Journal of Financial Economics 137 (2020) 451–469

Contents lists available at ScienceDirect

Journal of Financial Economics

journal homepage: www.elsevier.com/locate/jfec

The economic impact of right-to-work laws: Evidence from collective bargaining agreements and corporate policies[☆]

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A R T I C L E I N F O

Article history:
Received 2 October 2018
Revised 1 July 2019
Accepted 30 July 2019
Available online 8 February 2020

JEL classification:
J31
J50
G31
G32
G38

Keywords:
Right to work
Collective bargaining
Unions
Wage growth
Investment

A B S T R A C T

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 27 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, and on several

☆ All authors are affiliated with the Scheller College of Business at the Georgia Institute of Technology. We thank Ashwini Agrawal, Gerard Hoberg, Debarshi Nandy, Paige Ouimet, Andrea Weber, Liu Yang, an anonymous referee, as well as the editor (Bill Schwert) for very helpful comments. We thank conference participants at the American Economic Association 2019, the European Finance Association 2018, the University of Kentucky Finance Conference 2018, the Northeastern University Finance Conference 2018, the Labor and Finance Group meeting 2017, the DFG Textual Analysis Workshop at the ZEW in Mannheim 2017, and seminar participants at Emory University, Central European University, University of Alabama, and Queen's University at Kingston for their feedback. Sudheer Chava can be reached at sudheer.chava@scheller.gatech.edu; 800 West Peachtree Street NW, Atlanta, GA 30308. András Danis (corresponding author) can be contacted at andras.danis@scheller.gatech.edu; 800 West Peachtree Street NW, Atlanta, GA 30308. Alex Hsu can be reached at alex.hsu@scheller.gatech.edu; 800 West Peachtree Street NW, Atlanta, GA 30308. We thank Nisarg Shah for excellent research assistance.

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

https://doi.org/10.1016/j.jfineco.2020.02.005
0304-405X/

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 pp over approximately one year. The unconditional average wage growth in our sample is 2.9% and average consumer price index 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 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, and profitability and a higher labor-to-assets ratio. Extensions of this canonical theory, such as Matsa (2010), Michaels et al. (2019), and 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 free 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, 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 show 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 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 five years after RTW adoption. Economically, operating profitability is almost 3 pp 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 pass-through of industry sales growth to firm 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), and 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 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 allow us to measure the contractual wages of exactly those workers who are most likely to be affected by RTW laws and are 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 show 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.

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:

$$\max_{K,L} \{ pQ(K, L) - W(\theta)L - rK\}, \tag{2}$$

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 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

[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.”

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 decrease 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: Eq. (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 make 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, 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), and 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 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 had 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 Eq. (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 Eq. (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

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

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					

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 Online Appendix A.

Eq. (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, Eq. (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 Eq. (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 Eqs. (3) and (5) is best illustrated graphically. Fig. A1 in the Online Appendix shows a stylized plot for the empirical pattern that Eq. (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. Fig. 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 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.

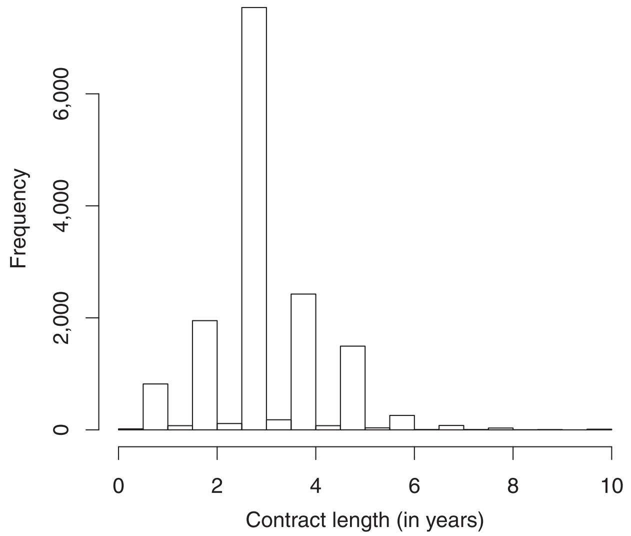

Fig. 1. Histogram of the length of collective bargaining agreements. The sample of collective bargaining agreements is from Bloomberg BNA, with sample period 1988–2016.

