# Labor Constraints and the Impact on Firm Investment: Evidence from Right-to-Work Laws

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**Abstract**

We analyze the impact of the introduction of state level Right-to-Work (RTW) laws, a proxy for relaxed labor constraints for firms, on corporate policies. To identify the causal effect of RTW, we exploit the fact that different states introduced the law at different points in time. A difference-in-differences estimation shows that investment increases after the introduction of the law. To further rule out endogeneity concerns, we also use a geographic regression discontinuity design. The investment-asset ratio is 1.67% higher for firms headquartered in a RTW county that is within 150 miles of a county in a non-RTW state. We explore several potential mechanisms behind the effect on investment. Our results are consistent with the wage channel, where RTW leads to lower wages, which creates an incentive for firms to invest. We also present evidence for the debt channel, where RTW affects the leverage ratio, which then influences investment. Our results highlight the role of labor constraints in corporate policies and how RTW laws led to an increase in the relative bargaining power of firms vis-a-vis employees.

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# 1 Introduction

Labor is an important and critical input to a firm's production function. Just as firms face financial constraints ((Fazzari, Hubbard, and Petersen (1988); Lamont, Polk, and Saá-Requejo (2001); Kaplan and Zingales (1997), Whited and Wu (2006))), they can also face labor constraints and adjustment costs (see Bond and Reenen (2007)). Bloom (2009) documents significant labor adjustment costs using plant level data and Belo, Lin and Bazdresch (2014) show that labor adjustment costs are important for explaining the cross-sectional variation in asset prices. Right-to-work (RTW) laws have a significant impact on the relative bargaining power of firms and employees, among other things, and are one of the most debated state level labor law changes. For example, Elwood and Fine (1987) document a 5-10 percent reduction in unionism as a result of the passage of RTW laws. In this paper, we use the introduction of RTW laws as a shock to the labor constraints and adjustment costs that firms face and analyze their impact on firm level investment behavior.

Our main result is that Right-to-Work passage has a positive and significant impact on firm-level investment. For the full panel of firms, the investment-to-asset and investment-to-PPE ratios are 0.97% and 1.28% higher, respectively, after Right-to-Work implementation relative to before. The economic effects are large compared to the unconditional means of these ratios in the sample: 6.7% for investment-to-asset and 14.1% for investment-to-PPE. We account for potential endogeneity problems by using a difference-in-differences specification, where we exploit the introduction of RTW in different states at different points in time.

To further improve the identification of the causal effect of RTW, we estimate a geographic regression discontinuity design where we only focus on firms located close to the Right-to-Work (RTW) vs. non-Right-to-Work (non-RTW) borders. For firms headquartered in a RTW county that is within 150 miles of a non-RTW county, the investment-asset ratio is 1.67% higher with Right-to-Work treatment. If we decrease the distance to 100 miles, then

the relative difference of investment-to-asset is even greater at 3.07%.

Our results are consistent with the broad notion that RTW laws relax labor constraints for firms. This idea is analogous to the literature on financial constraints. However, we show that RTW, our measure of a relaxation of labor constraints, is independent from a relaxation of financial constraints. In particular, we show that after controlling for financial constraints, the effect of RTW on investment is still positive and significant.

After having established the effect of RTW on investment, we investigate the potential mechanisms behind it. We show that the increase in firm-level investment is partially driven by declining wages. For the panel with all firms, we find that operating profitability loads significantly and positively on the *RTW* dummy. Holding marginal revenue constant, the increase in operating profits has to be driven by a decrease in marginal cost. Since Right-to-Work law is designed to combat labor unionization, it is more plausible that Right-to-Work implementation lowers the firm's marginal cost through falling wages as opposed to declining return on capital. In the full panel study, Right-to-Work introduction accounts for a 1.96% increase in the firm's operating profitability, which is nontrivial compared to the sample mean of 12.7%. The impact of RTW on profitability is robust to the geographic regression discontinuity design, and the slope coefficient of operating income-to-asset regressed on the *RTW* dummy remains positive and significant.

Since the effect of RTW on profitability is only indirect evidence for the wage channel, we repeat the main difference-in-differences estimation using County Business Pattern (CBP) data,[1] provided by the U.S. Census Bureau, to see the effect of RTW on wages. The data is an annual series of business activity aggregated from establishment surveys for both private and public companies so the coverage is greater than that of Compustat. To calculate county-level average labor cost, we take the total annual payroll and divide it by the total number of employees in each county each year. Assuming the number of hours each employee provide

[1] See http://www.census.gov/econ/cbp/ for more details.

does not vary significantly from year to year, the change in payroll should reflect any change in wage. Using county-year observations, we regress average payroll on the *RTW* dummy variable and show that average annual payroll drops significantly by about $2,100 following introduction of Right-to-Work legislation. The average annual payroll in the CBP data for all counties and all industries is $23,780. These results are consistent with the idea that RTW has real effects on the cost of labor which in turn allows firms to invest more.

We also find evidence that RTW affects investment through the debt channel. We split the sample into firms with low and high unionization rates. Since firm-level unionization rates are not easily observable, we use industry-level union membership rates as a proxy. For the subsample of low unionization firms, we find that RTW increases leverage and investment. This is consistent with the empirical results in Serfling (2015), who shows that for low unionization firms, less protection of employees leads to lower borrowing costs and higher leverage. We argue that RTW reduces the protection of employees, which reduces borrowing costs, which in turn increases leverage and investment. For the subsample of high unionization firms, we find that RTW reduces leverage and increases investment. We argue that RTW improves the bargaining power of firms relative to employees, so firms have to rely less on leverage as a strategic tool for bargaining power. The reduction in leverage alleviates the debt overhang problem, which leads to higher investment. This is consistent with Matsa (2010), who reports that firms strategically use leverage to increase the probability of bankruptcy, which allows them to negotiate lower wages. He finds that his results are stronger for high unionization firms.

In addition to the wage channel and the debt channel, we also explore two alternative mechanisms. Under the state demand mechanism, RTW leads to higher demand in the state where the law is introduced, which then creates an incentive for firms in that state to invest more. Under the labor adjustment cost mechanism, RTW reduces the cost of hiring and firing workers, instead of affecting wages directly. This additional flexibility could create an incentive for firms to invest more. However, we do not find evidence for these two alternative

mechanisms.

Our paper contributes to different strands of the literature. We focus on three of these: the Right-to-Work literature in labor economics, the labor-and-finance literature in corporate finance, and the literature on unions and investment. The Right-to-Work literature is very extensive and explores the effects of RTW on many outcome variables such as wages, unionization rates, union organization activity, and NLRB elections. Moore (1998) provides a survey of this field. For brevity, we focus only on those parts of this literature that are related to the mechanism behind the effect of RTW on firm investment.

First, our results are connected to the papers on the relationship between RTW and unionization. According to Moore (1998), there are several theoretical predictions for this relationship. The "Free-Rider Hypothesis" argues that RTW creates a free-rider problem among workers. An individual worker prefers not to pay union dues, while enjoying the benefits of collective bargaining. This hypothesis predicts that unionization rates fall after the introduction of RTW laws. The "Bargaining Power Hypothesis", similarly to the Free-Rider Hypothesis, predicts that unionization rates fall after RTW laws are introduced. However, there are additional long-term effects at play. Lower unionization rates mean that strikes are less effective, which reduces unions' bargaining power relative to firms. As a consequence, workers perceive the benefits of joining a union to be lower, which reduces union organization activity and decreases unionization rates even further. The so-called "Taste Hypothesis" states that RTW introduction is rather a consequence of anti-union sentiment in a particular state, rather than a cause of lower unionization rates. Therefore, according to this hypothesis, RTW has no causal effect on unionization.

Moore (1998) argues that while the evidence is mixed, the majority of studies conclude that RTW has a negative effect on unionization rates. Examples of studies with this conclusion are Davis and Huston (1995), Ichniowski and Zax (1991), and Hundley (1988). This negative relationship is important because, as we argue in this paper, it can explain the

effect of RTW on investment. For example, if a decrease in unionization rates leads to a decrease in labor adjustment costs (DiNardo and Lee (2004) and Chen, Kacperczyk, and Ortiz-Molina (2011)), then RTW can also have have an effect on investment.

