The Effect of Right-to-Work Laws on Workers and Firms

Sudheer Chava   András Danis   Alex Hsu*

ABSTRACT

The recent slowdown in wage growth in the US has been attributed to low productivity growth, globalization, and automation. We argue that the decline of unions could be an additional factor in explaining low wage growth. In states with right-to-work (RTW) laws, new employees can join a unionized firm without having to pay union dues. We use the staggered introduction of these laws across the US as a negative shock to the bargaining power of unions. Using data from collective bargaining agreements (CBAs), we show that wages drop at unionized firms as a result of the introduction of RTW laws. We show that the number of CBAs and union membership rates drop as well, which suggests that the effect on wages indeed occurs because of a reduction in union bargaining power. The effects on firms mirror the effects on workers: Firms increase investment and employment, and reduce financial leverage. Finally, we show that RTW laws not only reduce wages of unionized workers, but also have an effect on the average worker.

JEL Classification: J31, J41, J50, G30

Keywords: right to work, collective bargaining, unions, wage growth, workers

---

*All authors are affiliated with the Scheller College of Business at the Georgia Institute of Technology. We thank conference participants at the DFG Textual Analysis Workshop at the ZEW in Mannheim 2017, and seminar participants at the University of Alabama and Queen's University at Kingston for their feedback. Sudheer Chava can be reached at sudheer.chava@scheller.gatech.edu, or at 404 894 4371. András Danis can be contacted at andras.danis@scheller.gatech.edu, or at 404 385 4569. Alex Hsu can be reached at alex.hsu@scheller.gatech.edu, or at 404 385 1123. We thank Nisarg Shah for excellent research assistance.

I. Introduction

Over the recent decades, real wage growth in the US has slowed down significantly, which has been attributed to low productivity growth, globalization, and automation. We examine whether there could be an additional explanation: the decline of unions, and the corresponding decline in workers' bargaining power. To test this hypothesis, we use the staggered introduction of state-level right-to-work (RTW) laws as a negative shock to union strength. In simple terms, states with RTW laws allow new employees to join a unionized establishment without having to pay union dues. Consequently, we expect these laws to weaken unions and to shift bargaining power from workers to firms. Our tests of the effects of RTW on workers and firms support the idea that the decline of unions has contributed to low wage growth.

The decline of wage growth is a well-known and important puzzle in the academic literature. For example, Acemoglu and Autor (2011) show that since 1973, average annual real wage growth of the 50th percentile of the wage distribution has been close to zero. Over the years, the popular press has highlighted this problem numerous times, which illustrates that this is an important question outside of academia as well.[1] The phenomenon cannot be easily explained by low productivity growth, because wage growth has slowed down more than productivity growth. In fact, the puzzle is sometimes referred to as the decoupling of productivity and wages (e.g., Cyrille, Kappeler and Pionnier (2017)). Another potential explanation is that technological progress in automation creates downward pressure on wages (e.g., Brynjolfsson and McAfee (2011)). However, this view is contested, as technological innovation has historically improved both productivity and wages (e.g., Autor (2015)). Autor, Dorn, Hanson and Song (2014) and Acemoglu, Autor, Dorn, Hanson and Price (2016) offer an explanation by showing that globalization has negative effects on the US labor market. We provide evidence that supports an additional explanation, namely that the decline of unions has contributed to the decline of wage growth. Our findings are consistent with recent conjectures by Summers (2017) and Krugman (2017).

We present both worker and firm level evidence supporting our hypothesis. For our tests focusing on workers, we extract wage growth data from 19,574 collective bargaining agreements (CBAs) in the US, spanning the period 1988–2016. As our identification strategy, we exploit the staggered introduction of RTW laws across five states, Oklahoma, Indiana, Michigan, Wisconsin, and West Virginia, using a difference-in-differences methodology. In some of our other tests, we are able to lengthen the sample period and to increase the number of RTW introductions to twelve. The difference-in-differences methodology automatically

[1] The Economist alone has written on the subject on 2017/9/1, 2017/8/24, 2015/4/14, and 2014/9/6.

controls for time-invariant differences in wage levels across states, as well as time-specific shocks that are common to all states. Additionally, we control for economic conditions at the state-level, industry fixed effects, and state-specific growth rates in wages, which rules out several omitted variable concerns. Furthermore, our tests for the parallel trends assumption are inconsistent with reverse causality.

Our main finding is that RTW laws reduce nominal wage growth by -0.6 percentage points over approximately one year. This effect is quite large, because annual real wage growth is very low. The unconditional average wage growth in our sample is 2.9%, and average CPI inflation is 2.6%. This suggests that RTW laws eliminate a substantial fraction of real wage growth, albeit only over one year. We cannot test the effect on wage levels, because the CBA data is mostly available only in wage growth rates, but even a temporary effect on wage growth can have permanent effects on wage levels. Subsample tests suggest that the effect is driven by the private sector, as opposed to the public sector.

We argue that the mechanism responsible for the effect of RTW laws on wage growth is the decline in union strength. However, union strength, or union bargaining power, is unobservable. Therefore, to support this mechanism, we use two proxy variables for union strength. First, we use the number of CBAs at the state-year level. If RTW laws reduce union strength so much that some establishments de-unionize, we should observe a drop in the number of CBAs after the passage of a law. This is exactly what we find. Second, we use union membership rates at the state-year level. We expect union membership rates to fall after the introduction of a RTW law, and our tests confirm this conjecture. Union membership rates fall in the overall sample, and also in the private and public sector subsamples.

One potential concern with our tests using CBA data is that the results only apply to unionized workers, which represent a relatively small fraction of the workforce. In other words, the effects on the average worker might be negligible. To rule out this concern, we perform a back-of-the-envelope calculation, using the fact that the estimated effect on workers covered by a CBA is -0.6%, and that the average union coverage rate during our sample period is 15%. Depending on the assumption about the unobservable effect on workers that are not covered by a CBA, we estimate the effect on the average worker is in the range of -0.09% to -0.6% for one year. This effect is actually quite large, considering that the average wage growth in our sample is 2.9% per year, and that average annual inflation is 2.6%.

Our firm-level results mirror the findings at the worker level. This confirms our main hypothesis, since we think of RTW laws as a shift in bargaining power from unions to firms. Using the CRSP-Compustat merged dataset, we use our difference-in-difference methodology to explore how firms react to the introduction of RTW laws. Due to the longer sample period, the number of states that introduce a RTW law increases to twelve. We find that firms invest more and increase employment, which are both consistent with a drop in wages. Also, firms reduce financial leverage, which is consistent with Matsa (2010), who finds that firms use financial leverage leverage as a strategic tool to threaten bankruptcy and thereby increasing their bargaining power against unions. Our results are consistent with the view that after introduction of a RTW law, firms' bargaining power increases, and they no longer need to use high leverage as a bargaining tool.

The paper's contributions are related to different strands of the literature. The first contribution is to provide evidence for a relatively under-explored explanation for the low wage growth puzzle in the US. Multiple explanations have been proposed in the literature. Some of these explanations, such as the slowdown of productivity growth and the rise of automation, are not fully satisfactory. Productivity growth in the US has slowed down compared to before the early 1970s, but not by enough to explain the decline in wage growth. Gordon (2014) shows that annual productivity growth has decreased from 2.36% over 1891–1972 to 1.38% over 1972–1996. According to Acemoglu and Autor (2011), median real wage growth over the period 1973–2008 was much lower, approximately 10% in total.[2] Also, as explained in Acemoglu and Autor (2011), before 1973, wage growth of different parts of the earning distribution to move together. Since then, however, wage growth for the 90th percentile of the earnings distribution kept up with productivity growth, but wage growth for the 50th percentile did not. The rise of automation could potentially explain the decline of wage growth, but that is a contested argument. This is because technological progress has historically increased the productivity of workers, which has lead to higher, not lower, wage growth.

A compelling explanation for the wage growth puzzle is globalization, in particular the offshoring of jobs to low-wage countries. Autor et al. (2014) and Acemoglu et al. (2016) provide evidence for this explanation. We propose an alternative explanation to the existing ones, but none of these explanations are mutually exclusive. To the best of our knowledge, our paper is the first rigorous econometric study of the effect of RTW laws on wages and on firms. The effect of the decline of unions on wage growth has been conjectured in Summers (2017) and Krugman (2017), neither author provides rigorous empirical evidence for the effect. There is also a parallel literature on the effect of declining unions on increasing wage inequality (e.g., Lemieux (1998), Card (2001), Card, Lemieux and Riddell (2004), and Western and Rosenfeld (2011)). We focus on wage growth, not wage inequality, but of course

[2]Figure 7a in Acemoglu and Autor (2011) shows that the 50th percentile of the earnings distribution experienced wage growth of approximately 10 log points, which is approximately 10 percent, since $\exp(0.1) - 1 = 0.105$.

the two are related.

The second contribution of the paper is to provide evidence for the negative effect of RTW laws on wages. This is important, because 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, Dunlevy and Newman (1986), and Hundley (1993) find no effect. Also, our paper has several methodological advantages compared to the existing literature. For example, our identification strategy is stronger than that of existing papers. The sample period is longer, which allows us to use more RTW introductions. Also, we use a difference-in-difference estimation, and in addition we control for state-specific growth rates and industry fixed effects. Also, we combine worker level tests with tests of the effects on firms.