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 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, the results from these predictive regressions further reduce 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

In this section, we describe in detail the dataset used for our contract-level wage analysis as well as the firm-level data used to study the impact of RTW legislation.

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 Standard Industrial Classification (SIC) and North American Industry Classification System (NAICS) codes, and a short summary of the agreed-upon terms concerning the change in wages.[4]

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. Fig. 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

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

Table 2
Summary statistics for change in log wage.
This table presents summary statistics for log wage growth in a sample of collective bargaining agreements (CBAs) from Bloomberg BNA. The sample period is 1988–2016. The first row is the entire sample. The second rows contains CBAs negotiated in a nonright-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	15,125	0.029	0.029	−0.223	0.015	0.027	0.037	0.635
Non-RTW obs.	14,827	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	9,604	0.033	0.032	−0.223	0.020	0.030	0.039	0.565
Public sector	5,521	0.022	0.021	−0.105	0.010	0.021	0.030	0.635

algorithm and the sample construction methodology can be found in Online 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 Online 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 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).

[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 versus clerks).

We provide additional summary statistics tables in the Online 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 two-digit SIC codes. The number of observations varies strongly across industries. This is because collective bargaining is much more prevalent in some industries than others. For example, the public sector has a large number of 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 use 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-sample 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 across industries between the two samples 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 represents 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 headquarter ZIP codes to Federal Information Processing Standards (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, employees (emp), cash (che), total long-term debt (dltt), total liabilities (lt), and dividend (dv). Observations with a capital expenditure to property, plant and equipment (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

We present results of our empirical analysis in this section. We examine outcome variables that can be affected by RTW adoption through a shift in bargaining power from labor to the firm.

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.[8] 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 Eq. (5) using an RTW dummy variable that takes a value of one in the year when a state introduces an RTW

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

[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.

[8] See the survey papers by Moore and Newman (1985) and Moore (1998)

Table 3
The effect of RTW laws on wage growth.
This table presents estimation results for the difference-in-differences specification in Eq. (5). 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 an RTW law. Other explanatory variables are dummies that indicate the years before and after the introduction of an 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 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

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

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 pp. Even the most conservative coefficient of −0.6 pp represents a 20.7% reduction in wage growth relative to the unconditional mean of 2.9%.[9] 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 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 pp, 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 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 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 Fig. A1 and A2 in the Online Appendix.

[9] 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. (2019), among others.

Table 4
The permanent effects of RTW laws on wage growth.

This table presents estimation results for the difference-in-differences specification in Eq. (5). 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 *RTW*, a dummy that takes a value of one in the year of the introduction of an RTW law and in all subsequent years. An additional control variable is the growth rate of the 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

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

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 research and development (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 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:

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

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), and 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 one 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 ($\frac{at_t+equity_t-be_t-txbd_t}{at_t}$), cash flow ($\frac{dp_t+ib_t}{at_{t-1}}$), 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 recommendation of Petersen (2008) for Compustat panel data.[10]

Table 5 presents the baseline regression results of estimating Eq. (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.

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

Table 5
The effect of RTW laws on firm investment, employment growth, and leverage.

We use a sample of firm-year observations from the CRSP-Compustat merged database. 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 one. 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 (*GSPgrowth*) 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)
Cash flow	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	77,684	77,684	77,684
Adjusted R^2	0.558	0.133	0.648

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

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 year of implementation, and a $\Delta RTW^{<(-5)}$ dummy is assigned to all observations before the pre-RTW five-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:

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

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.[11] 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 three 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 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 Fig. 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, Fig. A3 and A4.

[11] 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.

Table 6
Dynamic effect of RTW laws on firm investment, employment growth, and leverage.

