The Economic Impact of Right-to-Work Laws: Evidence from Collective Bargaining Agreements and Corporate Policies

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Abstract

We analyze the economic and financial impact of right-to-work (RTW) laws in the US. Using data from collective bargaining agreements, we show that there is a decrease in wages for unionized workers after RTW laws. Firms increase investment and employment but reduce financial leverage. Labor-intensive firms experience higher profits and labor-to-asset ratios. Dividends and executive compensation also increase post-RTW. Our results are consistent with a canonical theory of the firm augmented with an exogenous bargaining power of labor and suggest that RTW laws impact corporate policies by decreasing that bargaining power.

1. Introduction

Employees are critical stakeholders in firms and their wages have a significant operational and financial impact on employers.[1] As wages are endogenous, we use right-to-work (RTW) laws, which have been passed by twenty-seven states in the US, as an exogenous negative shock to the bargaining power of workers. In RTW states, employees can join a unionized establishment without having to pay union fees. All employees, even if they are not members of the union, are protected by the collective bargaining agreement (CBA) negotiated by the union. In this paper, we show that RTW laws, and the consequent decrease in union bargaining power, has a significant impact on wages, investment, employment, profitability, as well as on several financial policies such as leverage, dividends, and executive compensation.

We use wage growth data from 19,574 CBAs in the US. To the best of our knowledge, this is the first paper to use the wage information embedded in these contracts. Our identification strategy exploits the introduction of RTW laws across five states during 1988–2016: Oklahoma, Indiana, Michigan, Wisconsin, and West Virginia. While we cannot completely rule out the omitted variable problem, a wide range of fixed effects and additional control variables as well as robustness tests help mitigate many plausible omitted variable concerns. We find that RTW laws reduce nominal wage growth by 0.6 percentage points over approximately one year. The unconditional average wage growth in our sample is 2.9% and average CPI inflation is 2.6%, suggesting that RTW laws eliminate a substantial fraction of real wage growth, albeit only over one year. We cannot directly test the effect on wage levels because the CBAs mostly contain data on wage growth rates. However, even a temporary effect on wage growth is consistent with a permanent negative effect on wage levels.

One of our main assumptions is that this reduction in wage growth is a result of a decline in union bargaining power. This relation is difficult to test directly because union strength is hard to measure. However, we provide two indirect tests for our hypothesis. We first show

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[1]Autor et al. (2017) document that the payroll (wages and salaries) to sales ratio is on average 37% in the services sector and 15% in manufacturing.

that there is a drop in the number of CBAs after the passage of an RTW law, which suggests that RTW laws reduce union strength by so much that some establishments de-unionize. In the second test, we use state-level union membership data to show that the free-rider problem between workers increases after RTW introduction. Both tests are consistent with the idea that RTW laws reduce union bargaining power.

A canonical theory of the firm, with labor and capital as the only inputs of production, predicts that a reduction in wages leads to higher investment, employment, profitability, and a higher labor-to-assets ratio. Extensions of this canonical theory, such as Matsa (2010), Michaels et al. (2019), or Ellul and Pagano (2019), further predict that a positive shock to firms' bargaining power leads to a reduction in financial leverage. These authors argue that firms use leverage as a bargaining chip in negotiations with workers. As union strength drops after RTW adoption, that need weakens, and one should expect leverage to go down. However, there is a competing hypothesis, which predicts that operating leverage decreases due to lower employee wage bargaining power. As a result, firms should be able to borrow more as future cash flows are freed up.

We use the CRSP-Compustat merged data set to explore how firms react to the introduction of RTW laws. Due to the longer sample period of 1950–2016, the number of states that introduce an RTW law increases to 14. We find that firms invest more and increase employment, both of which are consistent with a drop in wages. Also, firms reduce financial leverage, which is consistent with Matsa (2010), who finds that firms use financial leverage as a strategic tool to threaten bankruptcy, and thereby increasing their bargaining power against unions. Our results suggest that after the introduction of an RTW law, firms' bargaining power increases, and they no longer need to use high leverage as a bargaining tool.

As the next step, we use spline regressions to investigate the dynamic effect of RTW on firm outcomes. We document that there is an average three-year delay after RTW laws for the positive impact on investment and employment growth to materialize. On the other

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hand, firm de-leveraging happens earlier—one year after RTW introduction.

There is no statistically significant effect of RTW on operating profitability for the average firm. However, when we focus on labor-intensive firms, defined by those with a high labor-to-assets ratio, profitability is significantly higher 5 years after RTW adoption. Economically, operating profitability is almost 3 percentage points higher in year 5 post-RTW. Similarly, we do not find a statistically significant effect on the labor-to-assets ratio for the average firm. However, for labor-intensive firms, we find a significant increase in the labor-to-assets ratio four years after the introduction of RTW.

In additional tests, we also look at payout policy, cash holdings, and executive compensation. We find that RTW increases payout through higher dividends. Our results for the effect on share repurchases and cash holdings are inconclusive. Notably, the timing of the dividend increase is in line with those on investment and employees growth. The spline regression shows dividend payout is significantly higher in year 3 following RTW adoption compared to the benchmark in the year immediately prior. Using the ExecuComp database, we find that RTW laws have a positive effect on CEO compensation. Executives receive increases in base salary, the value of options granted, and other compensation, such as contributions to pension plans. We do not find a statistically significant effect on the value of stock-based grants. Overall, these preliminary results are consistent with the rest of our results on the impact of RTW laws on firms.

Finally, we examine the impact of RTW laws on the unemployment insurance provision between firms and workers. Under the implicit contract framework of Baily (1974) and Azariadis (2015), firms can act as buffers by absorbing adverse shocks on behalf of their employees in exchange for lower wages. An extension of the theory in the RTW context implies that as the bargaining power shifts to firms after RTW introduction, wages fall and the incentive to provide insurance declines. To test this hypothesis, we follow Ellul et al. (2018) by comparing pre- and post-RTW sensitivities of firm-level employees growth to industry-level sales shocks. We find that the passthrough of industry sales growth to firm

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employees growth is significantly larger after RTW adoption relative to before. In other words, firms headquartered in RTW states are more likely to decrease their labor force due to a negative industry-wide shock than firms located in a non-RTW state, which confirms our hypothesis.

Taken together, our results are consistent with the view that RTW laws reduce the bargaining power of workers. This has significant effects on both workers and firms. However, our findings cannot be immediately used to measure the aggregate welfare effects of RTW laws, because they have both positive and negative effects. On the one hand, workers who are covered by a collective bargaining agreement seem to be the most negatively affected. Our wage growth results suggest that their salaries drop in the year when RTW is introduced and stay at that lower level. On the other hand, equity holders and executives of large corporations, and potentially non-unionized workers as well, seem to gain from RTW laws.

Our paper contributes to four different strands of the literature. The first contribution is to the growing literature on labor and finance. Most of the existing papers, such as Matsa (2010), Agrawal and Matsa (2013), Simintzi et al. (2015), or Serfling (2016) focus on the relation between labor market legislation and financial leverage. Among these papers, ours is most closely related to Matsa (2010), who shows that firms use financial leverage as a strategic bargaining tool against unions. Our results on the effect of RTW laws on leverage are consistent with his findings, although we use a different methodology and a longer sample period. Similarly, our finding that RTW laws lead to an increase in firm investment extend the literature on the negative effect of unions on investment, which includes Hirsch (1992), Bronars and Deere (1993), Fallick and Hassett (1999), and Bradley et al. (2017). The main contribution of our paper to this literature is the use of RTW laws as a shock to union bargaining power.

Next, our paper provides evidence for the negative effect of RTW laws on the wages of unionized workers. This is important because evidence from the existing literature on this question is mixed. Carroll (1983) and Garofalo and Malhotra (1992) find that RTW

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laws reduce wages, but Moore (1980), Wessels (1981), Moore et al. (1986), and Hundley (1993) find no effect. Our paper has several methodological advantages compared to the existing literature. For example, many of the existing papers use wage data aggregated at the establishment or state level or rely on a single cross-section. By contrast, our CBA data allows us to measure the contractual wages of exactly those workers that are most likely to be affected by RTW laws and is therefore arguably less noisy than aggregate data. Also, our relatively long sample period allows us to use the changes in RTW laws for identification as opposed to simply comparing RTW states to non-RTW states. Most of the studies mentioned above that find no evidence of RTW laws affecting wages rely on cross-state variations in a given point in time due to the lack of RTW adoptions between 1963 to 2001 (Louisiana in 1976 and Idaho in 1986 are the exceptions).[2] Finally, we combine worker-level tests with tests to determine the effects of RTW adoption on firms.

We also contribute to the research on the causes of the decline of unions in the US. In particular, our results relate to the literature on the effect of RTW laws on unions. Importantly, some papers in the literature find a negative effect on union membership rates while others find no effect, so the "issue of whether or not RTW laws reduce unionization remains an open question" (Moore, 1998, p. 453). We use a substantially longer sample period than previous studies allowing us to include a higher number of RTW introductions and an identification strategy based on the difference-in-differences method. We show that the number of CBAs has decreased, which suggests that some establishments may have de-unionized. More importantly, our results document the gap between the union coverage rate and the union membership rate. This demonstrates an increase in the percentage of workers who are free-riding on the union bargaining agreements. Both these results show how the passage of RTW laws leads to a decline in union bargaining power.

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[2]See Hundley (1993). "The cross-sectional analysis conducted in this study does not permit as strong a test for causal inferences as would a data set where changes in individual coverage and membership states are matched with changes in important bargaining law variables... Since, with a couple of exceptions, state bargaining law provisions have remained substantially unchanged since the late 1970s, it is not possible to equate changes in bargaining laws with changes in coverage/membership states."

The rest of the paper proceeds as follows. Section 2 presents the conceptual framework behind our main tests. We discuss the empirical specification and identification challenges in Section 3. Data sources and variable definitions are described in Section 4. Our main empirical results are presented in Section 5, followed by additional results in Section 6. Finally, Section 7 concludes.

2. Conceptual framework

We present a simple static, partial equilibrium conceptual framework that provides the foundation for our main empirical tests. It allows us to examine the effect of a shift in the relative bargaining power between firms and workers on wages and several corporate policies. A complete theoretical model would be beyond the scope of the paper. Instead, we use the simplest possible model to derive most of our core predictions and inform our analysis.

We assume that each firm has a Cobb-Douglas production function of the form

$$Q(K, L) = AK^{\alpha}L^{\beta}, \tag{1}$$

where Q is output, A is total factor productivity, and $\alpha > 0$ and $\beta > 0$ are exogenous parameters of the production function. Physical capital and labor input are denoted by K and L, respectively. The assumption of a Cobb-Douglas functional form is not crucial, as most predictions are robust to a wide range of production functions.

The firm takes output and input prices as given and chooses capital and labor to maximize profit:

$$\max_{K,L} \big\{ p\,Q(K, L) - w(\theta)L - rK \big\}, \tag{2}$$

where p is the price at which the firm can sell its product, w is the wage per unit of labor, and r is the rental cost of capital.

We assume that the wage w is a decreasing function of the relative bargaining power of

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the firm, θ. This is a central element of the framework because we argue that the introduction of RTW laws can be thought of as a positive shock to θ. For brevity, we do not explicitly model the bargaining game between workers and the firm. However, this simple model can be easily extended to incorporate such bargaining, as in Michaels et al. (2019).

This framework, while extremely simple, can be used to derive most of our core predictions. An exogenous increase in the bargaining power of the firm, θ, affects both workers and the firm in various ways.

Wages: An increase in the bargaining power of the firm should lead to lower wages. This is by assumption, because the function $w(\theta)$ is decreasing in θ. As mentioned before, it is straightforward to extend the model to allow for bargaining between the firm and workers.

Employment: The optimal level of labor input, L^*, will increase. Empirically, we test this using the number of employees of the firm.

Investment: With a Cobb-Douglas production function, a reduction in wages will also lead to a higher amount of capital, K^*. Interestingly, this prediction holds for other commonly used production functions as well, as long as the cross-derivative $\partial^2 Q / \partial K \partial L > 0$.[3]

Labor-to-capital ratio: With a Cobb-Douglas production function, a reduction in the wage rate will lead to an increase in the labor-to-capital ratio. However, both the numerator and denominator increase, causing the increase to be smaller than one would intuitively expect.

Profitability: Equation (2) suggests that a decrease in the wage rate will—ceteris paribus—lead to an increase in profits. However, under the assumption of a competitive output market, the equilibrium price p will adjust so that the net effect on profits will be zero. In the simple framework presented here, we made a strong assumption of such a competitive output market. If we allowed for imperfect competition, then the firm would charge a price p at a markup relative to marginal costs, which implies that a reduction in wages can lead to an increase in profits. Thus, depending on the assumption on competitiveness,

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[3]This result can be found in `https://people.ucsc.edu/~wittman/classes/econ-204a/`, among others.

the predicted effect on profitability ranges from zero to some positive amount.

Leverage: Our simple model does not distinguish between different types of financing, such as equity and debt. However, several papers have extended this setup to allow for endogenous financing decisions, such as Matsa (2010), Michaels et al. (2019), or Ellul and Pagano (2019). All three of these models predict that leverage can be used as a strategic variable to improve the firm's wage bargaining outcome. An exogenous increase in the firm's bargaining power θ will, therefore, reduce the need for high leverage as a bargaining tool, which reduces the amount of debt financing used by the firm.

3. Empirical specification and identification

Our research question is whether the introduction of RTW laws has an effect on wages and firm outcome variables such as investment, profitability, and leverage. However, estimating the causal effect of these laws is challenging. Legislation is not random, and right-to-work laws are no exception. We argue that the main endogeneity concerns are an omitted variable that is correlated with the law and correlated with wage growth, and, to a lesser extent, reverse causality.

One plausible omitted variable is globalization. Offshoring jobs to low-wage countries could simultaneously apply downward pressure on wage growth and force US states to pass RTW legislation in order to be competitive. Another possibility is that anti-union sentiment—which is hard to measure—increases over time, allowing firms to lobby for the passage of RTW laws while wage growth also trends downward. Either one of these scenarios raises endogeneity concerns for estimating the causal link between RTW introduction and wage growth.

Concerning reverse causality, it is possible that some states experience lower wage growth than others and this lower wage growth causes the introduction of RTW laws. Voters in states with low wage growth could believe that unions are responsible for low worker income,

which then induces state legislatures to pass RTW laws. The result would be a negative observed correlation between wage growth and RTW introductions, but the causality would be opposite to our story.

We use several methods to address these endogeneity problems. Our main approach is a difference-in-differences regression that exploits the fact that some states have introduced an RTW law while other states have not. Table 1 summarizes the introduction years and shows that by 2017, 27 states have implemented such a law. The difference-in-differences methodology reduces the risk that unobservable time-invariant state characteristics or unobservable time shocks confound the estimation of the effect of RTW laws on wage growth. However, a remaining concern is that of time-varying unobservable state characteristics. We address this issue after presenting our regression specification.

A typical difference-in-differences specification in this context would look like this:

$$\log(w_{ist}) = bRTW_{st} + \lambda_t + \delta_s + \varepsilon_{ist}, \tag{3}$$

where the dependent variable is the log of wages in contract i, in state s, in year t. The variable RTW is a dummy that takes a value of one in all state-year observations in which a RTW law is in effect, and a value of zero in all other state-years. The specification includes year fixed effects, λ_t, as well as state fixed effects, δ_s.

However, in our main data set, the level of wages is not observable, only the growth rate is. Therefore, we estimate a difference-in-differences specification in changes rather than in levels:

$$\Delta \log(w_{ist}) = \beta \Delta RTW_{st} + \psi_t + \epsilon_{ist}. \tag{4}$$

As shown in Angrist and Pischke (2009), under certain assumptions, this is equivalent to Equation (3), and the first-difference operator removes the need for state fixed effects. The transformed year fixed effects are denoted as ψ_t.

We augment Equation (4) with additional control variables. The specification that we

estimate is

$$\Delta \log(w_{ijst}) = \beta \Delta RTW_{st} + \gamma \Delta GSP_{st} + \psi_t + \rho_j + \phi_s + \epsilon_{ijst}. \tag{5}$$

The dependent variable is the change in log wages, $\Delta \log(w) = \log(w_t/w_{t-1})$. ΔRTW is a dummy variable, which takes a value of one only in the state-year observations where a RTW law is introduced. We add the real growth rate of the gross state product, ΔGSP_{st}, as a control variable. We do this because local economic conditions can be an important determinant of wage growth. Also, it is possible that local economic growth affects the introduction of an RTW law. Our results are very similar if we do not control for ΔGSP, as shown in Appendix A.

Equation (5) also includes year fixed effects, ψ_t, industry fixed effects, ρ_j, and state fixed effects, ϕ_s. It should be noted that the dependent variable is $\Delta \log(w)$, not $\log(w)$, so time-invariant differences in wage levels between states are already controlled for. Therefore, Equation (5) would qualify as a difference-in-differences specification even without state fixed effects. However, we add state fixed effects to allow for the possibility that wage growth rates vary between states. The coefficient β in Equation (5) can, therefore, be interpreted as the deviation in the wage growth rate in the year of the law's introduction from each state's average wage growth rate, averaged across all treated states.

The difference between Equations (3) and (5) is best illustrated graphically. Figure A1 shows a stylized plot for the empirical pattern that Equation (3) is supposed to capture. The vertical axis shows log wages and the horizontal axis shows time. The dots represent contracts in a state that introduces an RTW law during the sample period. The crosses indicate observations in a state that does not pass such a law. Figure A2 shows an analogous stylized plot, but with the *change* in log wages on the vertical axis. The two figures illustrate that even if RTW laws have a permanent negative effect on wages, that will manifest itself as a temporary negative effect on wage growth rates.

Our difference-in-differences approach exploits the fact that states introduced their RTW law at different points in time. In the sample period of our collective bargaining agreement

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data, the five introductions are Oklahoma (2001), Indiana (2012), Michigan (2013), Wisconsin (2015), and West Virginia (2016). This reduces the risk that some omitted shock, such as globalization or anti-union sentiment, is driving the change in wage growth because that omitted variable would have to change in these five states exactly in those respective five years when the laws are introduced.

Ideally, we would use an even larger number of RTW introductions, but the sample period for our CBA data set is relatively short. However, existing studies in this research area use an even smaller number of RTW introductions. Most papers in the RTW literature only use a single cross-section of data, without looking at any law changes (Moore, 1980; Wessels, 1981; Garofalo and Malhotra, 1992; Hundley, 1993; Holmes, 1998). A few papers use a sample period with one RTW introduction (Carroll, 1983; Moore et al., 1986), and the sample period in Matsa (2010) contains three RTW introductions. Finally, our tests using firm-level data have a longer sample period, and this allows us to significantly increase the number of RTW introductions.

Our second approach to address the endogeneity problem is to provide detailed evidence for the mechanism through which RTW laws lead to lower wage growth. Also, we use firm-level data to show that RTW laws have the opposite effects on firms compared to workers. While these two approaches do not use a different source of exogenous variation from the wage test, they further reduce the likelihood that some omitted variable is driving our results.

In our last test for omitted variables, we examine which state-level political and economic variables predict the introduction of RTW laws. This approach is also used in Simintzi et al. (2015), among others. In a first stage, we estimate a predictive regression in which we use predictors such as (a) the political orientation of the governor, (b) a measure for the importance of imports from China, (c) the state-level union membership rate, and (d) the gross state product growth rate, among others. In a second stage, we use the significant predictors from the first stage as additional controls in our main difference-in-differences regression. This test sheds light on the political economy of RTW laws, and it further reduces

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the likelihood that time-varying variables such as globalization or anti-union sentiment drive our results.

Our predictive regressions for the introduction of RTW laws support the view that the political preferences of voters are one of the main determinants of these laws. Although political outcomes are hard to disentangle from economic variables, this further reduces the likelihood that globalization of trade or low wage growth was the reason for the laws' passage. These predictive results are also consistent with findings in the political science literature that the party affiliation of the president has a positive effect on the likelihood that the opposing party wins gubernatorial elections (Piereson, 1975; Holbrook-Provow, 1987) and state legislature elections (Campbell, 1986).

