset</i>	<i>Union dataset</i>	<i>Wage dataset</i>
Full-time hours (35+), usually full-time	69.58%	74.80%	70.94%
Part-time for non-economic reasons, usually full-time	5.23%	5.62%	5.53%
Not at work, usually full-time	2.13%	2.29%	2.01%
Full-time hours, usually part-time for economic reasons	0.17%	0.18%	0.23%
Full-time hours, usually part-time for non-economic reasons	0.41%	0.44%	0.48%
Part-time for economic reasons, usually full-time	1.05%	1.13%	1.41%
Part-time hours, usually part-time for economic reasons	3.19%	3.43%	4.69%
Part-time hours, usually part-time for non-economic reasons	10.58%	11.37%	13.80%
Not at work, usually part-time	0.68%	0.73%	0.92%
Unemployed, seeking full-time work	6.09%	—	—
Unemployed, seeking part-time work	0.89%	—	—
Total	100%		

*Note:* This table presents the descriptive statistics of full-time and part-time status across our three datasets. See the main text for details.

Table 19: p-values for T-Tests Comparing Full-Time and Part-Time Employment Status Across Employment, Union, and Wage Datasets (see Table 18)

Label	<i>Employment vs. Union</i>	<i>Employment vs. Wage</i>	<i>Union vs. Wage</i>
Full-time hours (35+), usually full-time	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$
Part-time for non-economic reasons, usually full-time	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$	$0.004^{***}$
Not at work, usually full-time	$< 2.2\text{e-}16^{***}$	$8.9\text{e-}10^{***}$	$< 2.2\text{e-}16^{***}$
Full-time hours, usually part-time for economic reasons	$4.2\text{e-}6^{***}$	$< 2.2\text{e-}16^{***}$	$5.5\text{e-}12^{***}$
Full-time hours, usually part-time for non-economic reasons	$8.2\text{e-}13^{***}$	$2.9\text{e-}14^{***}$	$1.8\text{e-}5^{***}$
Part-time for economic reasons, usually full-time	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$
Part-time hours, usually part-time for economic reasons	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$
Part-time hours, usually part-time for non-economic reasons	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$
Not at work, usually part-time	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$

*Note:* This table presents the t-tests p-values for identifying statistically significant differences in the means of full-time and part-time employment status across our three datasets. Significance levels are denoted as follows:  $*** p < 0.01$ ,  $** p < 0.05$ ,  $* p < 0.1$ . See the main text for details.

Table 20: Industry Sector Employment Distribution across Employment, Union, and Wage Datasets

Label	<i>Employment dataset</i>	<i>Union dataset</i>	<i>Wage dataset</i>
Business and personal services	13.61%	13.32%	12.40%
Construction	6.85%	6.42%	6.67%
FIRE	7.88%	8.09%	6.52%
Manufacturing	14.38%	14.44%	15.02%
Trade	23.07%	22.76%	25.55%
Transportation and utilities	6.49%	6.52%	6.24%
Welfare and education	27.73%	28.46%	27.59%
Total	100%		

*Note:* This table presents the descriptive statistics of industry categories distribution across our three datasets. See the main text for details.

Table 21: p-values for T-Tests Comparing Industry Sector Employment Distribution Across Employment, Union, and Wage Datasets (see Table 20)

Label	<i>Employment vs. Union</i>	<i>Employment vs. Wage</i>	<i>Union vs. Wage</i>
Business and personal services	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$
Construction	$< 2.2\text{e-}16^{***}$	$4.5\text{e-}7^{***}$	$4.1\text{e-}14^{***}$
FIRE	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$
Manufacturing	$0.007^{***}$	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$
Trade	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$
Transportation and utilities	0.14	$8.9\text{e-}15^{***}$	$< 2.2\text{e-}16^{***}$
Welfare and education	$< 2.2\text{e-}16^{***}$	$0.02^{**}$	$< 2.2\text{e-}16^{***}$

*Note:* This table presents the t-tests p-values for identifying statistically significant differences in the means of industry categories across our three datasets. Significance levels are denoted as follows:  $*** p < 0.01$ ,  $** p < 0.05$ ,  $* p < 0.1$ . See the main text for details.

Table 22: Educational Attainment Distribution across Employment, Union, and Wage Datasets

Label	<i>Employment dataset</i>	<i>Union dataset</i>	<i>Wage dataset</i>
Graduate studies (postgraduate)	35.66%	36.92%	28.67%
Higher education (undergraduate)	59.66%	58.78%	66.31%
None to primary education	0.39%	0.38%	0.41%
Secondary education	4.29%	3.93%	4.61%
Total	100%		

*Note:* This table presents the descriptive statistics of education categories distribution across our three datasets. See the main text for details.

Table 23: p-values for T-Tests Comparing Educational Attainment Distribution Across Employment, Union, and Wage Datasets (see Table 22)

Label	<i>Employment vs. Union</i>	<i>Employment vs. Wage</i>	<i>Union vs. Wage</i>
Graduate studies (postgraduate)	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$
Higher education (undergraduate)	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$
None to primary education	$0.02^{**}$	$0.02^{**}$	$6.1\text{e-}4^{***}$
Secondary education	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$

*Note:* This table presents the t-tests p-values for identifying statistically significant differences in the means of educational attainment distribution across our three datasets. Significance levels are denoted as follows: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . See the main text for details.

Table 24: Distribution of Work Potential Experience across Employment, Union, and Wage Datasets

Label	<i>Employment dataset</i>	<i>Union dataset</i>	<i>Wage dataset</i>
0-10 Years	25.93%	25.23%	26.71%
10-20 Years	24.91%	25.05%	24.47%
20-30 Years	24.40%	24.68%	23.46%
30-40 Years	24.76%	25.04%	25.35%
Total	100%		

*Note:* This table presents the descriptive statistics of potential experience categories distribution across our three datasets. See the main text for details.