The third contribution is to the growing literature on labor and finance. Most of the existing papers, such as Matsa (2010), Simintzi, Vig and Volpin (2015), and Serfling (2016), among others, focus on the relationship between firms' labor market considerations and the financial leverage chosen by the firms. Out of 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 results that RTW laws lead to an increase in firm investment extend the literature on the negative effect of unions on investment, such as Hirsch (1992), Bronars and Deere (1993), Fallick and Hassett (1999), and Bradley, Kim, Incheol and Tian (2017). The main contribution of our paper is the use of RTW laws as a shock to union bargaining power.

Finally, we contribute to the research on the causes of the decline of unions in the U.S. In particular, our results are related to the literature on the effect of RTW laws on unions, reviewed by Moore (1998). Importantly, Moore writes that some papers 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). Compared to previous studies, we use a substantially longer sample period, which implies a higher number of RTW introductions, and an identification strategy based on the difference-in-differences method.

II. Identification

Our main research question is whether the introduction of RTW laws has a negative effect on wages. However, estimating the causal effect of these laws is challenging. Legislation is not created randomly, and right-to-work laws are no exception. We argue that the main endogeneity concern is an omitted variable that is correlated with the introduction of the law and with wage growth, followed by reverse causality concerns.

One plausible omitted variable is globalization. Offshoring to low-labor countries could simultaneously put downward pressure on wage growth and force US states to pass RTW legislation in order to be competitive. Another possibility could be 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 is also trending 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 that this lower wage growth causes the introduction of RTW laws. It is not easy to come up with a good reason why low wage growth should lead to such laws. One possibility would be that voters in these low wage growth states believe that unions are responsible for their low 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 this endogeneity problem. Our main approach is a difference-in-differences regression, which exploits the fact that some states have introduced a RTW law, while other states have not. Table I summarizes the introduction years, and shows that 27 states have implemented such a law by 2017. The idea of the difference-in-differences estimation is to compare the difference between RTW states and non-RTW states, and the difference between the post-RTW to the pre-RTW period. This reduces the risk that unobservable state characteristics or unobservable time shocks confound the estimation of the effect of RTW laws on wage growth: Unobservable time-invariant state characteristics are controlled for by the first difference, and unobservable shocks that affect all states at the same time are accounted for by the second difference.

Our difference-in-differences approach also exploits the fact that states introduced the law at different points in time. The five introductions in the sample period of our CBA data are Oklahoma (2001), Indiana (2012), Michigan (2013), Wisconsin (2015), and West Virginia (2016). This reduces the risk that some omitted shock that coincides with the RTW introduction 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. For example, Michigan introduced a RTW law in 2013. It is possible that a large manufacturing plant laid off several thousand workers in Michigan in the same year, which could have temporarily reduced wage growth. That reduction in wage growth might have nothing to do with the contemporaneous introduction of a RTW law, so it would confound our estimation of the effect of the law on wage growth. However, it is unlikely that such a shock happens not only in Michigan in 2013, but also in Oklahoma in 2001, in Indiana in 2012, etc. The staggered introduction of RTW laws reduces the likelihood that globalization, anti-union sentiment, or other omitted variables are biasing the estimation of the treatment effect.

Ideally, of course, we would use an even larger number of RTW introductions, but the sample period for our main dataset is relatively short. However, compared to existing studies in this research area, five staggered introductions is not such a small number. For example, Holmes (1998) uses a single cross-section in his analysis, and Matsa (2010) uses three RTW introductions. Some of our additional tests, using firm-level data, have a longer sample period, which 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, in some of our tests 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, they further reduce the likelihood that some omitted variable is driving our results.

In our last test for omitted variables, we examine which state-level political and economic variables predict the introduction of RTW laws. This approach is also used in Simintzi et al. (2015), among others. In a first stage, we estimate a predictive regression where we use the political orientation of the governor, a measure for the importance of imports from China, the state-level union membership rate, the gross state produce growth rate, among others, as predictors. 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 further reduces the likelihood that globalization or anti-union sentiment drives our results.

Finally, we address the reverse causality problem by estimating a modified difference-in-differences specification where we separately estimate the effect of RTW laws in the years prior, during, and after they go into effect. We show that there is no effect of RTW laws on wage growth prior to the laws' passage. 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.

To conclude, we cannot rule out all possible endogeneity concerns, because RTW laws are not randomly introduced. However, we believe that our identification strategy allows us to estimate the treatment effect of RTW laws as accurately as possible.

III. Data and methodology

The data for our main tests concerning the effect of RTW laws on workers is a sample of CBAs from Bloomberg BNA. The initial sample contains 19,574 contracts from the US, covering the period 1988–2016. Among others, the data includes the employer name, union name, effective date of the agreement, the length of the contract, city and state of the workers' location, the employer's SIC and NAICS codes, and a short summary of the agreed terms concerning the change in wages. The same dataset is used in Klasa, Maxwell and Ortiz-Molina (2009) and Yi (2016).

The total change in wages specified in each CBA is difficult to summarize in a single number. Also, the wage information is embedded in a separate text string for each contract, and these strings are heterogeneous across contracts. For these reasons, we create a text extraction algorithm to obtain the wage increase over the first year of each contract and use that as a proxy for the total increase in wages. Within the text extraction algorithm, we separately search for absolute and relative wage changes. We transform all absolute wage changes to relative ones, by scaling them by the level of wages, if available in the text. If the level of wages is not available, we use the average wage from the Census Bureau's County Business Patterns (CBP) dataset. To approximate the actual wage of the covered workers more precisely, we calculate the average wage for each year and for each industry, where industries are defined using 2-digit SIC codes until 1997, and 2-digit NAICS codes after that. Average wages are calculated as total payroll in Q1, divided by the total number of workers at the end of Q1.

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

We verify the accuracy our text extraction algorithm by manually collecting the wage increase for a random sample of 500 contracts and comparing the wage variable to the algorithmically collected one. We remove observations from the sample if the change in the first-year wage is higher than 100% or lower than −100%. Also, we remove observations from the sample if the contract length exceeds ten years, which is very rare. Finally, we remove states that introduced a RTW law prior to 1988, which is the beginning of our sample period. This leaves us with a final sample of 15,125 wage contracts.

The difference-in-differences regression specification to estimate the effect of RTW laws on wage growth is

$$\Delta \log(w_{t,s,e,u,i}) = \alpha + \beta RTW_{s,t} + \gamma \Delta GSP_{s,t} + f_t + f_i + f_s + \epsilon_{t,s,e,u,i}, \tag{1}$$

where the unit of observation is a contract, indexed by year t, state s, employer e, union u, and 2-digit SIC industry i. RTW is a dummy variable, defined at the state-year level, which identifies the treated observations. We will use different versions of this dummy variable, depending on the particular test. The simplest version of the dummy variable takes a value of one in the year when a state introduces a RTW law, and a value of zero in all other state-years. Standard errors are clustered at the state level.

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

The regression in equation (1) includes the variable $\Delta GSP_{s,t}$, which is real growth rate of the gross state product of state s in year t. We add this control variable because we believe that RTW laws are not introduced randomly in certain states. It is quite plausible that state policy makers try to boost the local economy by introducing such a law. Therefore, a weak economic environment might increase the likelihood of the introduction of the law. Since the economic environment is also an important determinant of wage growth, it is important to control for it. To calculate $\Delta GSP_{s,t}$, we use the nominal gross state products provided by the Bureau of Economic Analysis, and convert them to real annual growth rates using the GDP implicit price deflator from the Federal Reserve Bank of St. Louis website.

Equation (1) also includes year fixed effects, f_t, industry fixed effects, f_i, and state fixed effects, f_s. It should be noted that the dependent variable is $\Delta \log(w)$, not $\log(w)$, so time-invariant differences in wage levels across states are already controlled for. Therefore, equation (1) 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 both average wage levels and average wage growth rates vary across states. Similarly, industry fixed effects are not strictly necessary for a difference-in-differences estimation. We include them to control for differences in wage growth across industries. This rules out the possibility that jobs in RTW states migrate from high-wage to low-wage industries around the time of the introduction of the law. Such a change in the distribution of jobs across industries might bias the estimation of the treatment effect if one did not control for industry fixed effects.

Table II 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 is 2.9% in our sample. These growth rates in in nominal terms. The table also shows that there are many more treated than control observations. For the purpose of this table, a treated observation is defined as a CBA that covers workers in a RTW state and has an effective date in or after the year of the introduction of the law. There are two reasons why there are relatively few treated observations. First, only five states introduce a RTW law during our sample period. Second, most of the RTW introductions occur towards the end of our sample period. To rule out the possibility that the low ratio of treated to control observations affects our results, we include a robustness test where we drop all non-RTW states from the sample, which does not materially affect our results.

Table II already reveals that, in a simple univariate comparison, average wage growth in the treated subsample (1.3%) is lower than average wage growth in the control subsample (2.9%). Finally, also shown in Table II, 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). To alleviate concerns that our results are driven by the public sector and do not hold in the private sector, we perform separate tests on both subsamples.

We provide additional summary statistics tables in the appendix. Table A1 shows how the sample is distributed across states. It contains fewer than 50 states, because we drop those states from the sample that introduced a RTW law prior the beginning of our sample period. The table reveals one of the caveats of the Bloomberg BNA dataset, which is that some states have more observations than others. In particular, some of our treated states like Oklahoma and West Virginia have very few observations. There are multiple reasons for this: The coverage of Bloomberg BNA varies across states, unions are more common in some states than others, and some states have much bigger 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 might also be explained by relatively high inflation in the late 1980s and early 1990s.

