# Assessing the Employee Welfare Impact of Right-to-Work Laws: Insights from State-Level Legislation

**Abstract:**

We examine the effect of Right-to-Work (RTW) laws on employee welfare using the stacked difference-in-differences (DiD) methodology. We posit that RTW laws weaken collective bargaining power, increase free-riding, and ultimately reduce employee welfare. We document a significant employee welfare decline following RTW adoption, especially among firms in highly unionized industries. The deterioration is driven by reductions in employee involvement, profit-sharing, and work-life-balance provisions. Results remain robust to alternative specifications, local unobservables, endogeneity concerns, and multiple identification checks. Overall, our findings highlight important implications for policymakers, labour unions, and employers considering the broader consequences of RTW legislation.

*Keywords: Bargaining power, Employee welfare, Free rider, Right-to-Work Law, Unionization*

# 1. INTRODUCTION

The Labor-Management Relations Act of 1947, also known as the Taft-Hartley Act, permitted state legislatures to enact Right-to-work (RTW) laws while upholding the provision that all covered workers must receive equal application of contract terms, irrespective of their membership status in a labor union. RTW laws, which have been passed by 28 states in the US, prevent unions from enforcing contracts that require workers to join a union and give employees the right to choose whether or not to join or financially support a labor union (Miller, 1976; Farber, 1984; Meszaros and Quistorff, 2023). This means that even non-members benefit from the terms negotiated in collective bargaining[1] agreements (Bono-Lunn, 2024), allowing them to access union services without financially supporting the union (Devinatz, 2011).

However, it is argued that it is unfair for union members to financially support the union and provide a free ride for those employees who are non-members (Delaney, 1998). As such, there is ongoing debate over RTW legislation since it started to be implemented by individual states over 80 years ago (Devinatz, 2011). One side of the argument is the belief that RTW laws weaken the financial stability and bargaining power of unions because all employees obtain collective bargaining services although some provide no financial support to the union (Devinatz, 2011). This creates a "free rider" problem, where non-members enjoy the benefits of union representation without sharing the costs (Ichniowski and Zax, 1991). However, on the other hand, employers argue that employees should not be mandated to join or financially support a labor organization/union as a condition of their employment (Devinatz, 2011).

Recent research highlights the broad influence of unions on labor market outcomes, including job satisfaction (Artz et al., 2022; Blanchflower and Bryson, 2020; Laroche, 2017), productivity and wages (Barth et al., 2020; Benassi and Vlandas, 2022; Callaway and Collins,

[1] Unions primarily engage in collective bargaining, wherein they negotiate agreements with employers on behalf of their members (Ewing and Hendy, 2017).

2018; Frandsen, 2021), income inequality (Farber et al., 2021), and gender pay gap (Card et al., 2020; Triventi, 2013). These studies underscore unions' central role in shaping both pecuniary and non-pecuniary aspects of employment. If the unions' collective bargaining powers are weakened as a result of the adoption of RTW laws, it is likely that non-monetary working conditions may also deteriorate (Cassar and Meier, 2018; Earle and Pencavel, 1990; Mas and Pallais, 2017). For example, weaker unions may leave employees exposed to poorer working environments without sufficient financial compensation (Ashenfelter et al., 2022; Lang and Majumdar, 2004; Manning, 2021).

To examine this empirically, we study the causal impact of union strength on employee welfare by examining the adoption of state-level RTW laws using the stacked difference-in-differences (DiD) approach. We construct an employee welfare index using data from the KLD STATS database, based on the difference between identified strengths and concerns in the "employee relations" category, while excluding union-related items to capture broader aspects of employee treatment.[2] We are the first to show that RTW laws exhibit a significant adverse effect on employee welfare within these states through increasing free ridership and diminishing collective bargaining power. We also find that the negative impact of RTW on employee welfare is most pronounced in firms operating in highly unionized industries. The negative impact emerges gradually and becomes statistically significant from the year of adoption through the post-adoption period, highlighting the causal effect of RTW on employee welfare.

Moreover, we disaggregate employee welfare components, revealing that the overall decline in employee welfare is primarily driven by reductions in employee involvement, profit sharing, and work-life balance provisions. While our baseline DiD models control for observable state-

[2] We thank the editor for suggesting the exclusion of union-related items from our employee welfare measure. Please refer to Appendix C for a detailed explanation of the construction of our employee welfare measure.

level economic conditions, unobserved local factors could still bias our results. To address this, we re-estimate our models using control firms located in neighboring non-RTW states, which share similar local dynamics. The results remain qualitatively similar, suggesting that our findings are driven by RTW adoption rather than unobserved local conditions. Our findings also remain robust when accounting for potential endogeneity, alternative measures of the dependent variable, refined sample criteria, and the addition of further firm- and state-level political and economic variables.

Our paper has several important contributions. First, our study contributes to the existing literature on RTW laws by providing empirical evidence on the effect of these laws on employee welfare. Previous studies show that RTW laws reduce the collective bargaining power of workers and, consequently, negatively affect the wages of unionized workers (Chava et al., 2020). In addition, it is also documented that RTW laws are associated with substantial increases in free riding among private and public sector workers (Bono-Lunn, 2024). We contribute to this literature by showing that reduced collective bargaining power and increased instances of free riding are associated with lower employee welfare.

Second, the paper adds to the literature looking at the impact of labor laws on employees' working conditions, such as job satisfaction, productivity and wages, income inequality, gender pay gap, and non-pecuniary working conditions (Artz et al., 2022; Ashenfelter et al., 2022; Barth et al., 2020; Benassi and Vlandas, 2022; Blanchflower and Bryson, 2020; Callaway and Collins, 2018; Card et al., 2020; Cassar and Meier, 2018; Farber et al., 2021; Frandsen, 2021; Earle and Pencavel, 1990; Laroche, 2017; Lang and Majumdar, 2004; Manning, 2021; Mas and Pallais, 2017). In this context, understanding the implications of RTW laws for employee welfare is of critical importance. A closely related study is that of Makridis (2019); he uses individual-level data from Gallup's US Daily Poll, a nationally representative survey of approximately 1,000 respondents per day since 2008, to explore the effects of RTW laws on

self-reported well-being measures, including current and expected life satisfaction. While his findings suggest a modest increase in perceived well-being following RTW adoption, our study offers a complementary perspective by focusing on objective indicators of employee welfare at the firm level. Whereas Makridis (2019) relies on subjective, perception-based outcomes at the individual level, our study employs firm-level data from the KLD STATS database to examine observable indicators of employee welfare. This database provides a robust proxy for employee well-being, capturing observable and verifiable corporate practices such as retirement benefits, profit-sharing, and stock options-measures that reflect actual employer behavior and have been widely adopted in the literature as credible proxies for internal stakeholder treatment (Amini et al., 2023; Rind et al., 2022). In doing so, this study not only complements Makridis (2019) by providing a firm-level perspective but also addresses a key limitation of the Gallup data. Since Gallup identifies respondents by state of residence rather than workplace, it may misclassify workers who live in one state but are employed in another, particularly in metropolitan or border regions where state boundaries are close but economic areas are highly integrated.[3] Consequently, using residence-based identifiers may lead to inaccurate assignment of RTW exposure, whereas our firm-level data capture the actual policy environment in which employment relationships occur.

We also extend this literature by incorporating additional objective measures of employee welfare. In particular, we analyze employee turnover, calculated as the number of departures divided by the average number of employees, which is a meaningful proxy for worker satisfaction and organizational stability. By including this complementary metric, we provide a more holistic picture of how RTW laws affect employee outcomes. Our study also provides a novel lens through which to evaluate the welfare implications of RTW laws. Specifically, we

[3] For example, many individuals commute from Indiana or Wisconsin, both of which adopted RTW laws during our sample period, to Illinois (Chicago) for work.

highlight the critical roles of union collective bargaining power and the potential for free riding in shaping these effects, offering a clearer understanding of how RTW laws impact overall employee welfare. We find that RTW adoption is associated with a significant deterioration in employee welfare, particularly in firms with higher collective bargaining power and higher levels of free ridership. By moving beyond individual perceptions and focusing on observable corporate responses, our analysis offers a distinct and meaningful contribution to understanding the broader consequences of RTW legislation.

Third, the paper contributes by unpacking the composite employee welfare index to assess whether the effects of RTW laws are concentrated in specific welfare dimensions. We break the index into three key components, employee involvement, financial benefits, and broader welfare practices, and find that RTW adoption significantly reduces employee involvement and broader welfare provisions such as profit-sharing and work/life balance.

Fourth, our research is particularly timely, given that six states have enacted RTW laws since 2001. Our paper is leveraging the introduction of RTW laws in multiple states in recent years to isolate the specific impact of these laws, while accounting for state-specific effects and other firm characteristics that may affect firms' employee welfare. This contribution is especially relevant in light of current policy debates among policymakers regarding the adoption of the Protecting the Right to Organize (PRO) Act, which is a US legislative initiative designed to enhance workers' ability to organize and engage in collective bargaining. Our findings hold significance for policymakers, labor unions, and business stakeholders, offering insights into the potential ramifications of the PRO Act on the labor market dynamics and the balance of power between employers.

The rest of the paper is structured as follows. Section 2 outlines the theoretical background and develops the hypotheses. Section 3 describes the research methodology. Section 4 reports the empirical results and robustness checks. Finally, section 5 concludes.

## 2. THEORETICAL BACKGROUND AND HYPOTHESES

RTW laws have long been central to debates on labor market institutions and the distribution of power between employers and employees. By prohibiting mandatory union membership, RTW laws allow employees to benefit from union-negotiated agreements without contributing to the costs of collective representation. This institutional design fosters "free riding", thereby reducing union membership and financial resources and, ultimately, constraining unions' capacity to bargain effectively on behalf of workers. As a result, the erosion of union power serves as a key mechanism through which RTW laws may influence employee welfare. The literature frames the effects of RTW laws on the labor market and union power through three main hypotheses: the *free ridership hypothesis*, the *bargaining power hypothesis*, and the *taste hypothesis* (Moore, 1998; Moore and Newman, 1985).

The *free ridership hypothesis* focuses on the economic incentives created by RTW laws and their effects on union membership and resources. According to the *free ridership hypothesis*[4], the presence of RTW laws increases the costs associated with establishing and maintaining unions (Devinatz, 2011). This is because mandatory union membership can no longer be specified in the collective bargaining agreement (Moore and Newman, 1985). As a result, unions may face greater difficulties in funding their operations and representing workers effectively, as they are dealing with a larger proportion of free riders who benefit from union negotiations without contributing financially. Consequently, unions in RTW states may be less

[4] See Bennett and Johnson (1979), Booth and Bryan (2004), and Eren (2009) for free ridership problems in unions and Davis and Huston (1993), Freeman and Medoff (1984), Hirsch (1980), Sobel (1995), and Zax and Ichniowski (1991) for RTW laws inducing free riding.

able to provide comprehensive services and support to their members and may have limited capacity to engage in collective bargaining and advocacy efforts on behalf of their workers. Using Current Population Survey (CPS) data to empirically identify the impact of state RTW laws on union status, Bono-Lunn (2024) shows that RTW laws are associated with substantial increases in free riding among private and public sector workers. His findings indicate that increased free riding combined with declines in union coverage leads to a decline in union membership.

Therefore, under the *free ridership hypothesis*, by permitting non-union members to benefit from union services, the costs associated with organizing and maintaining unions increase, resulting in a constraint on the supply of union services (Davis and Huston, 1993; Sobel, 1995; Zax and Ichniowski, 1991). A decline in union-provided services can have significant negative consequences for workers, including lower wages[5] (Carroll, 1983; Fortin et al., 2023; Garofalo and Malhotra, 1992; Mishel, 2001; Moore, 1980; Wessels, 1981), reduced job satisfaction (Blanchflower and Bryson, 2020), and deteriorating working conditions including weaker benefit standards, longer working hours, and diminished protections against workplace hazards (Dawkins, 2010; Gihleb et al., 2023; Malinowski et al., 2015; Wright, 2016; Zoorob, 2018)[6]. As unions lose membership and financial resources, their ability to negotiate effectively declines. This trajectory of declining union membership and resource availability, as explained by the free ridership hypothesis, subsequently informs the *bargaining power hypothesis*, which posits that RTW laws lead to a decrease in the negotiating power of unions. This is because RTW laws result in a decline in union membership, reducing the unions' ability to initiate

[5] We acknowledge that the existing literature finds mixed effects on the impact of RTW laws on wages (Chava et al., 2020; Kalenkoski and Lacombe, 2006; Reed, 2003).