Table 25: p-values for T-Tests Comparing Work Potential Experience Distribution Across Employment, Union, and Wage Datasets (see Table 24)

Label	<i>Employment vs. Union</i>	<i>Employment vs. Wage</i>	<i>Union vs. Wage</i>
0-10 Years	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$
10-20 Years	$3.6\text{e-}7^{***}$	$2.0\text{e-}13^{***}$	$< 2.2\text{e-}16^{***}$
20-30 Years	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$
30-40 Years	$< 2.2\text{e-}16^{***}$	$< 2.2\text{e-}16^{***}$	$1.7\text{e-}7^{***}$

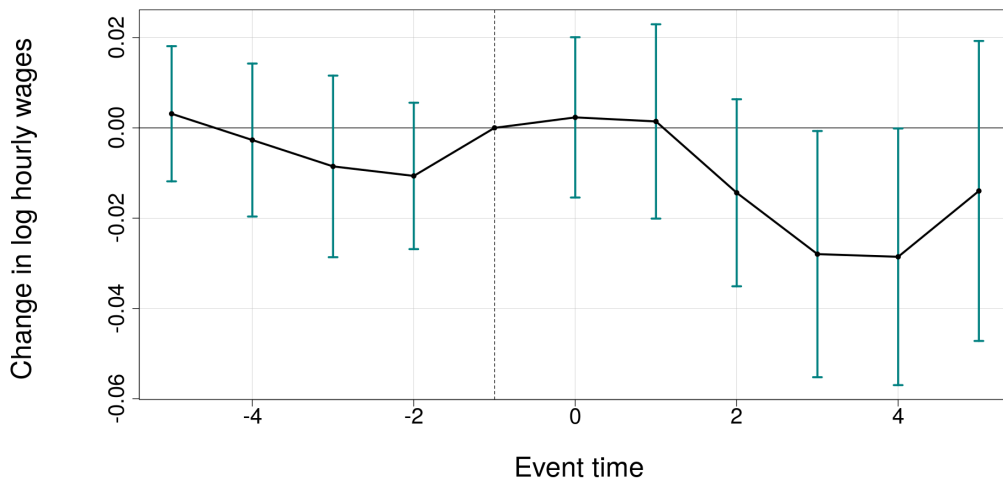
*Note:* This table presents the t-tests p-values for identifying statistically significant differences in the means of work potential experience distribution across our three datasets. Significance levels are denoted as follows: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . See the main text for details.

Table 26: First Stage Results of the Heckman Sample Selection Model

Dependent variable:	Employment
<i>Variables</i>	
Number of children under five in the household	-0.0393*** (0.0037)
<i>Fit statistics</i>	
Observations	4,749,830
R <sup>2</sup>	0.044
Within R <sup>2</sup>	0.071

*Note:* This table presents the outcomes from the first-stage regression of the Heckman two-step estimation procedure. Standard errors, clustered at the state level, are displayed in parentheses. Significance levels are denoted as follows: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Due to space constraints, not all covariates are listed; complete results are available in the GitHub repository at [outputs/tables/heckman\\_selection\\_model/](#). These estimations are computed using the `fixest` package in R. See the main text for details.

Figure 15: RTW Law on Log Hourly Wages Without Accounting for Sample Selection



*Note:* The figure plots the estimated effects of RTW law on log hourly wages when we do not account for sample selection, including 90% confidence intervals. Standard errors are clustered by state. Results are detailed in [Table 28](#). These estimations are computed using the `fixest` package in R. See the main text for details.

Table 27: Static and Dynamic TWFE Models of RTW Law on Labor Market Outcomes

Dependent variable:	Unionization rate		Log hourly wages		Employment	
Model:	Static	Dynamic	Static	Dynamic	Static	Dynamic
<i>Variables</i>						
RTW law	-0.0032*** (0.0005)		0.0013 (0.0078)		0.0126*** (0.0046)	
RTW law $\times$ time to treat = -5		-0.0010 (0.0012)		0.0040 (0.0090)		-0.0042 (0.0061)
RTW law $\times$ time to treat = -4		-0.0004 (0.0011)		-0.0030 (0.0091)		-0.0079 (0.0096)
RTW law $\times$ time to treat = -3		-0.0012 (0.0010)		-0.0089 (0.0110)		-0.0052 (0.0059)
RTW law $\times$ time to treat = -2		-0.0008 (0.0011)		-0.0108 (0.0093)		-0.0026 (0.0029)
RTW law $\times$ time to treat = 0		-0.0030** (0.0013)		0.0028 (0.0105)		1.27e-5 (0.0025)
RTW law $\times$ time to treat = 1		-0.0040*** (0.0014)		0.0167 (0.0138)		0.0034* (0.0017)
RTW law $\times$ time to treat = 2		-0.0030** (0.0014)		0.0009 (0.0130)		0.0054* (0.0028)
RTW law $\times$ time to treat = 3		-0.0034* (0.0017)		-0.0130 (0.0170)		0.0101** (0.0038)
RTW law $\times$ time to treat = 4		-0.0027* (0.0015)		-0.0138 (0.0178)		0.0107*** (0.0039)
RTW law $\times$ time to treat = 5		-0.0042*** (0.0010)		0.0026 (0.0194)		0.0147*** (0.0034)
Inverse Mills' Ratio	-0.0059 (0.0072)	-0.0057 (0.0073)	0.7034*** (0.0824)	0.7085*** (0.0838)		
<i>Fit statistics</i>						
Observations	4,418,333	4,228,141	602,095	572,653	4,749,830	4,546,156
R <sup>2</sup>	0.012	0.012	0.402	0.403	0.039	0.039
Within R <sup>2</sup>	0.010	0.010	0.385	0.385	0.030	0.030

*Note:* This table presents the outcomes from both static and dynamic TWFE models of RTW law on unionization rate, employment, and log hourly wages. Standard errors, clustered at the state level, are displayed in parentheses. Significance levels are denoted as follows: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Results from dynamic models are plotted in [Figure 1](#), [2](#), and [3](#). Due to space constraints, not all covariates are listed; complete results are available in the GitHub repository at [outputs/tables/twfe/](#). These estimations are computed using the `fixest` package in R. See the main text for details.