Table A3 breaks down the sample by 2-digit SIC codes. The number of observations varies strongly across industries. This is because collective bargaining is much more prevalent in some industries compared to others. For example, there are a lot of observations in the construction, food, local transit, communications, electric services, food stores, health services, and education industries as well as in the public sector. While these differences in coverage are to be expected, they also illustrate one of the caveats of our sample.

IV. The effect of RTW laws on workers

A. *The effect on wages*

To examine the effect of RTW laws on wages, we estimate equation (1) using a RTW dummy variable that takes a value of one in the year when a state introduces a RTW law, and a value of zero in all other years. We denote this variable RTW^0. Table III summarizes the results of different specifications, subsequently adding more fixed effects in columns (1) through (4). Most importantly, the coefficient of RTW^0 is negative and significant at the 1% level in all columns. This is true even in the most conservative specification in column (4), 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 a RTW law by 1.9 to 0.6 percentage points. Even the most conservative coefficient of -0.6 percentage points represents a 20.7% reduction in wage growth relative to the unconditional mean of 2.9%.[3] Also, since all of these growth rates are in nominal terms, the effect of RTW laws on real wage growth is even larger.

In Table IV we estimate the same regression specifications, but with a different definition of the RTW dummy. This dummy, denoted simply as RTW, takes a value of one in the year a state introduces a RTW law, and continues with that value for all subsequent years in that state. We can see in Table IV 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. However, we know from Table III that there is a temporary effect on wage growth, which can very well lead to a permanent effect on wage levels, as illustrated in Figure 1. Unfortunately, for the vast majority of CBAs we only observe wage growth, but not the level of wages, which means we cannot test directly whether there is a permanent effect on wage levels.

To see the exact timing of the impact of RTW laws on wage growth and to test the parallel trends assumption, we perform spline regressions where we split up the RTW dummy variable

[3]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 (4), 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, Chen, Wang, Zhang and Zhang (2017), among others.

into five separate dummies. We have a dummy variable for all years up to two years prior to the law's introduction, a dummy for two years, one for the year of the law, one for the year after the law, and a dummy for two years after the passage of the law. We denote these variables as $RTW^{<(-2)}$, RTW^{-2}, RTW^{0}, RTW^{+1}, and RTW^{+2}, respectively. The year prior to the introduction of the law is omitted so that it serves as the reference year. We drop treated observations which occur later than two years after the introduction of the law.

Table V contains the estimation results of our spline regressions. The coefficients of the variables $RTW^{<(-2)}$ and RTW^{-2} allow us to test the parallel trends assumption, which is important in any difference-in-differences estimation. In columns (1) to (3) of Table V, some of these coefficients are significant, which suggests that the parallel trends assumption might be violated. Another possible interpretation is that the passage of the law is anticipated, with pre-treatment effects on wage growth. To be conservative, we will focus on column (4), which includes state fixed effects, and where none of the pre-treatment coefficients are significant. In this column, the coefficient of RTW^{0} is negative and highly significant, suggesting that RTW laws reduce wage growth in the year of the law's passage. The magnitude of the effect is 0.3 percentage points, which is a bit smaller than the treatment effect in Table III. Interestingly, the coefficients of RTW^{+1} and RTW^{+2} are negative but insignificant. The most 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 RTW^{+1} and RTW^{+2} has low power. This can happen if there are very few CBAs after the introduction of a RTW law. We present some evidence to support this latter interpretation in Section IV.B.

So far we have shown that RTW laws reduce wage growth, but we have not distinguished between different types of CBAs. For example, it is conceivable that the treatment effect is different for private sector workers compared to employees in the public sector. To test this hypothesis, we split the sample into a private sector, where the two-digit SIC code is lower than 90, and a public sector, where the the SIC code is equal to or higher than 90. We then repeat the same regressions as in Table III. The first three columns of Table VI provide the results for the private sector observations, and shows that the coefficient of RTW^{0} is negative and significant across all specifications. The magnitudes of the coefficients are comparable to Table III. Similarly, columns (4)-(6) of Table VI present the results for the public sector contracts. The effect of RTW laws on wage growth is still negative, and is highly significant in columns (4) and (5), but is insignificant in the sixth column. Given that, according to the spline regressions, the specification in column (6) is the only one where the parallel trend assumption is not violated, one should be careful with the interpretation of the result in

Table VI. To be conservative, we focus on the results in column (6), which suggests that there is no effect of RTW laws on public unions.

To summarize, our results are consistent with the view that RTW laws have a significant negative effect on wage growth immediately around the introduction of the law. While there is not permanent effect on wage growth, the results suggest a permanent negative effect on wage levels. Also, the results seem to be mostly driven by private sector CBAs.

B. The effect on union strength

According to our story, 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 that they are less able no negotiate large wage increases for their members. If our story is true, we should see a reduction in union strength after the adoption of these laws. Unfortunately, union strength is not directly observable. Therefore, we use two proxies for it: the number of CBAs for each state-year, and the average union membership rate at the state-year level.

To calculate the number of CBAs, we use the same Bloomberg BNA data as in our previous tests, and count the number of observations for each state-year. We then use this as the dependent variable, and regress it on a RTW dummy, similarly to equation (1). Our regression controls for GSP growth as well as year and state fixed effects. As before, 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, as in IV. Following our story, we would expect the coefficient of the RTW dummy to be negative. The interpretation would be that at some firms, the strength of unions is reduced by so much that the unions are no longer able to negotiate a contract with the firms, and the firms becomes effectively de-unionized. The results of this difference-in-differences estimation are presented in Table VII. Column (1) contains no fixed effects, while columns (2) and (3) add year and state fixed effects, respectively. In the most conservative specification, in column (3), the coefficient of the RTW dummy is negative and significant at the 5% level. The point estimate is -7.75, which is quite large compared to the (unreported) average number of CBAs per state-year of 17.9. This suggests that, compared to the unconditional average, RTW laws reduce the number of CBAs by almost a half.

This result is interesting for two reasons. First, it confirms our story that RTW laws reduce wage growth through their effect on union strength. Second, it suggests that the treatment effects in Tables III to VI underestimate the true effect of RTW laws on wage growth. This is because it is quite plausible that reduction in wage growth after RTW 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 stay unionized, the estimated treatment effect will be biased towards zero. In other words, RTW laws might reduce wage growth be even more than our estimates suggest.[4]

The second proxy for union strength is the average union membership rate at the state-year level. To calculate this variable, we use the data from unionstats.com, which is explained in detail in Hirsch and MacPherson (2003). For each state-year, we calculate the average union membership rate, and denote this variable as *UnionMembership*. We also calculate the same variable separately for private and public sector unions. The state-level union membership data goes back to 1983, which allows us to expand the sample period to 1983–2016. This has the benefit of adding Idaho, which introduced RTW in 1986, to the list of treated states (see Table I).

We estimate an analogous regression specification to equation (1), where the dependent variable is *UnionMembership*, and the main explanatory variable is a RTW dummy. We estimate two versions of this regression, one with a simple RTW dummy, denoted RTW^0, and one with a permanent dummy variable, denoted RTW. According to our proposed mechanism, RTW laws should reduce union strength by decreasing union membership rates, so we expect the RTW dummies to have negative coefficients. The regressions control for GSP growth and year and state fixed effects. Standard errors are clustered at the state level.

Table VIII, column (1), presents the estimation results of the union membership regressions using the simple RTW dummy, and Table IX, column (1), presents the results employing the permanent RTW dummy. While both RTW coefficients are negative, only the simple RTW dummy is statistically significant. The point estimate of the significant coefficient is -2.03 percentage points, which is a substantial reduction compared to the (untabulated) unconditional average membership rate of 16%. This suggests that RTW laws have at least a temporary negative effect on union membership rates.

Column (2) of Table VIII and column (2) of Table IX estimate the effect of RTW laws on private sector unions. Again, the both coefficients are negative, but only the coefficient of the RTW^0 dummy is significant. RTW laws reduce union membership in the private sector by -1.75 percentage points, which is quite large compared to the average private sector union membership rate of 10.75% (untabulated). Finally, column (3) of Tables VIII and IX estimates the effect on union membership in the public sector. Somewhat differently to columns (1) and (2), it is now the coefficient of the permanent RTW dummy, in Table IX, that is statistically significant. The point estimate is -4.88 percentage points, which can be compared to an average union membership rate in the public sector of 41.78%. Please note

[4]We would like to thank Gerard Hoberg for this insight.

that, unconditionally, union membership is higher in the public sector than in the private sector, which is the most likely explanation for why the coefficients column (3) are higher than in column (2), in both Tables VIII and IX.

To conclude, we have shown that RTW introductions substantially reduce the number of CBAs and union membership rates. To the extent that these two variables are good proxies for union strength, 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.

C. *The effect on the average worker*

So far we have shown that RTW laws reduce wage growth for unionized workers, and that these laws reduce union strength. However, one concern with these findings might be that these findings are irrelevant for the average worker in the US, since only a small fraction of the workforce is unionized. In order to add anything to the discussion about the determinants of the decline in US wage growth, which is what we mentioned at the beginning of this paper, we need to show that the decline of unions strength has a meaningful effect on the average worker.