[6] As mentioned by Blanchflower and Bryson (2022), economists have recognized, since the seminal studies by Freeman (1977) and Borjas (1979), that there exists a negative correlation between trade union membership and job satisfaction. However, as documented by Blanchflower and Bryson (2022) using data from the United States and Europe on nearly two million respondents, "this is no longer true: today the partial correlation is positive".

strikes and weakening their overall bargaining position to negotiate wages and working conditions. In such a situation, the unions will supply fewer benefits to their members, which results in lower motivation of the employees to join the union and an ultimate lower demand for union services (Moore and Newman, 1985). This decline in collective bargaining power makes it harder for unions to secure collective agreements that benefit all employees in the workplace (Popejoy, 2010).

Current studies suggest that such a decrease in union collective bargaining power has a significant impact on both employees and firms. For instance, Chava et al. (2020) show that RTW laws reduce the collective bargaining power of workers and consequently, negatively affect the wages of unionized workers. At the same time, they show that after the introduction of RTW laws, firms' collective bargaining power increases, which leads to higher investment, higher profitability (for labor-intensive firms), and an increase in employment. With weakened leverage during negotiations, unions may find it challenging to maintain or improve wages, benefits, and working conditions for their members (Cassar and Meier, 2018; Chava et al., 2020). This can result in diminished overall benefits for workers covered by collective bargaining agreements in industries or workplaces affected by RTW laws, as employers experience less pressure to offer competitive compensation or maintain adequate workplace standards. This shift in bargaining power can drive down wages, erode job quality, and ultimately reduce employee welfare. In the absence of strong union representation, the workplace environment becomes less favorable for workers (Devinatz, 2011; Moen, 2017).

More specifically, strong unions serve as a vital mechanism for promoting employee welfare by providing workers with a collective voice to express concerns, advocate for their rights, and influence workplace policies (Kochan et al., 2019). Through collective bargaining and representation, unions empower employees to negotiate better wages, safer working conditions, and more equitable treatment, thereby enhancing their overall quality of life. This

empowerment fosters a more inclusive and democratic work environment where workers feel heard and valued (Hayter and Visser, 2021). Moreover, unions play a crucial role in building a sense of solidarity and community among workers, which can significantly boost morale, job satisfaction, and psychological well-being (Blanchflower and Bryson, 2020). The presence of a supportive union structure can reduce feelings of isolation and increase trust in the workplace, contributing to a healthier organizational culture. In addition to advocacy, unions often provide tangible benefits that directly support employee welfare. These may include access to professional development opportunities, legal assistance, health insurance, retirement plans, and other social protections (Hagedorn, 2016). Such services not only improve the material conditions of workers but also contribute to long-term stability and security, reinforcing the union's role as a cornerstone of employee welfare.

Taken together, under the *free ridership* and *bargaining power hypotheses* (see Figure 1 for the overall conceptual framework), we expect that the adoption of RTW laws deteriorates employee welfare due to unions' weakened position and their lower ability to provide such services and benefits to their members. Therefore, we hypothesize that:

*H1a: The adoption of RTW laws leads to a decline in employee welfare.*

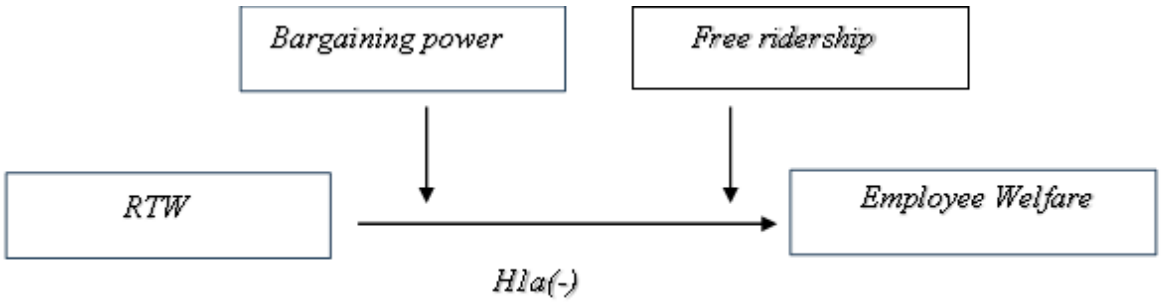


*Figure 1: The conceptual framework- RTW and employee welfare*

Building on the previous hypothesis, which posits that RTW laws reduce employee welfare by weakening union power and limiting the ability of unions to provide services and protections, we argue that these effects are not uniform across all firms. Instead, the impact of RTW laws on employee welfare depends on how strong union representation was before the law's adoption. In highly unionized firms, RTW laws represent a significant institutional shift that directly undermines established collective bargaining structures, leading to greater disruptions in wages, benefits, and working conditions supported by unions.

In contrast, firms with low union coverage, i.e. where unions were already weak or absent, are likely to experience minimal changes following RTW adoption as union influence was already minimal and employment practices were largely shaped by non-union norms. A broad body of empirical evidence supports this view. Numerous studies show that RTW laws lead to significant declines in union membership and activity (e.g., Feigenbaum et al., 2018; Hogler et al., 2004; Chun, 2023; VanHeuvelen, 2023; Zullo, 2008). For example, Fortin et al. (2023) suggest that the impact of RTW varies based on the underlying unionization rates of different industries. These findings suggest that RTW laws primarily weaken unions where they are most active and embedded, reducing the benefits unions can offer and thus having a stronger negative effect on workers in those firms. Recent empirical studies support this view, showing that the economic effects of RTW policies, such as increases in firm innovation and shifts in bargaining dynamics, are concentrated among highly unionized firms (Nguyen and Qiu, 2022). In line with this perspective, we argue that the negative impact of RTW laws on employee welfare is not uniform across all firms, but instead depends on the firm's pre-existing level of unionization. Specifically, we argue that the adverse consequences of RTW laws, particularly declines in employee welfare, are likely to be concentrated among firms with higher union coverage. We, therefore, hypothesize that:

*H1b: The negative impact of RTW laws on employee welfare is concentrated among highly unionized firms.*

In contrast to the free ridership and bargaining power hypotheses, the *taste hypothesis* offers an alternative explanation. It suggests that the adoption of RTW laws is primarily driven by widespread anti-union sentiment among workers within a state, rather than the laws themselves directly influencing unionization outcomes (Chava et al., 2020). In this view, RTW laws reflect the prevailing "tastes" or preferences regarding unionism (Farber, 1984; Lumsden and Petersen, 1975; Wessels, 1981). As such, RTW law does not independently alter the demand for or the supply of union representation, nor does it directly diminish union membership itself. Instead, RTW laws primarily reflect a state's widespread negative sentiments towards unionism. Therefore, it is the hostility towards unionism, and not the presence of the RTW law itself, that makes employees less interested in union representation (Moore and Newman, 1985). This implies that the enactment of RTW laws may be more reflective of the political and social attitudes towards unions within those states rather than a direct response to specific economic or labor market conditions. In this vein, Farber (1984), Lumsden and Peterson (1975), Moore and Newman (1975), and Wessels (1981) show that the lower percentage of the unionized workforce in RTW states was attributed to the tastes and preferences of the population rather than the presence of RTW laws.

Existing research on the effects of RTW laws offers mixed evidence regarding the *taste hypothesis* (Devinatz, 2015) but provides stronger empirical support for the *free ridership* and *bargaining power hypotheses*. This suggests that while the direct impact of RTW laws on unionization rates may be inconclusive, there is consistent evidence that these laws significantly influence union dynamics and labor relations. Overall, the wide variation in empirical findings around the *taste*, *free rider*, and *bargaining power hypotheses* reflects the

complexity of the RTW issue (Popejoy, 2010)[7]. Nevertheless, according to the taste hypothesis, RTW laws primarily reflect workers' pre-existing preferences for non-union employment rather than exerting a direct influence on union membership decisions (Farber, 1984). Consequently, employees are assumed to make rational choices regarding union participation based on the perceived costs and benefits of membership, independently of the presence of RTW legislation. Given that unions are generally intended to enhance employee welfare through improvements in wages and working conditions (Bryson et al., 2004), under the *taste hypothesis*[8], RTW laws are not expected to exert a significant impact on employee welfare.

In light of these competing theoretical perspectives, the following section outlines the methodological approach employed to empirically assess the influence of RTW laws on employee welfare.

# 3. RESEARCH METHOD

## 3.1 Data collection

We commence our sample construction by identifying all Compustat[9] firms headquartered in the United States during the period spanning 2001 to 2019. We chose 2001 as our starting point because data on employee welfare, produced by the KLD STATS database, is accessible from 1995 and there is no RTW adoption between 1995 and 2001.[10] Also, KLD data ceased in 2019, marking our ending point. Consequently, six states, Oklahoma (2001), Indiana (2012), Michigan (2013), Wisconsin (2015), West Virginia (2016), and Kentucky (2017), recognized

[7] With respect to the effect of RTW law on union membership, refer to Farber (1984), Lumsden and Peterson (1975), Moore and Newman (1975), and Wessels (1981) supporting the taste hypothesis, Warren and Strauss (1979) supporting the free ridership hypothesis, and Moore and Newman (1985) supporting the bargaining power hypothesis.

[8] This is despite the controversy in the literature on the impact of union membership on job satisfaction and employee well-being (Green and Heywood, 2015; Hipp and Givan, 2015; Laroche, 2017).

[9] Please note Compustat includes financial statements and market data for publicly traded companies so our analysis focuses exclusively on public firms.

[10] In the analysis section, we construct cohorts covering the five years prior to RTW adoption using the stacked DiD framework and conduct pre-trend analyses. Specifically for Oklahoma, which adopted RTW in 2001, we include data starting from 1996 to ensure sufficient pre-treatment observations.

the RTW during our sample period. Our approach is similar to Chava et al. (2020), who study five RTW introductions in their sample period. They argue that studying a smaller number of RTW introductions reduces the risk of omitted variable bias. It is unlikely that any unobserved factors, including globalization or anti-union sentiment, would coincide exactly with the timing of these laws in those specific states and years.[11] We exclude financial firms (SIC codes: 6000–6999) due to their distinct statutory capital requirements compared to non-financial firms, as well as utility firms (SIC codes: 4900–4999) because they are subject to state-specific regulations. Additionally, we impose criteria requiring all firm years to exhibit non-negative sales revenue and total assets. As a result, our baseline sample comprises 6,132 unique firms spanning the 2001-2019 period.