Table 28: Static and Dynamic TWFE Models of RTW Law on Log Hourly Wages Without Accounting for Sample Selection

Dependent variable:	Log hourly wages	
Model:	Static	Dynamic
<i>Variables</i>		
RTW law	-0.0133* (0.0070)	
RTW law $\times$ time to treat = -5		0.0031 (0.0088)
RTW law $\times$ time to treat = -4		-0.0027 (0.0100)
RTW law $\times$ time to treat = -3		-0.0085 (0.0118)
RTW law $\times$ time to treat = -2		-0.0106 (0.0095)
RTW law $\times$ time to treat = 0		0.0023 (0.0104)
RTW law $\times$ time to treat = 1		0.0014 (0.0126)
RTW law $\times$ time to treat = 2		-0.0144 (0.0122)
RTW law $\times$ time to treat = 3		-0.0280* (0.0160)
RTW law $\times$ time to treat = 4		-0.0286* (0.0167)
RTW law $\times$ time to treat = 5		-0.0140 (0.0195)
<i>Fit statistics</i>		
Observations	602,095	
R <sup>2</sup>	0.402	
Within R <sup>2</sup>	0.384	

*Note:* This table presents the outcomes from both static and dynamic TWFE models of RTW law on log hourly wages when we do not account for sample selection. Standard errors, clustered at the state level, are displayed in parentheses. Significance levels are denoted as follows: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Results from dynamic model are plotted in [Figure 15](#). Due to space constraints, not all covariates are listed; complete results are available in the GitHub repository at [outputs/tables/twfe/](#). These estimations are computed using the `fixest` package in R. See the main text for details.

Table 29: Diagnosis of Negative Weights in the TWFE Models

	Unionization rate		Log hourly wages		Employment	
	ATTs	Sum of weights	ATTs	Sum of weights	ATTs	Sum of weights
Positive weights	37	1	37	1	37	1
Negative weights	0	0	0	0	0	0

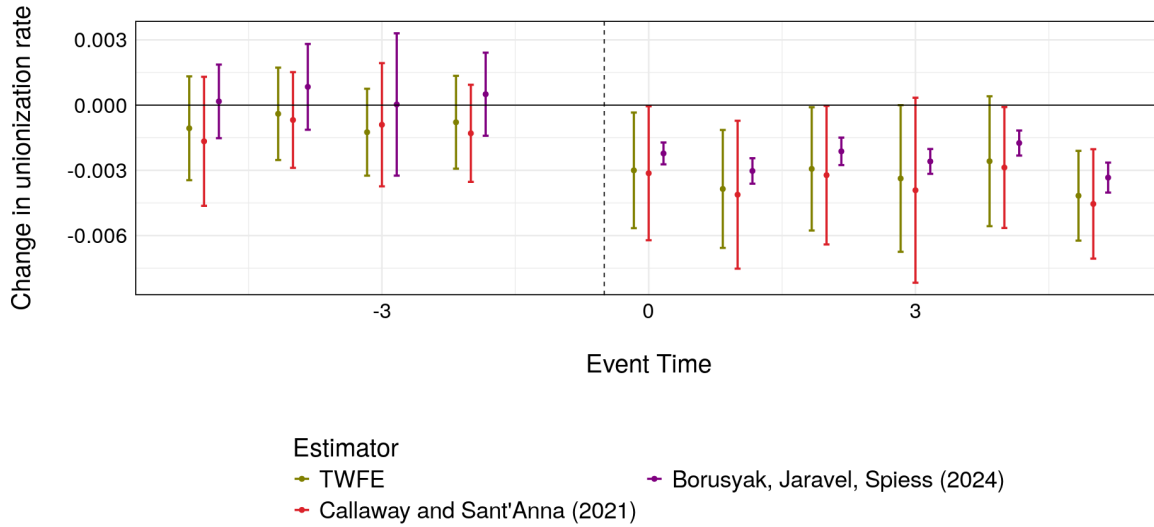
*Note:* This table displays the number of ATTs that receive negative weights in the TWFE models. It uses the methodology of [de Chaisemartin and D'Haultfoeuille \(2020\)](#). These estimations are computed using the `TwoWayFEWeights` package in R. See the main text for details.