We will use a back-of-the-envelope calculation to estimate the effect on the average worker, using the point estimates from the CBA data. The most conservative specification in Table III suggests that there is a one-year effect of RTW laws on wage growth of -0.6%. What makes the estimation of the effect on the average worker difficult is that we do not observe the effect on workers that are not covered by CBAs. Therefore, we have to make a strong assumption on what that effect might be.

One such strong assumption might be that RTW laws have no effect on non-unionized workers at all. Under that assumption, the effect on the average worker could be calculated as -0.6% times the average union coverage rate. Using the data at unionstats.com, we calculate that the average union coverage rate in the US for all wage and salary workers over 1988–2016 is 15.0%.[5] Please note that the coverage rate is slightly higher than the union membership rate, because it includes employees who are covered by a CBA but are not members of a union. It follows that the effect on the average worker is $-0.6\% \times 15\% = -0.09\%$.

The opposite strong assumption would be that the effect of RTW laws on workers not covered by CBAs is the same as for covered workers. Under that assumption, the effect on the average worker would be -0.6%. Most likely, the true effect is somewhere between -0.09% and -0.6%. While these effects seem small, one should keep in mind that these are

[5]The data can be found here: `http://unionstats.gsu.edu/All-Wage-and-Salary-Workers.xls`

nominal effects. Average nominal wage growth in our sample is 2.9%, and average inflation over 1988–2016, using the Consumer Price Index for all urban consumers, is 2.6%, so real wage growth is relatively low to begin with. Therefore, our calculation, while very simplistic, suggests that the reduction in wage growth caused by RTW laws for approximately one year is a substantial fraction of real wage growth.

In summary, our simple calculation suggests that RTW laws have an effect on the average worker in the economy, not just on those workers that are covered by a CBA. This is important, because covered workers represent a relatively small subset of US workers, who are clustered in certain industries. Additionally, it is important because it means that RTW laws and, more broadly, the weakening of unions, might have contributed to the overall decline in wage growth in recent decades in a way that is different from existing explanations such as globalization or automation.

V. Right to work introduction and firm impact

A. Data and methodology

We obtain firm location and accounting data from the Compustat fundamental annual file from 1950 to 2014. We then match firm headquarters to counties by converting headquarters ZIP Codes to FIPS county codes using a link file provided by the U.S. Census Bureau.[6] RTW data are compiled from the National Right to Work Legal Defense Foundation, Inc. There are twelve states that 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), and Michigan (2013). Observations originating from states that introduced RTW legislation before 1950 are dropped from the sample. From the wage study, we notice the impact of RTW is more transitory rather than permanent. As a result, 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 use a difference-in-differences approach to estimate the effect of labor constraints. Our treatment group consists of firm-year observations in RTW states after the law was

[6]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.

introduced, and the control group consists of firm-year observations in RTW states prior to the law was introduced as well as all firm-year observations in states that never introduced RTW during the sample period. Using the enactment of RTW laws as a proxy for the relaxation of a treated firm's labor constraints, we compare firm-level characteristics before and after the RTW treatment across states with and without the law. Using a dummy variable to denote all firm-year observations in a state that has passed the legislation, we estimate the following baseline regression:

$$Y_{i,j,t} = \beta RTW_{j,t} + Controls_{i,t-1} + \lambda GDP\ Growth_{j,t} + f_i + f_t + \epsilon_{i,j,t}, \tag{2}$$

where Y stands for the dependent variable of interest. The main dependent variables are investment (CAPX) scaled by assets, employment growth rate, operating income (OIBDP) over assets, as well as book leverage. The subscripts stand for firm i, state j, and year t. *RTW* is a dummy variable that is set to 1 if a firm-year observation belongs to a state that has passed RTW legislation during or before the observation year. *Controls* are firm-level characteristics including the log of assets, Tobin's q, cashflow, leverage, profitability, and asset tangibility. All the control variables are lagged by one period. *GDPGrowth* is the growth rate of state-level real GDP, and f_i and f_t denote firm and year fixed effects, respectively. We estimate this equation by clustering standard errors at the state and year level.

In our baseline specification in equation (2), we implicitly assume that RTW laws are introduced randomly. However, it is possible that this assumption may not hold in reality. Perhaps RTW laws are introduced after periods of weak local economic growth, potentially in an effort to boost the local economy. Therefore, we control for the local economic environment, as proxied by state-level GDP growth. Without this control, the estimation of the causal effect of the RTW dummy might be biased. Roberts and Whited (2011) show that even if the assignment of the treatment dummy is not completely random, controlling for the determinants of the treatment dummy will restore the ability of the difference-in-differences estimator to measure the treatment effect.

We screen out observations with equity value totaling less than \$10 million, as well as book equity-to-market equity ratio less than 0.01 or greater than 100. We also restrict return on equity (ROE) to be greater than -100%. Observations with a CAPX-to-PPE ratio greater than 50% are eliminated to rule out mergers and acquisitions. We drop 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. Lastly, we use the Whited and Wu (2006) index as a measure of financial constraints, and all observations are required to have a non-missing Whited-Wu index value to be included in the final sample.

B. Investment, hiring, profitability, and leverage

B.1. Difference-in-differences estimation

Table X presents the results of the baseline regression in equation (2). The dependent variable in columns (1) to (4) are investment over assets, employment growth, profitability and leverage. All regression specifications contain firm fixed effects and year fixed effects plus firm-level controls (lagged assets, lagged Tobin's q, lagged cashflow, lagged book leverage, lagged profitability and lagged tangibility) as well as state-level GDP growth. The coefficient of the RTW dummy is positive and significant at the 5% level in columns (1) and negative and significant in column (4). Investment as a share of total assets is 0.83% higher in the six years during and after RTW introduction, while debt as a share of total assets declines, on average, by 1.15% in the same window. These results suggest that RTW adoption leads to more firm investment and lower leverage.

The panel regression results in Table X demonstrate the treatment effect of the RTW law on firm characteristics, assuming that the treatment effect starts at the time the law is introduced and lasts for five years after. The results do not provide any insight on the timing of the law changes in relation to when they actually impact firm decisions. To get a sense for the lead-lag relation between the enactment of RTW laws and when the effects of these laws are realized, we employ spline regressions to examine the dynamic interaction between RTW laws and firm characteristics in Section V.B.3.

B.2. Labor constraints vs. financial constraints

In the baseline firm-level regression of equation (2), we do not explicitly control for financial constraints. However, it is possible that firms are not investing optimally because they are financially constrained prior to the RTW law implementation, and the law simply alleviates financial constraints so average investment after law enactment is higher. If this is the case, then we should see an insignificant effect of an RTW law on investment for financially unconstrained firms, and a positive effect for constrained firms. To test this hypothesis, we extend the regression specification of equation (2) to the following:

$$\begin{aligned} Y_{i,j,t} &= \beta RTW_{j,t} + \gamma FCDummy_{i,t-1} + \omega FC_{i,t-1} \times RTW_{j,t} \\ &+ Controls_{i,t-1} + \lambda GDP\ Growth_{j,t} + f_i + f_t + \epsilon_{i,j,t}, \end{aligned} \tag{3}$$

where *FC Dummy* is an indicator variable denoting a firm-year observation for a firm that is financially constrained and $FC \times RTW$ is the interaction term between the *RTW* dummy and the *FC Dummy*. Financial constraint is defined by the Whited and Wu (2006) index. For each year, we sort firms based on the WW index into four bins and label the top quartile constrained, the bottom quartile unconstrained, and the middle two quartiles mid-constrained. *FC Dummy* is equal to 1 for the constrained quartile and is 0 everywhere else. We also use a *Mid Dummy* to encompass the middle two bins based on the WW index. *Mid Dummy* and its interaction with *RTW* are also included in the regression in equation (3) but are not reported. Therefore, the coefficient loadings γ and ω are both relative to the unconstrained quartile.

Table XI presents the results of the regression with financial constraint dummies outlined in equation (3). The columns represent regression results with different dependent variables. All regressions contain firm fixed effects, year fixed effects, firm-level controls, and state-level GDP growth. There are three observations. First, the β coefficients on the *RTW* dummy is positive and statistically significant at the 5% level after controlling for financial constraints only in column (1) for investment. The economic significance of the *RTW* dummy increases in the presence of the financial constraint dummy: magnitudes of β are larger in Table XI than in Table X, suggesting the rise in investment is mainly driven by unconstrained firms. Second, the *FC Dummy* by itself is statistically significant across all columns. The γ coefficients are significant at the 1% level. In general, financially constrained firms have lower investment, smaller employment growth rate, lower operating income and take on more debt. This is consistent with the literature on the effect of financial constraints on firm characteristics. Third, the coefficient of the interaction term $FC \times RTW$ is negative and statistically significant at the 1% level for investment in column (1), while positive and strongly significant for leverage in column (4). This means that the positive impact of RTW on investment is much weaker for financially constrained firms than for unconstrained ones. On the other hand, RTW adoption does alleviate some level of financing pressure allowing these constrained firms to borrow more.