## 3.2 Empirical model and measures

Since several states adopted RTW laws at different times, a staggered DiD approach would be a natural choice. However, recent econometric research (e.g., Callaway and Sant'Anna, 2021; Sun and Abraham, 2021) suggests that even with random treatment assignment, standard DiD regressions with staggered timing often fail to provide valid estimates of the average treatment effect on the treated (ATT). As a result, several studies have adopted the event-based stacked DiD methodology, which uses alternative groups as controls (e.g., Gormley and Matsa, 2011; Baker et al., 2022). The stack-cohort approach creates event-specific "clean 2 × 2" datasets with outcome variables and controls for treated cohorts and "clean" controls within the treatment window (Baker et al., 2022). Following Gormley and Matsa (2011), we define each RTW event as a cohort and combine the treatment and control firms' data across cohorts. For each cohort, we keep observations within a –5 to +5-year window, ensuring control firms

[11] The majority of articles in the RTW literature rely solely on a single cross-section of data without considering any legislative changes (Garofalo and Malhotra, 1992; Hundley, 1993; Holmes, 1998). Additionally, Matsa's (2010) study stands out for encompassing a sample period containing three RTW introductions.

remain untreated for the next 5 years resulting in 98,595 firm-year observations.[12] This transforms the firm-year sample into a firm-year-cohort sample, allowing firm and year fixed effects to vary by cohort, a more conservative approach than simple fixed effects, and hence we estimate the following model, Equation (1):

$$Employee\ welfare_{i,s,t+1} = \alpha + \beta RTW_{s,t} + \gamma X_{i,s,t} + \omega_{ic} + \tau_{tc} + \varepsilon_{i,c,s,t} \quad \text{Eq. (1)}$$

where *i* denotes firm *i*, *s* indicates the firm's headquarters state, *c* refers to the treatment cohort, and *t* is the current year. RTW is a dichotomous variable indicating whether the *RTW* has been adopted by the firm's headquarters state by year *t*. *X* is a vector of firm and state-level control variables defined in Table A1 and measured in year *t*, $\omega_{ic}$ is the firm-cohort fixed effects controlling for any fixed differences between firms, and $\tau_{tc}$ is the year-cohort fixed effects to control for any secular time trends within cohorts. We cluster standard errors at the state level, given that the key independent variable of interest, RTW, is a state-specific variable.

To measure employee welfare, we use the KLD STATS database, focusing on the "employee relations" dimension for a given year. Following the widely adopted proxy for employee relations (Ghaly et al., 2015; Rind et al., 2022; Amini et al., 2023), we calculate employee welfare by summing the identified strengths score and subtracting the aggregate concerns score to determine the net score for each year, as outlined below.[13] Employee welfare is measured at t + 1 to reduce simultaneity and reverse-causality concerns, allowing time for RTW laws to influence firm policies and workforce outcomes.[14]

$$Employee\ welfare1_{i,t+1} = \sum Strength_{i,t+1} - \sum Concern_{i,t+1} \quad \text{Eq. (2)}$$

[12] We define control states as those that either (1) never adopted RTW, or (2) adopted RTW outside of the [-5, +5] window relative to the treatment year of a given cohort. Importantly, states that adopted RTW laws after the +5-year window are still eligible to serve as control states for earlier-treated cohorts. The list of controlled states for each treated state is in Appendix B.

[13] Appendix C provides further detail on how employee welfare is constructed. We further verify the robustness of our results by employing an alternative proxy for employee welfare ($Employee\ welfare2_{i,t+1}$), as detailed in Appendix C.

[14] This approach follows Chava et al. (2020), who note that it takes time for the impact of RTW adoption to materialize. For instance, union members may not leave their unions immediately after the law's implementation.

where $Employee\ welfare1_{i,t+1}$ is the employee welfare score for firm *i* in year *t+1*; $\sum Strength_{i,t+1}$ is the sum of strengths for firm *i* in year *t+1*; $\sum Concern_{i,t+1}$ is the sum of weaknesses/concerns for firm *i* in year *t+1*.

We incorporate a comprehensive set of time-varying firm-level and state-level characteristics into our analysis to account for potential influences on employee welfare. Specifically, at the firm level, we include firms' size, *Size*, measured as the natural logarithm of total assets. Leverage, *Lev*, defined as the ratio of total debt (comprising short-term and long-term debt) over total assets. Return on assets, *ROA*, calculated as operating income before depreciation scaled by total assets. Cash ratio, *Cash*, representing cash plus short-term investments over total assets. Data for constructing firm-level control variables is sourced from Compustat.[15] Regarding state-level control variables, to capture state economic conditions, we include the natural logarithm of per capita income of a firm's headquarter state (*PC_state income*), the natural logarithm of income of a firm's headquarter state (*Ln_state income*), and the growth rate of total income of a firm's headquarter state (*Gr_state income*). These variables are constructed using data from the Bureau of Economic Analysis. Detailed definitions of all variables can be found in Table A1. Additionally, to mitigate the influence of outliers, we apply winsorization to all continuous variables at their 1st and 99th percentiles.

# 4. ANALYSIS AND RESULTS

## 4.1 Descriptive statistics

Summary statistics and correlations are presented in Table A2, Panels A and B, respectively. In Panel A, column (2) represents the number of observations. Column (3) shows the mean of

[15] As robustness checks in Table A8, we control for additional variables. However, due to a significant decrease in sample size, those controls are not included in the main analysis.

the variables. Column (4) is the standard deviation. Column (5) is the 25$^{th}$ percentile. Column (6) is the 50$^{th}$ percentile. Column (7) is the 75$^{th}$ percentile. The results suggest that, on average, a sample firm maintains financial leverage of 20%, loses return on assets of 13%, and holds 20% of total assets in cash and cash equivalents. In Panel B, considering the firm-level variables, the results report that employee welfare (*Empl welfare1* $_{t+1}$) is significantly and negatively related to the RTW law (*RTW*= -0.021, p<0.05), size (*Size*= -0.060, p<0.05*)*, leverage *(Lev*= -0.002, p<0.05), cash (*Cash*= -0.044, p<0.05) while it is significantly and positively related to return on assets (*ROA*= 0.024, p<0.05).

## 4.2 Parallel-trends assumption: Dynamic DiD

To establish a causal link between RTW enactments and firm employee welfare in our DiD regressions, we rely on the parallel trends assumption that employee welfare outcomes for treatment and control firms would have evolved similarly absent the policy change. We first create a new set of adoption indicator variables: *Pre5, Pre4, Pre3, Pre2, RTW0, Post1, Post2, Post3, Post4,* and *Post5,* with *Pre1* serving as the reference category. Each variable takes the value of one if the firm is headquartered in a state that will adopt RTW laws in five, four, three, or two years (*Pre5*–*Pre2*), adopts the law in the current year (*RTW0*), or adopted it one to five years earlier (*Post1*–*Post5*), and zero otherwise. The results are reported in Table A3, suggesting that employee welfare deterioration occurs only after RTW laws take effect. This reduces concerns about reverse causality or violations of the parallel trends assumption. Figure 2 also illustrates that, in the pre-treatment period (*Pre5* to *Pre2*), the coefficients are statistically insignificant and close to zero, indicating no evidence of pre-treatment differences between treated and control firms.[16] In the post-treatment period, starting at *RTW0* and particularly in

[16]Figure A1 illustrates the dynamic trend for the alternative measure of employee welfare, *Empl welfare2*.

*Post1* through *Post5*, we observe a consistent negative treatment effect on employee welfare, *Empl welfare1*$_t$ in Model (1). [17]

[ Figure 2]

## 4.3 Hypothesis testing

To test Hypothesis *H1a*, we examine the disparity in employee welfare between firms headquartered in jurisdictions with RTW laws and those in other states employing Equation (1). The findings are presented in Table 1. In addressing concerns related to endogenous controls (see Angrist and Pischke, 2009), we conduct regressions in two forms: one without the inclusion of any control variables and another incorporating the full list of control variables. Model (1) omits any controls, whereas Model (2) incorporates a set of firm-level controls. Model (3) extends control to both firm-level and state-level variables. The coefficient estimates indicate a negative association between RTW and employee welfare, supporting our first Hypothesis (*H1a*), which suggests that a state's employee welfare is negatively affected by the adoption of RTW law. Moreover, the evidence does not support the alternative perspective proposed by the taste hypothesis, which suggests that RTW laws would have no significant effect on employee welfare.

The magnitude of the treatment effect is also economically significant. For instance, the estimated coefficients of RTW in Columns (1) to (3) are $\beta$ = -2.059 ($p < 0.01$), -2.064 ($p < 0.01$), and -1.306 ($p < 0.05$), indicating that employee welfare decreases by -0.931 (= -

[17]Recent research (see Roth, 2022) has raised concerns that standard statistical tests for pre-trends have low statistical power to detect economically meaningful violations. We further reinforce identification with clean control firms, specifically, firms located in neighboring, non-adopting states in Table A6. This control group helps mitigate concerns about unobserved heterogeneity and ensures comparability with treated firms.

2.059/2.212), -0.933 (= -2.064/2.212), and -0.590 (= -1.306/2.212) sample standard deviation of employee welfare for an average treated firm, respectively.[18]

Additionally, our analysis reveals that firm- and state-level characteristics significantly influence employee welfare, supporting the previous literature. For example, consistent with Rind et al. (2022), highly leveraged firms and those with a high cash ratio experience a decline in employee welfare, whereas firms with a higher ROA show improved employee welfare. Our findings indicate that the relationship between employee welfare and size is not significant. Previous studies provide mixed evidence on the relationship between firm size and employee welfare. While Rind et al. (2022) report that larger firms exhibit higher employee welfare, Ghaly et al. (2015) find no significant correlation between the two. Considering the state-level characteristics, per capita state income (*PC_state income*) is negative while the natural logarithm of state income (*Ln_state income*) is positively associated with employee welfare.

In Model (4), we test Hypothesis *H1b* by examining whether the effect of RTW laws on employee welfare varies depending on a firm's exposure to unionization prior to policy adoption. While we do not observe unionization at the firm level, we proxy for union presence using industry-level union coverage measured at the 4-digit SIC level from the Union Membership and Coverage Database. We compute the average union coverage over the three years prior to the RTW indicator year ($t-3$ to $t-1$). Firms then use a dummy of highly unionized (*HighlyUnionized*), which classifies firms into high-union coverage based on whether their industry's pre-treatment unionization rate is above the cross-sectional sample median. We re-estimate the baseline DiD using the interaction between RTW and highly unionized (*RTW*HighlyUnionized*). The results in Model (4) show that the coefficient of the interaction

[18] We also re-estimate our main specification (Model 3 from Table 1) separately for each of the six treated states. The results are reported in the Online Table 1. By using this consistent specification for each treated state, we ensure that any variation in the estimated treatment effects reflects true differences across states.

effect of RTW and highly unionized is numerically larger than the coefficient of RTW variable and statistically significant. It provides evidence that the impact of RTW laws on employee welfare is stronger among firms in more unionized industries. This result is in line with our Hypothesis *H1b,* which suggests that the negative impact of RTW laws on employee welfare is stronger among highly unionized firms.[19]

[Table 1]

## 4.4 Testing bargaining power and free ridership

Earlier, we theoretically argued that the adoption of RTW policies leads to an increase in free riders but decreases the bargaining power of the firms, thereby undermining employee welfare. To empirically test this, we conduct two analyses: First, we examine the direct impact of the RTW laws on free ridership and collective bargaining power. Free riders are employees who are covered by collective bargaining agreements but are not union members (Chaison and Dhavale, 1992). Free ridership is, therefore, measured as the proportion of workers covered by union agreements but not union members. It is calculated as (union coverage minus union membership)/union coverage and calculated annually at the state and 4-digit SIC industry level. We then define high free-ride (*High free-ride*) as a binary indicator equal to 1 if the free ridership ratio is above the sample median, and 0 otherwise.

Bargaining power is proxied by the degree of union coverage, measured as the percentage of employed workers who are covered by a collective bargaining agreement. This measure is calculated annually at the state and 4-digit SIC industry level. Hereafter, we use *union coverage* as a proxy for *bargaining power,* acknowledging that while it does not encompass all

[19] In Table A4, we we use an alternative measure of employee welfare by taking the differences between average strengths and average concerns to obtain the net average score per year for the employee relations dimension (*Empl welfare2*). The results remain consistent with those in Models (1) to (4).

dimensions of bargaining power, it provides a consistent and widely accepted indicator of union influence across states and industries (Chava et al., 2020 and OECD, 2022)[20].

We then define high union coverage (*High union coverage*) as a binary indicator equal to 1 if union coverage is above the sample median, and 0 otherwise.[21] Second, we examine the relationship between RTW laws and employee welfare, considering free ridership and union coverage as conditional factors.