Finally, we address the reverse causality problem by estimating a modified difference-in-differences specification in which we separately estimate the effect of RTW laws in the years before, during, and after the laws go into effect. We show that RTW laws have no effect on wage growth before the passage of the law. In another test, we do not find that declining union membership predicts RTW laws. While these tests do not completely rule out the possibility of reverse causality, they at least reduce its probability.

4.   Data

4.1.   Bloomberg BNA data

For our tests of the effect of RTW laws on workers, our data set is a sample of CBAs from the Settlement Summaries database of Bloomberg BNA. The initial sample contains 19,574 contracts from the US, covering the period 1988–2016. Among others, the data include the employer name, the union name, the effective date of the agreement, the length of the contract, the city and state of the workers' location, the employer's SIC and NAICS codes, and a short summary of the agreed-upon terms concerning the change in wages.[4]

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[4]Klasa et al. (2009) and Yi (2016) use Bloomberg BNA data, but do not extract the wage information.

The total change in wages specified in each CBA is difficult to summarize in a single number, because most contracts cover several years, with different wage increases in each year. Figure 1 shows that the typical contract length is three years. Also, the wage information is embedded in a separate text string for each CBA, and the structure of these strings is heterogeneous across contracts. For these reasons, we have developed a text extraction algorithm to obtain the wage increase over the first year of each contract, and we use this first-year wage increase as a proxy for the total increase in wages. Finally, we remove states that introduced an RTW law before 1988, which is the beginning of our sample period. This leaves us with a final sample of 15,125 wage contracts. The details of our algorithm and of the sample construction methodology can be found in Appendix B.

Each contract enters exactly one time in the sample, even though the typical contract has a maturity of more than a year. The wage growth for the first year of a contract is not duplicated for all subsequent years of a contract, as this might introduce a bias.[5] In a robustness test, explained in Appendix A, we also extract the wage increase over the second year of each contract, and we find that our main findings are similar to those using the first year.

Ideally, we would have multiple observations for the same firm across many years. However, for the vast majority of firms, this is not the case. As a result of this data limitation, we are comparing average bargaining outcomes before and after RTW laws, even if they correspond to different firms.

Table 2 provides summary statistics for the main variable of interest: the change in log wages, $\Delta \log(w) = \log(w_t/w_{t-1})$. It shows that the unconditional first-year wage growth in our sample is 2.9%. These growth rates are in nominal terms. The table also shows that there are many more control observations than treated observations. For the purpose of this table, a treated observation is defined as a CBA that covers workers in an RTW state and

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[5]Please note that more than one observation is possible for each year and for each employer. This can be due to different plant locations for the same firm, where each location is covered by its own contract. Another reason is that the same firm can have separate contracts for different occupations (e.g., manufacturing workers vs. clerks).

has an effective date that is in or after the year of the introduction of the law. There are relatively few treated observations for two reasons. First, only five states introduce an RTW law during our sample period. Second, most of the RTW introductions occur toward the end of our sample period.

Table 2 already reveals that, in a simple univariate comparison, average wage growth in the treated subsample (1.8%) is lower than average wage growth in the control subsample (2.9%). Table 2 also shows that about two-thirds of our observations are from the private sector (SIC codes below 90) and about one-third are from the public sector (SIC codes of 90 or higher).

We provide additional summary statistics tables in the Appendix. Table A1 shows how the sample is distributed across states. It contains fewer than 50 states because we omit from the sample those states that introduced an RTW law before our sample period. The table reveals one of the caveats of the Bloomberg BNA data set, which is that some states have more observations than others. In particular, some of our treated states (e.g., Oklahoma, West Virginia) have very few observations. The reasons for this difference are (a) the coverage of Bloomberg BNA varies across states, (b) unions are more common in some states than in others, and (c) some states have much larger economies than others.

Table A2 presents the distribution of the sample across time. Column (1) shows that the coverage of Bloomberg BNA is relatively stable over time, although it has slightly fewer observations in the early years of the sample period. Column (2) shows that average wage growth varies substantially over time, with a noticeable decreasing long-term trend. Some of this downward trend might be caused by the staggered introduction of RTW laws, but a substantial portion may also be explained by relatively high inflation in the late 1980s and early 1990s.

Table A3 breaks down the sample by 2-digit SIC codes. The number of observations varies strongly across industries. This is because collective bargaining is much more preva-lent in some industries than others. For example, the public sector has a large number of

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observations, as do certain industries, such as construction, food, local transit, communications, electric services, food stores, health services, and education. While these differences in coverage are to be expected, they also illustrate one of the caveats of our sample. To alleviate the concern that the public sector is driving our results, we add a robustness test in Table A4 in the Appendix where we split the sample into private- and public-sector subsamples. The results suggest that our results are driven mostly by CBAs in the private sector.

To check whether the sample of collective bargaining agreements we are using in the empirical analysis is representative of the universe of all outstanding CBAs, we compare the distribution of our sample across states and across industries against the Contract Listing data set, also provided by Bloomberg BNA. The Contract Listing agreements consist of all private sector union negotiations reported to the Federal Mediation and Conciliation Service (FMCS) between 1990 and August of 2017,[6] whereas the Settlement Summaries data set, which is the sample used for most of our analysis, is collected by Bloomberg using union publications and other press. We use the Contract Listing data set as an approximation of the universe of private sector CBAs.

Tables A5 and A6 compare the distribution of our Settlement Summaries sample to the Contract Listing data. In Table A5, the geographical distribution of our sample matches up well with the universe of negotiated contracts. The percentages of contracts in each state as a fraction of the total sample in Columns (2) and (4) are similar. In Table A6, we compare the contract distributions across one-digit SIC major industry codes using Contract Listing data from 2012 onwards because SIC industry classification is missing prior to 2012 in the Contract Listing data. The top panel shows that our Settlement Summaries data over-samples the public sector (SIC Major 9) relative to the listing data. This is not surprising, given the listing data is explicitly said to be for private sector union contracts reported to the FMCS. In the bottom panel of Table A6, we remove all observations in SIC Major 9 and recalculate the distribution. Again, Columns (2) and (4) show that distributions

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[6]Contract negotiation outcomes like wage are not available in the listing data.

across industries between the two sample are close, with manufacturing (SIC Major 2 and 3) making up the majority of the observations. This exercise provides some assurance that our settlement data is representative of the universe of CBAs.

4.2. Firm-level and macroeconomic data

We obtain firm location and accounting data from the Compustat fundamental annual file for fiscal years 1950 to 2016. We then match firm headquarters to counties by converting headquarters ZIP Codes to FIPS county codes using a link file provided by the US Census Bureau.[7] We also try to be as accurate as possible in assigning firm headquarters to states by using a file containing historical headquarter locations for Compustat firms between 1991 and 2008. A firm-year observation is dropped in the matching process given one of the following conditions: (i) the state variable is missing, (ii) the observation is not in the historical headquarter file, or (iii) the ZIP code cannot be mapped using the FIPS link file. Fourteen states enacted RTW legislation during our sample period: Nevada (1952), Alabama (1953), South Carolina (1954), Utah (1955), Kansas (1958), Mississippi (1960), Wyoming (1963), Louisiana (1976), Idaho (1986), Oklahoma (2001), Indiana (2012), Michigan (2013), Wisconsin (2015), and West Virginia (2016). We omit from this sample any observations that originate from states that introduced RTW legislation before 1950. We exclude all firm-year observations beyond five years after RTW introduction. Furthermore, GDP price deflators were obtained from the FRED database hosted by the Federal Reserve Bank of St. Louis, and state-level GDP data were gathered from the Bureau of Economic Analysis. We convert all dollar variables to real terms by deflating them to 2009 dollars, or inflate them if a value was recorded before 2009.

We screen out observations in which equity value totals less than $10 million. Observations with negative values for any of the following are dropped: total assets (at), sale,

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[7]The headquarters location need not always be where the firm's manufacturing operations are located. But, Henderson and Ono (2008) show that firms consider geographical proximity to their production facilities, possibly due to communication and coordination costs, in choosing their headquarters location.

employees (emp), cash (che), total long-term debt (dltt), total liabilities (lt), and dividend (dv). Observations with a CAPX-to-PPE ratio greater than 50% are eliminated to rule out mergers and acquisitions. We omit financial firms (SIC 6000-6999) and utilities (SIC 4900-4999) from the sample. Also, we winsorize all variables at the 1% and 99% quantiles to reduce the effect of outliers. The screens we use are a combination of the ones used in Vuolteenaho (2002) and Whited and Wu (2006).

5. Economic effects of lower union bargaining power

5.1. Wages

It is not clear a priori what the effect of RTW laws on wages will be. Labor economists distinguish between the *bargaining power hypothesis* and the *taste hypothesis* when it comes to the labor market consequences of RTW laws (see the survey papers by Moore and Newman (1985) and Moore (1998)). Under the bargaining power hypothesis, RTW laws reduce the bargaining power of unions, which reduces its ability to negotiate high wages. The predicted outcome is a lower unionization rate and lower wages for unionized workers. Under the taste hypothesis, however, the main reason for the introduction of an RTW law is the anti-union sentiment among workers in the state. After controlling for this anti-union sentiment, the estimated effect of an RTW treatment on the unionization rate should be zero. Additionally, Farber (1984) argues that wages of unionized workers with an RTW law might be higher. The reason is that workers in these states have a preference against unions. Therefore, workers will only join a union if the wage premium on top of the non-union wage is large enough to compensate them for that disutility.

Another argument in favor of higher wages is that RTW laws might boost local economic growth. Newman (1983, 1984), Schmenner et al. (1987), and Holmes (1998) provide empirical evidence for a positive effect on industrial growth and economic development.

To examine the effect of RTW laws on wages, we estimate Equation (5) using an RTW

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dummy variable that takes a value of one in the year when a state introduces an RTW law and a value of zero in all other years. We denote this variable as ΔRTW. Table 3 summarizes the results of different specifications, subsequently adding more fixed effects in Columns (1)–(3). Standard errors are clustered at the state level. Most importantly, the coefficient of ΔRTW is negative and significant at the 1% level in all columns. This is true even in the most conservative specification in Column (3), which controls for year, industry, and state fixed effects.

The estimated effects are economically quite large: Depending on the specification, wage growth is reduced in the year of an RTW law by 0.6–1.1 percentage points. Even the most conservative coefficient of -0.6 percentage points represents a 20.7% reduction in wage growth relative to the unconditional mean of 2.9%.[8] Also, since all these growth rates are in nominal terms, the effect of RTW laws on real wage growth is even larger.

To see the exact timing of the impact of RTW laws on wage growth, and to test the parallel trends assumption, we perform a spline regression in which we include five separate RTW dummies. We have one dummy variable for all years up to two years before the law's introduction. Then, we have one dummy two years before the law is passed, one dummy for the year in which the law is passed, one dummy for the year after the law is passed, and one dummy for two years after the passage of the law. We denote these variables as $\Delta RTW^{<(-2)}$, ΔRTW^{-2}, ΔRTW, ΔRTW^{+1}, and ΔRTW^{+2}, respectively. The year before the introduction of the law is omitted so that it serves as the reference year. We omit treated observations that occur later than two years after the introduction of the law.

Column (4) of Table 3 contains the estimation results of our spline regression. The coefficients of the variables $\Delta RTW^{<(-2)}$ and ΔRTW^{-2} allow us to test the parallel trends assumption. The pre-treatment coefficients are not statistically significant, which suggests

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[8]This is actually an approximation. To calculate the exact effect, note that since $d[\log(1+y)]/dx$ can be written as $[1/(1+y)]dy/dx$, it follows that $dy = d[\log(1+y)]/dx \times (1+y)dx$. In the case of Column (3), this means that the change in wage growth evaluated at the unconditional mean of 0.029 is $-0.006 \times (1 + 0.029) \times 1 = -0.0062$. This is a decrease of 21.4% relative to the unconditional mean of 0.029. A similar calculation can be found in Chang et al. (2018), among others.

that the parallel trends assumption is not violated. Also, confirming our previous findings, the coefficient of ΔRTW is negative and highly significant, suggesting that RTW laws reduce wage growth in the year of the law's passage. The magnitude of the effect is -0.3 percentage points, which is slightly smaller (in absolute values) than the treatment effect in Columns (1)–(3).

Interestingly, the treatment effect is immediately significant in year 0, without a lag. One might have assumed that it takes some time until union members actually leave their union and that therefore the results show up in year $+1$ or $+2$. One possible explanation for this is that both the firm and the union anticipate in year 0 that the union will lose a lot of members in years $+1$, $+2$, and so on. This anticipation might already reduce the union's bargaining power in year 0.

Another interesting observation is that the coefficients of ΔRTW^{+1} and ΔRTW^{+2} are negative but insignificant. One plausible interpretation of these results is that there is a very short-term effect on wage growth, which can still lead to permanent effects on wage levels. However, another interpretation is that the test for the significance of ΔRTW^{+1} and ΔRTW^{+2} has low power. This can happen if there are very few CBAs after the introduction of an RTW law. We present some evidence to support this latter interpretation in Section 5.4.

In Table 4, we estimate a similar set of regression specifications, but with a different definition of the RTW dummy. This dummy, denoted as RTW, takes a value of one in the year a state introduces an RTW law and maintains this value for all subsequent years. We can see in Table 4 that, while the permanent dummy RTW is still negative, it is insignificant in most specifications. This suggests that RTW laws have no permanent effect on the growth rate of wages. Given that we estimate a regression in changes and not in levels, it is not very surprising that there is no permanent effect. If there were a permanent negative effect on wage growth, then over many years it could compound to a very large negative effect on wage levels. As a result, wage levels in RTW states would be much lower than in non-RTW

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states, which would hardly be a long-run equilibrium.

While Table 4 shows that there is no permanent effect on wage growth, Table 3 shows a temporary effect, which can very well lead to a permanent effect on wage levels, as illustrated in Figures A1 and A2. Unfortunately, for the vast majority of CBAs, we can only observe wage growth, but not the level of wages. Therefore, we cannot directly quantify the permanent effect of RTW laws on wage levels.

To summarize, our results suggest that RTW laws have a significant negative effect on wage growth immediately around the introduction of the law. While there is no permanent effect on wage growth, our results are consistent with a permanent negative effect on wage levels.

5.2. Impact on firm investment, employment, and leverage

In the conceptual framework proposed in Section 2, we hypothesize that RTW laws can drive firms to invest more and to hire at a more rapid rate as wages drop post-introduction. Consistent with this idea, Connolly et al. (1986) show that firms in highly unionized industries invest less in R&D and have lower returns on R&D. Hirsch (1992) presents evidence that unionized firms invest less. Bronars and Deere (1993) also find a negative effect of unionization on both the tangible and intangible capital of firms. Furthermore, Fallick and Hassett (1999) show that after a successful union certification election, firm investment declines. More recently, Bradley et al. (2017) find a negative impact of labor unions on innovation in terms of patent quality and R&D investment.

We also explore the impact of RTW on firm leverage as two competing hypotheses suggest that the decline in labor constraint may cause firms to adjust their capital structure. On the one hand, firms may reduce leverage after an RTW law is enacted, because RTW introduction reduces firms' incentives to rely on strategic leverage as in Matsa (2010). On the other hand, firms may increase leverage after RTW adoption as Simintzi et al. (2015) find that weaker protection of workers leads to an increase in financial leverage.

To be clear, our Compustat tests have some limitations compared to the collective bargaining agreement (CBA) tests. First, not all firms in Compustat are unionized. Second, even if a firm is unionized, CBAs are not necessarily renegotiated immediately after RTW introduction. Third, there is noise in Compustat, because it only contains headquarters location, but the affected workers could be in other locations too. On the other hand, our contract-level tests do not suffer from these limitations: We focus only on unionized firms, an observation only enters in our regression if the contract is renegotiated, and we know the exact location of the affected workers. This said, as we will demonstrate here, RTW introduction does have a significant impact on the general population of public firms.

We use a difference-in-differences approach to estimate the effect of RTW laws. Our treatment group consists of firm-year observations in RTW states after the law was introduced, and the control group consists of (a) firm-year observations in RTW states before the law was introduced and (b) all firm-year observations in states that did not introduce RTW laws during the sample period. We estimate the following baseline regression:

$$Y_{ijst} = \beta RTW_{st} + Controls_{it-1} + \gamma \Delta GSP_{st} + \psi_t + \rho_j + \phi_i + \epsilon_{ijst}, \tag{6}$$

where Y stands for the dependent variable of interest. The main dependent variables are investment (capx) scaled by assets, the growth rate of the number of employees (emp), as well as book leverage, defined as the sum of debt in current liabilities (dlc) and long-term debt (dltt) over assets. The subscripts stand for firm i, industry j, state s, and year t. RTW is a dummy variable that is set to 1 for all observations after the year that a state has passed RTW legislation. The vector $Controls$ contains firm-level characteristics including the log of assets ($log(at_t)$), Tobin's q ($\frac{at_t + equity_t - be_t - txbd_t}{at_t}$), cashflow ($\frac{dp_t + ib_t}{at_{t-1}}$), profitability ($\frac{oibdp_t}{at_{t-1}}$), and asset tangibility ($\frac{ppegt_t}{at_t}$). All the control variables are lagged by one period. ΔGSP is the growth rate of state-level real GDP, and η_i, ϕ_j, and ψ_t denote firm, industry, and year fixed effects, respectively. We double-cluster standard errors at the state-year level following the

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recommendation of Petersen (2008) for Compustat panel data.[9]

Table 5 presents the baseline regression results of estimating Equation (6). The dependent variables in Columns (1) to (3) are investment over assets, employment growth, and leverage. The coefficient of the RTW dummy is positive and significant at the 5% level for investment in Column (1), positive and significant at the 1% level for employment growth in Column (2), and negative and significant at the 1% level for leverage in Column (3). In the five years after RTW introduction, investment as a share of total assets is 0.64% higher. Employment growth is 1.66% stronger. Debt as a share of total assets declines, on average, by 2.82% in the same window. These results suggest that RTW adoption leads to more firm investment, a stronger hiring rate, and lower leverage.

The investment result is consistent with a long line of literature mentioned at the beginning of this section on the negative effect of unionization and union bargaining power on firms' tangible and intangible capital. RTW passage reduces unions' bargaining power, thus allowing firms to invest more. The leverage result is in line with the Matsa (2010) strategic debt hypothesis such that firms use leverage as a bargaining tool against unions. RTW is a negative shock to union power, thus lessening the need on firms' part to employ leverage as a strategic tool in contract negotiations.

The panel regression results in Table 5 demonstrate the treatment effect of the RTW law on firm outcome variables, assuming that the treatment effect starts at the time the law is introduced and lasts for five years after. The difference-in-differences setup does not provide any insight into the timing of the law changes in relation to when they actually impact firm decisions. We explore the lead-lag relation between the time RTW laws are enacted and the time when the effects of these laws are realized next.

We perform spline regressions to examine the timing of the effect of RTW introduction on firms. We assign yearly dummies to firm-year observations in the five-year window before and after each RTW introduction. A ΔRTW dummy is assigned to observations during the

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[9]In unreported results, the statistical significance of our findings does not change with clustering only at the state level.

year of implementation, and a $\Delta RTW^{<(-5)}$ dummy is assigned to all observations before the pre-RTW 5-year window. All observations in non-RTW states and observations in the year immediately before RTW introduction (ΔRTW^{-1}) are in the control group. Finally, the same control variables and fixed effects are employed as in Table 5. The specification for our spline regressions is the following:

$$
\begin{aligned}
Y_{ijst} &= \sum_{k=2}^{<5} \Phi_k \Delta RTW_{st}^{(-k)} + \beta \Delta RTW_{st} + \sum_{k=1}^{5} \Psi_k \Delta RTW_{st}^{(+k)} \\
&+ Controls_{it-1} + \lambda \Delta GSP_{st} + \psi_t + \rho_j + \phi_i + \epsilon_{ijst},
\end{aligned}
\tag{7}
$$

where Φ, β, and Ψ are coefficient loadings on the ΔRTW dummies. Notice that we omit ΔRTW^{-1} from the regression to serve as the benchmark, so all estimated coefficients are relative to the values in the year before RTW enactment. The regression specifications include different fixed effects, firm-level control variables, and state-level control variables. Robust standard errors with double clustering at the state and year level are used to calculate the t-statistics.