Second, our paper is related to the studies on the effect of RTW on wages. As reviewed in a different survey by Moore and Newman (1985), the theoretical predictions for this effect can be both positive and negative. Some authors argue that wages must increase after RTW in order to provide an incentive to workers to join the union. According other researchers, however, RTW weakens unions' bargaining power by reducing unionization rates, which leads to lower wages. Moore (1998) finds that, while the empirical literature is inconclusive, RTW seems not to have a significant effect on wages. Examples of papers in this literature are Moore (1980), Wessels (1981), Carroll (1983), Moore, Dunlevy, and Newman (1986), Garofalo and Malhotra (1992), and Hundley (1993). The effect of RTW on wages is important because we use it as a possible mechanism for the effect on investment. For example, it is possible that firms invest more after RTW is introduced because wages decrease, which allows the firm to increases its size.

Third, a part of the RTW literature is concerned with the effect of RTW on the economic development of states. Several papers document a positive effect of RTW, possibly in conjunction with other business-friendly policies, on economic development. Holmes (1998), using a geographic discontinuity design, shows that the employment share of manufacturing is higher on the RTW side of state borders, compared to the non-RTW side. He also shows that the growth rate of the employment share of manufacturing is higher on the RTW side. Cobb (1982) and Plaut and Pluta (1983) find that the "business climate" of states, which is positively correlated with RTW laws, explains industrial growth. Schmenner (1982) provides survey evidence that a "favorable labor climate" is the most important determinant of industry location. Interestingly, however, Woodward and Glickman (1991) report that RTW has a negative effect on employment associated with foreign direct investment at the state level, and Coughlin, Terza, and Arromdee (1991) argue that RTW has a negative effect on

foreign direct investment. These findings are difficult to reconcile with the positive state-level effects of RTW mentioned before. While the overall evidence on the effects of RTW on the economic development of states is mixed, it shows that it is important to control for state specific economic conditions when estimating the effect of RTW on investment.

The second strand of literature examines the relationship between labor and finance and is much younger compared to the RTW literature. Matsa (2010) shows that firms use financial leverage as a strategic instrument against unions. The intuition is that higher leverage allows firms to improve their bargaining power relative to unions, through the threat of bankruptcy. Agrawal and Matsa (2013) use state-level unemployment insurance laws to show that higher unemployment benefits lead to higher financial leverage. They argue that unemployment insurance reduces employees' fear of bankruptcy, which allows the firm to choose a riskier capital structure. Simintzi, Vig, and Volpin (2015) measure employment protection across countries and find that stronger protection leads to a decrease in financial leverage. They argue that employment protection increases operating leverage, which makes high financial leverage prohibitively expensive for firms. Consistent with Simintzi, Vig, and Volpin (2015), Serfling (2015) uses U.S. state laws that protect employees against their employers to show that more protection leads to lower financial leverage. These papers are important for us because they suggest that there is a link between labor protection and financial leverage. Since leverage and debt financing are also determinants of investment, we hypothesize that the effect of RTW on investment might happen through the debt channel.

The third strand of literature investigates the effect of unions on corporate investment. Connolly, Hirsch, and Hirschey (1986) show that firms in highly unionized industries have lower returns on R&D and invest less in R&D. Hirsch (1992) presents evidence that firms with unions invest less. Bronars and Deere (1993) also find a negative effect of unionization on both tangible and intangible capital. Finally, Fallick and Hassett (1999) show that after a successful union certification election, investment falls. Our contribution to this literature is twofold. First, we use an identification strategy that allows us to interpret the effect on in-

vestment in a causal way. In our difference-in-differences estimation, we use the introduction of RTW in several states and at different points in time as a staggered natural experiment. Also, to further improve the identification of the causal effect of RTW, we estimate a geographic discontinuity design. Second, our main explanatory variable is the introduction of RTW, not unionization rates. Therefore, our approach allows for the possibility that RTW affects investment through a different channel than a drop in union membership.

The rest of the paper is organized as follows. Section 2 describes the data and methodology. The empirical analysis and results are documented in Section 3. We discuss and provide some suggestive evidence on the transmission mechanism linking Right-to-Work laws and the relaxation of labor constraint to firm investment in Section 4. Section 6 concludes the paper.

## 2 Data and Methodology

We obtain firm location and accounting data from the Compustat fundamental annual file from 1966 to 2014. We then match firm headquarters to counties by converting headquarter zip codes to FIPS county codes using a link file provided by the U.S. Census Bureau. Right-to-Work data is compiled from the National Right to Work Legal Defense Foundation, Inc. There are six states that enacted Right-to-Work legislation in our sample period: Louisiana (1976), Idaho (1985), Texas (1993)[2], Oklahoma (2001), Indiana (2012), and Michigan (2013). Furthermore, GDP price deflators were obtained from the FRED database hosted by the St. Louis Fed, and state level GDP data was gathered from the Bureau of Labor Statistics. 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.

[2]According to the Texas Labor Code, Title 3, Employer-Employee Relations, Chapter 101, "A contract that permits or requires the retention of part of an employee's compensation to pay dues or assessments on the employee's part to a labor union is void unless the employee delivers to the employer the employee's written consent to the retention of those sums." This was enacted in 1993 even though the statute was first passed in 1947.

Using the enactment of RTW laws as a proxy for the relaxation of a treated firm's labor constraints, we compare firm-level investment before and after the RTW treatment across states. Using a dummy variable to denote all firm-year observations in a state that has passed the legislation, we expect the coefficient loading when investment is regressed on the post-RTW dummy to be positive and significant if the relaxation of labor constraints generates a spill over effect on corporate investment. Specifically, the baseline regression looks like the following:

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

where $Y$ stands for one of the six dependent variable of interest: investment (CAPX) scaled by either assets or property, plants, and equipment (PPE), employment growth rate, sales over assets, 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 which is set to 1 if a firm-year observation belongs to a state which has passed Right to Work 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.[3] All the control variables are lagged by one period. $GDPGrowth$ is the growth rate of state-level real GDP. Finally, $f_i$ and $f_t$ denote firm and year fixed effects, respectively.

We screen out observations with equity value totaling less than \$10 million, and 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 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 WW index value to be included in the final panel.

[3] Asset tangibility is defined as the ratio of property, plant, and equipment to total assets.

### 2.1 Summary Statistics

Table I summarizes the Right-to-Work states in the U.S. including the District of Columbia. There are a total of 25 Right-to-Work states as of 2015 with Wisconsin being the most recent one to adopt the statute. The majority of the states that have enacted Right-to-Work legislation did so before 1966, and all the firm-year observations in these states are excluded in the baseline regressions. With our sample starting in 1966 and ending in 2014, there are six RTW adoptions in our dataset, and they serve as the basis of our natural experiment. It should be noted that we hand collect the RTW years in Table I by scanning state constitutional amendments and labor codes to ensure the years correspond to when the law actually took effect or enacted. Texas, for example, first passed the law in 1947 by statute, but the RTW did not come into effect until 1993.[4] Another case is Michigan, where the Right-to-Work provision was passed at the end of 2012 but only went into effect in March of 2013.[5]

Table II Panel A provides the summary statistics of the variables used in the analysis. Using Compustat abbreviations, $Asset$ is at, $Cash$ is che, $Dividends$ is dv, $Inv/A$ is capx divided by lagged at, $Inv/PPE$ is capx divided by lagged ppegt, $EmpGr$ is emp divided by lagged emp, $Sales/A$ is sale divided by at, $Debt/A$ is dltt plus dlc divided by at, $OI/A$ is oibdp divided by lagged at, $Tangibility$ is ppegt divided by at, $Cashflow$ is dp plus ib divided by lagged at. The remaining definitions for $Tobin's\ q$, $BE/ME$, and $ROE$ are standard. Panel B is the summary statistics of the Whited and Wu index. Please see Whited and Wu (2006) for the detailed empirical specification. Finally, Panel C in Table II outlines the number of RTW-treated observations in the sample. Out of the total of 76,520 observations, there are 6,407 firm-years with the $RTW$ dummy equal to 1. In columns (3) and (5), we see the number of treated observations in Indiana and Michigan are small due to very recent adoptions of Right-to-Work, in 2012 and 2013, respectively. Texas, in column

[4] See http://www.statutes.legis.state.tx.us/Docs/LA/htm/LA.101.htm

[5] See Michigan Employment Relations Commission Act 176 of 1939.

(7), dominates the number of observed Right-to-Work firm-years with more than 80% of the total.

# 3 Analysis

## 3.1 Panel Study

After applying all the appropriate screens, the baseline panel has 76,520 observations. We include firm and year fixed effects, and standard errors are clustered at the state and year level.