Table 30: CS and BJS Estimators of RTW Law on Labor Market Outcomes

Dependent variable: Estimator:	Unionization rate		Log hourly wages		Employment	
	CS	BJS	CS	BJS	CS	BJS
<i>Variables</i>						
RTW law $\times$ time to treat = -5	-0.0017 (0.0015)	-0.0002 (0.0009)	0.0024 (0.0097)	0.0091 (0.0250)	-0.0007 (0.0068)	-0.0039 (0.0065)
RTW law $\times$ time to treat = -4	-0.0007 (0.0011)	0.0008 (0.0010)	-0.0025 (0.0084)	0.0021 (0.0197)	-0.0036 (0.0105)	-0.0075 (0.0099)
RTW law $\times$ time to treat = -3	-0.0009 (0.0014)	0.0000 (0.0017)	-0.0090 (0.0100)	-0.0034 (0.0210)	-0.0029 (0.0068)	-0.0048 (0.0061)
RTW law $\times$ time to treat = -2	-0.0013 (0.0011)	0.0005 (0.0010)	-0.0096 (0.0138)	-0.0052 (0.0255)	-0.0020 (0.0031)	-0.0025 (0.0031)
RTW law $\times$ time to treat = 0	-0.0031** (0.0016)	-0.0022*** (0.0003)	0.0023 (0.0098)	0.0076*** (0.0016)	0.0003 (0.0029)	0.0037** (0.0015)
RTW law $\times$ time to treat = 1	-0.0041** (0.0017)	-0.0030*** (0.0003)	0.0159 (0.0144)	0.0210*** (0.0023)	0.0031* (0.0021)	0.0069*** (0.0022)
RTW law $\times$ time to treat = 2	-0.0032** (0.0016)	-0.0021*** (0.0003)	0.0009 (0.0135)	0.0048* (0.0030)	0.0044* (0.0026)	0.0088*** (0.0027)
RTW law $\times$ time to treat = 3	-0.0039* (0.0022)	-0.0026*** (0.0003)	-0.0144 (0.0173)	-0.0093** (0.0043)	0.0111* (0.0063)	0.0135*** (0.0028)
RTW law $\times$ time to treat = 4	-0.0029** (0.0014)	-0.0017* (0.0003)	-0.0176 (0.0183)	-0.0102* (0.0055)	0.0108** (0.0046)	0.0145*** (0.0029)
RTW law $\times$ time to treat = 5	-0.0045*** (0.0013)	-0.0033*** (0.0004)	-0.0026 (0.0185)	0.0070 (0.0053)	0.0143*** (0.0034)	0.0187*** (0.0030)
Observations	4,228,141	4,228,141	572,653	572,653	4,546,156	4,546,156

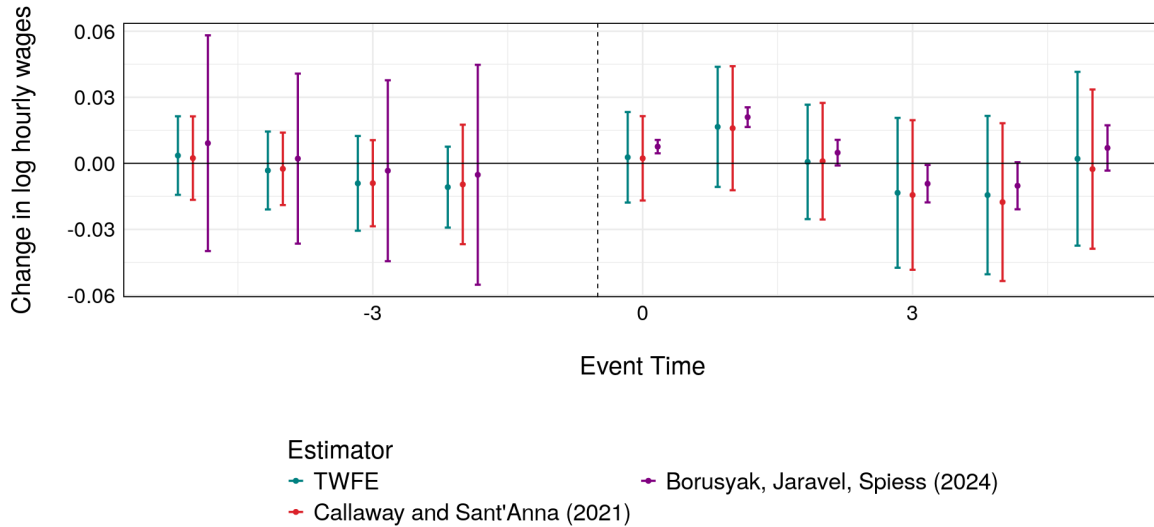
*Note:* This table presents the outcomes from both CS and BJS estimators of RTW law on unionization rate, employment, and log hourly wages. Standard errors, clustered at the state level, are displayed in parentheses. Significance levels are denoted as follows: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . These results are plotted in [Figure 4](#), [5](#), and [6](#). These estimations are computed using the `did` and `didimputation` packages in R. See the main text for details.

Figure 16: RTW Law on Unionization Rate: Comparison of TWFE, CS, and BJS Estimators