Table 6 presents the results of the spline regressions in which the dependent variables, in order, are: investment, employment growth, and leverage.[10] To ensure that the spline regressions are valid, we check the statistical significance of the coefficient loadings on the ΔRTW dummies before RTW laws are implemented. In Table 6, none of the estimated coefficients are statistically significant before treatment across all columns, suggesting that the parallel trend condition is not violated.

In Column (1), investment scaled by total assets is higher relative to the control group 3 years after RTW adoption. This is evident by the positive and significant coefficient loadings on the ΔRTW^{+3} dummy. In Column (2), the employment growth rate is also significantly higher in year 3 after RTW introduction. In Column (3) of Table 6, book leverage is, on

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[10]In Appendix Table A7, we show that the dynamic impact of RTW on our baseline results (investment, employees growth, and leverage) is preserved in the short sample between 1988 and 2016 to be consistent with the union contract sample.

average, significantly lower between years +1 and +4 after RTW introduction relative to the year immediately prior. The deleveraging in ΔRTW^{+4} is especially strong as leverage drops by 5.26%, and it is statistically significant at the 1% level. Overall, the implications of the spline regressions are consistent with Table 5: RTW adoption allows firms to invest more, hire more employees, and borrow less. However, the impact of RTW laws has an average delay of three to four years on these firm variables. This is likely caused by the fact that, recalling from Figure 1, union contracts are on average three years in length. The staggered nature of union contract renegotiations can result in the delay between RTW adoption and a treated firm making financing and hiring adjustments. We provide suggestive evidence for this explanation in Appendix A, Figures A3 and A4.

One challenging aspect of our study is the fact that not all firms in Compustat have a unionized workforce. Considering that the bargaining power mechanism we investigate relies on union contract negotiations, it would be ideal to focus on the subset of firms with high unionization rates. Unfortunately, we do not have access to firm-level unionization rates data. Instead, we construct a measure of firm-level labor intensity using the employees-to-assets ratio (Emp/A) as a proxy. Each year, we sort firm-year observations based on the employees-to-assets ratio into quartiles. We then label the observations in the top three quartiles as *high labor intensity* and the bottom quartile of observations as *low labor intensity*. We then perform the dynamic spline regression in Equation (7) on investment, employees growth, and leverage for only the high labor intensity firms.[11] The idea is that we should see an amplified response to RTW introduction since these firms are more likely to benefit from the shift in bargaining power due to the enactment of the law. Table 7 presents the regression results for labor-intensive firms analogous to the results for the full sample in Table 6.

Column (1) of Table 7 shows that investment as a share of total assets increases relative to the year prior to RTW adoption starting in year 1 (ΔRTW^{+1}), and the estimated coefficients

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[11]We also perform the analog of Table 5 to examine the impact of RTW on investment, employees growth rate, and leverage for these labor-intensive firms. The results are presented in Appendix Table A8. They are consistent with the spline results discussed here.

stay statistically significant for two additional years. Compared with the investment response in Table 6, Column (1), labor-intensive firms appear to raise their capital expenditure share earlier than the average firm (ΔRTW^{+3} for the latter). For the employees growth rate in Column (2), a comparison between Tables 6 and 7 shows that high labor intensity firms boost the hiring rate more over two years (ΔRTW^{+3} and ΔRTW^{+4}) than the full sample of firms. Finally, concerning leverage in Column (3), high and low labor intensity firms seem to behave in a similar fashion dynamically post-RTW. Overall, findings in Table 7 support the conjecture that the impact of RTW adoption is accentuated in firms with high labor intensity, which are more likely to have unionized workers.

Our firm-level evidence suggests that RTW adoption has a positive effect on firm investment and hiring decisions. At the same time, RTW introduction helps to alleviate the debt burden some firms bear in exchange for better bargaining position against organized labor. Moreover, investment and hiring rate outcomes are especially noticeable in firms that rely on high labor share.

5.3. Profitability and labor-to-assets ratio

Under our proposed economic framework, we also argue that—under certain assumptions—firm profitability and labor-to-capital ratio should both increase as bargaining power is shifted from unions to firms after RTW enactment. In the literature, Draca et al. (2011) document that increases in minimum wages significantly reduce firm profits. Therefore, we conjecture that RTW laws, which put downward pressure on union wage outcomes, can result in more profitable firms. However, the increase in profitability might be difficult to capture in the data as standard economic theory suggests that profitability is also a function of the level of competition the firm is faced with. In a perfectly competitive world, the effect on profitability will be zero in equilibrium as the price is set to the marginal cost of production. In reality, few markets are perfectly competitive, and firms under imperfect competition can set the price to be the marginal cost plus some markup. Similarly, holding the rental cost

of capital constant, lower wage implies an increase in the labor share relative to capital. However, as we see in Section 5.2, both the investment rate and the employees growth rate rise after RTW adoption so the numerator and denominator of the labor-to-capital ratio rise simultaneously, which can make any changes in the labor-to-capital ratio post-RTW hard to detect.

Given that the impact of RTW on firm investment and employees growth is stronger for labor-intensive firms, we also expect to see RTW laws to have an outsized effect on profitability in that subsample. Furthermore, Appendix Table A8, Column (2) shows that the magnitude of the increase in the employees growth rate (RTW dummy) is bigger in the subsample of high labor intensity firms in comparison with the estimated coefficient in Table 5, Column (2) for the full sample. At the same time, the point estimates of the effect of RTW on investment do not differ by much in Column (1) of these tables. Therefore, high labor intensity firms may be the appropriate sample to analyze the change in labor share. We investigate firm profitability and labor share using dynamic spline regressions for both the full sample and the high labor intensity subsample as before. We present the findings in Table 8. We measure the labor share by Emp/A.

Table 8 presents the spline regression results. The dependent variable in Column (1) is operating profitability, and the employees-to-assets ratio is used in Column (2). Both regressions are for the full sample of firms and contain firm-level controls and state-level GSP growth (not shown). Year, industry, and firm fixed effects are also included. As before, the year before RTW introduction serves as the benchmark. Next, we repeat the exercise on the same variables but conduct the regressions for only labor-intensive firms. The spline coefficients are shown in Columns (3) and (4) in Table 8. In all four cases, the parallel trend assumption holds, as there is no noticeable pre-trend. For the full sample of firms, RTW adoption has no effect on profitability or on the labor-to-assets ratio because none of the estimated coefficients on the post-RTW dummies are statistically significant. On the other hand, in Columns (3) and (4), RTW enactment has a positive and significant

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effect on profitability and labor share for labor-intensive firms in years +5 and +4 post-RTW, respectively. For a representative firm in the high labor intensity sample, operating profitability is almost 3% higher in the fifth year (ΔRTW^{+5}) after RTW relative to the year immediately prior to adoption.

The results shown in Table 8 demonstrate the fact that RTW legislation has a positive and significant impact on firm profitability and on the labor share if we focus on firms where labor input is more essential. This is consistent with our finding that RTW negatively affects the wage outcome of labor contract negotiations. As wage growth slows down, firms become more profitable and increase their labor share.

5.4. Union bargaining power

In our conceptual framework, the main mechanism through which RTW laws affect wage growth is union strength. Our hypothesis is that RTW laws reduce union strength, or union bargaining power, so unions are less able to negotiate large wage increases for their members. Our assumption is that there is a reduction in union bargaining power after the adoption of RTW laws. Unfortunately, union bargaining power is not directly observable. Therefore, we develop two indirect tests to validate the union strength mechanism in our conceptual framework: One based on the number of CBAs for each state-year, and the other based on union membership rates at the state-year level.

To calculate the number of CBAs, we use the same Bloomberg BNA data as in our previous tests, and we count the number of observations for each state-year. We then use this as the dependent variable and regress it on an RTW dummy, similar to Equation (5). Our regression controls for GSP growth as well as year and state fixed effects. Standard errors are clustered at the state level. Since the dependent variable is measured in levels and not in changes, we use a permanent RTW dummy variable. We expect the coefficient of the RTW dummy to be negative as the strength of unions at some firms is reduced so severely that the unions are no longer able to negotiate a contract with the firms, and the

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firms effectively become de-unionized.

The results of this difference-in-differences estimation are presented in Table 9. Column (1) contains no fixed effects, while Columns (2) and (3) add year and state fixed effects, respectively. In the most conservative specification, in Column (3), the coefficient of the RTW dummy is negative and significant at the 5% level. The point estimate is -7.75, which is quite large compared to the (unreported) average number of CBAs per state-year of 17.9. This suggests that, compared to the unconditional average, RTW laws reduce the number of CBAs by almost a half.

This result is interesting for two reasons. First, it confirms our hypothesis that RTW laws reduce wage growth through their effect on union bargaining power. Second, it suggests that the treatment effects in Tables 3–4 underestimate the true effect of RTW laws on wage growth. This is because it is quite plausible that the reduction in wage growth after the passage of an RTW law is strongest in those firms that become de-unionized as a result of the law. However, since we can only observe CBAs at those firms that remain unionized, the estimated treatment effect will be biased towards zero. In other words, RTW laws might reduce wage growth even more than our estimates suggest.[12]

Our second test of the union mechanism is based on data on union membership and coverage at the state-year level. We define a new variable, *UnionCovMem*, as the difference between the union coverage rate and the union membership rate, both measured at the state-year level, scaled by the union membership rate.[13]

The variable *UnionCovMem* measures the gap between the fraction of workers covered by a CBA and the fraction of workers who are union members. This gap is typically positive because workers who are not members of a union are often still covered by the CBA negotiated by the union. An intuitive interpretation of this variable is that it measures the severity of the free-rider problem within a unionized firm. According to our story, the

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[12]We would like to thank Gerard Hoberg for this suggestion.

[13]We divide by the membership rate to control for the fact that the public sector contains more union workers relative to the private sector. Appendix Table A9 contains a robustness test where we do not scale by the union membership rate.

free-rider problem will become more severe after the introduction of an RTW law. If this happens, then we expect union membership rates to fall more than coverage rates, which should increase the value of *UnionCovMem*.

To calculate the variable *UnionCovMem*, we use the data from unionstats.com, which is explained in detail in Hirsch and MacPherson (2003).[14] We estimate spline regressions analogous to Table 3. The dependent variable is *UnionCovMem*, and the main explanatory variables are a sequence of RTW dummies: $\Delta RTW^{<(-2)}$, ΔRTW^{-2}, ΔRTW, ΔRTW^{+1}, ΔRTW^{+2}, and ΔRTW^{+3}. According to our proposed mechanism, RTW laws should increase the severity of the free-rider problem within unionized firms, so we expect that the RTW dummies for the years after a law's passage will have positive coefficients. Similar to Table 3, we drop observations in RTW states that are more than three years after the law's passage. The regressions control for GSP growth and year and state fixed effects. Standard errors are clustered at the state level.

Table 10 presents the estimation results of the spline regressions. Columns (1), (2), and (3) contain the results for the total workforce, private sector unions, and public sector unions, respectively. In all columns, the coefficients for $\Delta RTW^{<(-2)}$ and ΔRTW^{-2} are statistically insignificant. This is important because the parallel trends assumption would be violated if they were. However, the most interesting finding is that the coefficients for the years after the law's passage are positive and statistically significant in all columns.

The exact timing of the effect varies a bit across the different columns. For the total workforce, the effect is mostly concentrated in years +1 and +2 after the law. For private sector unions, the effect is only marginally significant in years +1 and +2, but it is highly significant in year +3. Finally, for public unions, the effect on the free-rider problem is only significant in year +1. Compared to Table 3, the results become significant approximately one year later. One possible explanation for this is that it takes some time until union

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[14]The state-level union membership data go back to 1983, which allows us to expand the sample period to 1983–2016. This has the benefit of adding Idaho, which introduced an RTW in 1986, to the list of treated states (see Table 1).

members actually leave their union, which is why the results in Table 10 show up with a lag. Wage negotiations, however, might be affected sooner than that, for example because both the firm and the union anticipate in year 0 that the union will lose a lot of members in years +1, +2, and so on. This might explain why the results are instantaneous in Table 3.

To judge the economic significance of these coefficients, we compare them to the unconditional averages of the dependent variables. The (untabulated) averages for the total workforce, the private sector, and the public sector are 11.9%, 10.2%, and 15.5%, respectively. The point estimates for years +1 and +2 for the overall economy are 3 pp and 2.6 pp, respectively, which seems substantial, compared to the mean of 11.9%.

To conclude, we have shown that RTW introductions substantially reduce the number of CBAs, and they increase the severity of the free-rider problem between workers. Both of these results are consistent with our proposed mechanism: RTW laws reduce wage growth by reducing union strength. Weaker unions, having less bargaining power, are less able to negotiate high wage growth rates.

6. Additional results

6.1. Payout policy and cash holdings

In this section, we examine various additional effects of RTW laws that do not directly follow from our conceptual framework in Section 2 but are nevertheless important to understand the positive and negative effects of these laws.

Table 11, Columns (1) and (2) present the spline regressions around RTW adoptions for two payout variables: dividends (dv) and share repurchases (prstkc), both scaled by total assets. The regression specification follows that of Equation (7) with controls of log assets, Tobin's q, cashflow, and state GSP growth. Again, the year immediately prior to RTW adoption serves as the benchmark such that all estimated coefficients are relative to the level in year −1. We use our full sample of Compustat observations. Results on dividends are

shown in Column (1). None of the pre-RTW dummy variables are statistically significant, which suggests that the parallel trends assumption is not violated. In the post-RTW window, Div/A loads positively on ΔRTW^{+3}, and the coefficient is statistically significant at the 5% level.[15] The timing of this finding is interesting because, in line with Inv/A and EmpGr in Table 6, the positive impact of RTW materializes three years after its introduction. The point estimate suggests that dividends as a share of total assets are 0.32% higher in year 3 after RTW adoption relative to the value immediately prior to the adoption. In Column (2), the regression results are mixed for repurchases as evidenced by the parallel trends violation in ΔRTW^{-4}. All things considered, the findings in Table 11 imply equity holders receive higher payouts after RTW introduction through dividends.

Column (3) of Table 11 documents the same spline regression results for cash and short-term investments (che) divided by assets. We see immediately that none of the estimated coefficients are statistically significant at the 10% level. These results suggest that the average firm does not change its cash holdings after the introduction of RTW laws.

6.2. Executive compensation

While our simple conceptual framework in Section 2 does not explicitly predict an effect of RTW laws on executive compensation, there are reasons to expect such an effect. One reason is that we show in Section 5 that firms invest more in physical capital and hire more workers following RTW passage. We know from the existing literature that firm size is one of the main determinants of executive compensation (e.g., Gabaix and Landier, 2008). Therefore, it is plausible to expect that RTW laws lead to an increase in executive pay.

We merge our existing Compustat panel data set with the ExecuComp database, which results in a shortened sample period of 1992–2016. We focus on the compensation of CEOs, and construct four dependent variables: base salary (Salary), the value of options granted during the fiscal year (Options), the value of stocks granted during the fiscal year (Stocks),

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[15]In Appendix Table A10, we show that the dividend payout increase after RTW passage is strengthened for high labor intensity firms.

and other compensation (OthComp), which includes perquisites and contributions to pension plans, among other things. All variables are in logs and are adjusted for inflation to reflect 2016 dollars. The details of our sample construction can be found in Appendix C.

Table 12 presents our results using the ExecuComp sample. Column (1) shows that RTW laws have a positive effect on the base salary of CEOs, starting three years after the introduction of the law. The coefficients in years +3 and +5 are significant at the 5% and 1% levels, respectively. The coefficient of $RTW^{<(-5)}$ is also significant, which suggests a potential parallel trends violation. However, this violation is arguably minor, given that it occurs at least six years prior to the passage of the law. In Column (2) we see that there is a positive effect on option-based compensation, which becomes significant at the 1% level five years after the law's introduction. We interpret this as suggestive evidence because the coefficient in year +3 is negative and marginally significant. Interestingly, there is no significant effect on stock compensation, as shown in Column (3). Finally, we see in Column (4) that there is a positive effect on other compensation, which is significant at the 5% level in year +5. We add the caveat that there is a potential parallel trends violation, although it occurs five years prior to RTW passage.[16]

Taken together, the results in Table 12 are consistent with the view that RTW laws increase CEO compensation. We interpret this as suggestive evidence, especially since our analysis is limited in scope. Keeping these caveats in mind, it is interesting to observe how these results differ from our results from collective bargaining agreements in Section 5. While RTW laws reduce wage growth for unionized workers, who are usually blue-collar employees, our results on CEO pay are consistent with a positive effect on executives.

6.3. Firm-provided unemployment insurance

To the extent that workers are more risk-averse, or firms have better risk-absorbing capacity, it is reasonable to see firms as providers of insurance such that they shield their

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[16]In Appendix Table A11, we show that the positive impact of RTW on base salary and other compensation is reinforced for high labor intensity firms as the parallel trends violations disappear.

workers from negative shocks in exchange for lower wages. This can be rationalized in an implicit contract setting as theorized by Baily (1974) and Azariadis (2015). More recently, empirical work by Sraer and Thesmar (2007) and Ellul et al. (2018) verifies that, indeed, firms provide unemployment insurance to their workers, especially at family-owned firms.

Following Ellul et al. (2018), we study the impact of RTW on unemployment insurance provision by focusing on the sensitivity of firm-level hiring to industry-level shocks in a difference-in-differences setting. We regress firm i's employment growth on industry sales growth (excluding firm i's own sales) and the interaction between industry sales growth and the RTW dummy in the following regression model:

$$
\begin{aligned}
EmpGr_{ijst} \;=\; & \beta RTW_{st} + \omega IndSalesGrowthEx_{it} + \eta RTW_{st} \times IndSalesGrowthEx_{it} \quad (8) \\
& + Controls_{it-1} + \gamma \Delta GSP_{st} + \psi_t + \rho_j + \phi_i + \epsilon_{ijst},
\end{aligned}
$$

where $IndSalesGrowthEx_{it}$ is the industry sales growth calculated based on total sales excluding firm i's own sales.

We hypothesize that if RTW legislation indeed decreases the bargaining power of labor relative to employers, firms might be more willing to hire and fire employees due to industry-wide growth shocks after RTW introduction relative to before RTW. So, we expect RTW adoption to elevate the sensitivity of employment growth rate with respect to industry sales shocks (the interaction term should have a positive and significant coefficient loading, η).

Table 13 presents our finding on the pass-through from industry sales growth to firm employment growth pre- and post-RTW. Column (1) is the baseline regression shown in Equation (8), and Column (2) adds State Unemployment Benefits to the list of control variables, following Agrawal and Matsa (2013). Two observations are worth pointing out. First, the coefficient loading ω on $IndSalesGrowthEx_{it}$ is positive and significant. The point estimate implies that a 1% drop in industry sales growth leads to a 53 bps decline in employment growth, which confirms the result in Ellul et al. (2018). Second, the coefficient

loading on the interaction term, η, is positive and highly significant.[17] This means industry sales growth pass-through to the firm-level hiring rate is even stronger after RTW adoption, which is consistent with our hypothesis. RTW depresses the bargaining of workers, which in turn lowers the amount of insurance provided by firms to their employees.

7. Conclusion

Five states in the US have enacted RTW laws since 2010 and more than half of the states have RTW laws. We hypothesize that the passage of these RTW laws has a negative impact on union bargaining power and thus has a negative impact on the wage growth of unionized workers in those states. We find that the introduction of RTW laws reduces wage growth for workers covered by CBAs. While the strength of unions is not easily measurable, we provide indirect empirical evidence that is consistent with declining union strength. These laws reduce the number of existing CBAs and increase the severity of the free-rider problem between workers at unionized firms. This suggests that the effect on wage growth occurs through the union bargaining power channel.

As predicted by a canonical theory of the firm augmented with an exogenous bargaining power of labor, after the passage of an RTW law, firms headquartered in that state increase investment and employment, but reduce their use of strategic leverage. These actions are all consistent with a shift in bargaining power from workers to firms. Consistent with recent conjectures by Summers (2017) and Krugman (2017), our CBA-level and firm-level evidence suggests that the decline of unions, and the corresponding decline in workers' bargaining power, has contributed to a decline in wage growth of unionized workers in states that have RTW laws.