The results are reported in Table 2. Model (1) presents the results for the impact of RTW on high free ridership (*High free-ride*). In line with our expectation, Model (1) shows a significant positive coefficient for RTW at the 1% level. The coefficient of 0.025 for the RTW variable in Model (1) suggests that RTW adoption leads to almost a 2.5 percentage-point increase in free ridership compared to industry peers in non-RTW states. This increase is economically significant. This result is consistent with Chava et al. (2020) who find that the adoption of RTW law increases the percentage of non-member employees who are covered by union agreements, and hence, increases the severity of the free ridership problem. The result is also consistent with Chun (2023) who reports a positive RTW effect on free-riding behavior. Model (2) shows the results of the DiD regressions of employee welfare (*Empl welfare1*$_{t+1}$)[22] on RTW indicator, where we enter the interaction of high free ridership (*High free-ride*) in the models (*RTW*High free-ride*). If RTW laws exacerbate free riding and diminish employee welfare, we expect to observe a more pronounced impact on firms with higher levels of free ridership. The presented results in Model (2) are in line with this expectation, where the adverse impact of the enactment

[20]https://www.oecd.org/content/dam/oecd/en/publications/reports/2022/09/oecd-employment-outlook-2022_7a5a73b3/1bb305a6-en.pdf

[21] Free ridership and union coverage variables are sourced from the Union Membership and Coverage Database: https://www.unionstats.com/

[22] We report the results for our alternative measure of employee welfare (*Empl welfare1*$_{t+2}$) in the appendix, Table A5.

of RTW laws on firm employee welfare is greater for those treated firms with higher levels of free ridership.

Model (3) reports the results for the impact of RTW laws on high union coverage (*High union coverage)*. Consistent with our arguments developed earlier in this paper, Model (3) shows a significant negative coefficient for RTW at the 1% level. This result indicates a considerable decline in union coverage, for affected firms after RTW adoption. The result in Model (3) shows a coefficient of -0.041, suggesting that RTW adoption leads to a 4 percentage-point decrease in union coverage, compared to industry peers in non-RTW states. This decrease is economically significant. This result is consistent with Chun (2023) who reports that RTW laws reduce the share of workers covered by union contracts (union coverage). It is also in line with Chava et al. (2020) who find that RTW laws reduce the number of workers covered by collective bargaining agreements. Model (4) shows the results of the DiD regressions of employee welfare (*Empl welfare1*$_{t+1}$) on the RTW indicator while incorporating the interaction of high union coverage (*RTW*High union coverage*). If RTW laws increase the bargaining power of the firm over the labor union, leading to a significant decrease in employee welfare, we expect to observe a more pronounced impact on firms with higher levels of union coverage. The presented results in Model (4) are in line with this expectation, where the negative treatment effect of the RTW enactment on firm employee welfare is more pronounced for those treated firms having higher union coverage.[23] Overall, our findings suggest that the implementation of RTW laws increases free ridership and reduces union coverage (a proxy for bargaining power) of the labor union, thereby negatively impacting employee welfare within contracting firms.[24]

[23] In Table A4, we use an alternative measure of employee welfare by taking the differences between average strengths and average concerns to obtain the net average score per year for the employee relations dimension (*Empl welfare2*). The results remain consistent with those in Models (2) and (4).

[24] Following Schnabel (2020) and Hirsch and Berger (1984) who argue that union density serves as a key indicator of union strength and bargaining power, in untabulated tables, we check the robustness of our results by using

[Table 2]

## 4.5 Further analysis

### *4.5.1 Component-level analysis*

To better understand the underlying drivers of our main findings, we conduct a component-level analysis of the employee welfare measure. Specifically, we break the employee welfare measure into three main components and examine whether the results are loading on particular dimensions in Table 3. Model (1), *Emp_Involvement*, captures employee engagement by measuring participation in decision-making, net of indicators of workforce reduction. Model (2), *Emp_Benefits*, focuses on core financial security, measuring retirement-related benefits while subtracting associated employee concerns. Model (3), *Emp_Others*, captures broader, non-core welfare components including profit-sharing schemes and work/life balance practices, adjusted for any concerns raised by employees in these areas. The results reveal that RTW adoption is associated with significant declines in two key areas: employee involvement (Model 1) and broader welfare indicators capturing profit-sharing and work-life balance (Model 3). Specifically, the coefficients for RTW in Models (1) and (3) are negative and statistically significant at the 5% level. This indicates that after RTW laws are enacted, firms in affected states are less likely to promote employee participation in decision-making and to provide broader supportive practices. In contrast, Model (2), which focuses on retirement-related benefits, shows a negative but statistically insignificant coefficient. Overall, the results in Table 3 suggest that the enactment of RTW laws may weaken employee voice and diminish the quality of the overall work environment, even if core financial benefits remain unchanged.

[Table 3]

unionization (the percentage of employees who are union members) as a proxy for bargaining power. The results remain consistent.

*4.5.2 Alternative dependent variable (Turnover)*

As an additional analysis, we examine the effect of RTW laws on an alternative dependent variable (employee turnover) to capture employees' reactions to unpleasant working environments and their ability to voice their concerns. Dundon et al. (2004) conceptualize "employee voice" as the ability of individual employees to express dissatisfaction to their line manager and participate in decision-making processes aimed at resolving conflicts. Research suggests that opportunities for voice play a critical role in fostering workplace equity and reducing employee turnover. For instance, Eigen and Litwin (2014: 173) emphasize that "improved perceived legitimate opportunities for voice and greater perceived access to procedurally fair and neutral processes for resolving disputes should increase employees' organizational commitment and decrease turnover." This aligns with Hirschman's Exit-Voice theory (1970), which posits that rather than voicing concerns, employees dissatisfied with their working conditions may choose to exit the organization.

In our analysis presented in Table 4, we re-estimate our DiD regressions using an alternative dependent variable, employee turnover *(Empl turnover*), calculated as the number of departures divided by the average number of employees. We use this measure as a proxy for employee voice and overall workplace conditions. The table suggests that RTW law has a significant and positive effect on employee turnover across all three models (controlling for firm-level and state-level variables). We propose that higher turnover rates may reflect a perceived lack of legitimate avenues for voice, leading them to leave the organization rather than addressing their dissatisfaction. In general, the presented result is in line with our baseline result (Table 1) and supports Hypothesis *H1a* which suggests that the adoption of RTW laws leads to a decline in employee welfare. The higher employee turnover rates observed in this analysis reflect lower employee satisfaction, which in turn suggests a decrease in overall welfare for employees.

[Table 4]

*4.5.3 Additional Robustness checks: Firm age heterogeneity, additional controls and sample refinement*

While we initially incorporated observable local economic conditions into our regression analyses, it is crucial to consider the potential impact of unobserved local economic factors that could be associated with both the implementation of the RTW policy and employee welfare. In this subsection, we delve deeper into this issue by employing a more refined approach. Following Nguyen and Qiu (2022), we carefully select control firms located in contiguous neighboring states to the treated firms. Our aim is to investigate whether our findings remain robust when accounting for these refined conditions. We anticipate that if our results are influenced by unobserved local economic conditions, they may weaken or disappear when controlling for these factors. This is because both treated and control firms are likely to be exposed to similar local economic conditions, and any observed effects may be a result of these shared factors. For this comprehensive analysis, we construct a new dataset comprising both treated firms and their corresponding control firms. The control firms are identified as those headquartered in states that had not adopted RTW legislation by year *t* but share a common border with the specific treated state in each of the six adapting states. We then reassess all the baseline regressions outlined in Table 1 using this revised sample. The results of these analyses are presented in Table A6, Models (1) and (2), confirming that the effect of RTW laws on employee welfare remain negative and statistically significant, indicating that the detrimental effect of RTW adoption on employee welfare is not driven by local economic conditions.

To further address potential endogeneity in RTW adoption, we employ instrumental variable (2SLS) estimation.[25] We use political and economic predictors, specifically state-level political

[25] It is important to note that the adoption of RTW laws may be endogenously determined and correlated with unobservable political or economic factors (Pagano and Volpin, 2005; Perotti and Von Thadden, 2006). Following Chava et al. (2020) and Simintzi et al. (2015), we test which state-level characteristics predict RTW adoption to mitigate this concern.

leaning and a single lag of GDP, as instrumental variables using the same sample of states in our baseline regression in Table 1. The first stage results are presented in Model 3. Diagnostic tests (first-stage F > 15; significant Kleibergen–Paap and Cragg–Donald statistics) confirm instrument validity and strength. The second-stage results (Models 4-5 in Table A6) continue to yield significantly negative RTW coefficients, supporting Hypothesis *H1a* which suggests that the adoption of RTW laws leads to a decline in employee welfare. Finally, after adding controls for state-level uncertainty, GDP growth, and population (Models 6-7 in Table A6) within the same sample of states used in the baseline regression in Table 1, the results remain robust. Together, the presented results here reinforce the conclusion that RTW laws adversely affect employee welfare beyond regional or political-economic variation, supporting our main findings in Table 1 in line with Hypothesis *H1a* which suggests that the adoption of RTW laws leads to a decline in employee welfare.

Next to test the robustness of our findings, we focus on firm age heterogeneity to examine whether the impact of RTW laws on employee welfare differs across firms at various stages of organizational maturity. Specifically, we address firm age heterogeneity by adding an interaction between RTW and firms' age. This interaction allows us to assess whether the effect of RTW laws on employee welfare differs between younger and more established firms. The results presented in Table A7 show a significant effect of firm age but no significant interaction between age and RTW adoption. This suggests that while older firms may differ in baseline employee welfare, the impact of RTW laws is broadly consistent across firms of different ages. In other words, the effect of RTW laws remains robust across different stages of firm maturity, suggesting that the observed impact is not dependent on a firm's age.[26]

[26] In the Online Table 2, we also check the sensitivity of our findings for new entrant firms. New entrant firms typically possess unique characteristics that set them apart from established companies, which can significantly influence how RTW laws affect employee welfare. These firms often exhibit fewer established practices and limited resources (Amini et al., 2023), which might shape their response to RTW legislation differently. The results support our baseline findings.

Finally, we assess the robustness of our results by incorporating additional control variables in Table A8. Following Rind et al. (2021) and Brockman et al. (2020), we include additional firm-level controls to account for a wider range of firm characteristics that could confound the relationship between RTW laws and employee welfare. Specifically, we add the market-to-book ratio (*MTB*) to capture growth opportunities, dividend dummy (*Div*) to reflect payout behavior, tangibility (*Tangibility*) as a proxy for capital intensity, R&D intensity (*RD*) to control for innovation-related expenditures, and sales growth (*Sale_gr*) to account for firm expansion dynamics. Importantly, the results remain robust and consistent with our baseline findings: the adoption of RTW laws continues to exhibit a statistically significant negative effect on employee welfare. These findings support our first Hypothesis (*H1a*) that a state's adoption of RTW laws negatively affects employee welfare.

Furthermore, to address potential concerns about measurement error due to firms headquartered in non-RTW states but with establishments in RTW states (or vice versa), we conduct a robustness check by focusing on smaller firms, specifically those in the bottom 20th percentile of total assets, as these are less likely to be geographically dispersed. The subsample results (Table A8, Models (3) and (4)) show that the negative impact of RTW laws on employee welfare remains statistically significant and consistent with our main findings, indicating that multi-state exposure is unlikely to drive the observed effects.

Finally, to address concerns about potential bias from states that adopted RTW laws late in our sample period, particularly Wisconsin (2015), West Virginia (2016), and Kentucky (2017), we conduct a robustness check excluding these states from the treatment group. Since our dataset ends in 2019, these states contribute only limited post-treatment observations, which could affect the estimated dynamics of the treatment effect. As shown in Table A8, Models (5) and (6), the exclusion of these late-adopting states yields results that are qualitatively and

statistically consistent with our baseline findings. This suggests that our main conclusions are not sensitive to the inclusion of states with shorter post-RTW exposure and reinforces the overall robustness of our empirical strategy.

## 5. DISCUSSION AND CONCLUSION

RTW laws have been a topic of debate in labor relations. Supporters of these laws argue that they are enacted with the intention of making the state's labor force more attractive to new industries, stimulating faster economic growth (Palomba and Palomba, 1971), and having a positive effect on employment and wages (Ellwood and Fine, 1987; Hirsch, 1980; Holmes, 1998; Warren and Strauss, 1979). Critics contend that these laws weaken unions and undermine worker rights, leading to increased job insecurity, lower wages, and poorer working conditions (Chava et al., 2020; Devinatz, 2011). Given the continuing controversy over the RTW laws in the US labor market (Devinatz, 2011), as well as the contentious discourse regarding their impact on employees' subjective sense of well-being (Makridis, 2019), our study aims to empirically investigate the direct effect of RTW laws on employee welfare.