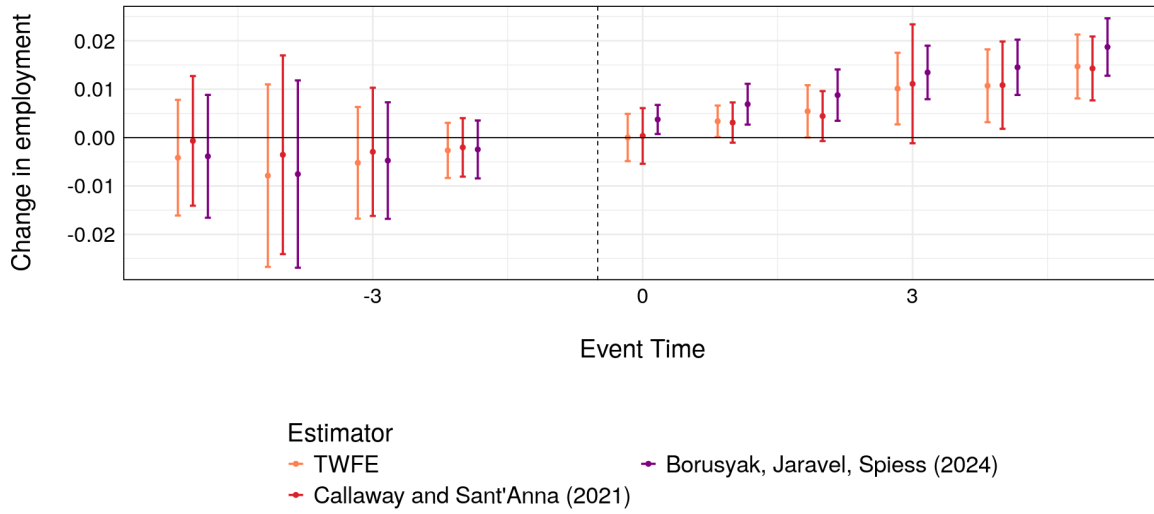
*Note:* The figure plot the estimated effects of RTW law on unionization rate using TWFE, [Callaway and Sant'Anna \(2021\)](#) and [Borusyak et al. \(2024\)](#) estimators, and includes 95% confidence intervals. Standard errors are clustered by state. Results are detailed in [Table 27](#) and [30](#). For the CS estimator, control states are never-treated ones. These estimations are computed using the `fixest`, `did`, and `didimputation` packages in R. See the main text for details.

Figure 17: RTW Law on Log Hourly Wages: Comparison of TWFE, CS, and BJS Estimators



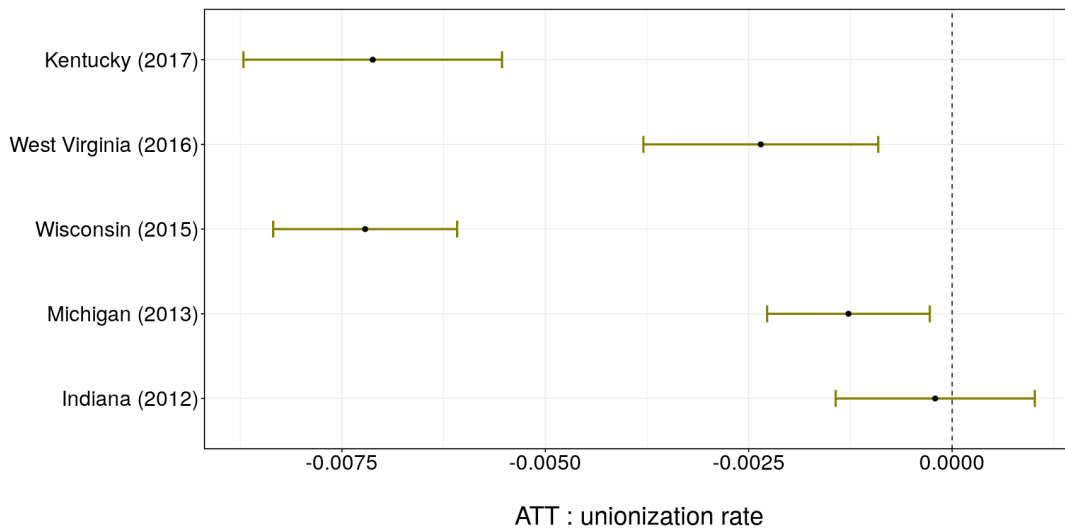
*Note:* The figure plot the estimated effects of RTW law on log hourly wages using TWFE, [Callaway and Sant'Anna \(2021\)](#) and [Borusyak et al. \(2024\)](#) estimators, and includes 95% confidence intervals. Standard errors are clustered by state. Results are detailed in [Table 27](#) and [30](#). For the CS estimator, control states are never-treated ones. These estimations are computed using the `fixest`, `did`, and `didimputation` packages in R. See the main text for details.

Figure 18: RTW Law on Employment: Comparison of TWFE, CS, and BJS Estimators



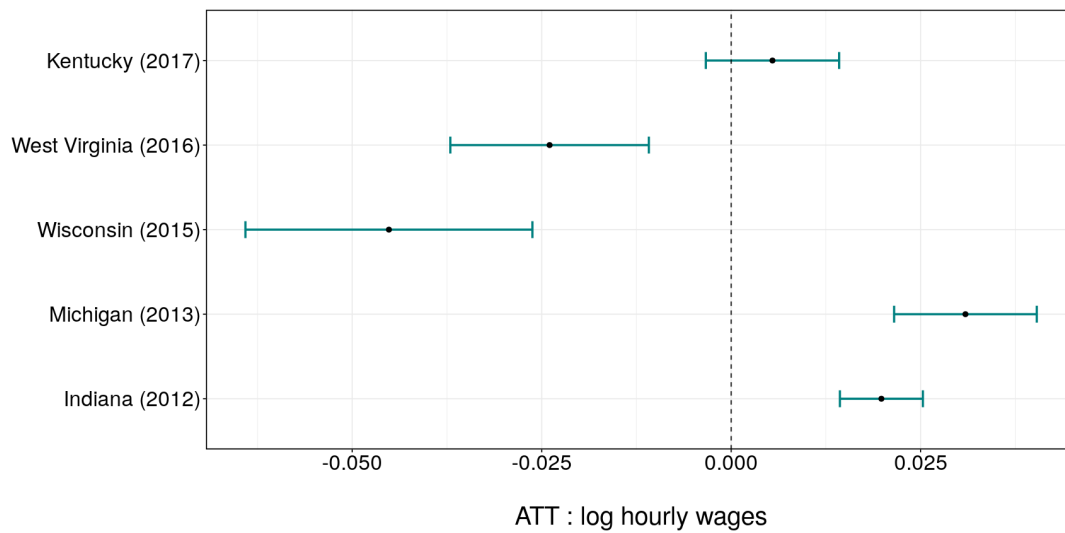
*Note:* The figure plot the estimated effects of RTW law on employment using TWFE, Callaway and Sant'Anna (2021) and Borusyak et al. (2024) estimators, and includes 95% confidence intervals. Standard errors are clustered by state. Results are detailed in Table 27 and 30. For the CS estimator, control states are never-treated ones. These estimations are computed using the `fixest`, `did`, and `didimputation` packages in R. See the main text for details.