We are cautious in interpreting the welfare effects or the policy implications of our findings. Our results cannot be interpreted in a way that RTW laws reduce aggregate welfare.

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[17]In Appendix Table A12, we show that the RTW impact on firm insurance provision is slightly stronger for high labor intensity firms.

On the one hand, our estimates suggest that the effect of RTW laws on the welfare of those workers who are already employed is likely negative. This comes both from a reduction in their wages and from a potential increase in income inequality since workers covered by collective bargaining are more likely to work in middle-income occupations (e.g., Card et al., 2004). On the other hand, there are also positive effects of RTW on aggregate welfare. For example, Holmes (1998) shows that the introduction of these laws creates higher employment, especially in manufacturing.

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Table 1: Summary statistics of state right-to-work laws in the US. This is a list of states in the US that have passed right-to-work legislation either by the state constitution or by a statute. *State* is the FIPS code of each state used by the US Census Bureau. *STUSAB* is the state abbreviation. *Name* is the name of the state. *Year RTW* is the year during which the legislation became effective. These data are hand-collected by reading either constitution amendments or labor codes.

State	STUSAB	Name	Year RTW	State	STUSAB	Name	Year RTW
1	AL	Alabama	1953	30	MT	Montana	
2	AK	Alaska		31	NE	Nebraska	1947
4	AZ	Arizona	1947	32	NV	Nevada	1952
5	AR	Arkansas	1947	33	NH	New Hampshire	
6	CA	California		34	NJ	New Jersey	
8	CO	Colorado		35	NM	New Mexico	
9	CT	Connecticut		36	NY	New York	
10	DE	Delaware		37	NC	North Carolina	1947
11	DC	D.C.		38	ND	North Dakota	1948
12	FL	Florida	1943	39	OH	Ohio	
13	GA	Georgia	1947	40	OK	Oklahoma	2001
15	HI	Hawaii		41	OR	Oregon	
16	ID	Idaho	1986	42	PA	Pennsylvania	
17	IL	Illinois		44	RI	Rhode Island	
18	IN	Indiana	2012	45	SC	South Carolina	1954
19	IA	Iowa	1947	46	SD	South Dakota	1947
20	KS	Kansas	1958	47	TN	Tennessee	1947
21	KY	Kentucky	2017	48	TX	Texas	1947
22	LA	Louisiana	1976	49	UT	Utah	1955
23	ME	Maine		50	VT	Vermont	
24	MD	Maryland		51	VA	Virginia	1947
25	MA	Massachusetts		53	WA	Washington	
26	MI	Michigan	2013	54	WV	West Virginia	2016
27	MN	Minnesota		55	WI	Wisconsin	2015
28	MS	Mississippi	1960	56	WY	Wyoming	1963
29	MO	Missouri					

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Table 2: Summary statistics for change in log wage. This table presents summary statistics for log wage growth in the Bloomberg BNA data. The first row is the entire sample. The second rows contains collective bargaining agreements (CBAs) negotiated in a non-right-to-work (non-RTW) state, as well as contracts from RTW states, but prior to the introduction of the law. The third row contains CBAs negotiated in a RTW state, after the passage of the law. The fourth and fifth rows distinguish CBAs negotiated at a public sector establishment from those negotiated at a private sector establishment. Each count in Column (1) represents a contract agreement. Column (3) is the standard deviation. Column (5) is the 25th percentile. Column (6) is the 50th percentile. Column (7) is the 75th percentile.

	(1) count	(2) mean	(3) sd	(4) min	(5) p25	(6) p50	(7) p75	(8) max
Total Sample	15125	0.029	0.029	-0.223	0.015	0.027	0.037	0.635
Non-RTW Obs.	14827	0.029	0.028	-0.223	0.015	0.028	0.037	0.565
RTW Obs.	298	0.018	0.040	-0.046	0.000	0.015	0.025	0.635
Private Sector	9604	0.033	0.032	-0.223	0.020	0.030	0.039	0.565
Public Sector	5521	0.022	0.021	-0.105	0.010	0.021	0.030	0.635

Table 3: The effect of RTW laws on wage growth. This table presents estimation results for the difference-in-differences specification in Equation (5). The unit of observation is a collective bargaining agreement (CBA). The sample period is 1988–2016. The dependent variable is the change in the log of wages. The main explanatory variable is ΔRTW, a dummy that indicates the year of the introduction of a right-to-work (RTW) law. Other explanatory variables are dummies that indicate the years before and after the introduction of a RTW law. ΔRTW^{+2} denotes two years after the introduction of the law, ΔRTW^{+1} denotes one year after the law, ΔRTW^{-2} is two years before the introduction, and $\Delta RTW^{<(-2)}$ stands for all years before then. An additional control variable is the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

| | Dependent variable: | | | |
| | $\Delta log(w)$ | | | |
	(1)	(2)	(3)	(4)
$\Delta RTW^{<(-2)}$				0.003
				(0.002)
ΔRTW^{-2}				−0.002
				(0.001)
ΔRTW	−0.011***	−0.010***	−0.006***	−0.003***
	(0.001)	(0.001)	(0.001)	(0.001)
ΔRTW^{+1}				−0.001
				(0.002)
ΔRTW^{+2}				−0.002
				(0.003)
GSP growth	0.088***	0.075***	0.059**	0.055**
	(0.030)	(0.028)	(0.025)	(0.026)
Year FE	Yes	Yes	Yes	Yes
Industry FE		Yes	Yes	Yes
State FE			Yes	Yes
Observations	15,125	15,125	15,125	15,026
Adjusted R^2	0.151	0.194	0.202	0.210

Note: *p<0.1; **p<0.05; ***p<0.01

Table 4: The permanent effects of RTW laws on wage growth. This table presents estimation results for the difference-in-differences specification in Equation (5). The unit of observation is a collective bargaining agreement (CBA). The sample period is 1988–2016. The dependent variable is the change in the log of wages. The main explanatory variable is RTW, a dummy that takes a value of one in the year of the introduction of a right-to-work (RTW) law and in all subsequent years. An additional control variable is the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

| | *Dependent variable:* | | | |
| | $\Delta log(w)$ | | | |
	(1)	(2)	(3)	(4)
RTW	-0.011**	-0.005	-0.004	-0.002
	(0.005)	(0.005)	(0.005)	(0.003)
GSP growth	0.107***	0.089***	0.075***	0.059**
	(0.027)	(0.031)	(0.028)	(0.025)
Constant	0.027***			
	(0.001)			
Year FE		*Yes*	*Yes*	*Yes*
Industry FE			*Yes*	*Yes*
State FE				*Yes*
Observations	15,125	15,125	15,125	15,125
Adjusted R^2	0.013	0.151	0.193	0.202
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 5: The effect of RTW laws on firm investment, employment growth, and leverage. This table reports the coefficient estimates of panel regressions by pooling all firm-year observations from 1950 to 2016. The RTW law indicator (RTW) is the main explanatory variable. The dependent variable in Column (1) is investment, defined as capital expenditure (CAPX) divided by lagged assets. The dependent variable in Column (2) is employees growth, defined as employees (emp) divided by lagged employees minus 1. The dependent variable in Column (3) is book leverage, defined as debt in current liabilities plus long-term debt (dlc + dltt) divided by lagged assets. All regressions include controls and year, industry as well as firm fixed effects. State-level year-over-year real GSP growth ($GSP\ Growth$) is the only control variable not measured at the firm level. Robust standard errors with double clustering at the state and year level are used in reporting the t-statistics in parentheses.

	(1) Inv/A	(2) EmpGr	(3) Debt/A
RTW	0.00637**	0.0166***	-0.0282***
	(2.05)	(2.95)	(-6.11)
LogAsset	-0.00712***	-0.0606***	0.0338***
	(-13.25)	(-14.83)	(14.91)
Tobin Q	0.00284***	0.0123***	-0.000580
	(6.74)	(8.07)	(-1.03)
Cashflow	0.00362**	0.0148**	
	(2.26)	(2.61)	
GSP Growth	0.0801***	0.175*	0.0166
	(3.41)	(1.84)	(0.42)
Profitability			-0.0953***
			(-7.62)
Tangiblity			0.0347***
			(3.03)
Constant	0.0907***	0.357***	0.0288**
	(30.62)	(16.12)	(2.06)
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Observations	77684	77684	77684
Adjusted R^2	0.558	0.133	0.648

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Dynamic effect of RTW laws on firm investment, employment growth, and leverage. This table reports the coefficient estimates of spline regressions on firm policies. The explanatory variables are dummies denoting each year in the 11-year (± 5) window around the RTW adoption plus one dummy denoting if a particular observation is more than five years before the enactment of the law ($\Delta RTW^{<(-5)}$). Observations in the one year immediately before the RTW law implementation do not have a RTW dummy and serve as the benchmark. Treated observations beyond ΔRTW^{+5} are omitted. The dependent variable in Column (1) is investment, defined as capital expenditure (CAPX) divided by lagged assets. The dependent variable in Column (2) is employment growth. The dependent variable in Column (3) is book leverage, defined as debt in current liabilities plus long-term debt (dlc + dltt) divided by lagged assets. All regressions include controls (not shown) and year, industry as well as firm fixed effects. Robust standard errors with double clustering at the state and year level are used in reporting the t-statistics in parentheses.

	(1)	(2)	(3)
	Inv/A	EmpGr	Debt/A
$\Delta RTW^{<(-5)}$	0.00223	0.0215	0.00441
	(0.52)	(1.09)	(0.38)
ΔRTW^{-5}	0.00102	-0.00600	0.00273
	(0.42)	(-0.21)	(0.15)
ΔRTW^{-4}	0.00330	0.0145	0.00255
	(1.20)	(0.46)	(0.15)
ΔRTW^{-3}	0.000591	0.0353	0.00398
	(0.09)	(1.38)	(0.33)
ΔRTW^{-2}	-0.00132	0.0315	-0.00404
	(-0.37)	(0.93)	(-0.44)
ΔRTW	0.00265	0.0382	-0.00922
	(0.76)	(1.66)	(-0.97)
ΔRTW^{+1}	0.00415	0.0266	-0.0194*
	(1.40)	(0.87)	(-1.91)
ΔRTW^{+2}	0.00234	0.0343	-0.0299***
	(0.41)	(1.22)	(-3.06)
ΔRTW^{+3}	0.0175**	0.0726***	-0.0207*
	(2.30)	(3.52)	(-1.97)
ΔRTW^{+4}	0.00573	0.0330	-0.0526***
	(0.76)	(1.55)	(-2.83)
ΔRTW^{+5}	0.0234	-0.000178	-0.0269
	(1.45)	(-0.01)	(-1.42)
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Observations	77684	77684	77684
Adjusted R^2	0.559	0.133	0.648

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Dynamic effect of RTW laws on firm investment, employment growth, and leverage - labor intensive firms only. This table reports the coefficient estimates of spline regressions on firm policies. The explanatory variables are dummies denoting each year in the 11-year (± 5) window around the RTW adoption plus one dummy denoting if a particular observation is more than five years before the enactment of the law ($\Delta RTW^{<(-5)}$). Observations in the one year immediately before the RTW law implementation do not have a RTW dummy and serve as the benchmark. Treated observations beyond ΔRTW^{+5} are omitted. The dependent variable in Column (1) is investment, defined as capital expenditure (CAPX) divided by lagged assets. The dependent variable in Column (2) is employment growth. The dependent variable in Column (3) is book leverage, defined as debt in current liabilities plus long-term debt (dlc + dltt) divided by lagged assets. All regressions include controls (not shown) and year, industry as well as firm fixed effects. Robust standard errors with double clustering at the state and year level are used in reporting the t-statistics in parentheses.

	(1)	(2)	(3)
	Inv/A	EmpGr	Debt/A
$\Delta RTW^{<(-5)}$	0.00438	0.0125	-0.000208
	(1.21)	(0.68)	(-0.01)
ΔRTW^{-5}	0.00258	-0.0307	-0.00387
	(0.58)	(-1.11)	(-0.18)
ΔRTW^{-4}	0.00624	-0.000282	0.00323
	(1.26)	(-0.01)	(0.19)
ΔRTW^{-3}	0.00100	0.0328	-0.00592
	(0.18)	(1.26)	(-0.33)
ΔRTW^{-2}	0.00318	0.00256	-0.00995
	(0.91)	(0.09)	(-0.64)
ΔRTW	0.00478	0.0178	-0.0110
	(1.66)	(0.96)	(-0.86)
ΔRTW^{+1}	0.00748**	-0.00305	-0.0241
	(2.62)	(-0.21)	(-1.62)
ΔRTW^{+2}	0.00704**	0.0337	-0.0341**
	(2.07)	(1.05)	(-2.69)
ΔRTW^{+3}	0.0112***	0.0573***	-0.0236*
	(2.75)	(2.99)	(-1.85)
ΔRTW^{+4}	0.00300	0.0608**	-0.0650**
	(0.74)	(2.17)	(-2.68)
ΔRTW^{+5}	0.0174*	0.0146	-0.0551***
	(1.88)	(0.53)	(-2.77)
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Observations	58464	58464	58464
Adjusted R^2	0.527	0.151	0.648

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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Table 8: Dynamic effect of RTW laws on firm profitability and the labor-to-assets ratio. This table reports the coefficient estimates of spline regressions for firm profitability and the labor-to-assets ratio. The explanatory variables are dummies denoting each year in the 11-year (± 5) window around the RTW adoption plus one dummy denoting if a particular observation is more than five years before the enactment of the law ($\Delta RTW^{<(-5)}$). Observations in the one year immediately before the RTW law implementation do not have a RTW dummy and serve as the benchmark. Treated observations beyond ΔRTW^{+5} are omitted. The dependent variable in Column (1) is profitability, defined as operating income (oibdp) divided by lagged assets. The dependent variable in Column (2) is the labor-to-assets ratio, defined as emp divided by total assets. Columns (3) and (4) repeat the same dependent variables as Columns (1) and (2) but for labor-intensive firms only, after dropping the bottom quartile of observations annually based on the labor-to-assets ratio. All regressions include controls (not shown) and year, industry as well as firm fixed effects. Robust standard errors with double clustering at the state and year level are used in reporting the t-statistics in parentheses.

	Full Sample		Labor Intensive	
	(1)	(2)	(3)	(4)
	OI/A	Emp/A	OI/A	Emp/A
$\Delta RTW^{<(-5)}$	0.00997	0.000342	0.0170	0.000110
	(0.52)	(0.90)	(1.60)	(0.21)
ΔRTW^{-5}	0.00555	0.000395	0.00241	0.000197
	(0.44)	(1.31)	(0.24)	(0.68)
ΔRTW^{-4}	-0.000931	0.000231	0.00195	-0.0000190
	(-0.10)	(0.67)	(0.31)	(-0.05)
ΔRTW^{-3}	-0.0131	0.0000577	0.00302	0.0000500
	(-0.72)	(0.16)	(0.27)	(0.11)
ΔRTW^{-2}	-0.00826	0.000299	-0.00377	0.000357
	(-0.80)	(0.85)	(-0.39)	(0.79)
ΔRTW	0.0112	0.000122	0.0126	0.000134
	(1.32)	(0.40)	(1.29)	(0.38)
ΔRTW^{+1}	0.000625	0.0000609	0.0154	0.0000862
	(0.04)	(0.29)	(1.25)	(0.33)
ΔRTW^{+2}	0.00126	0.000140	0.0126	0.000285
	(0.08)	(0.59)	(0.76)	(0.79)
ΔRTW^{+3}	-0.000105	0.000681	0.00710	0.000973
	(-0.01)	(1.09)	(0.29)	(1.34)
ΔRTW^{+4}	0.00375	0.000205	0.0169	0.000452**
	(0.23)	(1.41)	(0.79)	(2.70)
ΔRTW^{+5}	0.0138	0.000378	0.0298**	0.000805
	(0.87)	(0.82)	(2.63)	(1.24)
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Observations	77684	77684	58464	58464
Adjusted R^2	0.685	0.885	0.697	0.883

* p<0.10, ** p<0.05, *** p<0.01

Table 9: The effect of RTW on the number of CBAs. This table presents estimation results for a difference-in-differences regression, using the sample of collective bargaining agreements (CBAs) from Bloomberg BNA. The unit of observation is a state-year. The sample period is 1988–2016. The dependent variable is the number of CBAs per state-year. The main explanatory variable is RTW, a dummy that takes a value of one in the year of the introduction of a right-to-work (RTW) law and in all subsequent years. An additional control variable is the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

| | *Dependent variable:* | | |
| | Number of CBAs | | |
	(1)	(2)	(3)
RTW	−7.169	−12.285	−7.754**
	(6.570)	(8.102)	(3.544)
GSP growth	−45.339	−0.616	9.656
	(36.484)	(29.789)	(24.616)
Constant	19.257***		
	(3.504)		
Year FE		*Yes*	*Yes*
State FE			*Yes*
Observations	870	870	870
Adjusted R^2	0.004	0.114	0.736
Note:		*p<0.1; **p<0.05; ***p<0.01

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Table 10: The effect of RTW on unions. This table shows spline regressions used to estimate the timing of the effect of RTW laws on unions. The sample is based on union membership data from unionstats.com, and the unit of observation is a state-year. The sample period is 1983–2016. The dependent variable is *UnionCovMem*, defined as the difference between the union coverage rate and the union membership rate, divided by the membership rate. Column (1) is based on the entire workforce, Column (2) focuses on private sector unions, and Column (3) is based on the public sector. The main explanatory variables are a set of dummies that indicate when a right-to-work (RTW) law is introduced. ΔRTW^{+3} denotes three years after the introduction of the law, ΔRTW^{+2} denotes two years after the law, ΔRTW^{+1} denotes one year after the law, ΔRTW is the year of the introduction, ΔRTW^{-2} is two years before the introduction, and $\Delta RTW^{<(-2)}$ stands for all years before then. An additional control variable is the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:		
	UnionCovMem		
	Total	Private	Public
	(1)	(2)	(3)
$\Delta RTW^{<(-2)}$	−0.003	0.018	0.004
	(0.012)	(0.014)	(0.019)
ΔRTW^{-2}	0.016	0.017	0.019
	(0.015)	(0.020)	(0.022)
ΔRTW	0.009	0.002	0.040
	(0.014)	(0.011)	(0.027)
ΔRTW^{+1}	0.030***	0.022*	0.065***
	(0.009)	(0.013)	(0.016)
ΔRTW^{+2}	0.026***	0.052*	0.014
	(0.008)	(0.027)	(0.048)
ΔRTW^{+3}	0.011	0.025***	−0.003
	(0.008)	(0.009)	(0.030)
GSP growth	−0.013	0.005	−0.048
	(0.033)	(0.069)	(0.062)
Year FE	*Yes*	*Yes*	*Yes*
State FE	*Yes*	*Yes*	*Yes*
Observations	1,014	1,014	1,014
Adjusted R^2	0.734	0.472	0.764
Note:		*p<0.1; **p<0.05; ***p<0.01	

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Table 11: Dynamic effect of RTW laws on firm dividends, stock repurchases, and cash holdings. This table reports the coefficient estimates of spline regressions for firm policies. The explanatory variables are dummies denoting each year in the 11-year (± 5) window around the RTW adoption plus one dummy denoting if a particular observation is more than five years before the enactment of the law ($\Delta RTW^{<(-5)}$). Observations in the one year immediately before the RTW law implementation do not have a RTW dummy and serve as the benchmark. Treated observations beyond ΔRTW^{+5} are omitted. The dependent variable in Column (1) is dividends (dv) divided by lagged assets. The dependent variable in Column (2) is repurchases (prstkc) divided by lagged assets. The dependent variable in Column (3) is cash and short-term investments (che) divided by total assets. All regressions include controls (not shown) and year, industry as well as firm fixed effects. Robust standard errors with double clustering at the state and year level are used in reporting the t-statistics in parentheses.