### 3.1.1 Right-to-Work on Firm Characteristics

Table III presents the results of the baseline regression outlined in equation (1). Columns (1) to (6) represent, in order, the dependent variables investment-asset ratio ($Inv/A$), investment-PPE ratio ($Inv/PPE$), employment growth rate ($EmpGr$), sales-asset ratio ($Sales/A$), operating income-asset ratio ($OI/A$), and book leverage ($Debt/A$). In columns (1) to (5), the regressions include standard control variables for investment regressions found in the literature: lagged assets, lagged Tobin's q, lagged cashflow, and lagged book leverage. In column (6), the regression include standard control variables for leverage regressions: lagged assets, lagged Tobin's q, lagged profitability, and lagged asset tangibility. All regressions include state-level GDP growth rates to control for time-varying local economic conditions.

Focusing on the slope coefficient $\beta$ in equation (1), Table III shows the $RTW$ dummy is highly significant on all six dependent variables. Investment ratios increase significantly in columns (1) and (2). Employment growth rate, sales per asset, and operating income per asset all load positively on the indicator variable in columns (3) to (5). However, book

leverage declines significantly, in column (6). This is in contrast to recent works by Simintzi, Vig, and Volpin (2015) and Serfling (2015), where they show, in different settings, that increases in labor protection result in decreases in firm leverage. However, our finding is consistent with Matsa (2010), in which the author shows firms use leverage to better their bargaining position in labor negotiations. This implies that the need to carry high leverage on the firms' side is alleviated when their bargaining position improves due to exogenous factors.

The average investment-asset and investment-PPE ratios in our sample are 6.7% and 14.1%, respectively. Table III shows that the implementation of the Right-to-Work legislation increases $Inv/A$ and $Inv/PPE$ by 0.97% and 1.28%, respectively, according to the coefficient loadings on the $RTW$ dummy, making the effect on investment economically significant. Right-to-Work also have positive and significant impact on the employment growth rate and the operating income-to-asset ratio. The employment growth rate is 1.41% higher after the enactment relative to the average growth rate of 6.9% in the sample. The operating income-to-asset ratio increases by 1.96% compared to the average ratio of 12.7%. The sales-asset ratio is roughly 6.45% greater with RTW than without, but the economic significance is not as strong relative to the sample average of 127%. Finally, the slope coefficient of leverage on the $RTW$ dummy is -1.94%, which is economically sizable relative to the sample average leverage of 20.6%.

#### 3.1.2 Cross-sectional Heterogeneity

In the baseline regression of equation (1), we do not explicitly control for financial constraint. However, it is possible that firms are not investing optimally because they are financially constrained prior to the Right-to-Work implementation, and the law simply alleviates financial constraint so average investment after the enactment of the law is higher. If this is the case, then we should an insignificant effect of RTW on investment for uncon-

strained firms, and a positive effect for constrained firms. To test this hypothesis, we extend the regression specification of equation (1) to the following:

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

where *FC Dummy* is an indicator variable denoting a firm-year observation 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. 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 equation (1) but not reported. Therefore, the coefficient loadings $\gamma$ and $\omega$ are both relative to the unconstrained quartile.

Table IV presents the results of the Right-to-Work regression with financial constraint dummies outlined in equation (1). There are three observations. First, the $\beta$ coefficients on the *RTW* dummy are still statistically significant after controlling for financial constraint, and they are in the same direction as their counterparts in Table III across all six columns of dependent variables. In particular, investment-to-asset, investment-to-PPE, employment growth, sales-to-asset, and operating income-to-asset all increase while book leverage falls. Their economic significance also increase in the presence of the financial constraint dummy: magnitudes of $\beta$ are larger in Table IV than in Table III. Second, the *FC Dummy* by itself is statistically significant across the board. The $\gamma$ coefficients on investment, employment growth, operating income and leverage are significant at the 1% level; while the $\gamma$ on sales is significant at the 5% level. In general, the constrained firms have lower investment ratios, slower employment growth, lower operating income as a percentage of assets, and higher

leverage when compared to the unconstrained firms. All consistent with the basic intuition on what being financially constrained means.

Third, there is heterogeneous impact of the Right-to-Work implementation on firm characteristics in the cross section outside of the sales-asset ratio. This is evident from the $\omega$ coefficients on the interaction term $FC \times RTW$ in Table IV. The slope coefficients on the interaction term are negative and statistically significant at the 1% level for the investment ratios, employment growth rate as well as operating income scaled by assets. The interpretation is that the Right-to-Work implementation disproportionately impact the unconstrained firms more, allowing them to invest more than the constrained firms as a fraction of assets (or PPE), to add jobs at a faster rate than the constrained firms, and to become more profitable than their constrained counterparts. Moreover, the enactment of Right-to-Work contributes to the de-leveraging of the unconstrained firms more so than the constrained firms as indicated by the positive coefficient loading on $FC \times RTW$ in column (6) of Table IV, and the statistical significance is very high.

To understand the absolute impact of RTW on financially constrained firms as opposed to the relative impact, we add up the slope coefficients on $RTW$ and $FC \times RTW$ in Table IV for each of the columns (1) to (6). Roughly speaking, the sum of the $\beta$ and the $\omega$ coefficients are small or close to zero, except for $Sales/A$. This implies that RTW has very limited impact on investing, hiring, and financing activities for those firms which are financially constrained. To conclude, we do not find evidence that RTW relaxes financial constraint and therefore leads to higher investment. Instead, it is the financially unconstrained firms that can truly take advantage of the relaxation of labor constraints from the passage of Right-to-Work.

#### 3.1.3 Dynamic Impact of Right-to-Work Legislation

The panel regression results in tables III and IV demonstrate the treatment effect of the Right-to-Work law on firm investment, labor growth, and leverage before and after

the implementation. However, they 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 at the lead-lag relationship between the enactment of Right-to-Work laws and when the effects of these laws are realized, we employ spline regressions in this section to examine the dynamic interaction between RTW and the same six dependent variables as in the panel study.

To do so, we first construct thirteen additional dummies in the dataset. For each state, we designate five dummies for each year in the five year period prior to Right-to-Work implementation, one dummy for the year of implementation, and another five dummies for each year in the five year period following implementation. Additionally, two dummy variables are used to encompass all years outside of the two five-year windows before and after RTW implementation. Together, these thirteen dummies variables cover the entire sample period for each state. To estimate the spline, we combine all firm-year observations and run a pooled regression with the same six dependent variables and the newly constructed Right-to-Work year dummy variables:

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

where $\Phi$, $\beta$, and $\Psi$ 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 Right-to-Work enactment. The spline regression results are summarized in Table V. The dependent variables, in order from column (1) to column (6), are investment-to-asset, investment-to-PPE, employment growth, sales-to-asset, operating income-to-asset, and book leverage. As before, all regressions include the same control variables as tables III and IV with firm and year fixed effects. Robust standard errors with double clustering at the state- and year-level are used to calculate the $t$-statistics. Coefficient loadings on the control variables and fixed effects are excluded from the table.

The first important observation about column (1) in Table V is that the coefficients of the RTW dummy are mostly insignificant prior to the year of the law's introduction. Four out of five coefficients prior to the introduction are insignificant, while one coefficient is significant at the 10% level. This suggests that the treatment group behaves similarly to the control group in the years prior to the RTW law, and that the parallel trends assumption holds. The evidence is a bit weaker in column (2), where is use a different measure of firm investment. Three out of five coefficients are insignificant, one coefficient is significant at the 5% level, and one is significant at the 1% level. The most significant coefficient belongs to the RTW dummy five years prior to the introduction of the law, which is a relatively long time. Therefore, we conclude that the parallel trends assumption likely holds in our sample.

Columns (1) and (2) of Table V show that it takes about three years for the effect of Right-to-Work laws to take place in firm investment. The $\beta$ coefficients on $RTW$ 0 are positive but statistically insignificant in columns (1) and (2), meaning there is no immediate impact on investment from the relaxation of labor constraint through RTW. Coefficient loadings on $RTW$ +1 and $RTW$ +2 are also positive but insignificant. However, both investment ratios load strongly in the positive direction on $RTW$ +3 (1.36% and 1.16%, respectively) and beyond. This implies average firm investment is significantly higher three years after RTW enactment relative to the benchmark level in the year before enactment. In column (3), employment growth also does not respond immediately to RTW as evidenced by the insignificant coefficient loadings on $RTW$ 0 and $RTW$ +1. Two and four years after RTW implementation, we find employment growth rates to be significantly higher (1.81% and 2.18%, respectively) compared to the benchmark level in year minus one.