Figure 19: State-Level Aggregated ATTs of RTW Law on Unionization Rate using CS Estimator



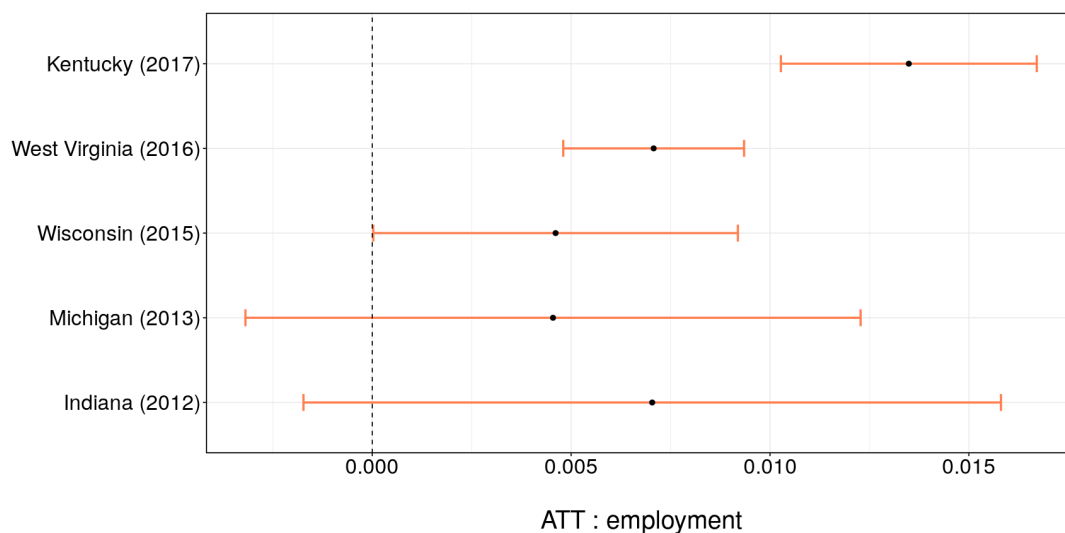
*Note:* The figure plot the estimated state-level aggregated ATTs of RTW law on unionization rate using Callaway and Sant'Anna (2021) estimator, and includes 95% confidence intervals. For each treated state, the adoption year of the RTW law is indicated in parentheses. Results are detailed in Table 31. These estimations are computed using the `did` package in R. See the main text for details.

Figure 20: State-Level Aggregated ATTs of RTW Law on Log Hourly Wages using CS Estimator



*Note:* The figure plot the estimated state-level aggregated ATTs of RTW law on log hourly wages using [Callaway and Sant'Anna \(2021\)](#) estimator, and includes 95% confidence intervals. For each treated state, the adoption year of the RTW law is indicated in parentheses. Results are detailed in [Table 31](#). These estimations are computed using the `did` package in R. See the main text for details.

Figure 21: State-Level Aggregated ATTs of RTW Law on Employment using CS Estimator



*Note:* The figure plot the estimated state-level aggregated ATTs of RTW law on employment using [Callaway and Sant'Anna \(2021\)](#) estimator, and includes 95% confidence intervals. For each treated state, the adoption year of the RTW law is indicated in parentheses. Results are detailed in [Table 31](#). These estimations are computed using the `did` package in R. See the main text for details.

Table 31: Summary of State-Level Aggregated ATTs of RTW Law on Labor Market Outcomes

Dependent variable:	Unionization rate	Log hourly wages	Employment
Kentucky (2017)	-0.0071*** (7e-04)	0.0055 (0.0040)	0.0135*** (0.0016)
West Virginia (2016)	-0.0024*** (6e-04)	-0.0240*** (0.0060)	0.0071*** (0.0011)
Wisconsin (2015)	-0.0072*** (5e-04)	-0.0452*** (0.0087)	0.0046** (0.0022)
Michigan (2013)	-0.0013*** (4e-04)	0.0309*** (0.0043)	0.0045 (0.0037)
Indiana (2012)	-0.0002 (5e-04)	0.0198*** (0.0025)	0.0070 (0.0042)

*Note:* This table summarizes the estimated state-level ATTs of RTW law on unionization, log hourly wages, and employment using the [Callaway and Sant’Anna \(2021\)](#) estimator. These results are plotted in [Figure 19](#), [20](#) and [21](#). These estimations are computed using the `did` package in R. See the main text for details.

Table 32: Summary of Sensitivity of Results to Violations of Parallel Trends Based on Relative Magnitude

$\bar{M}$	Unionization rate		Employment	
	Lower bound	Upper bound	Lower bound	Upper bound
0.1	-0.0063	-0.0007	0.0016	0.0134
0.2	-0.0071	-0.0002	-0.0015	0.0166
0.3	-0.0078	0.0004	-0.0051	0.0201
0.4	-0.0086	0.0010	-0.0089	0.0237
0.5	-0.0095	0.0016	-0.0127	0.0274

*Note:* This table summarizes the confidence intervals of our estimates for unionization rate and employment as a function of  $\bar{M}$  using the [Rambachan and Roth \(2023\)](#) approach. These results are plotted in [Figure 7](#) and [8](#). These estimations are computed using the `HonestDiD` package in R. See the main text for details.