Using the stacked difference-in-differences regression framework to analyze the impact of RTW adoption on employee welfare, our findings indicate that RTW laws significantly undermine employee welfare by promoting free ridership and weakening collective bargaining power (proxied by union coverage). We also show that the adverse effects of RTW laws on employee welfare are particularly strong in firms operating within highly unionized industries. A closer examination of the individual components of employee welfare indicates that this overall decline is largely driven by decreases in employee participation, profit-sharing opportunities, and work-life balance initiatives. To ensure the robustness of our results, we conduct additional analyses using firms located in neighboring states without RTW laws as controls, thereby accounting for potential unobserved local influences. The consistency of our

results across these specifications suggests that the observed effects are attributable to RTW law adoption rather than to unmeasured local characteristics. Furthermore, our results remain robust across a range of additional tests, including alternative dependent variables, stricter sample selection criteria, the inclusion of additional political and economic controls at both the firm and state levels, as well as adjustments for potential endogeneity.

These findings are of significant importance, as they contribute to the body of literature regarding the impacts of RTW laws. Indeed, existing literature shows that maintaining high levels of employee welfare is crucial for a firm's competitiveness, as it enables the company not only to outperform its competitors but also to retain its talented workforce (Bae et al, 2011; Guo et al., 2016; Rind et al., 2022; Zhang et al., 2020). Therefore, it can be argued that RTW laws can have adverse economic implications for firms by impacting employee welfare negatively. In addition, our result is important, given the ongoing controversy over the RTW laws and the fact that, over time they have undermined unions by encouraging free riding among workers covered by union agreements, which in turn diminishes union resources and weakens their bargaining power (Bono-Lunn, 2024). We suggest that RTW laws, by diminishing union collective bargaining power and increasing free ridership, have a detrimental impact on employee welfare, supporting the notion that RTW laws carry significant consequences for the future of the trade union movement in the US. In addition, by examining employee turnover as a reflection of employee voice, our study can provide valuable insights into how RTW laws influence the dynamics of employee satisfaction, organizational fairness, and retention.

Our findings are of interest to various stakeholders with several practical and policy implications. Firstly, labor unions, concerned about the working conditions of their members (Farber and Saks, 1980), find value in the findings of this study. Understanding that RTW laws

potentially result in diminished employee welfare reinforces their apprehensions and emphasizes the importance of addressing the challenges posed by such legislation. Secondly, employers generally view RTW laws as beneficial for reducing labor costs and increasing flexibility in recruitment and redundancy decisions (Bruno et al., 2015; Oas et al., 2016). Understanding the impact of RTW laws on employee welfare would help employers gauge the potential benefits and drawbacks of operating in RTW states. Thirdly, employers are interested in how RTW laws influence employee well-being and welfare, as these factors can potentially affect business performance. Fourthly, regulators might benefit from these findings to make informed decisions when evaluating or reforming labor policies and regulations. For example, recently, the US has been considering the adoption of the PRO Act as a major step toward restoring the right of workers to organize, collectively bargain, and advocate for better wages, benefits, and working conditions (Rhinehart, 2021). The results of this study have important policy implications in light of the PRO Act adoption and its potential impact on RTW laws. The PRO Act is a significant piece of labor law legislative proposal in the US (Rhinehart, 2021; Stock, 2023). One of its most notable provisions is the repeal of Section 14(b) of the Taft-Hartley Act, which currently authorizes states to enact RTW laws. By repealing this section, the PRO Act would effectively invalidate existing RTW laws in states that have adopted them. There is ongoing debate over the PRO Act, with supporters considering it a necessary step toward strengthening workers' rights (Rhinehart, 2021). However, opponents warn of potential economic consequences and unintended outcomes, suggesting that it clearly threatens the livelihoods of small business owners[27]. Our finding on the negative impact of RTW laws on employee welfare seems to suggest evidence in favor of the PRO Act.

[27] The PRO Act: A Radical Union Boss Wish List, Committee on Education and the Workforce. https://republicans-edlabor.house.gov/legislation/pro-act.htm

Lastly, while our focus is on public firms, the impact of RTW laws on private firms may differ due to their unique ownership structures, governance, and resource constraints. With less public scrutiny, private firms may approach employee welfare differently. Future research could explore RTW laws' effects on private firms to provide a broader understanding of their influence on employee welfare.

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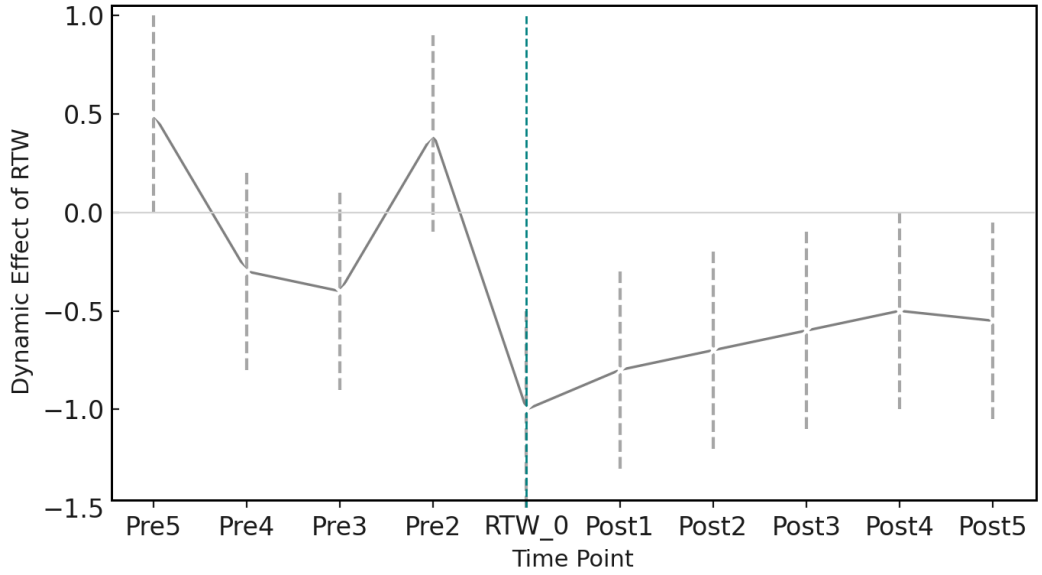
Table 1: The effect of RTW laws on employee welfare

| | (1) Empl welfare1$_{t+1}$ | (2) Empl welfare1$_{t+1}$ | (3) Empl welfare1$_{t+1}$ | (4) Empl welfare1$_{t+1}$ |
|---|---|---|---|---|
| RTW | -2.059*** | -2.064*** | -1.306** | -1.407** |
| | (-3.70) | (-3.79) | (-2.08) | (-2.37) |
| HighlyUnionized | | | | 0.356** |
| | | | | (2.00) |
| RTW*HighlyUnionized | | | | -1.495** |
| | | | | (-2.28) |
| Size | | 0.294 | 0.085 | 0.053*** |
| | | (0.70) | (1.57) | (5.48) |
| Lev | | -0.306*** | -0.307*** | -0.228*** |
| | | (-2.83) | (-2.87) | (-2.99) |
| ROA | | 0.581*** | 0.071 | 0.220*** |
| | | (2.95) | (0.66) | (3.83) |
| Cash | | -0.376*** | -0.259*** | 0.090 |
| | | (-2.92) | (-2.98) | (0.75) |
| PC_state income | | | -0.830*** | -6.154*** |
| | | | (-4.11) | (-7.97) |
| Ln_state income | | | 6.067** | 0.125 |
| | | | (2.09) | (0.60) |
| Gr_state income | | | -8.482*** | -3.146* |
| | | | (-2.77) | (-1.88) |
| Firm * Cohort FE | Yes | Yes | Yes | Yes |
| Year * Cohort FE | Yes | Yes | Yes | Yes |
| State Cluster | Yes | Yes | Yes | Yes |
| Observations | 98,595 | 98,595 | 98,595 | 98,595 |
| Adjusted R2 | 0.151 | 0.175 | 0.191 | 0.244 |

This table reports the results of the DiD regressions of the RTW laws indicator on firm employee welfare (*Empl welfare1$_{t+1}$*) in Models 1 to 3. In Model 4, we re-run our regressions using the subsamples of high/low unionized firms. Highly unionized is a binary indicator equal to 1 if the firm's industry-level average union coverage over the three years prior to the RTW (*t−3* to *t−1*) is above the cross-sectional sample median, and 0 otherwise. All regressions control for firm-cohort and year-cohort fixed effects. The standard errors are clustered at the state level. A detailed description of the variable construction is provided in Table A1. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Figure 2: Dynamic RTW effect on employee welfare




This figure reports the results of the dynamic effect of RTW laws on employee welfare *(Empl welfare1)*. A detailed description of the variable construction is provided in Appendix C.

Table 2: Testing free ridership and collective bargaining power

| | (1) High free-ride$_{t+1}$ | (2) Empl welfare1$_{t+1}$ | (3) High union coverage$_{t+1}$ | (4) Empl welfare1$_{t+1}$ |
|---|---|---|---|---|
| RTW | 0.025** | -0.587*** | -0.041*** | -1.054** |
| | (2.33) | (-3.99) | (-3.02) | (-2.01) |
| RTW*High free-ride$_{t+1}$ | | -1.010*** | | |
| | | (-4.58) | | |
| High free-ride$_{t+1}$ | | -0.798*** | | |
| | | (-7.00) | | |
| RTW*High union coverage$_{t+1}$ | | | | -0.147** |
| | | | | (-2.01) |
| High union coverage$_{t+1}$ | | | | 0.274** |
| | | | | (2.24) |
| Firm and state-level controls | Yes | Yes | Yes | Yes |
| Firm * Cohort FE | Yes | Yes | Yes | Yes |
| Year * Cohort FE | Yes | Yes | Yes | Yes |
| State Cluster | Yes | Yes | Yes | Yes |
| Observations | 98,595 | 98,595 | 98,595 | 98,595 |
| Adjusted R2 | 0.040 | 0.214 | 0.401 | 0.297 |

This table examines the dynamics of collective bargaining power and the free ridership hypothesis. Model 1 reports the results for the impact of RTW laws on high free ridership measured by union coverage-union membership)/union coverage, using the 4-digit SIC industry level each year and sourced from the Union Membership and Coverage Database. We define high free-ride (*High free-ride*) as a binary indicator equal to 1 if the free ridership ratio is above the sample median, and 0 otherwise. Model 2 shows the results of the DiD regressions of RTW indicator on employee welfare (*Empl welfare1$_{t+1}$*) using high free ridership (*High free-ride*). Model 3 reports the results for the impact of RTW on high union coverage, which is measured by the percentage of employed workers who are covered by a collective bargaining agreement using the 4-digit SIC industry level each year and sourced from the Union Membership and Coverage Database. We define high union coverage (*High union coverage*) as a binary indicator equal to 1 if the union coverage is above the sample median, and 0 otherwise. Model 4 shows the results of the DiD regressions of RTW indicator on employee welfare (*Empl welfare1$_{t+1}$*) using high/low (above/below cross-sectional median values) union coverage (*High union coverage*). The regressions include controls for size, leverage, return on assets (ROA), cash, pc_state income, ln_state income, and gr_state income (*Firm and state-level controls*). Also, all regressions control for firm-cohort and year-cohort fixed effects. Standard errors are clustered at the state level. A detailed description of the variable construction is provided in Table A1. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 3: The impact of RTW laws on employee welfare components

| | (1)<br>Emp_Involvement | (2)<br>Emp_Benefits | (3)<br>Emp_Others |
|---|---|---|---|
| RTW | -0.040** | -0.016 | -0.270** |
| | (-2.38) | (-0.47) | (-2.00) |
| Size | 0.003 | 0.007 | 0.027*** |
| | (1.56) | (0.83) | (11.67) |
| Lev | -0.027*** | -0.028 | 0.058*** |
| | (-2.90) | (-0.85) | (6.51) |
| ROA | 0.019 | 0.051* | 0.003 |
| | (1.19) | (1.87) | (0.27) |
| Cash | -0.034*** | -0.025 | -0.056*** |
| | (-2.60) | (-0.71) | (-4.89) |
| Firm and state-level controls | Yes | Yes | Yes |
| Firm * Cohort FE | Yes | Yes | Yes |
| Year * Cohort FE | Yes | Yes | Yes |
| State Cluster | Yes | Yes | Yes |
| Observations | 33,587 | 33,587 | 43,615 |
| Adjusted R2 | 0.090 | 0.060 | 0.071 |