To understand the absolute impact of a RTW law on financially constrained firms as opposed to the difference to unconstrained firms, one can add up the slope coefficients on *RTW* and $FC \times RTW$ in Table XI for each of the columns (1)-(4). Roughly speaking, the sums of the β and ω coefficients are small or close to zero except for leverage in column (4). This implies that a RTW law has very limited impact on the investment and hiring activities for those firms that are financially constrained. To conclude, we do not find evidence that a RTW law relaxes financial constraint and therefore leads to higher investment. Instead, our results are consistent with the view that it is the financially unconstrained firms that benefit from the loosening of labor constraints stemming from the passage of RTW laws.

B.3. Dynamic firm impact of right to work

Spline regressions are performed to examine the timing of the effect of RTW introduction on firms. To the extent that RTW laws are touted as pro-business legislation when the economy is lagging, it is important to understand if their effectiveness is instantaneous or delayed. We assign yearly RTW dummies to firm-year observations in the five year window before and after each RTW introduction. A RTW 0 dummy is assigned to observations during the year of implementation, and a RTW < -5 dummy is assigned to all observations prior to the pre-RTW 5-year window. All observations in non-RTW states and observations in the year immediately before RTW introduction (RTW -1) are in the control group. Finally, the same control variables and fixed effects are employed as the firm-level difference-in-difference regressions. To estimate the spline, we combine all firm-year observations and run a pooled regression:

$$\begin{aligned} Y_{i,j,t} \;&=\; \sum_{k=2}^{<5} \Phi_k RTW_{j,t}(-k) + \beta RTW_{j,t}(0) + \sum_{k=1}^{5} \Psi_k RTW_{j,t}(+k) \\ &\quad + Controls_{i,t-1} + \lambda GDP\ Growth_{j,t} + f_i + f_t + \epsilon_{i,j,t}, \end{aligned} \tag{4}$$

where Φ, β, and Ψ are coefficient loadings on the RTW dummies. Notice that we drop *RTW* -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.

Results of the spline regressions are presented in Table XII where the dependent variables, in order, are investment, employee growth, profitability, and leverage. To ensure the spline regression are valid, we check the statistical significance of coefficient loadings on the RTW dummies before RTW laws are implemented. In Table XII, none of the estimated coefficients are statistically significant at the 10% level or below before RTW 0 across all columns, suggesting the parallel trend condition is not violated. In column (1), investment scaled by total asset is higher, relative to the control group, 3 and 5 years after RTW adoption. This is evident by the positive and significant coefficient loadings on the RTW +3 and RTW +5 dummies. In column (2), employee growth rate is significantly higher in year 0 of RTW introduction and stays flat in the following 5 years. In column (3), none of the estimated coefficient is statistically significant, implying the adoption of RTW legislation has no dynamic effect on firm profitability. In column (4), book leverage is on average lower in the fourth and fifth year after RTW introduction relative to the year immediately prior. The de-leveraging in RTW +4 is especially strong as leverage drops by 2.87% and statistically significant at the 1% level. Overall, implications of the spline regressions are consistent with the difference-in-difference regressions: RTW adoption allows firms to invest more, hire more employees, and borrow less. However, the impact of RTW has an average delay of three to five years on investment and book leverage.

VI. Robustness tests

A. *What predicts the introduction of RTW laws?*

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

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

We use Carl Klarner's political data repository for data on governors up to 2010, and manually extend the data up to 2016.[7] The *Governor democrat* variable takes the value of 1 if the governor is a democrat and 0 in the case of a republican. Independent governors are coded as 0.5.

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

Second, we use state-level imports from China as a proxy for the effect of globalization. The data is from the U.S.A. Trade Online database (State of Destination) of the Census Bureau. We scale this variable by the nominal gross state product. Third, we include the state-level union membership rate as a proxy for union strength in the regressions. The real growth rate in the gross state product is also incorporated as a predicting variable. Other variables are the change in the union membership rate over the previous five years, and the annual change in the imports from China. Due to the limitations of the trade data, which is only available at the state level from 2008, we perform the predictive regressions from 2008 to 2017, which allows us to capture the five most recent RTW introductions in the BNA sample period (Indiana, Michigan, Wisconsin, West Virginia, and Kentucky). All predictors are lagged by one year. The dependent variable is RTW^0, the dummy variable that indicates the year of the law's introduction. For the five treated states, we remove observations after the introduction of the law. Also, as in our other regressions, we remove RTW states that introduced the law before the beginning of the sample period, which is 2008 in this case.

For brevity, regression results are documented in Table A4 of the Appendix. It shows that the political orientation of the governor is an important predictor of RTW laws. That variable is statistically significant in all columns. RTW legislation is more likely to be passed when the governor is republican. Interestingly, the other variables are not statistically significant. Therefore, it does not seem likely that globalization or union strength are responsible for RTW laws, although we cannot rule out that possibility.

In Table XIII, we estimate our base case specification from equation (1), but controlling for the *Governor democrat* variable. The table shows that the governor's party affiliation is significant in the most stringent specification, in column (4). A democratic governor has a positive but small effect on wage growth, with a coefficient of 0.001. Most importantly, the coefficients of the RTW^0 dummy are very similar, both in magnitude and in significance, to Table III. This suggests that our results are not driven by the most obvious predictors of RTW laws.

B. Using only RTW states

One of the potential concerns with our difference-in-differences specification in equation (1) is that non-RTW states are not a good control group for those states that introduce a RTW law. Also, one might criticize the addition of non-RTW states to the sample as an artificial increase in the sample size, which could lead to an excessive reduction of standard errors. To alleviate these concerns, drop all non-RTW states from the sample, and repeat the regressions from Table III, but only using observations from the five RTW states. In the resulting difference-in-differences regressions, the control group consists solely of observations in RTW states, but before the introduction of the RTW law.

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

C. Drop multistate observations from the sample

As explained in Section III, the raw CBA dataset contains some observations where the contract covers workers in multiple states. Since the state variable in the raw data contains the value 'multistate' for these observations, we split each of these observations into multiple observations, using information in the city variable. This raises the potential concern that the treatment of the multistate contracts artificially inflates the sample size, of that multistate contracts affect the estimation in some other special way. To account for this possibility, we remove all multistate observations from the sample, and re-estimate the regressions in Table III using the smaller sample.

Table XV shows that the resulting regression results are very similar to the results in Table III. The RTW coefficients are negative and highly significant in all specifications. The coefficient magnitudes are similar as well. These findings are not that surprising, since the sample size has only dropped a little bit, from 15,125 to 14,066, relative to Table III.

We conclude by noting that our results on the negative effect of RTW laws on wage growth around the introduction of RTW laws is quite robust. In particular, it is not driven by the composition of the control states, or by the addition of 'multistate' observations to the sample.

VII. Conclusion

The main hypothesis in this paper is that the decline of union strength in recent decades in the US has contributed to the decline in wage growth of middle-income workers, and that RTW laws can be viewed as negative shocks to union strength. While the strength of unions is not easily measurable, we provide indirect empirical evidence that is consistent with this hypothesis. The introduction of RTW laws reduces wage growth for workers that are covered by collective bargaining agreements. These laws also reduce the number of existing CBAs, as well as average union membership rates, which suggests that the effect on wage growth occurs through the union channel. We also show that the effects of RTW laws on firms mirror the effects on workers. In particular, firms increase investment and employment, and reduce their use of strategic leverage, which are all consistent with a shift in bargaining power from workers to firms.

One should be careful when it comes to the welfare effects or the policy implications of our findings. Our findings cannot be interpreted in a way that RTW laws reduce aggregate welfare. On the one hand, our results suggest that the effect of RTW laws on the welfare of those workers who are already employed is likely negative. This comes both from a reduction in their wages and from a potential increase in income inequality, since workers covered by collective bargaining are more likely to work in middle-income occupations (e.g., Card et al. (2004)). On the other hand, there are also positive effects of RTW on aggregate welfare. For example, Holmes (1998) shows that the introduction of these laws creates higher employment, especially in manufacturing.

Even if the aggregate welfare effect of RTW laws were negative, it is difficult to assess the optimal policy response. For example, it is not clear whether the reversal of RTW laws is the optimal response. Other policy measures, such as employment protection legislation (EPL), higher minimum wages, stricter antitrust legislation, a revenue-neutral increase in the slope of the marginal income taxes, among others, could potentially offset the negative welfare effect more efficiently.

REFERENCES

Acemoglu, Daron and David H Autor, "Skills, Tasks and Technologies: Implications for Employment and Earnings," in "Handbook of Labor Economics," Elsevier, 2011.

_ , _ , David Dorn, Gordon H Hanson, and Brendan Price, "Import Competition and the Great US Employment Sag of the 2000s," *Journal of Labor Economics*, 2016, *34* (S1), S141–S198.

Autor, David H, "Why Are There Still So Many Jobs? The History and Future of Workplace Automation," *Journal of Economic Perspectives*, 2015, *29* (3), 3–30.

_ , David Dorn, Gordon H Hanson, and Jae Song, "Trade Adjustment: Worker-Level Evidence," *Quarterly Journal of Economics*, 2014, *129* (4), 1799–1860.

Bradley, Daniel, Kim, Incheol, and Xuan Tian, "Do Unions Affect Innovation?," *Management Science*, July 2017, *63* (7), 2251–2271.

Bronars, Stephen G and Donald R Deere, "Unionization, Incomplete Contracting, and Capital Investment," *The Journal of Business*, January 1993, *66* (1), 117–132.