	(1) Div/A	(2) Repur/A	(3) Cash/A
$\Delta RTW^{<(-5)}$	0.0000971	0.000766	-0.00178
	(0.05)	(0.14)	(-0.24)
ΔRTW^{-5}	0.000790	-0.00174	0.00443
	(0.42)	(-0.28)	(0.46)
ΔRTW^{-4}	-0.0000749	-0.0107**	0.00115
	(-0.05)	(-2.56)	(0.09)
ΔRTW^{-3}	-0.000736	-0.00816	0.00671
	(-0.34)	(-1.64)	(1.17)
ΔRTW^{-2}	0.000316	-0.00340	0.00241
	(0.15)	(-0.63)	(0.43)
ΔRTW	-0.00106	-0.00263	0.00358
	(-0.59)	(-0.68)	(0.42)
ΔRTW^{+1}	-0.000131	-0.00894*	0.00339
	(-0.07)	(-1.92)	(0.40)
ΔRTW^{+2}	0.000384	-0.00789	0.0192
	(0.12)	(-1.28)	(1.11)
ΔRTW^{+3}	0.00319**	-0.00814	-0.00583
	(2.18)	(-1.07)	(-0.46)
ΔRTW^{+4}	0.00212	-0.00198	-0.00512
	(0.98)	(-0.24)	(-0.40)
ΔRTW^{+5}	0.000814	-0.00827	-0.0273
	(0.46)	(-1.64)	(-1.58)
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Observations	77684	69317	77684
Adjusted R^2	0.611	0.284	0.755

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 12: The effect of RTW laws on executive compensation. This table presents estimation results for Equation (7). The sample period is 1992–2016. The explanatory variables are dummies denoting each year in the 11-year (± 5) window around the RTW adoption plus one dummy denoting if a particular observation is more than five years before the enactment of the law ($\Delta RTW^{<(-5)}$). Observations in the one year immediately before the RTW law implementation do not have a RTW dummy and serve as the benchmark. The dependent variables are various measures of CEO compensation: base salary (Salary), options granted (Options), stocks granted (Stocks), and other compensation (OthComp). All dependent variables are in logs of thousand dollars. Control variables that are not displayed are lagged cash flow over assets, lagged Tobin's Q, lagged log of assets, and the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered by state and year.

	Salary	Options	Stocks	OthComp
	(1)	(2)	(3)	(4)
$\Delta RTW^{<(-5)}$	-0.116^{***}	-0.066	-0.209	-0.099
	(0.030)	(0.498)	(0.480)	(0.097)
ΔRTW^{-5}	0.005	0.636	-0.086	-0.218^{**}
	(0.046)	(0.403)	(0.516)	(0.111)
ΔRTW^{-4}	-0.043	0.481	-0.278	-0.125
	(0.084)	(0.383)	(0.411)	(0.134)
ΔRTW^{-3}	-0.037	-0.135	-0.389	-0.055
	(0.070)	(0.400)	(0.426)	(0.108)
ΔRTW^{-2}	-0.001	-0.070	-0.628	-0.041
	(0.052)	(0.317)	(0.446)	(0.081)
ΔRTW	-0.004	0.314	-0.025	0.119
	(0.028)	(0.610)	(0.343)	(0.075)
ΔRTW^{+1}	-0.008	-0.118	-0.200	0.178
	(0.025)	(0.543)	(0.448)	(0.137)
ΔRTW^{+2}	0.060	-0.217	0.265	0.015
	(0.039)	(0.457)	(0.476)	(0.133)
ΔRTW^{+3}	0.095^{**}	-0.538^{*}	-0.311	-0.005
	(0.038)	(0.313)	(0.270)	(0.160)
ΔRTW^{+4}	0.080	0.189	-0.124	0.008
	(0.053)	(0.368)	(0.304)	(0.139)
ΔRTW^{+5}	0.108^{***}	0.837^{***}	-0.041	0.826^{**}
	(0.038)	(0.290)	(0.561)	(0.321)
Year FE	*Yes*	*Yes*	*Yes*	*Yes*
Firm FE	*Yes*	*Yes*	*Yes*	*Yes*
Observations	20,471	20,347	20,457	20,468
Adjusted R^2	0.638	0.381	0.517	0.581
Note:			*p<0.1; **p<0.05; ***p<0.01	

Table 13: The effect of RTW laws on firm unemployment provision. This table reports the coefficient estimates of panel regressions by pooling all firm-year observations from 1950 to 2016. The RTW indicator and its interaction with industry sales growth excluding a given firm's own sale ($RTW \times IndSalesGrEx$) is the main explanatory variable. All regressions include controls and year, industry as well as firm fixed effects. State-level year-over-year real GSP growth ($GSP\ Growth$) is the only control variable not measured at the firm level. Robust standard errors with double clustering at the state and year level are used in reporting the t-statistics in parentheses.

	(1) EmpGr	(2) EmpGr
RTW	-0.0285	-0.0341*
	(-1.39)	(-1.72)
Ind. Sales Growth Ex.	0.00526**	0.00523**
	(2.59)	(2.25)
RTW $\times$ IndSalesGrEx	0.0425***	0.0449***
	(2.82)	(2.92)
LogAsset	-0.0573***	-0.0621***
	(-13.41)	(-10.16)
Tobin Q	0.0136***	0.0141***
	(9.56)	(8.98)
Cashflow	0.0253***	0.0239***
	(3.55)	(3.44)
GSP Growth	0.166*	0.124
	(1.78)	(1.17)
State Unemployment Benefits		0.000148
		(0.24)
Constant	0.329***	0.355***
	(14.52)	(10.75)
Year FE	Yes	Yes
Industry FE	Yes	Yes
Firm FE	Yes	Yes
Observations	74815	64272
Adjusted R^2	0.133	0.136

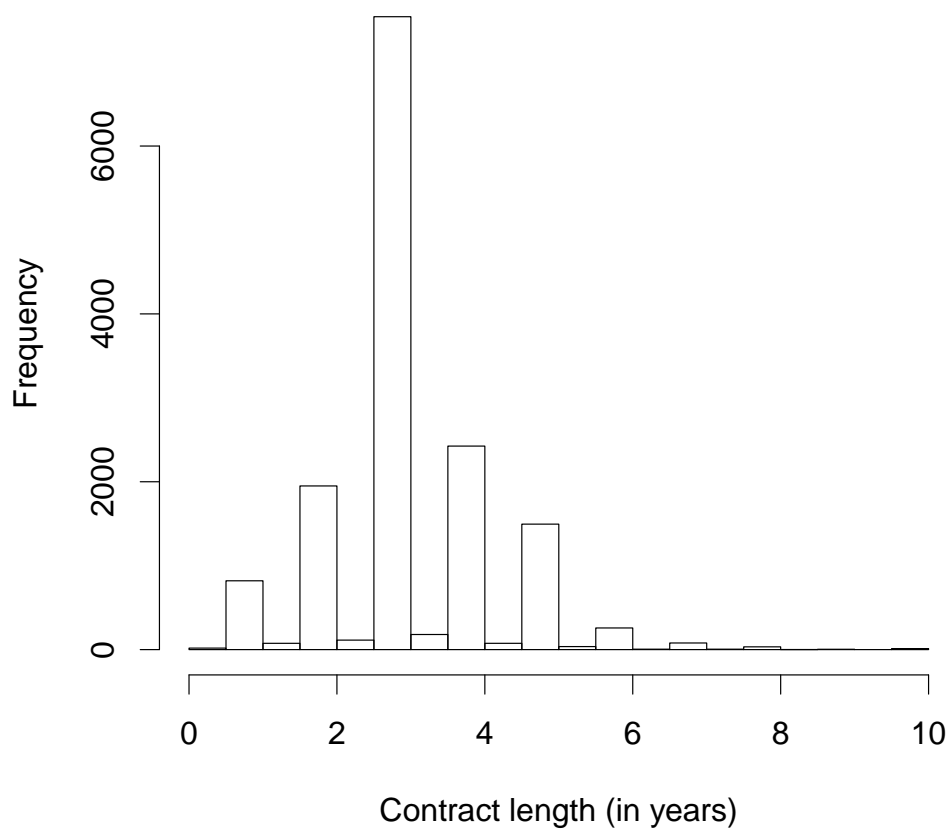* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Fig. 1. Histogram of the length of collective bargaining agreements.

Appendix A.   Robustness tests

What predicts the introduction of RTW laws?

Our difference-in-differences methodology reduces the likelihood that omitted variables, such as globalization or anti-union sentiment, are driving our results. However, we want to investigate, to the extent possible, what these omitted variables might be. Also, we want to understand what leads to the introduction of RTW laws. Therefore, we follow the approach of Simintzi et al. (2015) among others, and estimate a predictive regression using several state-level political and economic variables.

One of our predictive variables is the political orientation of a state's governor. It is plausible that the political party in power has an effect on this particular type of law. Of the five RTW introductions that we focus on in our BNA sample period, Oklahoma (2001), Indiana (2012), Michigan (2013), Wisconsin (2015), and West Virginia (2016); all occurred under either a Republican governor, a Republican state legislature, or both. Moreover, in the aftermath of the 2010 midterm elections, the Indiana state legislature tipped from an even split to Republican, the Michigan governorship and legislature shifted to Republican control, and the Wisconsin governorship and legislature flipped from Democratic to Republican. Over the next election cycle, all three states introduced RTW laws. For West Virginia, the state legislature has been controlled by the Republican party since 2014. Jim Justice was elected as the governor of West Virginia in 2016, and he switched party affiliation from Democratic to Republican as soon as he took office. Later in the same year, West Virginia joined the ranks of right-to-work states. Taken together, it is plausible to hypothesize that political party control at the state level might influence the likelihood of RTW adoption.

We use Carl Klarner's political data repository for data on governors up to 2010, and we manually extend the data up to 2016.[18] The *Governor democrat* variable takes the value of 1 if the governor is a Democrat and 0 in the case of a Republican. Independent governors are coded as 0.5.

Second, we use state-level imports from China as a proxy for the effect of globalization.[19] The data are from the USA Trade Online database (State of Destination) of the Census Bureau. We scale this variable by the nominal gross state product. Third, we include the state-level union membership rate in the regressions as a proxy for union strength. The real growth rate in the gross state product is also incorporated as a predicting variable. Other variables are (a) the change in the union membership rate over the previous five years and (b) the annual change in imports from China. Due to the limitations of the trade data, which are only available at the state level from 2008, we perform the predictive regressions from 2009 to 2017, which allows us to capture the five most recent RTW introductions (Indiana, Michigan, Wisconsin, West Virginia, and Kentucky). We start in 2009 because all predictors are lagged by one year. The dependent variable is ΔRTW, the dummy variable that indicates the year of the law's introduction. For the five treated states, we remove

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[18]http://klarnerpolitics.com/kp-dataset-page.html

[19]To further ease the concern that globalization, in particular trade with China, is simultaneously driving RTW introduction and low real wage growth, we check and verify that none of the commuting zones (with the exception of Milwaukee, WI) classified as high import exposure per worker by Autor et al. (2013) is in an RTW adoption state in our sample period.

observations after the introduction of the law. Also, as in our other regressions, we remove RTW states that introduced the law before the beginning of the sample period, which is 2009 in this case.

The regression results are documented in Table A13. It shows that the political orientation of the governor is an important predictor of RTW laws. That variable is statistically significant in all columns. RTW legislation is more likely to be passed when the governor is Republican. Interestingly, the other variables are not statistically significant. Therefore, it does not seem likely that globalization or union strength is responsible for RTW laws, although we cannot rule out that possibility.

In Table A14, we estimate our base case specification from Equation (5), while controlling for the *Governor democrat* variable.[20] The table shows that the governor's party affiliation is significant in the most stringent specification, in Column (4). A Democratic governor has a positive but small effect on wage growth, with a coefficient of 0.001. Most importantly, the coefficients of the ΔRTW dummy are very similar to Table 3, both in magnitude and in significance. This suggests that our results are not driven by the most obvious predictors of RTW laws.

Measuring wage growth over the second year of each contract

In all our tests using data from collective bargaining agreements, we measure wage growth by considering the first year of each contract, as explained in Section 4. Since wage growth over the first year is an imperfect measure of the overall value provided by a CBA to workers, this raises the question of whether our results are driven by our particular variable definition.

To see how sensitive our results are to this assumption, we collect the wage increase for the second year of each contract. This is more difficult to do than collecting data for the first year, and it results in fewer observations and a potentially noisier measure of wage growth. We collect this data by adopting our textual analysis algorithm introduced in Section 4.

We make a couple of assumptions to create a measure that is as accurate as possible. First, if the contract length is only one year, then we omit the observation from the sample. Second, if the contract length is more than a year, and if the wage increase for the second year is not mentioned explicitly, then we assume that the second-year wage increase is zero. Third, if a contract includes a wage reopener clause for the second year, we omit the observation from the sample. Fourth, if a CBA specifies that wages in the second year are conditional on future inflation, which is unknown ahead of time, then we also omit the data point from the sample. Finally, for a few observations, we must convert an absolute wage increase into a relative increase without knowing the exact level of wages at the beginning of the second year. For these observations, we use the wage level at the beginning of the first year, obtained as in Section 4, as an approximation. As a result of these assumptions, the final sample contains second-year wage information for 14,585 contracts, slightly below the 15,125 observations in Table 3.

We estimate similar difference-in-differences specifications as in Table 3, but with the second-year wage growth as the dependent variable. The results are shown in Table A15.

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[20]The number of observations is a bit lower than in Table 3 because the District of Columbia does not have a governor.

Most importantly, the ΔRTW dummy is negative and highly significant across all specifications. The magnitude of the coefficients is very similar to those in Table 3. This is reassuring because it suggests that our results are not driven by measuring wage growth only over the first year of each contract.

Using only RTW states

One of the potential concerns with our difference-in-differences specification in Equation (5) is that non-RTW states are not a good control group for those states that introduce an RTW law. Also, one might criticize the addition of non-RTW states to the sample as an artificial increase in the sample size, which could lead to an excessive reduction of standard errors. To alleviate these concerns, we omit all non-RTW states from the sample, and we repeat our base case regressions from Table 3, while using only observations from the five RTW states. In the resulting difference-in-differences regressions, the control group consists solely of observations in RTW states before the introduction of the RTW law.

Table A16 contains the results of the regressions using the reduced sample. They are very similar to the results in Table 3. The coefficients of ΔRTW are negative and highly significant in all columns. Even the magnitudes of the coefficients are similar. For example, in Column (4), the most conservative specification, the coefficient of ΔRTW is -0.006 in both tables. This highlights the robustness of our estimates, especially since the sample size has dropped from 15,125 to 2,278. Taken together, these findings suggest that adding non-RTW states to our control group does not affect our main conclusions.

We also construct a robustness test where we use the neighboring states of our five treated states as the control group. The list of neighboring states, within the subset of non-RTW states, contains Colorado, Illinois, Kentucky, Maryland, Minnesota, Missouri, New Mexico, Ohio, and Pennsylvania. We estimate our base case regression from Table 3 using only these neighboring states and our five treated states, and find qualitatively very similar results. We omit this table for brevity, but the results are available from the authors.

Drop multistate observations from the sample

As explained in Appendix B, the raw CBA data set contains some observations in which the contract covers workers in multiple states. Since the state variable in the raw data contains the value *multistate* for these observations, we divide each of these observations into multiple observations, using the information in the city variable. This raises the potential concern that the treatment of the multistate contracts artificially inflates the sample size, or that multistate contracts affect the estimation in some other special way. To account for this possibility, we remove all multistate observations from the sample, and we re-estimate our base case regressions from Table 3 using the smaller sample.

Table A17 shows that the resulting regression results are very similar to the results shown in Table 3. The ΔRTW coefficients are negative and highly significant in all specifications. The coefficient magnitudes are similar as well.

Drop early years with no RTW introductions

The sample period of our contract-level tests is 1988–2016. However, as can be seen in Table 1, all five RTW introductions that are used as the treated states occurred towards the end of the sample period. This possibly raises the concern that the early years of the sample period bias some coefficients in our regression specification in Equation (5). Therefore, we shorten the sample period to 2001–2016, and re-estimate our base case regressions from Table 3. The results are shown in Table A18, and they are very similar to the findings in Table 3. In the most conservative specification, in Column (4), the coefficient of ΔRTW drops slightly to -0.004, but is still significant at the 5% level.

Early contract renegotiation

One might be concerned about whether our results are biased by premature contract renegotiation. This is not likely to be the case. In the US, a contracting party cannot unilaterally change the terms of a contract. Also, a contract would be invalid if it is written in a way that allows one party not to perform its duties.[21] A rare exception to cancel an existing contractual obligation would be a Chapter 11 bankruptcy filing. However, since our results suggest that RTW laws help firms rather than hurting them, it is unlikely that RTW laws lead to more bankruptcies.

The typical way to prematurely renegotiate a contract would be a voluntary renegotiation, where both parties agree to change the key contractual terms, like the wage rate. Again, it is very unlikely that both parties have an incentive to renegotiate the contract. Our results suggest that RTW laws benefit employers and hurt the covered employees. Therefore, a renegotiation request by the employer will most likely be declined by the union.

Even though we believe that premature renegotiations are very unlikely, we examine how such renegotiations would affect our results. As an example, consider the possibility that all CBAs are prematurely renegotiated in the years prior to the passage of the RTW law, and that the newly negotiated wage rate reflects the shift in bargaining power to the employer. This might lead us to underestimate the true treatment effect after the introduction of the law, i.e., in years 0, $+1$, $+2$, etc. If this is the case, the parallel trends assumption in our main wage regression should be violated. Also, we should see a significant increase in the number of contracts in years -1 and -2. However, we know from Table 3 that the parallel trends assumption is not violated. Also, we check whether the number of contacts significantly increases in year -1 and -2 in Table A19, and this turns out not to be the case. The corresponding variables, ΔRTW^{-1} and ΔRTW^{-2}, are insignificant.

A second possibility would be that contracts are prematurely renegotiated in the year of the law's introduction, i.e., in year 0. If a random sample of contracts is renegotiated this way, that should not bias the treatment effects in years 0, $+1$, or $+2$. To get a bias that explains our findings, one must further assume that high wage growth contracts are more likely to be renegotiated early than low wage growth contracts. In that case, the treatment effect in the year of the law might be overestimated, while the treatment effect in years $+1$ and $+2$ might be underestimated (in absolute values). However, in this case, we should see

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[21]See the concept of an "illusory promise" in Black (1990), among others.

a significant increase in the number of contracts in year 0. We test for such an increase in year 0 in Table A19, but corresponding coefficient, ΔRTW, is insignificant.

Higher-order fixed effects

Our base case regressions in Table 3 already include year, industry, and state fixed effects. They control for a lot of unobservable variation in wage growth. However, we cannot rule out the possibility that there are omitted variables that are correlated with both our RTW dummy and with wage growth. To further alleviate such concerns, we take the spline regression specification in Column (4) in Table 3 and add higher-order fixed effects. In Table A20, Column (1) contains state and industry-year fixed effects, Column (2) contains year and state-industry fixed effects, and Column (3) combines industry-year and state-industry fixed effects. In all columns, the coefficient of ΔRTW is negative and has a similar magnitude as in Table 3, although the statistical significance is slightly lower.

Triple-difference estimation with unemployment rate

The bargaining power of unions is not only affected by RTW laws, but also by characteristics of the firm, the state, and the overall economy. In particular, it is likely that the state of the local economy has an influence on negotiations between employers and unions. We control for state-level economic growth in our regressions, but we do not explicitly control for the condition of the local labor market. If the local unemployment rate is high, it is plausible that the labor negotiations are affected. Also, it is possible that high unemployment interacts with the effect of RTW laws. For example, it could be that during times of high local unemployment, the bargaining power of unions is already low, and that the introduction of RTW laws hurts them even more.

To test this hypothesis, we obtain unemployment data at the state-year level from the Local Area Unemployment Statistics (LAUS) database of the Bureau of Labor Statistics. We then estimate an augmented version of Equation (5). It is a triple-difference regression where we add the change in the local unemployment rate, $\Delta Unemp$, as an additional control variable and also interact it with the ΔRTW dummy variable.