This table reports the results of the DiD regressions of the RTW laws indicator on firm employee welfare components. Model 1, *Emp_Involvement*, measures the employee welfare component as employee involvement minus workforce reduction. Model 2, *Emp_Benefits*, measures the employee welfare component as the employee retirement benefits minus their related concerns. Model 3, *Emp_Others*, measures employee welfare as other cash profit sharing and work/life balance minus other concerns. The regressions include controls for size, leverage, return on assets (ROA), cash, pc_state income, ln_state income, and gr_state income (*Firm and state-level controls*). Also, all regressions control for firm-cohort and year-cohort fixed effects. The standard errors are clustered at the state level. A detailed description of the variable construction is provided in Table A1. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 4: The effect of RTW laws on employee turnover (alternative dependent variable)

| | (1) Empl turnover$_{t+1}$ | (2) Empl turnover$_{t+1}$ | (3) Empl turnover$_{t+1}$ |
|---|---|---|---|
| RTW | 0.152** | 0.141** | 0.142* |
| | (1.98) | (2.11) | (1.81) |
| Firm_level controls | | Yes | Yes |
| Firm and state-level controls | | | Yes |
| Year * Industry FE | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes |
| State Cluster | Yes | Yes | Yes |
| Observations | 98,595 | 98,595 | 98,595 |
| Adjusted R2 | 0.289 | 0.295 | 0.298 |

This table reports the robustness results of the DiD regressions of the RTW laws indicator using an alternative measure of the dependent variable based on firm employee turnover (*Empl turnover$_{t+1}$*), which is measured as the number of departures divided by the average number of employees (Hom and Griffeth, 1995). Model 2 includes controls for firm-level variables, size, leverage, return on assets (ROA), and cash (*Firm-level controls*). Model 3 controls for firm- as well as state-level variables, which are pc_state income, ln_state income, and gr_state income (*Firm and state-level controls*). Also, all regressions control for firm-cohort and year-cohort fixed effects. The standard errors are clustered at the state level. A detailed description of the variable construction is provided in Table A1. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

# Appendix A: Additional Tables

## Table A1: Variables definitions

| Variables | Definitions | Source |
|---|---|---|
| *Panel A: Dependent variables* | | |
| Empl welfare1 | Employee welfare is constructed as employee strengths minus concerns in KLD employee index. Details on the construction of this variable are provided in Appendix C. | KLD |
| Empl welfare2 | Average strengths subtracted from average weaknesses in KLD employee index. Details on the construction of this variable are provided in Appendix C | KLD |
| Emp_Involvement | Employee involvement minus workforce reduction | KLD |
| Emp_Benefits | Employee retirement benefits minus their related concerns | KLD |
| Emp_Others | Other cash profit sharing and work/life balance minus other concerns | KLD |
| *Panel B: Right to Work (RTW) indicator* | | |
| RTW | An indicator variable indicating whether a firm's headquarter state has adopted the right to work laws by year t. | Authors' calculation |
| *Panel C: Main control and channel variables* | | |
| Size | Natural logarithm of total assets. | Compustat |
| Lev | Long- and short-term debt over total assets. | Compustat |
| ROA | Operating income before depreciation over total assets. | Compustat |
| Cash | Cash and cash equivalents over total assets. | Compustat |
| PC_state income | Natural logarithm of per capita income of the firm's headquarter state. | Bureau of Economic Analysis |
| Ln_state income | Natural logarithm of the total income of the firm's headquarter state. | Bureau of Economic Analysis |
| Gr_state income | Growth rate of the total income of the firm's headquarter state. | Bureau of Economic Analysis |
| High free-ride$_{t+1}$ | A binary indicator equal to 1 if the free ridership ratio is above the sample median, and 0 otherwise. The free ridership is calculated as (Union coverage minus union membership)/union coverage, calculated annually at the state and 4-digit SIC industry level. | Union Membership and Coverage Database: https://www.unionstats.com/ |
| High union coverage$_{+1}$ | A binary indicator equal to 1 if union coverage rate (Percentage of employed workers covered by a collective bargaining agreement calculated annually at the state and 4-digit SIC industry level) is above the sample median, and 0 otherwise. | Union Membership and Coverage Database: https://www.unionstats.com/ |
| HighlyUnionized | A binary indicator equal to 1 if the firm's industry-level average union coverage over the three years prior to the RTW indicator year (t−3 to t−1) is above the cross-sectional sample median, and 0 otherwise. | Union Membership and Coverage Database: https://www.unionstats.com/ |
| *Panel D: Other state control variables* | | |

| | | |
|---|---|---|
| GDP gr_state | State GDP annual growth rate. | Bureau of Economic Analysis |
| Political leaning | The ratio of votes cast for a Democratic presidential candidate to the votes cast for the Republican candidate in a state. | Dave Leip's Atlas of U.S. Presidential Elections. Economic policy uncertainty: http://uselectionatlas.org |
| Lagged GDP | Lag of GDP | Bureau of Economic Analysis |
| EPU_Composite | A composite set of terms that contains state-specific policy terms as well as the set of national policy terms. | Economic policy uncertainty: https://www.policyuncertainty.com/ |
| EPU_National | The level of uncertainty within a state that stems from specifically national policy-related sources. | Economic policy uncertainty: https://www.policyuncertainty.com/ |
| SPU_State | The level of uncertainty within a state comes from state and local policy issues. | https://www.policyuncertainty.com/ |
| Ln_state pop. | Natural logarithm of the population of the firm's headquarter state. | Bureau of Economic Analysis |
| *Panel C: Robustness variables* | | |
| Empl turnover | The number of departures divided by the average number of employees. | Compustat |
| Age | Firm age is the number of years since its incorporation year. | Orbis |
| Div | A dummy equals one if the firm pays dividends in a year and zero otherwise. | Compustat |
| Tangibility | Fixed assets/ total Assets. | Compustat |
| Sale_gr | Growth rate of the total sales. | Compustat |
| RD | R&D expenditures divided by total assets. | Compustat |
| MTB | Market value of equity plus book value of debt divided by total assets. | Compustat |

Table A2: Descriptive statistics and correlations

| *Panel A: Descriptive statistics* | | | | | | |
|---|---|---|---|---|---|---|
| Variables | N | Mean | SD | P25 | P50 | P75 |
| Empl welfare1$_{t+1}$ | 98,595 | -1.826 | 2.212 | -3.000 | -2.000 | 1.000 |
| Empl welfare2$_{t+1}$ | 98,595 | -0.456 | 0.553 | -0.750 | -0.500 | 0.300 |
| RTW | 98,595 | 0.008 | 0.086 | 0.000 | 0.000 | 0.000 |
| Size | 98,595 | 5.056 | 2.120 | 3.345 | 5.325 | 6.014 |
| Lev | 98,595 | 0.204 | 0.207 | 0.001 | 0.110 | 0.325 |
| ROA | 98,595 | -0.136 | 0.311 | -0.214 | 0.019 | 0.092 |
| Cash | 98,595 | 0.200 | 0.145 | 0.040 | 0.130 | 0.305 |
| PC_state income | 98,595 | 10.801 | 0.155 | 10.686 | 10.812 | 10.078 |
| Ln_state income | 98,595 | 13.366 | 0.870 | 12.700 | 13.241 | 14.211 |
| Gr_state income | 98,595 | 0.000 | 0.004 | 0.000 | 0.000 | 0.000 |

Table A2 Continues

*Panel B: Correlation coefficients*

| | | 1.1 | 1.2 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1.1 | Empl welafre1 $_{t+1}$ | 1.000 | | | | | | | | | |
| 1.2 | Empl welfare2$_{t+1}$ | | 1.000 | | | | | | | | |
| 2 | RTW | -0.021** | -0.005** | 1.000 | | | | | | | |
| 3 | Size | -0.060** | -0.025** | 0.024** | 1.000 | | | | | | |
| 4 | Lev | -0.002** | -0.000 | 0.015** | 0.144** | 1.000 | | | | | |
| 5 | ROA | 0.024** | 0.012** | 0.033** | 0.684** | 0.011** | 1.000 | | | | |
| 6 | Cash | -0.044** | -0.002** | -0.036** | -0.370** | -0.253** | -0.430** | 1.000 | | | |
| 7 | PC_state income | -0.412** | -0.258** | -0.088** | 0.006 | -0.006** | -0.118** | 0.151** | 1.000 | | |
| 8 | Ln_state income | -0.096** | -0.045** | -0.061** | -0.073** | -0.073** | -0.130** | 0.151** | 0.245** | 1.000 | |
| 19 | Gr_state income | -0.004 | -0.000 | 0.012** | 0.005 | 0.0068** | 0.013** | -0.020** | -0.034** | -0.106** | 1.000 |

Panel A of this table reports the sample descriptive statistics. Panel B reports the Pearson correlations for the dependent variables (*Empl welfare1*$_{t+1}$ and *Empl welfare2*$_{t+1}$) and control variables using 6,132 unique firms' observations over the 2001−2019 period. A detailed description of the variable construction is provided in Table A1. The symbol ** denotes statistical significance at the 5% level.

Table A3: Parallel-trends assumption: Dynamic DiD

| | (1)<br>Empl welfare1$_t$ | (2)<br>Empl welfare2$_t$ |
|---|---|---|
| Pre5 | 0.422 | 0.222 |
| | (0.621) | (1.08) |
| Pre4 | -0.275 | -0.013 |
| | (-1.44) | (-1.25) |
| Pre3 | -0.391 | -0.421 |
| | (-0.57) | (-0.50) |
| Pre2 | 0.298 | 0.208 |
| | (0.77) | (1.08) |
| RTW0 | -1.067*** | -0.767*** |
| | (-2.88) | (-3.08) |
| Post1 | -0.744*** | -0.604*** |
| | (-3.05) | (-4.05) |
| Post2 | -0.676** | -0.554** |
| | (-1.97) | (-2.01) |
| Post3 | -0.634** | -0.504** |
| | (-2.25) | (-1.95) |
| Post4 | -0.518** | -0.485** |
| | (-2.09) | (-1.99) |
| Post5 | -0.567** | -0.327** |
| | (2.04) | (-2.10) |
| Firm and state-level controls | Yes | Yes |
| Firm * Cohort FE | Yes | Yes |
| Year * Cohort FE | Yes | Yes |
| State Cluster | Yes | Yes |
| Observations | 98,595 | 98,595 |
| Adjusted R2 | 0.498 | 0.445 |
| F-test | 1.04 | 1.23 |