Regression results for sales and operating income, both scaled by assets, are displayed in columns (4) and (5) in Table V. Sales-to-asset ratio is general low in the years before Right-to-Work implementation given by the fact that most of the $\Phi$ coefficients are negative and significant. This is consistent with the idea that these law changes typically come about in the midst of weak macroeconomic conditions. After the enactment of RTW, sales ratio

is flat for at least five years, after which the coefficient loading on $RTW > +5$ is positive and significant at 7.72 %. On the other hand, Right-to-Work shows immediate impact on operating income in column (5). All $\Phi$ coefficients are insignificant in the pre-RTW window, but the $\beta$ coefficient on $RTW$ 0 is 0.78% and significant at the 5% level. The effect of RTW on $OI/A$ is persistent as most of the $\Psi$ coefficients are positive and significant. Finally, in column (6), we see the de-leveraging effect stemming from Right-to-Work enactment is much more long term. Even though the $\Psi$ coefficients are all negative starting with $RTW$ +1, their statistical significance is weak up to $RTW$ +5. Beyond the five-year post-RTW window, the coefficient loading on $RTW > +5$ is -2.08% with significance at the 5% confidence level.

Taken together, the empirical evidence from the spline regressions provides some insight into the underlying mechanism through which Right-to-Work laws are able to affect investment at the firm level. Since marginal profit equals to marginal revenue minus marginal cost, the fact that profitability ($OI/A$) increases immediately after RTW enactment, as indicated by the $\beta$ coefficient in column (5), suggests that either marginal revenue has gone up or marginal cost has gone down. Given that the sales ratio is actually lower at the same time, it is more likely the increase in profitability is driven by falling marginal cost. Assuming standard production technology using labor and capital as inputs, marginal cost is made up of two components: the marginal product of labor and the marginal product of capital. In a competitive equilibrium, the former is wage and the latter is the rental cost of capital. Due to the nature of Right-to-Work laws in shifting the bargaining power between firms and labor unions, it is straight forward to argue that the increase in profitability is a result of falling wages as opposed to falling cost of capital[6]. As wages decline, firms are able to hire at a faster rate than before as seen in column (3) of Table V starting two years after Right-to-Work enactment, which in turn leads to greater investment ratios in year 3 and beyond post RTW implementation. Surge in investment, employment growth, and profitability eventu-

[6]Another argument against the decline in the cost of capital is the delayed response of investment in columns (1) and (2) of Table V. Standard q-theory would suggest that firms will invest up to the point where marginal benefit equals to marginal cost. If indeed the cost of capital has declined after Right-to-Work implementation, we should not expect to see a three year gap between the enactment of the law and the uptick in investment.

ally result in higher output and lower leverage in the long run, illustrated by the positive and significant coefficient loadings on $RTW > +5$ in columns (4) and (6).

This is only circumstantial evidence of the transmission mechanism through the decline in wages. Since Compustat data does not provide wage bills, it is impossible to directly test this wage hypothesis in this setting. We look into this channel in more detail in Section 4 where we investigate the possible mechanisms using a different dataset.

### 3.2 Geographic Regression Discontinuity Design

One concern that arises from the panel regression results is the fact that unobserved state or regional characteristics instead of Right-to-Work enactment is actually driving the increase in investment. This is particular pressing for studies involving Right-to-Work laws because the majority of these legislatures are concentrated in the southern and southwestern states, where variables outside of the observed data such as weather and culture can be different from other regions of the U.S. where non-RTW states are concentrated. To mitigate the potential impact of these unobserved regional characteristics on firm investment, we turn to geographic regression discontinuity (GRD) design to tease out the effect of Right-to-Work implementation across state lines. GRD eliminates firm-year observations in the sample that are spatially far away from borders where RTW and non-RTW states meet. Since the treatment and control groups are very close to each other, it is very unlikely that unobservable differences between the two can bias the estimation of the treatment effect. By running regressions as specified in equation 1 on the GRD sample, we examine the treatment effect of Right-to-Work on investment, employment growth, sales, profitability, and leverage between firms headquartered a short distance apart on both sides of the RTW/non-RTW borders, effectively controlling for other unobserved geographical factors.

There have been total six changes in the list of states that have passed the Right-to-Work law since 1970: 1976, 1985, 1993, 2001, 2012, and 2013. For each year from these seven years

(including 1970 as a starting date of the sample), we created two lists of states. The first list contains names of states that have passed the Right-to-Work, and the other list contains names of states that have not passed the Right-to-Work law but that are neighboring the states in the first list. For example, by the end of 1976, the first list is created by having thirteen states have passed the Right-to-Work law: Arizona, Arkansas, Iowa, Nebraska, South Dakota, Tennessee, Virginia, North Dakota, Nevada, Utah, Kansas, Wyoming, and Louisiana. From this list of thirteen states, we create another list of the Non-Right-to-Work states which are bordered by the thirteen states. In 1976, this list contains sixteen states: California, Colorado, District of Columbia, Illinois, Kentucky, Maryland, Minnesota, Missouri, Montana, New Mexico, Oregon, Idaho, Oklahoma, West Virginia, Wisconsin, and Texas. Following the same procedure, we create two lists of states for each of six Right-to-Work implementations, and have total 14 lists at the end of this first step.

From these lists of RTW states and non-RTW states, we separately collect names of counties in the RTW states and the non-RTW states, compute their centroid using coordinates of polygons of counties. For all counties in the RTW states, we only keep those located within certain distances (50 miles, 100 miles, or 150 miles) from another county in a non-RTW state. By the end of this process, we have one list of counties in RTW states and the other list of counties in Non-RTW states, where each of the surviving county on both lists have at least one matched county no more than 50 miles, 100 miles, or 150 miles away on the other list.

Using the lists of neighboring counties in the RTW states and the non-RTW states, we create dummy variables called $d_{rtw}$ and $d_{nrtw}$. The dummy variable $d_{rtw}$ equals to one if the headquarter of a firm-year observation is located in the list of neighboring RTW counties and equals to zero otherwise. In a similar manner, the dummy variable $d_{nrtw}$ equals to one if the headquarter of a firm in the dataset is located in the list of neighboring non-RTW counties. For example, for the lists of neighboring RTW counties and non-RTW counties at the end of 1976, firm-year observation from 1976 to 1984 are applied and $d_{rtw}$ and $d_{nrtw}$

variables are created for those observations using lists at the end of 1976, because a next change in the list of the Right-to-Work law states is in 1985. For firm-year observations from 1970 to 1975, the lists of counties in RTW states and non-RTW states at the end of 1970 are used to create dummy variables.

Geographic discontinuity regressions are conducted using these dummy variables, $d_{rtw}$ and $d_{nrtw}$. First, we filter out all observations which are not in neighboring counties straddling the Right-to-Work border. Second, we regress investment-asset ratio, investment-PPE ratio, employment growth rate, sales-asset ratio, operating income-asset ratio, and debt-asset ratio on the *RTW* dummy while controlling for financial constraint similar to Table IV. The results of the GRD design using the 150-mile and 100-mile distance filters are shown in Table VI and Table VII, respectively. We do not report the GRD results using the 50-mile filter because the results are not statistically significant. This is probably caused by the low number of observations left in the sample. We keep observations in all Right-to-Work states for the GRD design.

In both tables VI and VII, the coefficient loadings on the *RTW* dummy is positive and significant on the investment-asset ratio in column (1). This means average investment is higher for firms headquartered on the RTW side of the Right-to-Work border relative to firms headquartered on the non-RTW side given the firms are in geographic proximity of one another. In column (2), the *RTW* dummy has positive impact on the investment-PPE ratio for both the 150-mile and 100-mile distance filters, but the statistical significance is weaker. The coefficient is significant at the 5% level in Table VI and at the 10% level in Table VII. It is puzzling that the *RTW* dummy is highly significant for investment-to-asset but becomes less significant for investment-to-PPE in the narrower band of the Right-to-Work border. One possible explanation is that the 100-mile radius leads to a smaller sample than the 150-mile distance.

Columns (3) and (4) in both GRD tables show that Right-to-Work implementation has

no discernible impact on the employment growth rate or the sales-to-asset ratio, in contrast to the full panel study in Table IV. This suggests that the increase in investment is not likely demand driven once we focus only on firms located in close geographic proximity. In Table VI, the *RTW* dummy does increase the operating income-asset ratio in column (5), although this is only significant at the 5% level. The same holds in column (5) in Table VII with much larger magnitude and stronger statistical significance. Further evidence that firms have more bargaining power on the RTW side of the border, and Right-to-Work might be contributing to decline in wages. Finally, the coefficient of leverage is negative in the last column of both Table VI and Table VII, but the slope coefficient is only statistically significant in the latter. Taken together with the positive coefficient of operating income in column (5), we conclude that the de-leveraging we see in the data is probably an outcome of the increase in profitability stemming from Right-to-Work enactment. This is consistent with the capital structure literature, which contains a lot of evidence that there is a negative within-firm relationship between profitability and leverage (for example, see Danis, Rettl, and Whited (2014) and Frank and Goyal (2015)).