Table 33: RTW on Labor Market Outcomes: Comparison Between Educated and Non-Educated Workers

Dependent variable:	Unionization rate		Log hourly wages		Employment	
Workers:	Educated	Non-educated	Educated	Non-educated	Educated	Non-educated
<i>Variables</i>						
RTW law $\times$ time to treat = -5	0.0003 (0.0015)	-0.0029 (0.0021)	0.0063 (0.0124)	-0.0004 (0.0103)	-0.0055 (0.0052)	-0.0009 (0.0082)
RTW law $\times$ time to treat = -4	0.0002 (0.0010)	-0.0012 (0.0022)	0.0084 (0.0135)	-0.0174** (0.0066)	-0.0074 (0.0085)	-0.0076 (0.0118)
RTW law $\times$ time to treat = -3	-0.0007 (0.0007)	-0.0021 (0.0018)	-0.0012 (0.0139)	-0.0168 (0.0102)	-0.0015 (0.0048)	-0.0096 (0.0085)
RTW law $\times$ time to treat = -2	-0.0004 (0.0011)	-0.0014 (0.0018)	-0.0067 (0.0066)	-0.0157 (0.0137)	0.0018 (0.0032)	-0.0084* (0.0043)
RTW law $\times$ time to treat = 0	-0.0021** (0.0010)	-0.0044* (0.0023)	0.0077 (0.0157)	-0.0027 (0.0046)	-0.0010 (0.0020)	0.0004 (0.0056)
RTW law $\times$ time to treat = 1	-0.0036*** (0.0010)	-0.0051* (0.0026)	0.0105 (0.0182)	-9.56e-5 (0.0079)	0.0007 (0.0020)	0.0060 (0.0046)
RTW law $\times$ time to treat = 2	-0.0022*** (0.0008)	-0.0049* (0.0027)	-0.0102 (0.0149)	-0.0114 (0.0150)	0.0035 (0.0023)	0.0058 (0.0058)
RTW law $\times$ time to treat = 3	-0.0022* (0.0012)	-0.0061** (0.0026)	-0.0208 (0.0191)	-0.0287* (0.0155)	0.0060 (0.0036)	0.0138** (0.0052)
RTW law $\times$ time to treat = 4	-0.0019 (0.0013)	-0.0046** (0.0021)	-0.0188 (0.0221)	-0.0323*** (0.0109)	0.0040 (0.0034)	0.0194*** (0.0067)
RTW law $\times$ time to treat = 5	-0.0029*** (0.0011)	-0.0069*** (0.0020)	-0.0093 (0.0217)	-0.0110 (0.0185)	0.0069* (0.0036)	0.0252*** (0.0054)
Inverse Mills' Ratio	-0.0003 (0.0102)	-0.0401*** (0.0129)	0.2552** (0.0827)	0.0467 (0.0732)		
<i>Fit statistics</i>						
Observations	2,772,932	1,455,209	347,245	225,408	2,921,662	1,624,494
R <sup>2</sup>	0.011	0.014	0.366	0.312	0.020	0.046
Within R <sup>2</sup>	0.009	0.012	0.344	0.301	0.012	0.035

*Note:* This table presents the outcomes for both educated and non-educated workers from TWFE models of RTW law on unionization rate, employment, and log hourly wages. A worker is considered non-educated if they did not complete any formal education beyond high school, or if they dropped out before finishing high school; all others are classified as educated. Standard errors, clustered at the state level, are displayed in parentheses. Significance levels are denoted as follows: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . For unionization rate and employment, these results are plotted in [Figure 9](#) and [10](#). Due to space constraints, not all covariates are listed; complete results are available in the GitHub repository at [outputs/tables/twfe\\_sensitivity\\_analysis/](#). These estimations are computed using the `fixest` package in R. See the main text for details.



Table 34: Unionization Rates per Industries for Treated and Untreated States

Industry category	Unionization rates	
	Treated states before adoption	Never treated states
Business and personal services	0.75%	1.10%
Construction	5.70%	4.74%
FIRE	0.62%	0.52%
Manufacturing	3.99%	2.35%
Trade	1.07%	1.23%
Transportation and utilities	4.70%	4.37%
Welfare and education	1.77%	2.24%
Median	1.77%	2.24%

*Note:* This table displays the unionization rates of each industry for treated states prior to the adoption of RTW laws, and never treated states. These results serve as a basis for categorizing industries into two groups: highly unionized and low unionized. See the main text for details.

Table 35: RTW on Labor Market Outcomes: Comparison Between Sectors with High and Low Unionization