The results of this regression are shown in Table A21. The coefficient of ΔRTW is still negative and significant at the 1% level in all columns. This is reassuring, because it helps us to rule out an additional omitted variable concern, according to which RTW laws are just a reaction to developments in the local labor market. Interestingly, the coefficient of $\Delta Unemp$ is significantly positive in all columns. This means that an increase in the unemployment rate is associated with higher wage growth for unionized workers. More importantly, however, the interaction term $\Delta RTW \times \Delta Unemp$ is negative. This suggests that during times with increasing unemployment, the negative effect of RTW laws on wage growth is even stronger, which is consistent with our hypothesis. However, the coefficient is only significant in Column (3), which is the most conservative specification.

Results without controlling for GSP growth

In our regression specification in Equation (5) we control for local economic conditions with GSP growth. This regression could be biased if GSP growth is itself affected by the introduction of an RTW law. In Table A22, we omit GSP growth and show that the results are both qualitatively and quantitatively very similar to Table 3.

Results without using CBP data

As explained in Appendix B, for a subset of contracts we need to convert the dollar increase in wages to a percentage increase. For some of these contracts we know the level of wages, which helps us with the conversion. For contracts where the level of wages is not available, we rely on County Business Patterns (CBP) data for an estimate of wage levels. In Table A23, we omit all contracts for which we use CBP data, and show that our results are very similar to Table 3.

Dynamic effect of RTW laws on firm policies between 1988 to 2016

The dynamic response of investment, employees growth, and leverage are unchanged compared with the full sample results documented in Table 6. Parallel trend assumptions are satisfied in all three columns in Table A7. The most significant rise in investment and employees growth materializes in year 3 after RTW introduction, consistent with the full sample. Moreover, leverage declines significantly in almost all years following RTW introduction in Column (3), but especially in RTW^{+2}. From this exercise, we are more confident that the five RTW adoptions in the short sample are similar to the fourteen RTW adoptions in the full sample between 1950 and 2016.

Merging large contract expirations with Compustat

The main CBA dataset in our paper only contains about 19,000 successful contract renegotiations, most of which are private firms, so merging with Compustat would produce a small sample size. However, we were able to use another Bloomberg BNA database, called Contract Listings, which contains a much larger number of contracts. While these contracts do not have wage data, they do contain the employer name and the expiration date. Under the assumption that a contract expiration is typically followed by a contract renegotiation, we are able to identify a subsample of Compustat firms that experience a contract renegotiation.

The Contract Listings database is the same database that we have used to show that our sample of collective bargaining agreements is not biased relative to the population of CBAs (see Table A5). In this robustness test, we manually match a subset of large contracts in the Contract Listings database to Compustat.

Our initial sample contains 433,082 contract expirations with a non-missing expiration date, covering 1995–2017. This sample is too large to be manually matched to Compustat. The matching is made difficult by the fact that the employer name in the Contract Listings database can either denote a parent company name, a subsidiary name, a plant name, or even a trade association name.

To reduce the sample size while focusing on the most important contract expirations, we drop observations with a bargaining unit size of less than 500 workers as well as contract expirations where the bargaining unit size is missing. The same threshold for the number of workers is used in Yi (2016). This reduces the number of observations to 18,390.

We only keep the five states that introduce RTW during our sample period (Oklahoma, Indiana, Michigan, Wisconsin, and West Virginia), which results in 1,983 contract expirations. After dropping all years prior to RTW passage and years later than five years after passage, the number goes further down to 324. For these contract expirations, we manually match each employer name (parent company, subsidiary, or plant name) using a Wikipedia search and a Google search to a publicly traded firm, if it exists, and then to a gvkey. The match is done not only based on name, but also based on year. Out of the 324 contract expirations, we have found gvkeys for 121.

Some firms have multiple contract expirations in the same year. After removing duplicate gvkey-expiration year pairs, the number of observations is reduced to 74. After merging with Compustat (based on gvkey and calendar year) and after dropping foreign firms, we end up with 57 expirations.

Within the 57 observations, some firms have multiple contract expirations after the introduction of an RTW law. For example, Kroger has four expirations: 2012 in Indiana, 2014 in Michigan, 2016 in Wisconsin, and 2017 in Michigan. For each firm, we focus on the first contract expiration after an RTW introduction. In the case of Kroger, this is 2012. As a result, we end up with 40 unique firms (gvkeys) with at least one large contract expiration after the introduction of RTW in one of our five RTW states.

We then perform an event study using these 40 firms, where the event year is the year of the contract expiration. We calculate average operating profitability (oibdp divided by lagged total assets) for each of the three years before and the three years after the introduction of RTW, as well as for the year of the introduction. Profitability is winsorized at the 1% level and we drop firm-years where operating profitability is below -1000%.

The results of the event study are presented in Figure A3. It shows that profitability is relatively constant in the years leading up to the event year, which is defined as the first contract expiration after the introduction of RTW. Following the event year, there is a steady increase in operating profitability. The vertical bars denote 95% confidence intervals for the mean. As can be seen from the figure, the increase in profitability is not statistically significant, which is not surprising, given the small sample of 40 firms. In year $+3$, the confidence interval is particularly wide, because most of our RTW introductions are towards the end of the sample period, so fewer than 40 firms remain in the sample by that year. However, the $p-$value of a two-sided $t-$test that compares the mean of year -3 to the mean of year $+2$ is $p = 10.3\%$. In other words, the increase in profitability is almost statistically significant.

We also construct a placebo test to support the view that the increase in profitability in Figure A3 is not a consequence of concurrent shocks. For each of the 40 gvkeys in Figure A3, we randomly select a firm-year observation in Compustat with the same 4-digit SIC code and the same fiscal year as the event year. By definition, firms in this control group do not experience a large contract expiration in one of our five RTW states immediately after the passage of the law. Figure A4 shows that for this control group, profitability is mostly flat over time. Similarly to Figure A3, the confidence intervals are relatively wide due to the

small sample size.

In spite of the limitations of a small sample size, Figures A3 and A4 provide suggestive evidence that profitability does increase after the introduction of RTW, especially if one focuses on those firms that are unionized and that experience a contract expiration in the years after RTW passage. The event study evidence presented here leads us to believe that two conditions are necessary for RTW laws to induce firm-level adjustment: RTW enactment and union contract renegotiation, which is consistent with the shift in union-firm bargaining power channel we propose.

Appendix B. Sample construction for collective bargaining agreements

Within the text extraction algorithm, we separately search for absolute and relative wage changes. We transform all absolute wage changes to relative changes by scaling them by the level of wages, if this is available in the text. If the level of wages is not available, we use the average wage from the Census Bureau's County Business Patterns (CBP) data set. To approximate the actual wage of the covered workers more precisely, we calculate the average wage for each year and for each industry, where industries are defined using 2-digit SIC codes until 1997 and 2-digit NAICS codes afterward. Average wages are calculated as total payroll in Q1 divided by the total number of workers at the end of Q1. In a robustness test in Appendix A we show that our results are very similar if we do not use the CBP data.

If the absolute change in wages is reported in weekly, monthly, or yearly amounts, we normalize them to hourly wage increases. If the BNA data set contains a range of wage increases, we use the midpoint of the range. In some cases, the *State* variable of the BNA data set specifies that the workers are located in multiple states, which is coded as *Multistate*. In those cases, we manually extract the states of the covered workers, if possible, by using the information in the *City* variable of the data set. For example, if a CBA covers workers in Maine and Tennessee, then we replace the single observation with two observations, one for each of the two states. In our robustness tests, we show that removing these Multistate observations from the sample does not affect our results.

We verify the accuracy of our text extraction algorithm by manually collecting the wage increases for a random sample of 500 contracts and then comparing the wage variable to the wage variable collected using the algorithm. We remove observations from the sample if the change in the first-year wage is higher than 100% or lower than −100%. Also, we remove observations from the sample if the contract length exceeds 10 years, which is very rare.

The regression in Equation (5) includes the variable ΔGSP_{st}, which is the real growth rate of the gross state product of state s in year t. To calculate ΔGSP_{st}, we use the nominal gross state products provided by the Bureau of Economic Analysis, and we convert them to real annual growth rates using the GDP implicit price deflator from the Federal Reserve Bank of St. Louis website.

Appendix C. Sample construction for executive compensation

In Section 6.1, we merge our Compustat panel data set with the ExecuComp Annual Compensation database. After merging based on fiscal year and GVKEY, we end up with a shorter sample period of 1992–2016, due to the late starting date of ExecuComp. We restrict the executive compensation data to the CEO of each firm-year, and construct four variables: Salary, Options, Stocks, and OthComp. For Salary, we use the Execu-Comp variable SALARY. For Options, we use OPTION_AWARDS_BLK_VALUE until 2005 and OPTION_AWARDS_FV after 2005. For Stocks, we use RSTKGRNT up to 2005 and STOCK_AWARDS_FV after 2005. Finally, for OthComp, we use the ExecuComp variable OTHCOMP.

To adjust for inflation, we use the Consumer Price Index for All Urban Consumers (All Items, Not Seasonally Adjusted) from the Federal Reserve Bank of St. Louis and express all amounts in 2016 dollars. To remove duplicate observations, we order the data set based on GVKEY, fiscal year, and executive name, and select the first observation (only one duplicate observation removed). For some of our variables, there are a few observations with negative values. In those cases, we set the value of the variable to "missing". All four variables are transformed as $\log(1 + x)$ and are winsorized at the 1% and 99% levels.

Table A1: Summary statistics for change in log wage growth broken down by state. This table presents summary statistics for log wage growth in the Bloomberg BNA data sorted by state. Collective bargaining agreements (CBAs) are matched to states through the location of the establishment at which contracts are negotiated. Each count in Column (1) represents a contract.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
	count	mean	sd	min	p25	p50	p75	max
Alaska	62	0.024	0.025	0.000	0.005	0.017	0.034	0.103
California	1580	0.036	0.033	-0.062	0.020	0.030	0.045	0.262
Colorado	128	0.031	0.053	-0.062	0.010	0.027	0.039	0.565
Connecticut	541	0.030	0.036	0.000	0.017	0.027	0.034	0.394
Delaware	37	0.037	0.024	0.000	0.029	0.032	0.041	0.127
District of Columbia	139	0.034	0.033	0.000	0.021	0.032	0.039	0.310
Hawaii	126	0.033	0.026	-0.041	0.017	0.030	0.049	0.113
Illinois	1250	0.030	0.026	-0.069	0.017	0.029	0.038	0.223
Indiana	433	0.026	0.025	-0.128	0.010	0.027	0.034	0.193
Kentucky	125	0.028	0.026	0.000	0.017	0.025	0.033	0.161
Maine	149	0.026	0.023	0.000	0.011	0.025	0.032	0.157
Maryland	241	0.033	0.033	0.000	0.015	0.030	0.040	0.215
Massachusetts	882	0.028	0.026	-0.030	0.015	0.025	0.034	0.278
Michigan	985	0.020	0.027	-0.105	0.000	0.019	0.030	0.269
Minnesota	535	0.024	0.028	-0.163	0.007	0.020	0.030	0.326
Missouri	287	0.033	0.022	0.000	0.021	0.030	0.039	0.186
Montana	91	0.037	0.049	0.000	0.021	0.030	0.037	0.280
New Hampshire	84	0.026	0.022	0.000	0.011	0.025	0.034	0.122
New Jersey	776	0.032	0.030	0.000	0.020	0.030	0.039	0.323
New Mexico	69	0.038	0.033	0.000	0.022	0.034	0.049	0.191
New York	1815	0.030	0.027	0.000	0.019	0.030	0.039	0.320
Ohio	1166	0.025	0.027	-0.030	0.010	0.025	0.030	0.441
Oklahoma	71	0.034	0.075	0.000	0.015	0.020	0.034	0.635
Oregon	436	0.028	0.027	-0.051	0.013	0.025	0.035	0.231
Pennsylvania	1449	0.028	0.025	-0.111	0.017	0.030	0.036	0.195
Rhode Island	225	0.028	0.023	0.000	0.016	0.030	0.034	0.144
Vermont	130	0.029	0.025	-0.030	0.017	0.029	0.039	0.165
Washington	524	0.029	0.027	-0.057	0.013	0.027	0.039	0.219
West Virginia	127	0.033	0.027	0.000	0.021	0.029	0.037	0.165
Wisconsin	662	0.027	0.022	-0.223	0.020	0.028	0.031	0.178
Total	15125	0.029	0.029	-0.223	0.015	0.027	0.037	0.635

Table A2: Summary statistics for change in log wage growth broken down by year. This table presents summary statistics for log wage growth in the Bloomberg BNA data sorted by year. Collective bargaining agreements (CBAs) are aggregated by the year of the effective date of each contract. Each count in Column (1) represents a contract.

	(1) count	(2) mean	(3) sd	(4) min	(5) p25	(6) p50	(7) p75	(8) max
1988	284	0.045	0.042	0.000	0.020	0.039	0.061	0.336
1989	173	0.071	0.064	0.000	0.032	0.050	0.094	0.565
1990	146	0.074	0.048	0.000	0.039	0.058	0.102	0.221
1991	143	0.057	0.046	0.000	0.030	0.044	0.077	0.306
1992	77	0.068	0.044	0.000	0.033	0.057	0.105	0.170
1993	97	0.045	0.037	0.000	0.023	0.034	0.059	0.179
1994	165	0.032	0.027	0.000	0.021	0.030	0.039	0.184
1995	337	0.033	0.046	0.000	0.010	0.030	0.039	0.394
1996	305	0.030	0.035	-0.010	0.018	0.028	0.032	0.296
1997	575	0.027	0.018	-0.062	0.020	0.030	0.034	0.138
1998	557	0.032	0.022	0.000	0.022	0.030	0.038	0.211
1999	623	0.034	0.025	0.000	0.025	0.030	0.039	0.320
2000	650	0.038	0.029	0.000	0.027	0.032	0.044	0.441
2001	642	0.037	0.021	0.000	0.029	0.034	0.045	0.219
2002	536	0.036	0.028	-0.051	0.023	0.030	0.041	0.265
2003	672	0.030	0.026	-0.051	0.017	0.030	0.038	0.205
2004	629	0.030	0.026	0.000	0.017	0.030	0.039	0.262
2005	758	0.032	0.024	0.000	0.020	0.030	0.039	0.186
2006	646	0.034	0.023	0.000	0.022	0.030	0.039	0.195
2007	720	0.034	0.025	-0.051	0.023	0.030	0.039	0.326
2008	776	0.032	0.021	-0.062	0.020	0.030	0.039	0.183
2009	842	0.016	0.018	-0.128	0.000	0.016	0.030	0.138
2010	767	0.013	0.020	-0.223	0.000	0.010	0.025	0.189
2011	841	0.015	0.033	-0.102	0.000	0.010	0.022	0.635
2012	755	0.016	0.020	-0.105	0.000	0.020	0.025	0.269
2013	725	0.019	0.017	-0.064	0.010	0.020	0.026	0.168
2014	626	0.022	0.021	-0.163	0.015	0.020	0.028	0.253
2015	582	0.024	0.020	-0.101	0.015	0.023	0.030	0.162
2016	476	0.026	0.021	-0.111	0.020	0.023	0.030	0.183
Total	15125	0.029	0.029	-0.223	0.015	0.027	0.037	0.635

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Table A3: Summary statistics for change in log wage growth broken down by industry. This table presents summary statistics for log wage growth in the Bloomberg BNA data sorted by industry. Collective bargaining agreements (CBAs) are aggregated by the industry of the establishment at which contracts are negotiated. Industry is defined by the 2-digit SIC code. Each count in Columns (1) and (4) represents a contract.

| | | (1) | (2) | (3) | | | (4) | (5) | (6) |
SIC	Name	count	mean	sd	SIC	Name	count	mean	sd
10	Metal	18	0.031	0.026	50	Wholesale-Durable	37	0.036	0.026
12	Coal	19	0.038	0.022	51	Wholesale-Non-Durable	37	0.031	0.022
14	Mining	14	0.038	0.046	53	General Merchandise	22	0.051	0.037
15	Building	55	0.041	0.030	54	Food Stores	499	0.033	0.035
16	Heavy Construction	471	0.034	0.023	55	Automotive Dealers	9	0.041	0.042
17	Contractors	414	0.042	0.037	56	Apparel Stores	19	0.046	0.017
20	Food and Kindred	433	0.027	0.021	58	Restaurants	44	0.061	0.043
21	Tobacco	2	0.027	0.000	59	Misc. Retail	61	0.044	0.036
22	Textile	46	0.038	0.040	60	Depository Inst.	11	0.041	0.041
23	Apparel	60	0.035	0.027	62	Brokers	8	0.020	0.013
24	Lumber	50	0.021	0.029	63	Insurance Carriers	35	0.028	0.011
25	Furniture	38	0.053	0.039	64	Insurance Agents	5	0.033	0.009
26	Paper	301	0.023	0.014	65	Real Estate	25	0.031	0.013
27	Printing	323	0.029	0.044	70	Hotels	111	0.042	0.038
28	Chemicals	195	0.031	0.026	72	Personal Services	38	0.038	0.035
29	Petroleum	36	0.024	0.011	73	Business Services	196	0.038	0.029
30	Rubber	103	0.034	0.031	75	Auto Repair	29	0.052	0.07
31	Leather	16	0.045	0.024	76	Misc. Repair	6	0.026	0.008
32	Stone	97	0.027	0.021	78	Motion Pictures	52	0.026	0.011
33	Primary Metal	196	0.022	0.026	79	Amusement Parks	145	0.033	0.054
34	Fabricated Metal	178	0.032	0.029	80	Health Services	1453	0.034	0.031
35	Industrial Machinery	194	0.030	0.028	81	Legal Services	2	0.016	0.023
36	Electronic Equip.	185	0.032	0.032	82	Education	506	0.030	0.037
37	Tranportation Equip.	393	0.024	0.020	83	Social Services	68	0.029	0.025
38	Measuring Instruments	54	0.035	0.028	84	Museums	10	0.056	0.036
39	Misc. Manufacturing	31	0.044	0.026	86	Membership Org.	41	0.033	0.018
40	Railroad Transportation	83	0.034	0.027	87	Engineering	50	0.051	0.048
41	Local Transit	966	0.038	0.034	89	Misc. Services	6	0.031	0.015
42	Motor Freight	41	0.040	0.036	90	Government	2378	0.022	0.019
43	USPS	1	0.025	.	91	General Government	17	0.043	0.056
44	Water Transportation	47	0.037	0.036	92	Justice	931	0.019	0.016
45	Air Transportation	75	0.047	0.055	93	Public Finance	1	0.026	.
47	Transportation Services	35	0.048	0.068	94	Human Resource	2	0.027	0.038
48	Communications	456	0.027	0.017	96	Economic	1	0.000	.
49	Electric Services	453	0.031	0.018	99	Nonclassifiable	2191	0.023	0.024

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Table A4: Simple RTW dummy, private vs public sector employees. This table presents estimation results for the difference-in-differences specification in Equation (5). The unit of observation is a collective bargaining agreement (CBA). The sample period is 1988–2016. The dependent variable is the change in the log of wages. The main explanatory variable is ΔRTW, a dummy that indicates the year of the introduction of a right-to-work (RTW) law. An additional control variable is the growth rate of the gross state product (GSP). Columns (1)–(3) are based on the subsample of private sector CBAs, in which the 2-digit SIC codes are < 90, and Columns (4)–(6) are focused on the public sector, in which the SIC codes are ≥ 90. Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:					
	$\Delta log(w)$					
	Private	Private	Private	Public	Public	Public
	(1)	(2)	(3)	(4)	(5)	(6)
ΔRTW	-0.009^{***}	-0.006^{***}	-0.004^{**}	-0.013^{***}	-0.013^{***}	-0.004
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.004)
GSP growth	0.069^{**}	0.059^{**}	0.046	0.100^{**}	0.100^{**}	0.075^{***}
	(0.032)	(0.029)	(0.031)	(0.040)	(0.040)	(0.027)
Year FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Industry FE		*Yes*	*Yes*		*Yes*	*Yes*
State FE			*Yes*			*Yes*
Observations	9,604	9,604	9,604	5,521	5,521	5,521
Adjusted R^2	0.118	0.165	0.170	0.185	0.185	0.234
Note:						*p<0.1; **p<0.05; ***p<0.01

Table A5: Comparison of contract distribution across states. This table presents the comparison of two data sets of collective bargaining agreements (CBAs), both provided by Bloomberg BNA. The Contract Listings data set approximately covers the entire universe of private-sector CBAs between 1990 and 2017. The Settlement Summaries data set is a smaller set of CBAs, and is the sample used in this paper. The table shows the distribution across US states for both data sets. Columns (1) and (2) present the number of contracts in each state and their percentages as a fraction of the total number of observations in the Contract Listings data set. Columns (3) and (4) contain the same two variables for Settlement Summaries data set.