Brynjolfsson, Erik and Andrew McAfee, *Race against the machine*, Digital Frontier Press, 2011.

Card, David, "The Effect of Unions on Wage Inequality in the U.S. Labor Market," *ILR Review*, 2001, *54* (2), 296–315.

_ , Thomas Lemieux, and W Craig Riddell, "Unions and wage inequality," *Journal of Labor Research*, 2004, *25* (4), 519.

Carroll, Thomas M, "Right to Work Laws Do Matter," *Southern Economic Journal*, October 1983, *50* (2), 494–509.

Chang, Xin Simba, Yangyang Chen, Sarah Qian Wang, Kuo Zhang, and Wenrui Zhang, "Credit Default Swaps and Corporate Innovation," *SSRN Electronic Journal*, September 2017.

Cyrille, Schwellnus, Andreas Kappeler, and Perre-Alain Pionnier, "Decoupling of Wages From Productivity: Macro-Level Facts," *https://www.oecd.org/eco/Decoupling-of-wages-from-productivity-Macro-level-facts.pdf*, 2017.

Fallick, Bruce C and Kevin A Hassett, "Investment and Union Certification," *Journal of Labor Economics*, July 1999, *17* (3), 570–582.

Garofalo, Gasper A and Devinder M Malhotra, "An integrated model of the economic effects of right-to-work laws," *Journal of Labor Research*, September 1992, *13* (3), 293–305.

Gordon, Robert J, "The Demise of U.S. Economic Growth: Restatement, Rebuttal, and Reflections," Technical Report, National Bureau of Economic Research, Cambridge, MA, Cambridge, MA February 2014.

Henderson, J Vernon and Yukako Ono, "Where do manufacturing firms locate their headquarters?," *Journal of Urban Economics*, 2008, *63* (2), 431–450.

Hirsch, Barry T, "Firm Investment Behavior and Collective Bargaining Strategy," *Industrial Relations: A Journal of Economy and Society*, January 1992, *31* (1), 95–121.

_ **and David A MacPherson**, "Union Membership and Coverage Database from the Current Population Survey: Note," *ILR Review*, 2003, *56* (2), 349–354.

Holmes, Thomas J, "The Effect of State Policies on the Location of Manufacturing: Evidence from State Borders," *Journal of Political Economy*, August 1998, *106* (4), 667–705.

Hundley, Greg, "Collective Bargaining Coverage of Union Members and Nonmembers in the Public Sector," *Industrial Relations: A Journal of Economy and Society*, January 1993, *32* (1), 72–93.

Klasa, Sandy, William F Maxwell, and Hernán Ortiz-Molina, "The strategic use of corporate cash holdings in collective bargaining with labor unions," *Journal of Financial Economics*, May 2009, *92* (3), 421–442.

Krugman, Paul, "Trucking And Blue-Collar Woes," May 2017.

Lemieux, Thomas, "Estimating the Effects of Unions on Wage Inequality in a Panel Data Model with Comparative Advantage and Nonrandom Selection," *Journal of Labor Economics*, 1998, *16* (2), 261–291.

Matsa, David A, "Capital Structure as a Strategic Variable: Evidence from Collective Bargaining," *The Journal of Finance*, May 2010, *65* (3), 1197–1232.

Moore, William J, "Membership and wage impact of right-to-work laws," *Journal of Labor Research*, June 1980, *1* (2), 349–368.

_ , "The determinants and effects of right-to-work laws: A review of the recent literature," *Journal of Labor Research*, June 1998, *19* (3), 445–469.

_ **, James A Dunlevy, and Robert J Newman**, "Do Right to Work Laws Matter? Comment," *Southern Economic Journal*, October 1986, *53* (2), 515–524.

Roberts, Michael R and Toni M Whited, "Endogeneity in Empirical Corporate Finance," *SSRN Electronic Journal*, 2011.

Serfling, Matthew, "Firing Costs and Capital Structure Decisions," *The Journal of Finance*, September 2016, *71* (5), 2239–2286.

Simintzi, Elena, Vikrant Vig, and Paolo Volpin, "Labor Protection and Leverage," *Review of Financial Studies*, January 2015, *28* (2), 561–591.

Summers, Lawrence H, "America needs its unions more than ever," 2017.

Wessels, Walter J, "Economic effects of right to work laws," *Journal of Labor Research*, March 1981, *2* (1), 55–75.

Western, Bruce and Jake Rosenfeld, "Unions, Norms, and the Rise in U.S. Wage Inequality," *American Sociological Review*, August 2011, *76* (4), 513–537.

Whited, Toni M and Guojun Wu, "Financial constraints risk," *Review of Financial Studies*, 2006, *19* (2), 531–559.

Yi, Irene, "Slashing Liquidity Through Asset Purchases: Evidence from Collective Bargaining," *SSRN Electronic Journal*, 2016.

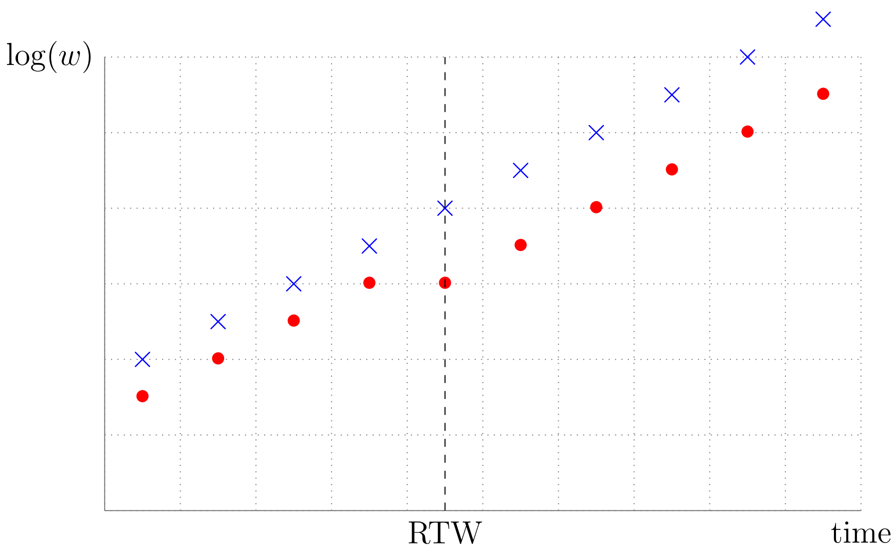

Figure 1. Stylized plot illustrating the identification strategy

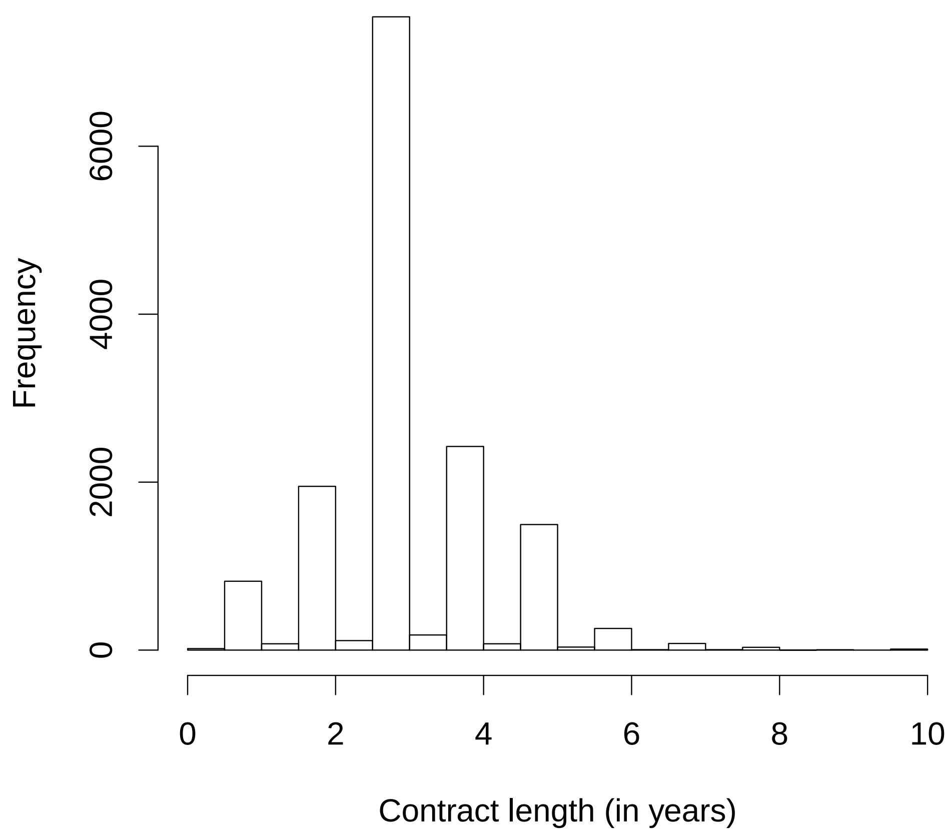

Figure 2. Histogram of the length of collective bargaining agreements

Table I

Summary Statistics of State Right-to-Work Laws in the US

This is a list of states in the US that have passed the 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 legislation became effective. This is 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					

Table II
Summary Statistics for Change in Log Wage

This table presents summary statistics for log wage growth in the Bloomberg BNA data. The first row is the entire sample. The second and third rows separate collective bargaining agreements (CBAs) negotiated in a Right-to-Work (RTW) state from those negotiated in a non-RTW state. The fourth and fifth rows distinguish CBAs negotiated at a public sector establishment from those negotiated at a private sector establishment. Each count in column (1) represents a contract agreement. Column (3) is the standard deviation. Column (5) is the 25th percentile. Column (6) is the 50th percentile. Column (7) is the 75th percentile.