Dependent variable:	Unionization rate		Log hourly wages	
Sector's unionization:	High	Low	High	Low
<i>Variables</i>				
RTW law $\times$ time to treat = -5	-0.0006 (0.0018)	-0.0015** (0.0007)	-0.0008 (0.0086)	0.0089 (0.0099)
RTW law $\times$ time to treat = -4	-0.0001 (0.0012)	-0.0005 (0.0011)	-0.0063 (0.0062)	-0.0009 (0.0141)
RTW law $\times$ time to treat = -3	-0.0016 (0.0016)	-0.0004 (0.0006)	0.0023 (0.0109)	-0.0266 (0.0167)
RTW law $\times$ time to treat = -2	-0.0016 (0.0016)	0.0004 (0.0009)	-0.0054 (0.0088)	-0.0215 (0.0199)
RTW law $\times$ time to treat = 0	-0.0041** (0.0017)	-0.0013 (0.0009)	0.0058 (0.0071)	-0.0025 (0.0162)
RTW law $\times$ time to treat = 1	-0.0054*** (0.0017)	-0.0019* (0.0011)	0.0094 (0.0113)	0.0180 (0.0202)
RTW law $\times$ time to treat = 2	-0.0046** (0.0021)	-0.0008 (0.0005)	-0.0025 (0.0101)	-0.0027 (0.0191)
RTW law $\times$ time to treat = 3	-0.0059** (0.0022)	-9.49e-7 (0.0011)	-0.0170 (0.0129)	-0.0159 (0.0248)
RTW law $\times$ time to treat = 4	-0.0045** (0.0020)	6.62e-5 (0.0011)	-0.0216 (0.0155)	-0.0131 (0.0209)
RTW law $\times$ time to treat = 5	-0.0060*** (0.0018)	-0.0017* (0.0010)	-0.0080 (0.0161)	0.0084 (0.0260)
Inverse Mills' Ratio	-0.0179 (0.0121)	0.0048 (0.0057)	0.2949** (0.1068)	0.9059*** (0.1001)
<i>Fit statistics</i>				
Observations	2,351,010	1,877,131	316,529	256,124
R <sup>2</sup>	0.010	0.005	0.345	0.403
Within R <sup>2</sup>	0.007	0.003	0.326	0.379

*Note:* This table presents the outcomes for workers from sectors with high unionization and low unionization, based on TWFE models of RTW law on unionization rate, and log hourly wages. Sectors with high unionization include construction, manufacturing, transportation and utilities, as well as welfare and education. Sectors with low unionization are FIRE, trade, business and personal services. Standard errors, clustered at the state level, are displayed in parentheses. Significance levels are denoted as follows: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . The results for unionization rate are plotted in [Figure 11](#). Due to space constraints, not all covariates are listed; complete results are available in the GitHub repository at [outputs/tables/twfe\\_sensitivity\\_analysis/](#). These estimations are computed using the `fixest` package in R. See the main text for details.

Table 36: RTW on Unionization Rate and Log Hourly Wages for Non-Educated Workers in Highly Unionized Sectors

<i>Variables</i>	Unionization rate	Log hourly wages
RTW law $\times$ time to treat = -5	-0.0032 (0.0036)	-0.0057 (0.0116)
RTW law $\times$ time to treat = -4	-0.0004 (0.0034)	-0.0153*** (0.0052)
RTW law $\times$ time to treat = -3	-0.0025 (0.0031)	-0.0101 (0.0179)
RTW law $\times$ time to treat = -2	-0.0022 (0.0028)	-0.0008 (0.0124)
RTW law $\times$ time to treat = 0	-0.0070* (0.0037)	-0.0048 (0.0062)
RTW law $\times$ time to treat = 1	-0.0073* (0.0037)	-0.0030 (0.0053)
RTW law $\times$ time to treat = 2	-0.0062 (0.0041)	-0.0120 (0.0175)
RTW law $\times$ time to treat = 3	-0.0105** (0.0042)	-0.0263 (0.0175)
RTW law $\times$ time to treat = 4	-0.0070* (0.0035)	-0.0324*** (0.0084)
RTW law $\times$ time to treat = 5	-0.0106*** (0.0036)	-0.0110 (0.0134)
Inverse Mills' Ratio	-0.0503** (0.0186)	-0.0362 (0.1042)
<i>Fit statistics</i>		
Observations	726,402	113,078
R <sup>2</sup>	0.013	0.274
Within R <sup>2</sup>	0.009	0.262

*Note:* This table presents the outcomes for non-educated workers from sectors with high unionization, based on TWFE models of RTW law on unionization rate, and log hourly wages. A worker is considered non-educated if they did not complete any formal education beyond high school, or if they dropped out before finishing high school. Sectors with high unionization include construction, manufacturing, transportation and utilities, as well as welfare and education. Standard errors, clustered at the state level, are displayed in parentheses. Significance levels are denoted as follows: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . The results are plotted in [Figure 12a](#) and [Figure 12b](#). Due to space constraints, not all covariates are listed; complete results are available in the GitHub repository at [outputs/tables/twfe\\_sensitivity\\_analysis/](#). These estimations are computed using the `fixest` package in R. See the main text for details.

## GitHub Repository Structure

- **Scripts:**

- `1_init_environment.R`: Initializes the R environment and loads necessary packages.
- `2_import_and_process_data.R`: Manages data importation and preliminary processing.
- `3_generate_descriptive_statistics.R`: Summarizes dataset characteristics through descriptive statistics.
- `4_heckman_selection_model.R`: Applies sample selection correction using the Heckman model.
- `5_conduct_event_studies.R`: Estimates RTW law impact with traditional TWFE estimators.
- `6_use_frisch_waugh_theorem.R`: Applies Frisch-Waugh theorem to estimate regression coefficients, controlling for other variables.
- `7_apply_robust_estimators.R`: Uses heterogeneity-robust estimators.
- `8_test_parallel_trend_sensitivity.R`: Tests the sensitivity of results to the parallel trend assumption.
- `9_perform_other_sensitivity_analysis.R`: Estimates RTW law impact on specific groups.

- **Modules, data, and outputs:**

- `data/`: Contains quality flags for earnings data.
- `modules/`: Contains reusable functions for scripts.
- `outputs/`: Contains plots and tables generated by scripts.

- **Final document:**

- `thesis.pdf`: Fully edited master thesis.

- **Other files:**

- `renv.lock`: Generated by `renv`, contains R package dependencies for the project.

The project repository is accessible on GitHub at: [https://github.com/JulienPeignon/master\\_thesis](https://github.com/JulienPeignon/master_thesis)