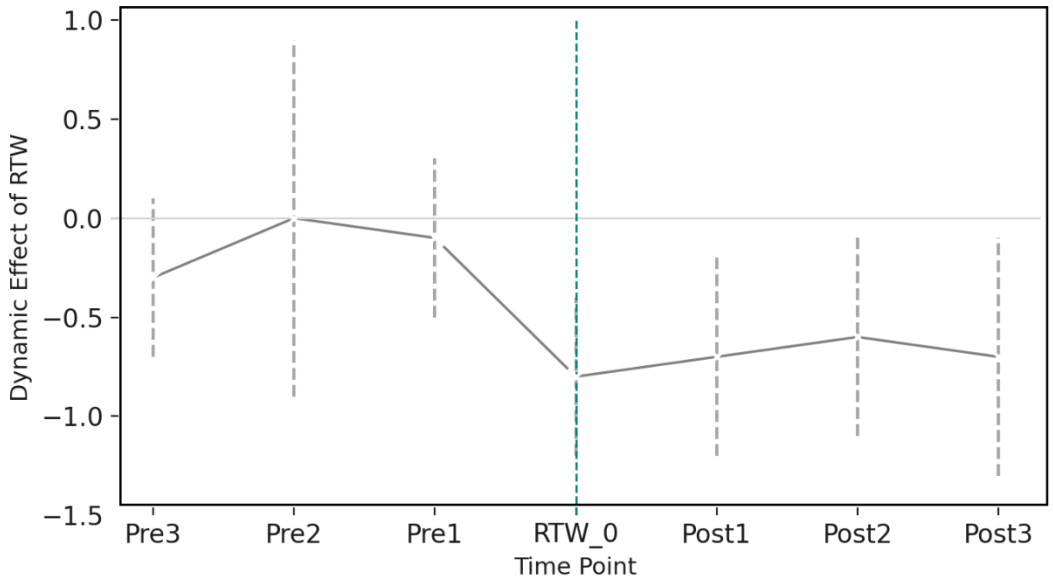
The table reports the results of the falsification test that counterfactually assumes that the RTW took place a few years before the actual event. The dependent variables are firm employee welfare (*Empl welfare1*$_{t+1}$ and *Empl welfare2*$_{t+1}$). Pre5, Pre4, Rre3, Rre2, RTW_0, Post1, Post2, Post3, Post4, and Post5 are indicator variables that indicate five years before, four years before, three years before, two years before, the current year of, one year after, two years after, three years after, four year after, and five or more years after the RTW (Pre1 is the reference point). All regressions control for firm- as well as state-level variables, which are size, leverage, return on assets (ROA), cash, pc_state income, ln_state income, and gr_state income (*Firm and state-level controls*). Also, all regressions control for firm-cohort and year-cohort fixed effects. The standard errors are clustered at the state level. P-values for a joint F-test on the pre-treatment coefficients are also reported. A detailed description of the variable construction is provided in Table A1. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A4: The effect of RTW laws on the alternative measure of employee welfare (Empl welfare2)

| | (1) Empl welfare2$_{t+1}$ | (2) Empl welfare2$_{t+1}$ | (3) Empl welfare2$_{t+1}$ | (4) Empl welfare2$_{t+1}$ |
|---|---|---|---|---|
| RTW | -0.515*** | -0.516*** | -0.327** | -0.432** |
| | (-3.00) | (-3.25) | (-2.25) | (-2.50) |
| HighlyUnionized | | | | 0.300** |
| | | | | (2.42) |
| RTW*HighlyUnionized | | | | -1.092*** |
| | | | | (-3.03) |
| Size | | 0.073 | 0.021** | 0.040*** |
| | | (0.40) | (2.55) | (4.07) |
| Lev | | -0.077*** | 0.077*** | 0.086 |
| | | (-2.88) | (3.27) | (1.14) |
| ROA | | 0.145*** | 0.018 | 0.115 |
| | | (2.65) | (0.54) | (1.59) |
| Cash | | -0.094*** | -0.065*** | 0.001 |
| | | (-2.88) | (-3.05) | (0.01) |
| PC_state income | | | -0.457*** | -5.627*** |
| | | | (-4.00) | (-10.20) |
| Ln_state income | | | 6.517*** | 0.104 |
| | | | (2.78) | (0.43) |
| Gr_state income | | | -7.120*** | 1.914 |
| | | | (-3.67) | (1.09) |
| Firm * Cohort FE | Yes | Yes | Yes | Yes |
| Year * Cohort FE | Yes | Yes | Yes | Yes |
| State Cluster | Yes | Yes | Yes | Yes |
| Observations | 98,595 | 98,595 | 98,595 | 98,595 |
| Adjusted R2 | 0.186 | 0.201 | 0.290 | 0.291 |

This table reports the results of the DiD regressions of the RTW indicator on firm employee welfare (*Empl welfare2$_{t+1}$*) in Models 1 to 3. In Model 4, we re-run our regressions using the subsamples of high/low unionized firms. Highly unionized is a binary indicator equal to 1 if the firm's industry-level average union coverage over the three years prior to the RTW indicator year (*t−3* to *t−1*) is above the cross-sectional sample median, and 0 otherwise. All regressions control for firm-cohort and year-cohort fixed effects. The standard errors are clustered at the state level. A detailed description of the variable construction is provided in Table A1. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Figure A1: Alternative measure of employee welfare (Empl welfare2)




This figure reports the results of the dynamic effect of RTW laws on employee welfare *(Empl welfare2)*. A detailed description of the variable construction is provided in Appendix C.

Table A5: Testing free ridership and collective bargaining power using the alternative measure of employee welfare (Empl welfare2)

| | (1)<br>Empl welfare2$_{t+1}$ | (2)<br>Empl welfare2$_{t+1}$ |
|---|---|---|
| RTW | -0.1983*** | -0.397* |
| | (-3.01) | (-2.04) |
| RTW*High free-ride$_{t+1}$ | -0.328*** | |
| | (-6.57) | |
| High free-ride$_{t+1}$ | -0.347*** | |
| | (-4.01) | |
| RTW*High union coverage$_{t+1}$ | | -0.014** |
| | | (-2.24) |
| High union coverage$_{t+1}$ | | 0.401*** |
| | | (8.14) |
| Firm and state-level controls | Yes | Yes |
| Firm * Cohort FE | Yes | Yes |
| Year * Cohort FE | Yes | Yes |
| State Cluster | Yes | Yes |
| Observations | 98,595 | 98,595 |
| Adjusted R2 | 0.347 | 0.352 |

This table examines the dynamics of collective bargaining power and the free ridership hypothesis. Model 1 shows the results of the DiD regressions of RTW indicator on employee welfare (*Empl welfare2$_{t+1}$*) using high/low (above/below cross-sectional median values) free ridership (*High free-ride*). Model 2 shows the results of the DiD regressions of RTW indicator on employee welfare (*Empl welfare2$_{t+1}$*) using high/low (above/below cross-sectional median values) union coverage (*High union coverage*). The regressions include controls for size, leverage, return on assets (ROA), cash, pc_state income, ln_state income, and gr_state income (*Firm and state-level controls*). Also, all regressions control for firm-cohort and year-cohort fixed effects. Standard errors are clustered at the state level. A detailed description of the variable construction is provided in Table A1. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A6: The effect of RTW laws on employee welfare considering the unobservable local economic conditions

| | (1) Empl welfare1$_{t+1}$ | (2) Empl welfare2$_{t+1}$ | (3) First-stage | (4) Empl welfare1$_{t+1}$ | (5) Empl welfare2$_{t+1}$ | (6) Empl welfare1$_{t+1}$ | (7) Empl welfare2$_{t+1}$ |
|---|---|---|---|---|---|---|---|
| RTW | -1.024** | -0.256** | | | | -1.013*** | -0.253** |
| | (-2.56) | (-3.54) | | | | (-2.84) | (-1.98) |
| Political leaning | | | -0.055** | | | | |
| | | | (-10.05) | | | | |
| Lagged GDP | | | 0.025** | | | | |
| | | | (-2.40) | | | | |
| RTW (inst.) | | | | -2.939*** | -1.235*** | | |
| | | | | (-16.64) | (-12.85) | | |
| Size | -0.037 | -0.009 | -0.005* | 0.070*** | 0.018*** | 0.044 | 0.011 |
| | (-1.06) | (-1.11) | (-1.94) | (5.46) | (3.58) | (1.47) | (1.00) |
| Lev | 0.146 | 0.037 | -0.004*** | 0.336*** | 0.084*** | 0.135* | 0.034** |
| | (0.65) | (0.15) | (-2.73) | (5.35) | (2.99) | (1.84) | (1.95) |
| ROA | 0.030 | 0.007 | 0.002 | 0.002 | 0.000 | 0.098 | 0.025 |
| | (0.14) | (1.05) | (1.57) | (0.03) | (0.11) | (0.97) | (0.08) |
| Cash | 0.063 | 0.016 | 0.004*** | -0.242*** | -0.061*** | -0.177* | -0.044* |
| | (0.35) | (1.05) | (2.74) | (-3.18) | (-3.25) | (-1.79) | (-1.88) |
| PC_state income | -8.078*** | -2.020*** | 0.276*** | 13.754*** | 10.938*** | -6.401*** | -1.600*** |
| | (-10.99) | (-8.09) | (11.36) | (4.84) | (4.21) | (-7.21) | (-5.45) |
| Ln_state income | 0.071 | 0.018 | -0.142*** | -10.221*** | -9.27*** | -0.933*** | -0.233*** |
| | (0.21) | (1.00) | (-7.32) | (-4.90) | (-6.97) | (-3.23) | (-3.07) |
| Gr_state income | 2.167 | 0.542 | -0.065 | 5.669 | 1.417 | 1.973 | 0.493 |
| | (1.13) | (1.10) | (-0.62) | (1.62) | (1.00) | (1.12) | (1.00) |
| GDP gr_state | | | | -2.939*** | -1.235*** | 0.003 | 0.001 |
| | | | | | | (0.09) | (0.14) |
| EPU_Composite | | | | | | -0.018 | -0.005 |
| | | | | | | (-0.86) | (-0.74) |
| EPU_National | | | | | | 0.003 | 0.001 |
| | | | | | | (0.22) | (1.04) |
| EPU_State | | | | | | 0.041** | 0.010** |
| | | | | | | (1.96) | (1.97) |
| Ln_state pop | | | | | | -0.657** | -0.164** |
| | | | | | | (-2.38) | (-2.34) |
| Firm * Cohort FE | Yes | Yes | | Yes | Yes | Yes | Yes |
| Year * Cohort FE | Yes | Yes | | Yes | Yes | Yes | Yes |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| State Cluster | Yes | Yes | | Yes | Yes | Yes | Yes |
| F-statistics value | | | 15 | | | | |
| 1. Endogeneity test Chi2 (p-value) | | | | 30.165 (0.000) | 41.100 (0.000) | | |
| 2. Underidentification test (Kleibergen-Paap rk LM statistic) | | | 9.441 (0.000) | | | | |
| 3. Weak identification tests: Cragg-Donald Wald F statistic | | | 26.170 | | | | |
| (Kleibergen-Paap rk Wald F status | | | 20.285 | | | | |
| Stock-Yogo weak ID test critical values: 10% maximal IV size) | | | 19.934 | | | | |
| Observations | 31,620 | 31,620 | 98,595 | 98,595 | 98,595 | 98,595 | 98,595 |
| Adjusted R2 | 0.153 | 0.172 | 0.351 | 0.257 | 0.334 | 0.200 | 0.239 |