### 3.3 State-by-State Analysis

Of the six Right-to-Work implementations in our sample period, Indiana (2012) and Michigan (2013) took place so close to the end of the sample period that there are only a limited number of RTW observations available: 45 in Indiana and 34 in Michigan, according to Table II, respectively. Also, there are very few observations in Idaho, because very few firms are headquartered there. For the state-by-state analysis in this section, we ignore Idaho, Indiana and Michigan and try to understand the impact of Right-to-Work laws in the remaining three states individually. More specifically, we only keep firm-year observations from Right-to-Work states in the sample one state at a time for Louisiana, Oklahoma, and Texas. Furthermore, we keep all firm-year observations from the non-RTW states as the control group. We then run panel regressions like the one in equation 1 on each of the

single-RTW-state samples to see if the enactment of Right-to-Work legislation affect firm-level investment and profitability. Since these single-RTW-state samples have only a limited number of treated observations, we only focus on industries with more unionized labor force to give the test more power. This means firm observations outside of major industries of mining and construction (SIC Major Group 1), and manufacturing (SIC Major Groups 2 and 3) are dropped.

Table VIII presents the estimated regression coefficients for the state-by-state analysis. Columns (1) and (2) are for regressions with Louisiana as the sole Right-to-Work state; columns (3) and (4) are for regressions with Oklahoma as the sole Right-to-Work state; columns (5) and (6) are for regressions with Texas as the sole Right-to-Work state. For compactness, we only show investment-to-asset ratios in the odd columns and operating income-to-asset ratios in the even columns. There are two results of note. First, the *RTW* dummy is positive and significant for investment regressions in columns (1), (3), and (5). This means, on average, the investment-to-asset ratios are higher following Right-to-Work enactments in Louisiana, Oklahoma, and Texas. On the other hand, the *RTW* dummy does not show significant impact on firm investment in Idaho, thus the regression results are omitted in Table VIII. This is potentially due to the low number of available treated observations in mining, construction, or manufacturing in that state. In unreported tests, we repeat the state-by-state analysis for Idaho, Indiana, and Michigan, and find statistically insignificant coefficients of RTW. This is not surprising, given the small number of observations with RTW=1 in these states.

The second notable result is the fact that profitability rises stemming from Right-to-Work legislation in Oklahoma and Texas in columns (4) and (6), but the *RTW* dummy is insignificant on profitability for Louisiana firms in column (2). Coincidentally, the statistical significance of the *RTW* dummy is also weaker on the investment ratio in Louisiana in column (1). Combining the evidence of Right-to-Work on profitability with that on investment in Table VIII implies that declining wage (increasing profitability) potentially contributes to

the rise in investment, but it is not the only mechanism driving the results. The cross-state heterogeneity suggests the presence of a second channel through which Right-to-Work implementation leads to higher investment. We will discuss the mechanism in more detail in Section 4 of the study.

The state-level diagnostics provide confidence that the impact of Right-to-Work on investment is not driven by a single state. Although the effect is not consistent across all states that have implemented Right-to-Work legislation, we believe this is largely stemming from small number of observations due to either lack of firms (Idaho) or lack of time (Indiana and Michigan). While it is true that Louisiana, Oklahoma, and Texas are in close geographic proximity, the three Right-to-Work enactments in those states span over 25 years, ensuring that temporal heterogeneity is preserved. Furthermore, the inclusion of state-specific real GDP growth and time fixed effects is meant to control for macroeconomic conditions so that the rise in investment ratios as indicated by the positive and significant slope coefficients on the $RTW$ dummy is not the result of an economic expansion.

## 4 Mechanism

We explore different mechanisms that might explain the observed effect of Right-to-Work laws and the easing of labor constraint to firm investment. We present four different potential mechanisms, or channels: The state demand channel, the wage channel, the adjustment cost channel, and the debt channel. The state demand mechanism allows for the possibility that the introduction of a RTW law leads to an increase in the local economic environment of the state where the law is introduced. This generates greater demand for output thus leading to higher investment.

According to the wage channel, post RTW, the firm has more bargaining power over the union. This is because RTW reduces union membership rates, or makes it more difficult for a

union to form. This allows firms to reduce wages, or to prevent wage increases, compared to the counterfactual where RTW is not introduced. With lower wages the firm will choose to hire more workers and to invest more in capital, under certain assumptions on the production function.

Under the adjustment cost mechanism, RTW shifts bargaining power from the union to the firm, as with the wage channel. The firm can better dictate the terms of the contract regarding renegotiation timing, severance, training, etc. The result is effectively lower costs of hiring and firing its labor force. Lower adjustment cost means the firm can better respond to future economic conditions, implying lower precautionary motives. As the share of future cashflow dedicated to labor input declines, the labor contract becomes less debt-like, allowing the firm to invest closer to the first-best level.

The debt channel is based on the existing literature on the relationship between financial leverage and labor. There are two popular views in this literature. First, as argued by Matsa (2010), firms use financial leverage strategically to force concessions from workers. Firms choose higher leverage to increase the probability of bankruptcy, which allows them to bargain for lower wages. According to this view, we expect firms to reduce leverage after RTW is introduced. This reduction in leverage should alleviate the debt overhang problem (e.g., Myers (1977)) and increase investment. Second, as proposed by Serfling (2015) and Simintzi, Vig, and Volpin (2015), less protection of employees leads to lower borrowing costs and to higher leverage. The extent that RTW can be seen as a reduction in employee protection, we expect RTW to reduce borrowing costs, which should lead to higher leverage and higher investment.

The two strands of literature on the relationship between labor and leverage lead to different predictions for the effect of RTW on leverage and investment. Also, the two views are not mutually exclusive. Since we do not know whether the effect in Matsa (2010) or the effect in Serfling (2015) and Simintzi, Vig, and Volpin (2015) dominates, it is difficult to

design an empirical test that identifies the debt mechanism. However, Matsa (2010) shows that his findings are much stronger for firms with high unionization rates. Similarly, Serfling (2015) reports much stronger results for firms with low unionization rates.[7] Therefore, if the debt mechanism plays a role, we expect that the effect of RTW on investment is positive for firms with high unionization rates, and negative for firms with low unionization rates.

Keep in mind that these four hypotheses are not mutually exclusive, and chances are that all of them work in tandem to contribute to the rise in investment post-RTW. Our aim is to decipher if any of the channels can be supported in the data and perhaps provide commentary on the importance of each. We present tests for each of these four mechanisms.

### 4.1 The Demand Channel

Before running any formal regressions to test the demand channel hypothesis, it should be noted that all panel and spline regressions in the current study contain control variables for state-level GDP growth. If demand is truly driving the increase in investment, then $GDP\ Growth$ should soak up variations in the investment ratios and render $RTW$ insignificant in equation (1). This is clearly not the case from Table III, which suggests that there is something beyond outside demand that is at work.

More formally, we use the sales per assets ratio as a reflection of demand in the data. In column (4) of Table III, the $\beta$ coefficient on $RTW$ is positive and highly significant at roughly 6%, and it holds true in column (4) of Table IV after controlling for financial constraint. This suggests that the average output of firms in the baseline sample is greater after the implementation of Right-to-Work laws, which gives some validity to the demand channel. However, the results of the spline regression for the sales-asset ratio in Table V tells a very different story: when the sales ratio is regressed on RTW year dummy variables in equation (2), the coefficient loadings $\Phi$ and $\beta$ are either insignificant or significantly negative. In the

[7] Simintzi, Vig, and Volpin (2015) do not report separate results for subsamples with low and high unionization rates.

five-year post-RTW window, all slope coefficients remain insignificant, which means the sales ratio after RTW is not statistically different from the year right before implementation. The only positive and significant coefficient in column (4) of Table V is for $RTW > +5$, at least three years after the impact of RTW is realized on investment ratios in columns (1) and (2) and four years after it starts to drive employment growth higher as illustrated in column (3).

The dynamic interactions of Right-to-Work, investment, employment growth, and sales help to shed light on the transmission mechanism of how labor constraint reduction results in greater investment. The demand channel is less plausible because RTW enactment raises investment ratios and the employment growth rate before gains in sales are observed.