We use a sample of firm-year observations from the CRSP-Compustat merged database. The sample period is 1950–2016. This table reports the coefficient estimates of spline regressions on firm policies. The explanatory variables are dummies denoting each year in the 11-year ($\pm$ 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) Inv/A	(2) EmpGr	(3) 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	77,684	77,684	77,684
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.

We use a sample of firm-year observations from the CRSP-Compustat merged database, 1950–2016. This table reports the coefficient estimates of spline regressions on firm policies. The explanatory variables are dummies denoting each year in the 11-year ($\pm$ 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) Inv/A	(2) EmpGr	(3) 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	58,464	58,464	58,464
Adjusted R^2	0.527	0.151	0.648

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

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 Eq. (7) on investment, employees growth, and leverage for only the high labor intensity firms.[12] 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 stay statistically significant for two additional years. Compared with the investment response in Table 6, Column (1), labor-intensive firms appear to raise

[12] 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 and are consistent with the spline results discussed here.

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 the 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) show 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 Gross State Product (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,

Table 8
Dynamic effect of RTW laws on firm profitability and the labor-to-assets ratio.

We use a sample of firm-year observations from the CRSP-Compustat merged database, 1950–2016. 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 ($\pm$ 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) OI/A	(2) Emp/A	(3) OI/A	(4) 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	77,684	77,684	58,464	58,464
Adjusted R^2	0.685	0.885	0.697	0.883

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

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 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 Eq. (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 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.

Table 9
The effect of RTW on the number of CBAs.

This table presents estimation results for a difference-in-differences regression, using a sample of 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 an RTW law and in all subsequent years. An additional control variable is the growth rate of the 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

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

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. The reasoning is that 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 toward zero. In other words, RTW laws might reduce wage growth even more than our estimates suggest.[13]

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.[14]

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 that was 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 free-rider problem will become more severe after the introduction of an RTW law. If this happens, then we

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

[14] 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.

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 an 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 GSP. Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable: UnionCovMem		
	Total (1)	Private (2)	Public (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

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

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, explained in detail in Hirsch and MacPherson (2003).[15] 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 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

We summarize additional empirical results here. In particular, we investigate how RTW adoption affect various firm stakeholders; namely, shareholders, managers, and workers.

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 Eq. (7) with controls of log assets, Tobin's q, cash flow, 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.

[15] 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).

Table 11
Dynamic effect of RTW laws on firm dividends, stock repurchases, and cash holdings.

We use a sample of firm-year observations from the CRSP-Compustat merged database, 1950–2016. 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	77,684	69,317	77,684
Adjusted R^2	0.611	0.284	0.755

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

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.[16] 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 shows 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 (Gabaix and Landier, 2008, e.g.,). 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), 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 Online 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.[17]

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

[17] 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.

Table 12
The effect of RTW laws on executive compensation.

We use a sample of firm-year observations from the CRSP-Compustat merged database, combined with ExecuComp. The sample period is 1992–2016. This table presents estimation results for Eq. (7). 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 GSP. Standard errors are shown in parentheses and are clustered by state and year.

	Salary (1)	Options (2)	Stocks (3)	OthComp (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

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

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 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:

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

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. Thus, 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 Eq. (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.[18] 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

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

Table 13
The effect of RTW laws on firm unemployment provision.

We use a sample of firm-year observations from the CRSP-Compustat merged database. 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* × *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 (*GSPgrowth*) 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 × *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)
Cash flow	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	74,815	64,272
Adjusted R^2	0.133	0.136

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

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[19] and Krugman,[20] 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. 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. The negative effect 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 (Card et al., 2004, e.g.,). 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.

[19] Larry Summers, September 3, 2017, America needs its unions more than ever, http://larrysummers.com/2017/09/03/america-needs-its-unions-more-than-ever/.

[20] Paul Krugman, May 23, 2017, Trucking and blue-collar woes, https://krugman.blogs.nytimes.com/2017/05/23/trucking-and-blue-collar-woes/.

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