	Contract Listings (universe)		Settlement Summaries (sample)	
	(1)	(2)	(3)	(4)
	Count	Percent of Total	Count	Percent of Total
Alaska	942	0.35%	62	0.41%
California	34794	12.85%	1580	10.45%
Colorado	2444	0.90%	128	0.85%
Connecticut	4607	1.70%	541	3.58%
District of Columbia	1645	0.61%	139	0.92%
Delaware	860	0.32%	37	0.24%
Hawaii	1819	0.67%	126	0.83%
Illinois	30856	11.40%	1250	8.26%
Indiana	8935	3.30%	433	2.86%
Kentucky	3323	1.23%	125	0.83%
Massachusetts	8520	3.15%	882	5.83%
Maryland	4497	1.66%	241	1.59%
Maine	727	0.27%	149	0.99%
Michigan	16499	6.09%	985	6.51%
Minnesota	11913	4.40%	535	3.54%
Missouri	11703	4.32%	287	1.90%
Montana	1547	0.57%	91	0.60%
New Hampshire	577	0.21%	84	0.56%
New Jersey	16392	6.05%	776	5.13%
New Mexico	1010	0.37%	69	0.46%
New York	31948	11.80%	1815	12.00%
Ohio	18123	6.69%	1166	7.71%
Oklahoma	1247	0.46%	71	0.47%
Oregon	5693	2.10%	436	2.88%
Pennsylvania	22649	8.36%	1449	9.58%
Rhode Island	1716	0.63%	225	1.49%
Vermont	492	0.18%	130	0.86%
Washington	12710	4.69%	524	3.46%
Wisconsin	10493	3.88%	662	4.38%
West Virginia	2097	0.77%	127	0.84%
Total	270778	100.00%	15125	100.00%

Table A6: Comparison of contract distribution across industries. This table presents the comparison of two data sets of collective bargaining agreements (CBAs), both provided by Bloomberg BNA. The Contract Listings data set approximately covers the entire universe of private-sector CBAs between 1990 and 2017. The Settlement Summaries data set is a smaller set of CBAs, and is the sample used in this paper. The table shows the distribution across major SIC industries for both data sets. Columns (1) and (2) present the number of contracts in each major industry and their percentages as a fraction of the total number of observations in the Contract Listings data set. Columns (3) and (4) contain the same two variables for Settlement Summaries data set. The Contract Listings data set covers the period 2012–2017 as SIC industry classification is missing prior to 2012. In the Settlement Summaries data set, agreements coded with SIC industry 99 are dropped. In the bottom panel, we eliminate public sector agreements (SIC code 9), and recalculate the fraction of contracts in each major industry.

| | Contract Listings 2012-2017 | | Settlement Summaries | |
| | (1) | (2) | (3) | (4) |
SIC Major	Count	Percent of Total	Count	Percent of Total
1	0	0.00%	991	7.66%
2	18347	21.97%	1484	11.47%
3	18566	22.23%	1447	11.19%
4	14913	17.86%	2157	16.68%
5	6680	8.00%	728	5.63%
6	10796	12.93%	84	0.65%
7	7411	8.87%	577	4.46%
8	4747	5.68%	2136	16.51%
9	2049	2.45%	3330	25.75%
Total	83509	100.00%	12934	100.00%

	Without Public Sector			
	Contract Listings 2012-2017		Settlement Summaries	
	(1)	(2)	(3)	(4)
SIC Major	Count	Percent of Total	Count	Percent of Total
1	0	0.00%	991	10.32%
2	18347	22.52%	1484	15.45%
3	18566	22.79%	1447	15.07%
4	14913	18.31%	2157	22.46%
5	6680	8.20%	728	7.58%
6	10796	13.25%	84	0.87%
7	7411	9.10%	577	6.01%
8	4747	5.83%	2136	22.24%
Total	81460	100.00%	9604	100.00%

Table A7: Dynamic effect of RTW laws on firm investment, employment growth, and leverage—post-1988 sample. This table reports the coefficient estimates of spline regressions on firm policies. The explanatory variables are dummies denoting each year in the 11-year (± 5) window around the RTW adoption plus one dummy denoting if a particular observation is more than five years before the enactment of the law ($\Delta RTW^{<(-5)}$). Observations in the one year immediately before the RTW law implementation do not have a RTW dummy and serve as the benchmark. Treated observations beyond ΔRTW^{+5} are omitted. The dependent variable in Column (1) is investment, defined as capital expenditure (CAPX) divided by lagged assets. The dependent variable in Column (2) is employees growth, defined as employees (emp) divided by lagged employees minus 1. The dependent variable in Column (3) is book leverage, defined as debt in current liabilities plus long-term debt (dlc + dltt) divided by lagged assets. All regressions include controls (not shown) and year, industry as well as firm fixed effects. Robust standard errors with double clustering at the state and year level are used in reporting the t-statistics in parentheses.

	(1) Inv/A	(2) EmpGr	(3) Debt/A
$\Delta RTW^{<(-5)}$	0.00390	0.0127	-0.00153
	(0.92)	(0.82)	(-0.11)
ΔRTW^{-5}	0.00144	-0.0292	-0.00162
	(0.57)	(-1.53)	(-0.08)
ΔRTW^{-4}	0.00287	0.00762	-0.00139
	(0.96)	(0.23)	(-0.07)
ΔRTW^{-3}	0.00209	0.0217	0.00214
	(0.37)	(1.14)	(0.16)
ΔRTW^{-2}	-0.00338	0.0153	-0.00410
	(-1.20)	(0.57)	(-0.41)
ΔRTW	0.00440*	0.0353	-0.00948
	(1.81)	(1.35)	(-0.98)
ΔRTW^{+1}	0.00457	-0.00572	-0.0227*
	(1.25)	(-0.44)	(-2.02)
ΔRTW^{+2}	-0.000779	0.0211	-0.0321***
	(-0.12)	(0.76)	(-3.02)
ΔRTW^{+3}	0.0111***	0.0675***	-0.0232*
	(2.82)	(3.92)	(-1.82)
ΔRTW^{+4}	-0.00325	0.0173	-0.0466**
	(-0.62)	(0.57)	(-2.12)
ΔRTW^{+5}	0.0109	-0.0111	-0.0187
	(0.48)	(-0.42)	(-0.73)
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Observations	55685	55685	55685
Adjusted R^2	0.585	0.139	0.677

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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Table A8: The effect of RTW laws on firm investment, employment growth, and leverage: Labor-intensive firms only. This table reports the coefficient estimates of panel regressions by pooling all firm-year observations from 1950 to 2016. The RTW law indicator (RTW) is the main explanatory variable. The dependent variable in Column (1) is investment, defined as capital expenditure (CAPX) divided by lagged assets. The dependent variable in Column (2) is employees growth, defined as employees (emp) divided by lagged employees minus 1. The dependent variable in Column (3) is book leverage, defined as debt in current liabilities plus long-term debt (dlc + dltt) divided by lagged assets. All regressions include controls and year, industry as well as firm fixed effects. State-level year-over-year real GSP growth ($GSP\ Growth$) is the only control variable not measured at the firm level. Robust standard errors with double clustering at the state and year level are used in reporting the t-statistics in parentheses.

	(1) Inv/A	(2) EmpGr	(4) Debt/A
RTW	0.00450***	0.0172*	-0.0291***
	(2.80)	(1.83)	(-3.86)
LogAsset	-0.00865***	-0.0699***	0.0311***
	(-12.12)	(-15.34)	(10.09)
Tobin Q	0.00325***	0.0127***	-0.000421
	(5.84)	(5.14)	(-0.60)
Cashflow	0.00673**	0.0276**	
	(2.46)	(2.61)	
GSP Growth	0.0432**	0.129	-0.0171
	(2.47)	(1.13)	(-0.48)
Profitability			-0.113***
			(-10.38)
Tangiblity			0.0234**
			(2.51)
Constant	0.0975***	0.397***	0.0502**
	(29.00)	(16.19)	(2.73)
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Observations	58464	58464	58464
Adjusted R^2	0.527	0.151	0.648

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A9: The effect of RTW on unions, using an alternative measure of the free-rider problem. This table shows spline regressions to estimate the timing of the effect of RTW laws on unions. The sample is based on union membership data from unionstats.com, and the unit of observation is a state-year. The sample period is 1983–2016. The dependent variable is the difference between the union coverage rate and the union membership rate. Column (1) is based on the entire workforce, Column (2) focuses on private sector unions, and Column (3) is based on the public sector. The main explanatory variables are a set of dummies that indicate when a right-to-work (RTW) law is introduced. ΔRTW^{+3} denotes three years after the introduction of the law, ΔRTW^{+2} denotes two years after the law, ΔRTW^{+1} denotes one year after the law, ΔRTW is the year of the introduction, ΔRTW^{-2} is two years before the introduction, and $\Delta RTW^{<(-2)}$ stands for all years before then. An additional control variable is the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:		
	Coverage membership absolute difference		
	Total	Private	Public
	(1)	(2)	(3)
$\Delta RTW^{<(-2)}$	0.027	0.087	0.086
	(0.153)	(0.146)	(0.309)
ΔRTW^{-2}	0.068	0.037	0.423
	(0.088)	(0.125)	(0.537)
ΔRTW	0.090	0.015	0.917*
	(0.140)	(0.102)	(0.508)
ΔRTW^{+1}	0.414*	0.196	1.786***
	(0.211)	(0.132)	(0.532)
ΔRTW^{+2}	0.392***	0.354***	0.835
	(0.132)	(0.088)	(0.868)
ΔRTW^{+3}	0.101	0.148**	0.366
	(0.093)	(0.074)	(0.761)
GSP growth	−0.421	0.285	−0.670
	(0.424)	(0.599)	(1.270)
Year FE	_Yes_	_Yes_	_Yes_
State FE	_Yes_	_Yes_	_Yes_
Observations	1,014	1,014	1,014
Adjusted R^2	0.677	0.537	0.660
Note:		*p<0.1; **p<0.05; ***p<0.01	

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Table A10: Dynamic effect of RTW laws on firm dividends, repurchases, and cash holdings: Labor-intensive firms only. This table reports the coefficient estimates of spline regressions on firm policies. The explanatory variables are dummies denoting each year in the 11-year (±5) window around the RTW adoption plus one dummy denoting if a particular observation is more than five years before the enactment of the law ($\Delta RTW^{<(-5)}$). Observations in the one year immediately before the RTW law implementation do not have a RTW dummy and serve as the benchmark. Treated observations beyond ΔRTW^{+5} are omitted. The dependent variable in Column (1) is dividends (dv) divided by lagged assets. The dependent variable in Column (2) is repurchases (prstkc) divided by lagged assets. The dependent variable in Column (3) is cash and short-term investments (che) divided by total assets. All regressions include controls (not shown) and year, industry as well as firm fixed effects. Robust standard errors with double clustering at the state and year level are used in reporting the t-statistics in parentheses.

	(1)	(2)	(3)
	Div/A	Repur/A	Cash/A
$\Delta RTW^{<(-5)}$	0.000337	0.00255	0.00535
	(0.14)	(0.49)	(0.98)
ΔRTW^{-5}	0.00115	-0.00379	0.00551
	(0.74)	(-0.72)	(0.68)
ΔRTW^{-4}	0.000203	-0.0106***	0.00345
	(0.11)	(-2.87)	(0.34)
ΔRTW^{-3}	-0.00105	-0.00773	0.00370
	(-0.40)	(-1.43)	(0.48)
ΔRTW^{-2}	0.000384	-0.000329	0.00113
	(0.15)	(-0.06)	(0.18)
ΔRTW	-0.00174	-0.00158	0.00279
	(-0.84)	(-0.35)	(0.30)
ΔRTW^{+1}	-0.000115	-0.00779*	0.00302
	(-0.05)	(-1.74)	(0.36)
ΔRTW^{+2}	0.00104	-0.00994	0.0121
	(0.27)	(-1.54)	(0.57)
ΔRTW^{+3}	0.00476***	-0.00403	-0.0178
	(3.10)	(-0.58)	(-1.29)
ΔRTW^{+4}	0.00479**	-0.00589	-0.0107
	(2.10)	(-1.51)	(-0.63)
ΔRTW^{+5}	0.00310	-0.00665	-0.0610***
	(1.24)	(-0.81)	(-3.42)
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Observations	58464	52567	58464
Adjusted R^2	0.622	0.296	0.719

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A11: The effect of RTW laws on executive compensation: Labor-intensive firms. This table presents estimation results for Equation (7). The sample period is 1992–2016. The explanatory variables are dummies denoting each year in the 11-year (±5) window around the RTW adoption plus one dummy denoting if a particular observation is more than five years before the enactment of the law ($\Delta RTW^{<(-5)}$). Observations in the one year immediately before the RTW law implementation do not have a RTW dummy and serve as the benchmark. The dependent variables are various measures of CEO compensation: base salary (Salary), options granted (Options), stocks granted (Stocks), and other compensation (OthComp). All dependent variables are in logs of thousand dollars. Control variables that are not displayed are lagged cash flow over assets, lagged Tobin's Q, lagged log of assets, and the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered by state and year.

	Salary	Options	Stocks	OthComp
	(1)	(2)	(3)	(4)
$\Delta RTW^{<(-5)}$	−0.041	0.243	−0.277	−0.045
	(0.034)	(0.586)	(0.416)	(0.121)
ΔRTW^{-5}	0.035	0.614	0.073	−0.120
	(0.045)	(0.458)	(0.379)	(0.095)
ΔRTW^{-4}	0.035	0.599	−0.045	−0.006
	(0.043)	(0.491)	(0.411)	(0.167)
ΔRTW^{-3}	−0.001	−0.135	−0.318	0.022
	(0.057)	(0.488)	(0.440)	(0.154)
ΔRTW^{-2}	0.012	−0.139	−0.462	0.053
	(0.055)	(0.450)	(0.395)	(0.121)
ΔRTW	−0.006	0.266	−0.052	0.292***
	(0.039)	(0.818)	(0.530)	(0.088)
ΔRTW^{+1}	−0.017	0.174	−0.128	0.277*
	(0.029)	(0.808)	(0.570)	(0.156)
ΔRTW^{+2}	0.041	0.180	0.263	0.147
	(0.041)	(0.592)	(0.755)	(0.124)
ΔRTW^{+3}	0.077**	−0.169	−0.208	0.270*
	(0.032)	(0.451)	(0.437)	(0.154)
ΔRTW^{+4}	0.113**	0.386	−0.160	0.083
	(0.053)	(0.292)	(0.560)	(0.160)
ΔRTW^{+5}	0.006	0.875*	0.428	1.288
	(0.026)	(0.490)	(0.350)	(0.911)
Year FE	*Yes*	*Yes*	*Yes*	*Yes*
Firm FE	*Yes*	*Yes*	*Yes*	*Yes*
Observations	14,906	14,805	14,895	14,903
Adjusted R^2	0.736	0.379	0.510	0.562

| *Note:* | *p<0.1; **p<0.05; ***p<0.01 |

Table A12: The effect of RTW laws on firm unemployment provision: Labor-intensive firms only. This table reports the coefficient estimates of panel regressions by pooling all firm-year observations from 1950 to 2016. The RTW indicator and its interaction with industry sales growth excluding a given firm's own sale ($RTW \times IndSalesGrEx$) is the main explanatory variable. All regressions include controls and year, industry as well as firm fixed effects. State-level year-over-year real GSP growth ($GSP\ Growth$) is the only control variable not measured at the firm level. Robust standard errors with double clustering at the state and year level are used in reporting the t-statistics in parentheses.

	(1) EmpGr	(2) EmpGr
RTW	-0.0338	-0.0351
	(-1.39)	(-1.52)
Ind. Sales Growth Ex.	0.00679***	0.00655***
	(3.57)	(2.84)
RTW × IndSalesGrEx	0.0491***	0.0481***
	(3.07)	(3.42)
LogAsset	-0.0664***	-0.0732***
	(-14.49)	(-11.73)
Tobin Q	0.0136***	0.0139***
	(5.30)	(4.57)
Cashflow	0.0279**	0.0259**
	(2.56)	(2.49)
GSP Growth	0.122	0.0728
	(1.06)	(0.52)
StateUI		-0.000679
		(-1.42)
Constant	0.369***	0.415***
	(15.41)	(13.78)
Year FE	Yes	Yes
Industry FE	Yes	Yes
Firm FE	Yes	Yes
Observations	56326	48284
Adjusted R^2	0.148	0.150

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A13: Predicting the introduction of RTW laws. This table contains the results of predictive regressions for the introduction of right-to-work (RTW) laws. The dependent variable is a dummy that takes the value of 1 in the year when a RTW law is introduced. The predictors are the political orientation of the state's governor, the ratio of a the state's imports from China to the state's gross state product, the average union membership rate of the state, the growth rate of the gross state product, the change in the state's union membership rate, the change in the ratio of imports from China and the gross state product, and a constant. Columns (1)–(3) contain OLS regressions, and Columns (4)–(6) present logistic regressions. All predicting variables are lagged by one year. The sample period is 2009–2017. Observations in RTW states after the introduction of a RTW law are omitted from the sample.