### 4.2 The Wage Channel

Throughout the analysis up to this point, we have seen some indirect evidence that Right-to-Work affects firm investment through the wage channel. Specifically, holding marginal revenue constant, the observed increase in operating income resulting from RTW can best be explained by declining wages as firms strengthen their bargaining position over their workers. From the spline regressions, sales and leverage remain stagnant following Right-to-Work implementation while operating income rises leading to higher investment ratios. However, we do not observe wage explicitly in Compustat data, and it is possible that the profitability boost associated with RTW enactment is not the result of lowering labor cost.

To test the falling wage hypothesis directly, we employ the County Business Patterns (CBP) data from the U.S. Census. We obtain historical data from 1986 to 2013 at annual frequency for the industry aggregate. In the county record layout, we obtain two variables of interest: $EMP$ is total number of employees and $AP$ is total annual payroll in thousands of dollars, both in county-year observations. To calculate labor cost, we simply divide $AP$ by $EMP$ to get annual payroll per employee. Assuming the average employee works the same number of hours from year to year, changes in the per capita annual payroll number

should reflect changes in average wage, which allows us to use it as the dependent variable in a panel regression where the *RTW* dummy is the explanatory variable. The regression specification is very similar to that of equation 1:

$$PR_{i,j,t} = \beta RTW_{j,t} + \lambda GDP\ Growth_{j,t} + f_t + \epsilon_{i,j,t}, \tag{3}$$

where $PR_{i,j,t}$ is per capita annual payroll in county $i$, state $j$, and year $t$. State-level real GDP growth is the only control variable. Notice that observations are in county-years rather than firm-years.

The results of the county wage regressions are summarized in Table IX. $t$-statistics are calculated using double clustered standard errors at the year- and state-level. We use the entire sample from 1986 forward in columns (1) and (2). In columns (3) and (4), we cut the sample down by including only observations in states that introduced Right-to-Work legislation after 1986. As reported, the results do not change between the two samples. Across the top row, we see that the average annual payroll per capita decreases by around \$2,100 from Right-to-Work implementation, and this is economically significant relative to the unconditional mean of \$23,780 per capita. The magnitude of this decline in average payroll provides support to our hypothesis that falling wages are driving the rise in operating income stemming from RTW introduction.

### 4.3 The Adjustment Cost Channel

Any study involving the effect of adjustment cost requires the use of a proxy for adjustment cost since it is rarely observed directly. In this case, we need a proxy for labor adjustment cost to generate heterogeneity in the cross section of firms. Unfortunately, the availability of such proxy is scarce in the existing literature.[8] As an early attempt, we pro-

[8]Donangelo (2014) and Belo and Lin (2014) use an occupation-specific measure of adjustment cost obtained from Occupational Information Network. Based on the JobZones index, this measure is specifically about the level of formal training that is necessary to perform a job adequately. While the index is useful for the research questions

pose two simple adjustment cost proxies in this section to test the adjustment cost channel for firm investment.

#### 4.3.1 Employment Volatility

The first proxy for labor adjustment cost is the volatility of the number of employees in Compustat. The idea is straightforward: controlling for all other firm characteristics, higher volatility of employment is a proxy for higher labor adjustment cost. The intuition is based on the empirical observation that employee layoffs are very lumpy: in most years, firms do not lay off workers, but if they do, they fire many workers at once (e.g., Davis, Faberman, and Haltiwanger (2006)). We assume that this is due to labor adjustment costs. Firms might be reluctant to fire workers because it is costly to fire them. Also, it might be costly to hire them back in the future, if economic conditions improve. Under this assumption, we can use the observed volatility of hiring and firing to infer the firm's labor adjustment costs. This intuition is similar to the literature on "lumpy investment." In that case, firms only invest when the return on investment is above some threshold, otherwise they do not invest.

Employment volatility in year $t$ is defined as the average of squared deviations of the number of employees from the mean number of employees over the ten years prior to year $t$. In constructing this measure, we require a firm-year observation to have ten years of lagged employment data with none-missing values in Compustat to be included in the sample. To ensure the smaller sample (after screening out firm-year observations without a value for employment volatility) is not systematically different from the original panel, we run regressions specified in equation (1) to check the estimated coefficients are the same. Although not reported here, we can confirm that the findings reported in Table III are replicated in the new sample.

Under the adjustment cost hypothesis, the introduction of RTW reduces labor adjustment

in those to papers, this proxy does not exactly measure the type of adjustment cost in the context of easing labor constraint from the implementation of Right-to-Work.

costs. Firms with high adjustment costs, i.e., with high employment volatility, should benefit more from this reduction. Therefore, we expect the effect of RTW on investment to be stronger for firms with high employment volatility. To test this idea, we define a dummy variable $Employment\ Vol_{i,t}$ which is one if employment volatility for firm $i$ in year $t$ is above the median in year $t$. We estimate the following regressions analogous to those in Table IV for financial constraint:

$$\begin{aligned} Y_{i,j,t} &= \beta RTW_{j,t} + \eta Employment\ Vol_{i,t} + \theta EmpVol_{i,t} \times RTW_{j,t} \\ &\quad + Controls_{i,t-1} + \lambda GDP\ Growth_{j,t} + f_i + f_t + \epsilon_{i,j,t}, \end{aligned} \tag{4}$$

where we replace the $FC\ Dummy$ with $Employment\ Vol$ and $FC \times RTW$ with $EmpVol \times RTW$ in the regression specification. The dependent variables of interest are the same: investment ratios, employment growth rate, sales-to-asset, operating income-to-asset, and leverage. All control variables and the fixed effects remain the same as those in equation (1).

Table X summarizes the estimated regression coefficients in equation (4). The primary coefficient of interest is $\theta$ on the interaction term between employment volatility and the post-RTW dummy. In particular, if the adjustment cost hypothesis is valid, Right-to-Work enactment has a stronger impact on investment of the high labor adjustment cost (high volatility) firms, and investment ratios have to load positively on the interaction term. This is the opposite of what we find in columns (1) and (2) of Table X. For investment-to-asset, $\theta$ is -0.0112 and statistically significant at the 5% level; while for investment-to-PPE, $\theta$ is -0.0098, but statistically insignificant. Overall, using employment volatility as a proxy for labor adjustment cost, we do not find evidence for the adjustment cost hypothesis.

#### 4.3.2 Unionization Rates

For the second test of the adjustment cost mechanism, we use unionization rates at the industry-year level as a proxy for adjustment costs. The logic is that firms with high

unionization rates cannot easily reduce the number of employees, because a large fraction of employees are protected by the union. We use the unionization rates from 1983 to 2014 as described in Hirsch and Macpherson (2003).[9] The data contain unionization rates by Census Industry Codes (CIC), which need to be mapped to SIC codes. For the time period 1992–2002, the raw dataset contains the SIC codes corresponding to each CIC code. We use the same link for the preceding time period 1983–1991. We cannot use this link for the remaining time period 2003–2014, because the Census Bureau significantly changed the definition of CIC codes at that time. Therefore, we first map CIC codes to NAICS codes using the link provided by the Census Bureau. In a second step, we map the NAICS codes to four-digit SIC codes.

We define the variable *Union Dummy* to be one if the unionization rate for firm $i$ in year $t$ is above the median unionization rate in year $t$. We add the *Union Dummy* to the base case specification in equation 1, and interact the new dummy with the RTW variable. Under the adjustment cost hypothesis, RTW leads to a reduction in labor adjustment costs. Firms with high unionization rates should benefit more from this reduction. Therefore, we expect the interaction term $Union \times RTW$ to be positive and significant.

Table XI reports the estimation results. We use the same dependent variables as in Table III. The table shows that the RTW dummy is still significant in all specifications. The RTW dummy is significant at the 1% level in the specifications for investment over assets, investment over PPE, employment growth, and profitability. The RTW dummy is significant at the 10% in the regression for sales over assets, and insignificant in the regression for leverage. The signs of the coefficients have not changed compared to our base case results in Table III.

The RTW dummy interacted with the *Union Dummy* is positive and significant at the 1% level in column (1), and insignificant in column (2). This means that RTW laws do

[9]The data is available at http://unionstats.com.

not have a larger effect on investment if unionization rates are high. This is inconsistent with the hypothesis that RTW affects investment though the adjustment cost channel. The interaction term is not significant in column (4), the regression for profitability. In other words, the effect of RTW on profitability is not different for firms with high unionization rate firms. This is evidence against the hypothesis that RTW affects investment through the wage channel. If RTW allowed firms to reduce wages, then firms with high unionization rates should profit more from falling wages, which is not the case.