This table reports the results of the DiD regressions of the RTW indicator on employee welfare (*Empl welfare1*$_{t+1}$ and *Empl welfare2*$_{t+1}$). Models 1 and 2 use control firms headquartered in neighboring states contiguous to the treated ones. Models 3-5 use instrumental variables. In the first stage (Model 3), we estimate a predictive regression in which we use political leaning, measured by the ratio of votes cast for a Democrat presidential candidate to the votes cast for the Republican candidate, and lagged GDP per capita to capture general economic development as our instruments for RTW indicators. These variables capture the political and economic factors that have been shown to influence the implementation of RTW laws (Vedder, 2011; Feigenbaum et al., 2018). The overidentification test is also reported. The first-stage F-statistic exceeds 15, indicating that the instruments possess sufficient strength to mitigate concerns of weak identification. The robustness of the instrumental variable strategy is further supported by several diagnostic tests. First, the endogeneity tests yield Chi-squared statistics of 30.165 and 41.100 (both with p = 0.000), confirming that the endogenous regressors are statistically different from exogenous variables and thus validating the need for instrumental variable estimation. Second, the Kleibergen-Paap underidentification tests produce significant linear model statistics (9.44, p = 0.000), rejecting the null hypothesis of underidentification and confirming that the instruments are sufficiently correlated with the endogenous regressors. Third, weak instrument diagnostics provide strong evidence of instrument relevance: the Cragg-Donald Wald F statistics (26.17 and Kleibergen-Paap rk Wald F statistics (20.28) substantially exceed the Stock-Yogo 10% maximal IV size critical value of 19.93. This indicates that the instruments are not weak and that concerns about bias due to weak identification are minimal. The results for the second stage are reported in Models 4 and 5. Models 6 and 7 control for more state-level characteristics. Specifically, we introduce state-level political and economic variables, including GDP growth (GDP gr_state), a measure of economic and political uncertainty within states represented by a composite set of terms (EPU_Composite), uncertainty stemming from national policy-related sources (EPU_National), and uncertainty originating from state and local policy issues (EPU_State). Additionally, we include the natural logarithms of states' populations (Ln_state pop) to further refine our analysis. All regressions control for firm-cohort and year-cohort fixed effects. Standard errors are clustered at the state level. A detailed description of the variable construction is provided in Table A1. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A7: The impact of RTW laws on employee welfare by firm age

| | (1)<br>Empl welfare1$_{t+1}$ | (2)<br>Empl welfare2$_{t+1}$ |
|---|---|---|
| RTW | -0.472*** | -0.118*** |
| | (-3.44) | (-2.87) |
| RTW *Age | 0.003 | 0.001 |
| | (1.11) | (1.07) |
| Age | -0.906*** | -0.227*** |
| | (-9.57) | (-4.88) |
| Size | 0.054*** | 0.014*** |
| | (3.72) | (4.54) |
| Lev | 0.361*** | 0.090*** |
| | (6.62) | (6002) |
| ROA | 0.163*** | 0.041*** |
| | (2.83) | (2.47) |
| Cash | -0.149** | -0.037** |
| | (-2.14) | (-2.05) |
| PC_state income | 29.336*** | 7.334*** |
| | (9.25) | (9.20) |
| Ln_state income | -9.070*** | -2.267*** |
| | (-16.76) | (-11.00) |
| Gr_state income | 4.597** | 1.149** |
| | (2.47) | (2.04) |
| Firm * Cohort FE | Yes | Yes |
| Year * Cohort FE | Yes | Yes |
| State Cluster | Yes | Yes |
| Observations | 50,678 | 50,678 |
| Adjusted R2 | 0.324 | 0.375 |

This table reports the robustness results of the DiD regressions of the RTW indicator on employee welfare (*Empl welfare1*$_{t+1}$ and *Empl welfare2*$_{t+1}$), considering firms' age. All regressions control for firm-cohort and year-cohort fixed effects. The standard errors are clustered at the state level. A detailed description of the variable construction is provided in Table A1. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A8: Additional analysis

| | (1) Empl welfare1$_{t+1}$ | (2) Empl welfare2$_{t+1}$ | (3) Empl welfare1$_{t+1}$ | (4) Empl welfare2$_{t+1}$ | (5) Empl welfare1$_{t+1}$ | (6) Empl welfare2$_{t+1}$ |
|---|---|---|---|---|---|---|
| RTW | -1.181* | -0.320* | -0.864** | -0.216** | -2.151*** | -0.538*** |
| | (-1.85) | (-1.94) | (-2.40) | (-2.51) | (-5.75) | (-5.14) |
| Size | 0.069* | 0.017 | 0.073 | 0.018* | 0.093*** | 0.023*** |
| | (1.75) | (1.66) | (1.11) | (1.78) | (2.82) | (3.05) |
| Lev | 0.160*** | 0.254*** | 0.182 | 0.046 | 0.287** | 0.072** |
| | (6.07) | (6.00) | (1.44) | (1.17) | (2.68) | (2.95) |
| ROA | 0.138 | 0.034 | 0.176 | 0.044 | 0.070 | 0.018 |
| | (1.20) | (1.11) | (1.14) | (1.05) | (0.69) | (0.60) |
| Cash | -0.182 | -0.045 | 0.089 | 0.022 | -0.260*** | -0.065*** |
| | (-1.07) | (-1.07) | 0.073 | (0521) | (-2.88) | (-2.07) |
| Div | 0.301*** | 0.075*** | | | | |
| | (4.02) | (3.02) | | | | |
| Tangibility | 0.042 | 0.010 | | | | |
| | (1.37) | (0.87) | | | | |
| Sale_gr | -0.071*** | -0.018*** | | | | |
| | (5-.29) | (-4.05) | | | | |
| RD | 0.659*** | 0.165*** | | | | |
| | (4.24) | (3.88) | | | | |
| MTB | -0.085*** | -0.021*** | | | | |
| | (-9.19) | (-8.11) | | | | |
| PC_state income | 3.683** | 8.171** | 3.466** | 7.867** | 7.765** | 6.941** |
| | (2.06) | (2.00) | (2.39) | (2.39) | (2.15) | (2.10) |
| Ln_state income | -4.855** | -8.714** | -3.005*** | -7.751*** | -3.798** | -7.450** |
| | (-2.72) | (-2.88) | (-2.82) | (-3.10) | (-2.78) | (-2.66) |
| Gr_state income | 4.433 | 1.108 | 6.233 | 1.558 | 4.937** | 1.234** |
| | (1.42) | (0.42) | (0.78) | (0.18) | (2.33) | (3.01) |
| Firm * Cohort FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year * Cohort FE | Yes | Yes | Yes | Yes | Yes | Yes |
| State Cluster | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 51,963 | 51,963 | 23,488 | 23,488 | 96,218 | 96,218 |
| Adjusted R2 | 0.240 | 0.314 | 0.123 | 0.158 | 0.192 | 0.254 |

This table reports the robustness results of the DiD regressions of the RTW indicator on employee welfare (*Empl welfare1$_{t+1}$* and *Empl welfare2$_{t+1}$*). In Models 1 and 2, we use additional control variables. Models 3 and 4 show the results for small firms measured as the bottom 20th centile of total assets. Models 5 and 6 present the results excluding Wisconsin, West Virginia, and Kentucky. All regressions control for firm-cohort and year-cohort fixed effects. The standard errors are clustered at the state level. A detailed description of the variable construction is provided in Table A1. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

## Appendix B: Treated and control states in the stacked DiD framework

| Treated states | RTW Year | Control states |
|---|---|---|
| Oklahoma | 2001 | Alaska, California, Colorado, Connecticut, Delaware, District of Columbia, Hawaii, Illinois, Maine, Maryland, Massachusetts, Minnesota, Missouri, Montana, New Hampshire, New Jersey, New Mexico, New York, Ohio, Oregon, Pennsylvania, Rhode Island, Vermont, Washington, Indiana, Michigan, Wisconsin, West Virginia, Kentucky |
| Indiana | 2012 | Alaska, California, Colorado, Connecticut, Delaware, District of Columbia, Hawaii, Illinois, Maine, Maryland, Massachusetts, Minnesota, Missouri, Montana, New Hampshire, New Jersey, New Mexico, New York, Ohio, Oregon, Pennsylvania, Rhode Island, Vermont, Washington |
| Michigan | 2013 | Alaska, California, Colorado, Connecticut, Delaware, District of Columbia, Hawaii, Illinois, Maine, Maryland, Massachusetts, Minnesota, Missouri, Montana, New Hampshire, New Jersey, New Mexico, New York, Ohio, Oregon, Pennsylvania, Rhode Island, Vermont, Washington |
| Wisconsin | 2015 | Alaska, California, Colorado, Connecticut, Delaware, District of Columbia, Hawaii, Illinois, Maine, Maryland, Massachusetts, Minnesota, Missouri, Montana, New Hampshire, New Jersey, New Mexico, New York, Ohio, Oregon, Pennsylvania, Rhode Island, Vermont, Washington |
| West Virginia | 2016 | Alaska, California, Colorado, Connecticut, Delaware, District of Columbia, Hawaii, Illinois, Maine, Maryland, Massachusetts, Minnesota, Missouri, Montana, New Hampshire, New Jersey, New Mexico, New York, Ohio, Oregon, Pennsylvania, Rhode Island, Vermont, Washington |
| Kentucky | 2017 | Alaska, California, Colorado, Connecticut, Delaware, District of Columbia, Hawaii, Illinois, Maine, Maryland, |

| | | |
|---|---|---|
| | | Massachusetts, Minnesota, Missouri, Montana, New Hampshire, New Jersey, New Mexico, New York, Ohio, Oregon, Pennsylvania, Rhode Island, Vermont, Washington |

To ensure valid identification in our stacked DiD framework, we construct treatment cohorts based on the year of RTW adoption in each treated state and define an event window of [–5, +5] years around the adoption year. We define control states as those that either (1) never adopted RTW, or (2) adopted RTW outside of the [-5, +5] window relative to the treatment year of a given cohort. Importantly, states that adopted RTW laws after the +5-year window are still eligible to serve as control states for earlier-treated cohorts. This approach avoids contamination from overlapping treatments while allowing for a sufficiently large and valid control group. For example, states like Indiana (RTW in 2012), Michigan (2013), and Kentucky (2017) can be valid controls in the Oklahoma cohort (RTW in 2001), because their RTW adoption occurred after Oklahoma's +5 window.

## Appendix C: Construction of employee welfare variable

To measure employee welfare we use the KLD STATS (Statistical Tool for Analyzing Trends in Social & Environmental Performance) database. Following corporate finance studies (Ghaly et al., 2015; Faleye and Trahan, 2011; Verwijmeren and Derwall, 2010) we calculate employee welfare by considering identified strengths and identified concerns included in the "employee relations" dimension in a given year. More specifically, we calculate employee welfare by summing the identified strengths score and subtracting the aggregate concerns score to determine the net score for each year, as outlined in Equation (2).[28] However, given that the main measures of employee welfare include measures that are directly related to unionization, we exclude union-related strengths and concerns from our employee welfare measure in this paper. Below are the components of our index as described by the KLD.

*Strengths:*

1. Cash profit sharing: the firm has a cash profit-sharing program through which it has recently made distributions to a majority of its workforce.

2. Employee involvement: the firm strongly encourages worker involvement and/or ownership through stock options available to a majority of its employees; gain sharing, stock ownership, sharing of financial information, or participation in management decision making.

3. Retirement benefits strength: the firm has a notably strong retirement benefits program.

4. Work/life benefits: the firm has outstanding employee benefits or other programs addressing work/family concerns (e.g., childcare, elder care, or flextime).

*Concerns:*

1. Health and safety concern: the firm has recently either paid substantial fines or civil penalties for willful violations of employee health and safety standards, or has been otherwise involved in major health and safety controversies.

2. Workforce reductions: the firm has made significant reductions in its workforce in recent years.

3. Retirement benefits concern: the firm has either a substantially underfunded defined benefit pension plan or an inadequate retirement benefits program.

4. Other concern: the firm is involved in an employee relations controversy that is not covered by other KLD ratings.

[28] In the KLD database, each of the categories receives a rating of either 0 or 1. However, retirement benefits strength and concerns have been discontinued after 2009. Therefore, to arrive at our employee welfare index, we average the KLD components of the five "strength" categories and subtract the average of the five components of the "concern" categories, creating an index ranging from − 1 to 1. By construction, higher values of the index indicate better employee welfare.

We further verify the robustness of our results by employing an alternative proxy for employee welfare. We also follow Mănescu (2011) and take average strengths and then subtract average concerns to obtain the net average score per year for the employee relations dimension (*Empl welfare2*) as follows.

$$Employee\ welfare2_{i,t+1} = \frac{\sum Strength_{i,t+1}}{u_{i,t+1}} - \frac{\sum Concern_{i,t+1}}{k_{i,t+1}} \quad \text{Eq. (3)}$$

where $Employee\ welfare2_{i,t+1}$ is the annual average employee welfare score for firm *i*; $u_{i,t+1}$ is the number of strengths for firm *i* in year *t+1*; $k_{i,t+1}$ is the number of concerns for firm *i* in year *t+1*; $Strength_{i,t+1}$ and $Concern_{i,t+1}$ remain the same, as defined in Equation (2).