	Dependent variable:					
	ΔRTW					
	OLS			*logistic*		
	(1)	(2)	(3)	(4)	(5)	(6)
Governor democrat	−0.050**	−0.049**	−0.055**	−2.298**	−2.232*	−2.211*
	(0.020)	(0.020)	(0.023)	(1.139)	(1.151)	(1.158)
Imports from China	0.101	0.071	0.084	8.553	2.747	2.543
	(0.730)	(0.731)	(0.830)	(40.690)	(45.342)	(45.477)
Union membership	−0.002	−0.002	−0.002	−0.115	−0.113	−0.104
	(0.002)	(0.002)	(0.002)	(0.126)	(0.129)	(0.128)
GSP growth	0.013	−0.002	−0.001	−0.060	−2.403	−2.879
	(0.373)	(0.373)	(0.450)	(19.681)	(19.684)	(19.564)
Union mem. chg.		−0.006	−0.006		−0.361	−0.314
		(0.006)	(0.007)		(0.355)	(0.355)
Chg. in imports from China			−0.278			4.751
			(4.155)			(218.561)
Constant	0.084**	0.078**	0.086**	−1.440	−1.779	−1.731
	(0.037)	(0.037)	(0.042)	(1.596)	(1.654)	(1.648)
Observations	240	240	212	240	240	212
Adjusted R²	0.012	0.013	0.007			
Log Likelihood				-21.127	-20.566	-20.153
Akaike Inf. Crit.				52.254	53.132	54.306

Note: *p<0.1; **p<0.05; ***p<0.01

Table A14: The effect of RTW laws on wage growth, controlling for governorship. This table presents estimation results for an augmented version of Equation (5). The unit of observation is a collective bargaining agreement (CBA). The sample period is 1988–2016. The dependent variable is the change in the log of wages. The main explanatory variable is ΔRTW, a dummy that indicates the year of the introduction of a right-to-work (RTW) law. We control for the political orientation of a state's governor, as well as for the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:			
	$\Delta log(w)$			
	(1)	(2)	(3)	(4)
ΔRTW	−0.019***	−0.011***	−0.010***	−0.005***
	(0.002)	(0.001)	(0.001)	(0.001)
Governor democrat	−0.002	0.001	0.001	0.001**
	(0.001)	(0.001)	(0.001)	(0.001)
GSP growth	0.099***	0.086***	0.072**	0.057**
	(0.029)	(0.031)	(0.029)	(0.025)
Constant	0.028***			
	(0.002)			
Year FE		*Yes*	*Yes*	*Yes*
Industry FE			*Yes*	*Yes*
State FE				*Yes*
Observations	14,986	14,986	14,986	14,986
Adjusted R^2	0.013	0.151	0.193	0.202

Note:	*p<0.1; **p<0.05; ***p<0.01

Table A15: The effect of RTW laws on wage growth, using the second year of each contract. This table presents estimation results for the difference-in-differences specification in Equation (5). The unit of observation is a collective bargaining agreement (CBA). The sample period is 1988–2016. The dependent variable is the change in the log of wages, calculated for the second year of each contract. The main explanatory variable is ΔRTW, a dummy that indicates the year of the introduction of a right-to-work (RTW) law. We control for the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:			
	$\Delta log(w)$			
	(1)	(2)	(3)	(4)
ΔRTW	-0.016^{***}	-0.013^{***}	-0.011^{***}	-0.006^{***}
	(0.001)	(0.002)	(0.002)	(0.002)
GSP growth	0.069^{***}	0.050^{*}	0.038^{*}	0.026
	(0.023)	(0.028)	(0.023)	(0.018)
Constant	0.027^{***}			
	(0.001)			
Year FE		*Yes*	*Yes*	*Yes*
Industry FE			*Yes*	*Yes*
State FE				*Yes*
Observations	14,585	14,585	14,585	14,585
Adjusted R^2	0.004	0.038	0.082	0.089

Note:	*p<0.1; **p<0.05; ***p<0.01

Table A16: The effect of RTW laws on wage growth, using only RTW states. This table presents estimation results for the difference-in-differences specification in Equation (5), using only the subsample of observations in an RTW state. The unit of observation is a collective bargaining agreement (CBA). The sample period is 1988–2016. The dependent variable is the change in the log of wages. The main explanatory variable is ΔRTW, a dummy that indicates the year of the introduction of a right-to-work (RTW) law. An additional control variable is the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:			
	$\Delta log(w)$			
	(1)	(2)	(3)	(4)
ΔRTW	-0.016^{***}	-0.006^{***}	-0.006^{***}	-0.006^{***}
	(0.001)	(0.001)	(0.002)	(0.002)
GSP growth	0.128^{***}	0.171^{***}	0.199^{***}	0.167^{***}
	(0.027)	(0.036)	(0.040)	(0.039)
Constant	0.023^{***}			
	(0.001)			
Year FE		_Yes_	_Yes_	_Yes_
Industry FE			_Yes_	_Yes_
State FE				_Yes_
Observations	2,278	2,278	2,278	2,278
Adjusted R^2	0.030	0.188	0.238	0.243
Note:				*p<0.1; **p<0.05; ***p<0.01

Table A17: The effect of RTW laws on wage growth, without multistate observations. This table presents estimation results for the difference-in-differences specification in Equation (5), using only the subsample of observations in which the collective bargaining agreement (CBA) does not cover multiple states. The unit of observation is a CBA. The sample period is 1988–2016. The dependent variable is the change in the log of wages. The main explanatory variable is ΔRTW, a dummy that indicates the year of the introduction of a right-to-work (RTW) law. An additional control variable is the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:			
	$\Delta log(w)$			
	(1)	(2)	(3)	(4)
ΔRTW	-0.020^{***}	-0.012^{***}	-0.011^{***}	-0.006^{***}
	(0.001)	(0.001)	(0.001)	(0.001)
GSP growth	0.110^{***}	0.092^{***}	0.081^{***}	0.061^{**}
	(0.028)	(0.031)	(0.029)	(0.025)
Constant	0.027^{***}			
	(0.001)			
Year FE		*Yes*	*Yes*	*Yes*
Industry FE			*Yes*	*Yes*
State FE				*Yes*
Observations	14,066	14,066	14,066	14,066
Adjusted R^2	0.013	0.156	0.197	0.206

Note:	*p<0.1; **p<0.05; ***p<0.01

Table A18: The effect of RTW laws on wage growth, with a short sample period. This table presents estimation results for the difference-in-differences specification in Equation (5), where the sample period is shortened to 2001–2016. The unit of observation is a collective bargaining agreement (CBA). The dependent variable is the change in the log of wages. The main explanatory variable is ΔRTW, a dummy that indicates the year of the introduction of a right-to-work (RTW) law. An additional control variable is the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:			
	$\Delta log(w)$			
	(1)	(2)	(3)	(4)
ΔRTW	−0.016***	−0.012***	−0.010***	−0.004**
	(0.001)	(0.001)	(0.001)	(0.002)
GSP growth	0.094***	0.098***	0.085***	0.042**
	(0.028)	(0.033)	(0.029)	(0.019)
Constant	0.024***			
	(0.001)			
Year FE		*Yes*	*Yes*	*Yes*
Industry FE			*Yes*	*Yes*
State FE				*Yes*
Observations	10,993	10,993	10,993	10,993
Adjusted R^2	0.010	0.113	0.172	0.193
Note:				*p<0.1; **p<0.05; ***p<0.01

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Table A19: The timing of the effect of RTW on the number of CBAs. This table presents estimation results for a difference-in-differences regression, using the sample of collective bargaining agreements (CBAs) from Bloomberg BNA. The unit of observation is a state-year. The sample period is 1988–2016. The dependent variable is the number of CBAs per state-year. The main explanatory variables are a set of dummies that indicate when a right-to-work (RTW) law is introduced. ΔRTW^{+3} denotes three years after the introduction of the law, ΔRTW^{+2} denotes two years after the law, ΔRTW^{+1} denotes one year after the law, ΔRTW is the year of the introduction, ΔRTW^{-1} is one year before the introduction, and ΔRTW^{-2} stands for two years before the introduction of the law. $\Delta RTW^{[1,3]}$ indicates the first three years after the introduction of the law. An additional control variable is the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:	
	Number of CBAs	
	(1)	(2)
ΔRTW^{-2}	−6.317	−6.341
	(5.814)	(5.804)
ΔRTW^{-1}	−5.118	−5.068
	(8.317)	(8.318)
ΔRTW	−2.520	−2.496
	(10.352)	(10.350)
$\Delta RTW^{[1,3]}$	−8.684***	
	(2.923)	
ΔRTW^{+1}		−13.248***
		(3.110)
ΔRTW^{+2}		−5.745
		(4.973)
ΔRTW^{+3}		−5.291*
		(2.750)
GSP growth	12.262	11.834
	(26.740)	(26.860)
Year FE	*Yes*	*Yes*
State FE	*Yes*	*Yes*
Observations	857	857
Adjusted R^2	0.735	0.735
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table A20: Dynamic regressions with higher-order fixed effects. This table presents estimation results for a modified version of the difference-in-differences specification in Equation (5). The unit of observation is a collective bargaining agreement (CBA). The sample period is 1988–2016. The dependent variable is the change in the log of wages. The main explanatory variables are a set of dummies that indicate when a right-to-work (RTW) law is introduced. ΔRTW^{+2} denotes two years after the introduction of the law, ΔRTW^{+1} denotes one year after the law, ΔRTW is the year of the introduction, ΔRTW^{-2} is two years before the introduction, and $\Delta RTW^{<(-2)}$ stands for all years before then. An additional control variable is the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:		
	$\Delta log(w)$		
	(1)	(2)	(3)
$\Delta RTW^{<(-2)}$	0.003	0.001	0.0002
	(0.002)	(0.001)	(0.001)
ΔRTW^{-2}	−0.001	0.001	0.0002
	(0.002)	(0.001)	(0.002)
ΔRTW	−0.003*	−0.002*	−0.004*
	(0.002)	(0.001)	(0.002)
ΔRTW^{+1}	−0.001	0.0001	−0.001
	(0.002)	(0.002)	(0.002)
ΔRTW^{+2}	−0.002	−0.001	−0.002
	(0.002)	(0.004)	(0.002)
GSP growth	0.063***	0.041*	0.046***
	(0.020)	(0.022)	(0.018)
Year FE		*Yes*	
State FE	*Yes*		
Industry-Year FE	*Yes*		*Yes*
State-Industry FE		*Yes*	*Yes*
Observations	15,026	15,026	15,026
Adjusted R^2	0.308	0.273	0.368
Note:		*p<0.1; **p<0.05; ***p<0.01

Table A21: The effect of RTW laws on wage growth, interacted with unemployment rate. This table presents estimation results for an augmented version of Equation (5). The unit of observation is a collective bargaining agreement (CBA). The sample period is 1988–2016. The dependent variable is the change in the log of wages. The main explanatory variable is ΔRTW, a dummy that indicates the year of the introduction of a right-to-work (RTW) law. We control for the change in the unemployment rate at the state-year level, denoted as $\Delta Unemp$, the interaction of the latter with ΔRTW, as well as for the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

		Dependent variable:	
		$\Delta log(w)$	
	(1)	(2)	(3)
ΔRTW	−0.011***	−0.010***	−0.009***
	(0.002)	(0.002)	(0.002)
$\Delta Unemp$	0.002***	0.002***	0.002***
	(0.0005)	(0.0005)	(0.0005)
$\Delta RTW \times \Delta Unemp$	0.001	0.001	−0.005***
	(0.001)	(0.002)	(0.002)
GSP growth	0.101***	0.088***	0.072***
	(0.034)	(0.032)	(0.027)
Year FE	*Yes*	*Yes*	*Yes*
Industry FE		*Yes*	*Yes*
State FE			*Yes*
Observations	15,125	15,125	15,125
Adjusted R^2	0.152	0.194	0.203
Note:			*p<0.1; **p<0.05; ***p<0.01

Table A22: The effect of RTW laws on wage growth, without controlling for GSP growth. This table presents estimation results for Equation (5). The unit of observation is a collective bargaining agreement (CBA). The sample period is 1988–2016. The dependent variable is the change in the log of wages. The main explanatory variable is ΔRTW, a dummy variable that indicates the year when a right-to-work (RTW) law is introduced. In contrast to Table 3, we do not control for the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:		
	$\Delta log(w)$		
	(1)	(2)	(3)
ΔRTW	−0.011***	−0.010***	−0.005***
	(0.001)	(0.001)	(0.001)
Year FE	*Yes*	*Yes*	*Yes*
Industry FE		*Yes*	*Yes*
State FE			*Yes*
Observations	15,125	15,125	15,125
Adjusted R^2	0.148	0.192	0.201
Note:		*p<0.1; **p<0.05; ***p<0.01

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Table A23: The effect of RTW laws on wage growth, without CBP data. This table presents estimation results for Equation (5). In contrast to Table 3, we omit all contracts where wage growth had to be estimated using auxiliary data from the County Business Patterns (CBP) data set. The unit of observation is a collective bargaining agreement (CBA). The sample period is 1988–2016. The dependent variable is the change in the log of wages. The main explanatory variable is ΔRTW, a dummy variable that indicates the year when a right-to-work (RTW) law is introduced. An additional control variable is the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

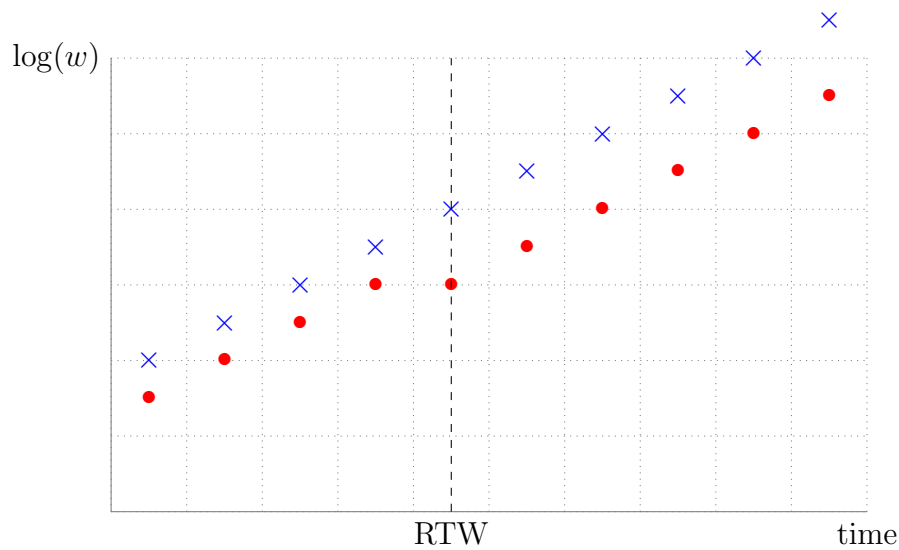|	*Dependent variable:*		
	$\Delta log(w)$		
	(1)	(2)	(3)
ΔRTW	−0.011***	−0.010***	−0.005***
	(0.001)	(0.001)	(0.001)
GSP growth	0.079***	0.068**	0.053**
	(0.030)	(0.027)	(0.022)
Year FE	*Yes*	*Yes*	*Yes*
Industry FE		*Yes*	*Yes*
State FE			*Yes*
Observations	13,381	13,381	13,381
Adjusted R^2	0.189	0.226	0.237
Note:		*p<0.1; **p<0.05; ***p<0.01	

Fig. A1. Stylized plot illustrating the identification strategy. The vertical axis shows log wages and the horizontal axis shows time. The dots represent contracts in a state that introduces an RTW law during the sample period. The crosses indicate observations in a state that does not pass such a law.
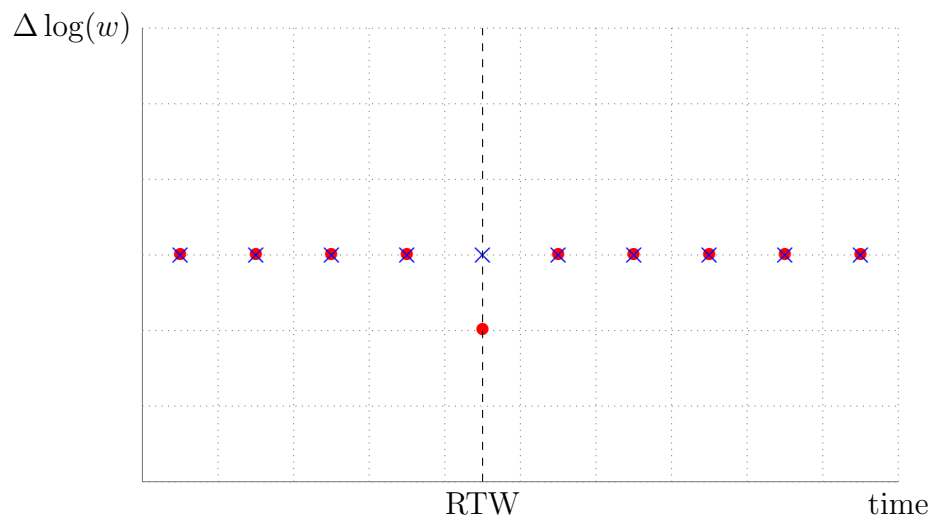

Fig. A2. Stylized plot illustrating the identification strategy. The vertical axis shows the *change* in log wages and the horizontal axis shows time. The dots represent contracts in a state that introduces an RTW law during the sample period. The crosses indicate observations in a state that does not pass such a law.
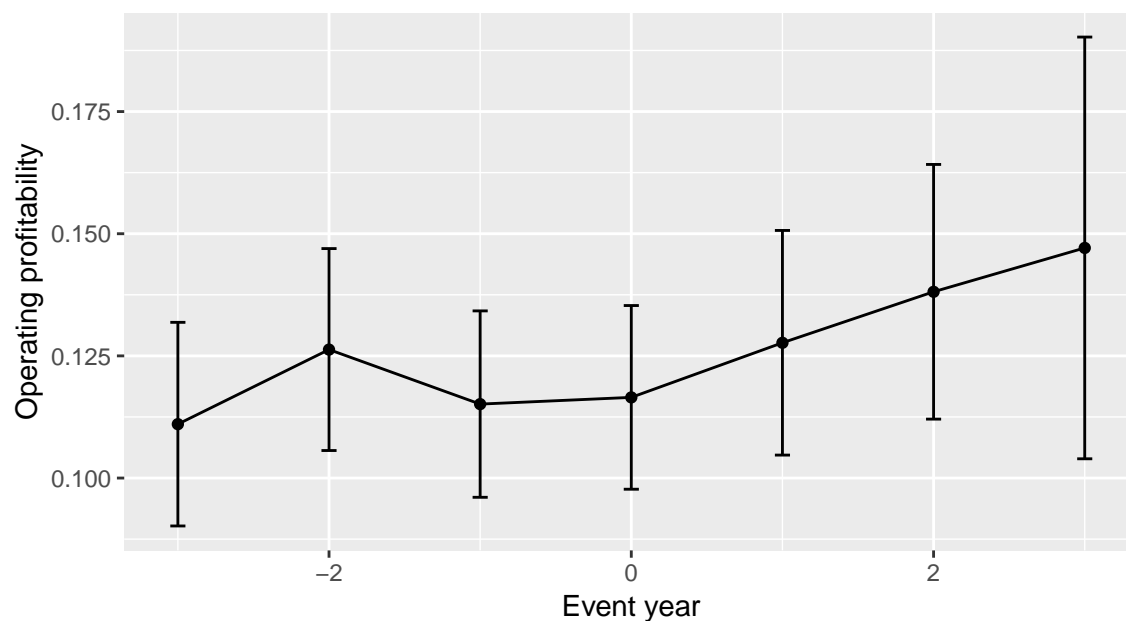
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Fig. A3. Event study for profitability after a post-RTW contract expiration. We identify firm-year observations in Compustat that experience a union contract expiration immediately after the introduction of RTW. We focus on large contracts which cover at least 500 workers and we limit our analysis to the five states that introduce RTW between 1995 and 2016. The contract expirations are from the Contract Listings database of Bloomberg BNA. The event year is defined as the year of the first contract expiration after the passage of RTW. The solid dots represent average operating profitability in a particular event year. The vertical lines denote 95% confidence intervals for the mean.
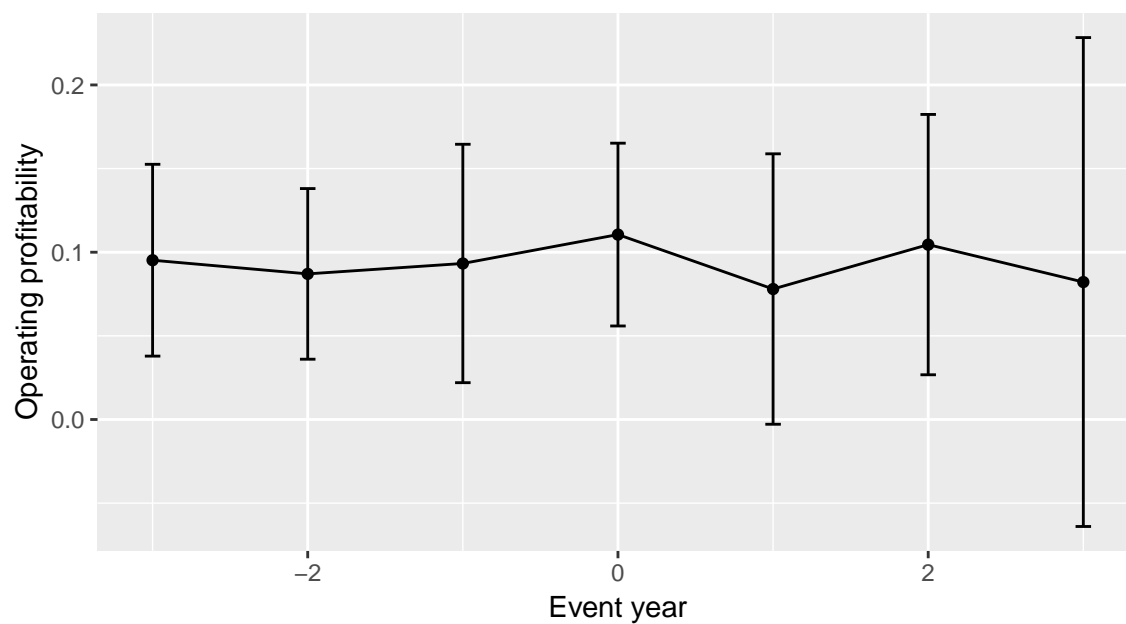
Fig. A4. Placebo event study for profitability. This event study is an extension of Figure A3 and shows the evolution of profitability for a matched control group of firms. For each of the 40 firms in our treatment group in Figure A3, we randomly select a matched control firm in the same 4-digit SIC code and the same fiscal year as the event year. The solid dots represent average operating profitability in a particular event year. The vertical lines denote 95% confidence intervals for the mean.