Column (6) in Table XI shows that the interaction term $Union \times RTW$ is negative and significant at the 5% level in the regression for leverage. At the same time, the coefficient of RTW is insignificant. In other words, firms with high unionization rates reduce leverage after the introduction of RTW, but low unionization firms do not. This is interesting because we know from Table III that in the full sample, RTW leads to a reduction in leverage. Together with the results in Table XI, we know that this is mostly happening in the subsample of high unionization rate firms. Also, we argue in Section 3 that the negative effect of RTW on leverage is likely due to the increase in profitability. Since there is no differential effect on profitability for high unionization firms (see column (5) of Table XI), there might be an additional mechanism at play here.

We conclude that using employment volatility and unionization rates as proxies for labor adjustment costs results in evidence that is inconsistent with the adjustment cost channel. Further, the evidence using unionization rates is inconsistent with the wage channel. However, we found evidence supporting the wage channel in Table IX. Also, the results in Table XI reveal an interesting differential effect of RTW on leverage for high and low unionization firms. We explore these differences in more detail in the next section.

### 4.4 The Debt Channel

Under the debt mechanism, we expect to see a different effect of RTW on investment for high and low unionization rate firms. Also, we expect to see a different effect of RTW on leverage for these two groups of firms. For firms with low union membership rates, we expect that RTW, which is assumed to reduce employee protection, to decrease borrowing rates, which should lead to higher leverage and higher investment. This economic mechanism is based on the empirical findings in Serfling (2015), who also shows that his results are stronger for low unionization firms.

For firms with high union membership rates, we expect that RTW, which can be seen as reducing the union's bargaining power with the firm, leads to less strategic debt issuance by the firm. Therefore, RTW should lead to a decrease in leverage, which in turn should alleviate the debt overhang problem and allow the firm to invest more. This mechanism is based on the empirical results in Matsa (2010), who also finds that the strategic effect is stronger for high unionization firms.

Since we use unionization rates for both the adjustment cost channel and the debt channel, it might seem that it is difficult to distinguish between the two mechanisms. This is not the case, however. The two mechanisms provide different predictions for the effect of RTW on leverage and investment, and on how these effects differ between low and high unionization firms. These differences allows us to identify which of the two mechanisms is at work. What we cannot exclude, however, since the two hypotheses are not mutually exclusive, is that the effect on investment through the debt channel is so strong that it dominates the effect through the adjustment cost channel.

We split the sample into low and high unionization firms, based on the median unionization rate for each year. We then estimate spline regressions as in Table V for the two subsamples separately. Table XII contains the results for the subsample of low unionization firms. Column (1) shows that there is no difference between our treatment and control group

in terms of investment over assets prior to the introduction of RTW. Even after the introduction, it takes some time to see a difference in investment rates. Starting with the coefficient $RTW + 2$, and until $RTW > +5$, the effect of RTW is positive and significant. Similarly, column (2) presents a similar effect of RTW on investment over PPE, except that the effect starts a year earlier, at $RTW + 1$. Column (6) shows that the effect of RTW is positive and statistically significant, although not as strong as the effect on investment. The effect on leverage starts at $RTW + 2$, i.e., with a lag after the introduction of RTW. It is interesting that the effect on investment and the effect on leverage starts to manifest themselves at the same time, which further suggests that there might be a causal link between the two effects. To summarize, the findings in Table XII are consistent with the hypothesis that RTW affects investment through the debt channel.

Table XIII contains the results for the subsample with high unionization firms. Column (1) and column (2) show that RTW has a positive and significant effect on investment of assets and investment over PPE, respectively. The effect starts with a lag of four years in both cases. Column (6) shows that RTW has a negative and significant effect on leverage. Firms start to reduce leverage four years after the introduction of RTW. It is interesting that both the effect on investment and the effect on leverage start with a lag of four years. This is consistent with the notion that there is a causal link between the effect on investment and the effect on leverage. To conclude, both the evidence in Table XII and the evidence in Table XIII support the hypothesis that RTW affects investment through the debt channel.

## 5 Robustness

In this section, we perform a number of robustness tests to address some of the concerns that have risen from the main analysis in Section 3.

### 5.1 Controlling for the Texas Effect

To further guard against the possibility that the state of Texas is driving the main results in terms of RTW's impact on firm investment, we extend the sample back to 1952, which is the earliest we can obtain necessary variables in the Compustat database. By the inclusion of 1950s and the first half of 1960s, we add 7 additional Right-to-Work introductions to the sample: Nevada (1952), Alabama (1953), South Carolina (1954), Utah (1955), Kansas (1958), Mississippi (1960), and Wyoming (1963). Since the implementation of RTW in Texas has shown to have an outsized influence on firm investment, we exclude all firm-year observations headquartered in Texas from the sample for robustness. As a result, there are a total of 12 RTW introductions in the extended sample: 7 mentioned above plus Louisiana (1976), Idaho (1985), Oklahoma (2001), Indiana (2012), and Michigan(2013).

Table XIV presents the results of the difference-in-differences regression similar to equation 1 in the extended sample excluding Texas. We focus on the 3 to 5 year window after the legislation is in place. To do so, we construct a RTW dummy that is equal to 1 if and only if a given observation is headquartered in a Right-to-Work state in years 3, 4 or 5 post RTW. We choose to examine the treatment effect of RTW on investment starting in year 3 to be consistent with the delay observed in the spline regression in column (1) of Table V. Furthermore, the window is restricted to 5 years or less because Table V shows the effect of Right-to-Work starts to dissipate after five years, suggesting that the shock is transitory as opposed to permanent. A simple story of competition between two neighboring states that introduced Right-to-Work legislation in different years can potentially generate the diminishing effect of RTW over time. Lastly, we eliminate the top decile of firms by total asset each year. By dropping the largest firms, we minimize the chances that a firm headquartered in a Right-to-Work state has manufacturing and/or retail locations in a non-Right-to-Work state, or vice versa.

Column (1) and column (2) in Table XIV confirm the impact of Right-to-Work on in-

vestment in the extended sample sans Texas: RTW introduction leads to a positive and insignificant increase in firms' investment-to-asset and investment-to-PPE ratios in the 3 to 5 years after implementation while controlling for financial constraint. Notice firm and year fixed effects are included, but due to data availability, we can no longer control for cashflow and state-level GDP growth in this specification. The remaining columns in Table XIV show that Right-to-Work implementation has no discernible effect on employment growth, sales, profitability, and leverage in the 3 to 5 year window post introduction. However, this does not necessarily mean the effect is non-existent since Right-to-Work can can have a more immediate or long-run impact on these other firm characteristics. To see the dynamic effect of Right-to-Work in the extended sample without Texas, we employ a spline regression as before.

The results of the spline regression are shown in Table XV. The same set of controls as the difference-in-differences specification are used. In columns (1), we see that RTW has no immediate impact on the investment-asset ratio in years 0 to +2, which is similar to the dynamics of the effect of RTW on investment in the post-1966 sample. Consistent with column (1) of Table XIV, investment positively responds to RTW in years +3 to +5, but statistical significance is only realized in year +5 with an estimated slope coefficient of 0.0329. Unlike Table V, the impact of RTW on investment fully dissipates after 5 years in the absence of Texas, making $RTW > +5$ insignificant in column (1). Moving on to profitability in column (5) of Table XV, we observe the same dynamics of the effect of RTW on the operating income-asset ratio after dropping Texas in the extended sample: Right-to-Work introduction provides an immediate boost to profitability in year 0 and then again in year +5. The lead-lag relationship between profitability and investment from the spline regression suggest the wage mechanism outlined in Section 4 is still a potential driver of firms' ability to invest more after Right-to-Work implementation. Overall, we have sufficient evidence to conclude that although Texas has an especially strong influence regarding the impact of Right-to-Work on firm investment, our main hypothesis survives in the remaining

RTW states in the absence of Texas.

## 6 Conclusion

This paper investigates the underlying link between labor constraints and firm investment. To the extent that wages are sticky and it is costly for firms to hire and fire workers, labor contracts behave similar to debt contracts on firms' balance sheets requiring commitment of current resources and future cash flows. As a result, labor constraints stemming from wages and labor inputs not adjusting instantaneously has real effect on firms' investment decision.