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

Table III

The effect of RTW laws on wage growth

This table presents estimation results for the difference-in-differences specification in equation (1). The unit of observation is a collective bargaining agreement (CBA). The sample period is 1988–2016. The dependent variable is the change in the log of wages. The main explanatory variable is RTW^0, a dummy that indicates the year of the introduction of a right-to-work (RTW) law. An additional control variable is the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:			
	$\Delta log(w)$			
	(1)	(2)	(3)	(4)
RTW^0	-0.019^{***}	-0.011^{***}	-0.010^{***}	-0.006^{***}
	(0.001)	(0.001)	(0.001)	(0.001)
GSP growth	0.106^{***}	0.088^{***}	0.075^{***}	0.059^{**}
	(0.027)	(0.030)	(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	0.012	0.151	0.194	0.202

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table IV

The permanent effects of RTW laws on wage growth

This table presents estimation results for the difference-in-differences specification in equation (1). The unit of observation is a collective bargaining agreement (CBA). The sample period is 1988–2016. The dependent variable is the change in the log of wages. The main explanatory variable is *RTW*, a dummy that takes a value of one in the year of the introduction of a right-to-work (RTW) law and in all subsequent years. An additional control variable is the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

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

Table V
Spline regressions: The timing of the effect of RTW laws on wage growth
This table presents estimation results for a modified version of the difference-in-differences specification in equation (1). The unit of observation is a collective bargaining agreement (CBA). The sample period is 1988–2016. The dependent variable is the change in the log of wages. The main explanatory variables are a set of a dummies that indicate when a right-to-work (RTW) law is introduced. RTW^{+2} denotes two years after the introduction of the law, RTW^{+1} denotes one year after the law, RTW^{0} is the year of the introduction, RTW^{-2} is two years prior to the introduction, and $RTW^{<(-2)}$ stands for all years prior to that. An additional control variable is the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:			
	$\Delta log(w)$			
	(1)	(2)	(3)	(4)
$RTW^{<(-2)}$	−0.001	−0.004***	−0.003***	0.003
	(0.001)	(0.001)	(0.001)	(0.002)
RTW^{-2}	−0.023***	−0.009***	−0.008***	−0.002
	(0.002)	(0.002)	(0.002)	(0.001)
RTW^{0}	−0.019***	−0.012***	−0.011***	−0.003***
	(0.001)	(0.001)	(0.001)	(0.001)
RTW^{+1}	−0.013***	−0.007**	−0.007***	−0.001
	(0.003)	(0.003)	(0.003)	(0.002)
RTW^{+2}	−0.015***	−0.009**	−0.009**	−0.002
	(0.003)	(0.004)	(0.004)	(0.003)
GSP growth	0.103***	0.070***	0.059**	0.055**
	(0.027)	(0.027)	(0.025)	(0.026)
Constant	0.027***			
	(0.001)			
Year FE		*Yes*	*Yes*	*Yes*
Industry FE			*Yes*	*Yes*
State FE				*Yes*
Observations	15,026	15,026	15,026	15,026
Adjusted R^2	0.018	0.160	0.203	0.210

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

Table VI
Simple RTW dummy, private vs public sector employees
This table presents estimation results for the difference-in-differences specification in equation (1). The unit of observation is a collective bargaining agreement (CBA). The sample period is 1988–2016. The dependent variable is the change in the log of wages. The main explanatory variable is RTW^0, a dummy that indicates the year of the introduction of a right-to-work (RTW) law. An additional control variable is the growth rate of the gross state product (GSP). Columns (1)–(3) are based on the subsample of private sector CBAs, and columns (4)–(6) are focused on the public sector. Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:					
	$\Delta log(w)$					
	Private	Private	Private	Public	Public	Public
	(1)	(2)	(3)	(4)	(5)	(6)
RTW^0	-0.009^{***}	-0.006^{***}	-0.004^{**}	-0.013^{***}	-0.013^{***}	-0.004
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.004)
GSP growth	0.069^{**}	0.059^{**}	0.046	0.100^{**}	0.100^{**}	0.075^{***}
	(0.032)	(0.029)	(0.031)	(0.040)	(0.040)	(0.027)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE		Yes	Yes		Yes	Yes
State FE			Yes			Yes
Observations	9,604	9,604	9,604	5,521	5,521	5,521
Adjusted R^2	0.118	0.165	0.170	0.185	0.185	0.234
Note:						*p<0.1; **p<0.05; ***p<0.01

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

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

Table VIII
The effect of RTW on union membership rates

	Dependent variable:		
	UnionMembership		
	Total	Private	Public
	(1)	(2)	(3)
RTW^0	-2.029^{**}	-1.746^{**}	-3.497
	(0.972)	(0.688)	(3.306)
GSP growth	3.109	3.364	4.046
	(2.829)	(2.788)	(5.043)
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	1,054	1,054	1,054
Adjusted R^2	0.927	0.908	0.951
Note:			*p<0.1; **p<0.05; ***p<0.01

Table IX
The effect of RTW on union membership rates

	Dependent variable:		
	UnionMembership		
	Total	Private	Public
	(1)	(2)	(3)
RTW	-1.541	-0.900	-4.878^{***}
	(1.160)	(1.133)	(1.883)
GSP growth	3.949	3.848	6.739
	(2.653)	(2.687)	(4.286)
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	1,054	1,054	1,054
Adjusted R^2	0.928	0.908	0.953
Note:			*p<0.1; **p<0.05; ***p<0.01

Table X
The Effect of RTW Laws on Firm Investment, Employment Growth, Operating Profitability and Leverage

This table reports the coefficient estimates of panel regressions by pooling all firm-year observations from 1950 to 2014. 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 employment growth. The dependent variable in column (3) is profitability defined as operating income (oibdp) divided by lagged assets. The dependent variable in column (4) is book leverage, defined as debt in current liabilities plus long-term debt (dlc + dltt) divided by lagged assets. All regressions include controls and both firm and year fixed effects. State-level year-over-year real GDP growth (*GDP Growth*) is the only control variable not measured at the firm level. Robust standard errors with double clustering at the state- and year-level are used in reporting the t-statistics in parentheses. There are a total of $75,065$ firm-year observations.

	(1) Inv/A	(2) EmpGr	(3) OI/A	(4) Debt/A
RTW Dummy	0.00827**	-0.00205	-0.00724	-0.0115**
	(2.14)	(-0.18)	(-0.67)	(-2.43)
LogAsset	-0.00716***	-0.0634***	-0.00117	0.0321***
	(-12.37)	(-16.46)	(-0.29)	(15.19)
Tobin Q	0.00290***	0.0121***	0.00148	-0.000955
	(6.91)	(6.77)	(1.03)	(-1.50)
Cashflow	0.00321**	0.0124**	0.0417***	
	(2.32)	(2.64)	(2.89)	
Leverage	-0.0244***	-0.104***	-0.0506***	
	(-6.79)	(-14.05)	(-7.55)	
GDP Growth	0.0813***	0.154	0.0947**	0.00651
	(3.28)	(1.47)	(2.22)	(0.18)
Profitability				-0.0958***
				(-6.34)
Tangiblity				0.0409***
				(3.15)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.558	0.160	0.693	0.654

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

Table XI
The Effect of RTW Laws and Financial Constraint on Firm Investment, Employment Growth, Operating Profitability and Leverage
This table reports the coefficient estimates of panel regressions by pooling all firm-year observations from 1950 to 2014. The RTW law indicator (RTW) is the main explanatory variable. FC Dummy and its interaction with the RTW Dummy (RTW x FC) are also included. Financial constraint is defined by the Whited and Wu (2006) index. 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 profitability defined as operating income (oibdp) divided by lagged assets. The dependent variable in column (4) is book leverage, defined as debt in current liabilities plus long-term debt (dlc + dltt) divided by lagged assets. All regressions include controls and both firm and year fixed effects. State-level year-over-year real GDP growth ($GDP\ Growth$) is the only control variable not measured at the firm level. Robust standard errors with double clustering at the state- and year-level are used in reporting the t-statistics in parentheses. There are a total of $75,065$ firm-year observations.

	(1) Inv/A	(2) EmpGr	(3) OI/A	(4) Debt/A
RTW Dummy	0.0293***	0.000229	0.0262*	-0.0135
	(3.38)	(0.01)	(1.91)	(-0.81)
FC Dummy	-0.0191***	-0.191***	-0.0888***	0.0471***
	(-10.44)	(-10.99)	(-7.43)	(9.28)
RTW x *FC*	-0.0418***	0.000577	-0.0344**	0.0765***
	(-4.98)	(0.03)	(-2.62)	(4.53)
LogAsset	-0.00944***	-0.0861***	-0.0118***	0.0377***
	(-15.58)	(-19.32)	(-3.39)	(18.27)
Tobin Q	0.00272***	0.0103***	0.000599	-0.000524
	(6.64)	(5.39)	(0.42)	(-0.84)
Cashflow	0.00308**	0.0110*	0.0408***	
	(2.25)	(1.88)	(2.90)	
Leverage	-0.0228***	-0.0884***	-0.0428***	
	(-6.66)	(-12.32)	(-6.05)	
GDP Growth	0.0811***	0.154	0.0932**	0.00634
	(3.26)	(1.50)	(2.28)	(0.17)
Profitability				-0.0927***
				(-6.00)
Tangiblity				0.0430***
				(3.31)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.562	0.182	0.703	0.656

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

Table XII

Dynamic Effect of RTW Laws on Firm Investment, Employment Growth, Operating Profitability and Leverage

This table reports the coefficient estimates of the spline regressions on firm characteristics. 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 ($RTW < -5$). Observations in the one year immediately before the RTW law implementation do not have a RTW dummy and serve as the benchmark. All observations beyond RTW +5 are dropped. 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 profitability defined as operating income (oibdp) divided by lagged assets. The dependent variable in column (4) 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 both firm and year fixed effects. Robust standard errors with double clustering at the state- and year-level are used in reporting the t-statistics in parentheses. There are 75,065 observations.

	(1) Inv/A	(2) EmpGr	(3) OI/A	(4) Debt/A
RTW <-5	-0.000734	0.0100	0.0100	0.00108
	(-0.13)	(0.55)	(0.32)	(0.11)
RTW -5	-0.00126	-0.0239	0.00950	-0.00116
	(-0.54)	(-0.92)	(0.54)	(-0.05)
RTW -4	0.00195	-0.00649	-0.00106	-0.00734
	(0.58)	(-0.21)	(-0.07)	(-0.37)
RTW -3	-0.000301	0.0193	-0.0212	-0.00443
	(-0.03)	(0.93)	(-0.76)	(-0.46)
RTW -2	-0.000516	0.0175	-0.0149	-0.00401
	(-0.08)	(0.40)	(-1.10)	(-0.66)
RTW 0	-0.000261	0.0439**	0.0103	0.00428
	(-0.07)	(2.21)	(1.24)	(0.70)
RTW +1	-0.00153	0.00816	-0.00722	-0.00273
	(-0.34)	(0.18)	(-0.37)	(-0.32)
RTW +2	-0.00364	0.0262	-0.00659	-0.0146
	(-0.41)	(0.68)	(-0.30)	(-1.18)
RTW +3	0.0285*	0.0386	-0.00425	-0.00392
	(1.73)	(0.87)	(-0.28)	(-0.35)
RTW +4	0.00868	-0.0192	-0.00437	-0.0287***
	(0.96)	(-0.66)	(-0.35)	(-3.03)
RTW +5	0.0264*	-0.0172	0.0158	-0.0252*
	(1.86)	(-0.59)	(0.77)	(-1.83)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.558	0.160	0.693	0.654

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

Table XIII
The effect of RTW laws on wage growth, controlling for governorship
This table presents estimation results for the difference-in-differences specification in equation (1). The unit of observation is a collective bargaining agreement (CBA). The sample period is 1988–2016. The dependent variable is the change in the log of wages. The main explanatory variable is RTW^0, a dummy that indicates the year of the introduction of a right-to-work (RTW) law. We control for the political orientation of a state's governor, as well as for the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:			
	$\Delta log(w)$			
	(1)	(2)	(3)	(4)
RTW^0	-0.019^{***}	-0.011^{***}	-0.010^{***}	-0.005^{***}
	(0.002)	(0.001)	(0.001)	(0.001)
Governor democrat	-0.002	0.001	0.001	0.001^{**}
	(0.001)	(0.001)	(0.001)	(0.001)
GSP growth	0.099^{***}	0.086^{***}	0.072^{**}	0.057^{**}
	(0.029)	(0.031)	(0.029)	(0.025)
Constant	0.028^{***}			
	(0.002)			
Year FE		*Yes*	*Yes*	*Yes*
Industry FE			*Yes*	*Yes*
State FE				*Yes*
Observations	14,986	14,986	14,986	14,986
Adjusted R^2	0.013	0.151	0.193	0.202
Note:				*p<0.1; **p<0.05; ***p<0.01

Table XIV

The effect of RTW laws on wage growth, using only RTW states

This table presents estimation results for the difference-in-differences specification in equation (1), using only the subsample of observations in a RTW state. The unit of observation is a collective bargaining agreement (CBA). The sample period is 1988–2016. The dependent variable is the change in the log of wages. The main explanatory variable is RTW^0, a dummy that indicates the year of the introduction of a right-to-work (RTW) law. An additional control variable is the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:			
	$\Delta log(w)$			
	(1)	(2)	(3)	(4)
RTW^0	-0.016^{***}	-0.006^{***}	-0.006^{***}	-0.006^{***}
	(0.001)	(0.001)	(0.002)	(0.002)
GSP growth	0.128^{***}	0.171^{***}	0.199^{***}	0.167^{***}
	(0.027)	(0.036)	(0.040)	(0.039)
Constant	0.023^{***}			
	(0.001)			
Year FE		Yes	Yes	Yes
Industry FE			Yes	Yes
State FE				Yes
Observations	2,278	2,278	2,278	2,278
Adjusted R^2	0.030	0.188	0.238	0.243
Note:				*p<0.1; **p<0.05; ***p<0.01

Table XV

The effect of RTW laws on wage growth, without multistate observations

This table presents estimation results for the difference-in-differences specification in equation (1), using only the subsample of observations where the collective bargaining agreement (CBA) does not cover multiple states. The unit of observation is a CBA. The sample period is 1988–2016. The dependent variable is the change in the log of wages. The main explanatory variable is RTW^0, a dummy that indicates the year of the introduction of a right-to-work (RTW) law. An additional control variable is the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:			
	$\Delta log(w)$			
	(1)	(2)	(3)	(4)
RTW^0	-0.020^{***}	-0.012^{***}	-0.011^{***}	-0.006^{***}
	(0.001)	(0.001)	(0.001)	(0.001)
GSP growth	0.110^{***}	0.092^{***}	0.081^{***}	0.061^{**}
	(0.028)	(0.031)	(0.029)	(0.025)
Constant	0.027^{***}			
	(0.001)			
Year FE		Yes	Yes	Yes
Industry FE			Yes	Yes
State FE				Yes
Observations	14,066	14,066	14,066	14,066
Adjusted R^2	0.013	0.156	0.197	0.206
Note:				*p<0.1; **p<0.05; ***p<0.01

Appendices

Table A1
Summary Statistics for Change in Log Wage Growth Broken Down by State
This table presents summary statistics for log wage growth in the Bloomberg BNA data sorted by state. Collective bargaining agreements (CBAs) are matched to states through the location of the establishment at which contracts are negotiated. Each count in column (1) represents a contract agreement.

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

Table A2
Summary Statistics for Change in Log Wage Growth Broken Down by Year
This table presents summary statistics for log wage growth in the Bloomberg BNA data sorted by year. Collective bargaining agreements (CBAs) are aggregated by the year during which contracts are negotiated. Each count in column (1) represents a contract agreement.

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

Table A3

Summary Statistics for Change in Log Wage Growth Broken Down by Industry

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

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

Table A4

Predicting the introduction of RTW laws.

This table contains the results of predictive regressions for the introduction of right-to-work (RTW) laws. The dependent variable is a dummy that takes the value of one in the year when a RTW law is introduced. The predictors are the political orientation of the state's governor, the ratio of a the state's imports from China to the state's gross state product, the average union membership rate of the state, the growth rate of the gross state product, the change in the state's union membership rate, the change in the ratio of imports from China and the gross state product, and a constant. Columns (1)-(3) contain OLS regressions, and columns (4)-(6) present logistic regressions. All predicting variables are lagged by one year. The sample period is 2008–2017. Observations in RTW states after the introduction of a RTW law are dropped from the sample.

	Dependent variable:					
	RTW^0					
	OLS			*logistic*		
	(1)	(2)	(3)	(4)	(5)	(6)
Governor democrat	-0.050^{**}	-0.049^{**}	-0.055^{**}	-2.298^{**}	-2.232^{*}	-2.211^{*}
	(0.020)	(0.020)	(0.023)	(1.139)	(1.151)	(1.158)
Imports from China	0.101	0.071	0.084	8.553	2.747	2.543
	(0.730)	(0.731)	(0.830)	(40.690)	(45.342)	(45.477)
Union membership	-0.002	-0.002	-0.002	-0.115	-0.113	-0.104
	(0.002)	(0.002)	(0.002)	(0.126)	(0.129)	(0.128)
GSP growth	0.013	-0.002	-0.001	-0.060	-2.403	-2.879
	(0.373)	(0.373)	(0.450)	(19.681)	(19.684)	(19.564)
Union mem. chg.		-0.006	-0.006		-0.361	-0.314
		(0.006)	(0.007)		(0.355)	(0.355)
Chg. in imports from China			-0.278			4.751
			(4.155)			(218.561)
Constant	0.084^{**}	0.078^{**}	0.086^{**}	-1.440	-1.779	-1.731
	(0.037)	(0.037)	(0.042)	(1.596)	(1.654)	(1.648)
Observations	240	240	212	240	240	212
Adjusted R^2	0.012	0.013	0.007			
Log Likelihood				-21.127	-20.566	-20.153
Akaike Inf. Crit.				52.254	53.132	54.306
Note:						*p<0.1; **p<0.05; ***p<0.01