Using Right-to-Work law introduction as a laboratory, we examine the impact of an exogenous shock that relaxes labor constraints on firm-level investment. We have three main findings. First, investment ratios for firms headquartered in Right-to-Work counties are significantly higher after RTW treatment relative to untreated firms headquartered in non-RTW states or in RTW states but prior to treatment. Second, Right-to-Work introduction causes wage to fall and boosts operating income, which partially contributes to the investment rise. Finally, we find evidence that RTW affects investment through the debt channel. RTW changes the firm's leverage ratio, which then changes it's investment rate.

Labor constraints have real effects on firms' investment and financing decisions not unlike financial constraints. However, the literature on labor constraints is not nearly as established as the one on financial constraints in finance. This paper provides a link between labor and corporate finance that is nontrivial and economically important.

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Table I: Summary Statistics of State Right-to-Work Laws in the U.S.

This table presents the list of states in the U.S. 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 U.S. Census Bureau. STUSAB is the state abbreviation. Name is the name of the state. Year RTW is the year during which Right-to-Work legislation becomes effective in a particular state. This is hand collected by reading either constitution amendments or labor codes. Texas, for example, passed the Right-to-Work statute in 1947, but it was not enacted until 1993.

| 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 | 1985 | 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 | | 48 | TX | Texas | 1993 |
| 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 | |
| 27 | MN | Minnesota | | 55 | WI | Wisconsin | 2015 |
| 28 | MS | Mississippi | 1960 | 56 | WY | Wyoming | 1963 |
| 29 | MO | Missouri | | | | | |

Table II: Summary Statistics of Firm Characteristics

This table reports summary statistics of key variables used in the analysis from 1966 to 2014, comprised of 76,520 firm-year observations from CRSP-Compustat. Financial and utility are dropped from the sample. Then, the following screens are applied: asset (atq), sales (saleq), cash (cheq), long-term debt (dlttq), liabilities (ltq) or dividend (dvq) less than zero; equity less than $10 million; book to market ratio less than 0.01 or greater than 100. Panel A reports the summary statistics of firm characteristics. ROE is return on book equity, where book equity values are constructed based on formula from Kenneth French's website. Panel B reports the summary statistics of the financial constraints index for completeness. Panel C summarizes the number of RTW observations in each of the Right-to-Work states in the sample.

| Panel A | (1) mean | (2) p25 | (3) p50 | (4) p75 | (5) sd | (6) max | (7) min |
|---|---|---|---|---|---|---|---|
| Asset | 1860 | 54 | 172 | 699 | 11663 | 750507 | 0.705 |
| Cash | 171 | 3.621 | 15 | 62 | 1280 | 77022 | 0 |
| Dividends | 34.291 | 0 | 0 | 3.891 | 279 | 10875 | 0 |
| Inv/A | 0.067 | 0.024 | 0.048 | 0.087 | 0.065 | 0.354 | 0 |
| Inv/PPE | 0.141 | 0.066 | 0.112 | 0.188 | 0.104 | 0.465 | 0 |
| EmpGr | 0.069 | -0.043 | 0.030 | 0.133 | 0.249 | 1.321 | -0.510 |
| Sales/A | 1.266 | 0.728 | 1.141 | 1.607 | 0.806 | 4.654 | 0.054 |
| Debt/A | 0.206 | 0.042 | 0.186 | 0.321 | 0.175 | 0.704 | 0 |
| OI/A | 0.127 | 0.073 | 0.139 | 0.207 | 0.151 | 0.503 | -0.461 |
| Tangiblity | 0.534 | 0.262 | 0.463 | 0.737 | 0.352 | 1.639 | 0.035 |
| Cashflow | 0.099 | 0.048 | 0.100 | 0.153 | 4.431 | 1113.000 | -65.932 |
| Tobin's q | 1.762 | 0.980 | 1.298 | 1.932 | 1.634 | 57.496 | -3.145 |
| BE/ME | 0.780 | 0.352 | 0.602 | 0.986 | 0.756 | 31.530 | 0.010 |
| ROE | 0.133 | 0.005 | 0.102 | 0.181 | 3.305 | 583.803 | -1.000 |

| Panel B | (1) mean | (2) p25 | (3) p50 | (4) p75 | (5) sd | (6) max | (7) min |
|---|---|---|---|---|---|---|---|
| Whited and Wu Index | -0.263 | -0.330 | -0.256 | -0.189 | 0.138 | 0.807 | -11.929 |

| Panel C | (1) Total | (2) Idaho | (3) Indiana | (4) Louisiana | (5) Michigan | (6) Oklahoma | (7) Texas |
|---|---|---|---|---|---|---|---|
| Num. of RTW Obs. | 6407 | 172 | 45 | 586 | 34 | 254 | 5316 |

Table III: Full Panel Study of Firm Accounting Variables on Right-to-Work

This table reports the coefficient estimates of panel regressions by pooling all firm-year observations from 1966 to 2014. The Right-to-Work law indicator (*Post RTW*) is the main explanatory variable. Six different firm characteristics are used as the dependent variables. Column (1) is for investment defined as capital expenditure (CAPX) over lagged assets. Column (2) is for investment defined as capital expenditure over lagged property, plant, and equipment (PPE). Column (3) is for the growth rate of number of employees. Column (4) is for sales over assets. Column (5) is for operating income (OIBDP) over lagged assets. Column (6) is for book leverage. All regressions include firm and year fixed effects. Robust standard errors with clustering at the firm level are used in reporting the t-statistics in parentheses. There are 76,520 firm-year observations.

| | (1) Inv/A | (2) Inv/PPE | (3) EmpGr | (4) Sales/A | (5) OI/A | (6) Debt/A |
|---|---|---|---|---|---|---|
| RTW | 0.00966*** | 0.0128** | 0.0141*** | 0.0645*** | 0.0196*** | -0.0194*** |
| | (4.34) | (2.38) | (3.67) | (3.02) | (6.12) | (-3.67) |
| LogAsset | -0.00691*** | 0.000659 | -0.0654*** | -0.130*** | -0.0145*** | 0.0352*** |
| | (-11.24) | (0.40) | (-12.31) | (-13.71) | (-6.13) | (20.00) |
| Tobin Q | 0.00375*** | 0.0112*** | 0.0151*** | -0.00267*** | 0.0101*** | -0.00316*** |
| | (4.45) | (6.85) | (6.98) | (-2.80) | (4.03) | (-4.77) |
| Cashflow | -0.000235 | 0.0000506 | 0.000623 | 0.000398 | 0.00220 | |
| | (-1.02) | (0.09) | (0.92) | (0.33) | (0.94) | |
| Leverage | -0.0347*** | -0.0839*** | -0.116*** | -0.0816*** | -0.0466*** | |
| | (-9.43) | (-15.79) | (-9.23) | (-2.79) | (-6.52) | |
| GDP Growth | 0.127*** | 0.194*** | 0.304** | 0.0573 | 0.181*** | -0.0217 |
| | (3.79) | (4.82) | (2.61) | (0.54) | (3.38) | (-0.75) |
| Profitability | | | | | | -0.0833*** |
| | | | | | | (-5.42) |
| Tangiblity | | | | | | 0.0347*** |
| | | | | | | (2.93) |
| Firm FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Adjusted $R^2$ | 0.585 | 0.379 | 0.165 | 0.848 | 0.617 | 0.671 |

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

Table IV: Full Panel Study of Firm Accounting Variables on Right-to-Work and Financial Constraints

This table reports the coefficient estimates of panel regressions by pooling all firm-year observations from 1966 to 2014. Regressions use the *RTW* dummy and the financial constraint indicator (*FC Dummy*) and the interaction ($FC \times RTW$) as the main explanatory variables. Six different firm characteristics are used as the dependent variables in both panels. Column (1) is for investment defined as capital expenditure (CAPX) over lagged assets. Column (2) is for investment defined as capital expenditure over lagged property, plant, and equipment (PPE). Column (3) is for the growth rate of number of employees. Column (4) is for sales over assets. Column (5) is for operating income (OIBDP) over lagged assets. Column (6) is for book leverage. All regressions include firm and year fixed effects. Robust standard errors with clustering at the firm level are used in reporting the t-statistics in parentheses. There are 76,520 firm-year observations.

| | (1) Inv/A | (2) Inv/PPE | (3) EmpGr | (4) Sales/A | (5) OI/A | (6) Debt/A |
|---|---|---|---|---|---|---|
| RTW | 0.0146*** | 0.0235*** | 0.0297*** | 0.0722*** | 0.0243*** | -0.0288*** |
| | (5.50) | (4.18) | (5.17) | (3.28) | (5.94) | (-4.26) |
| FC Dummy | -0.0183*** | -0.0268*** | -0.175*** | 0.0214** | -0.0900*** | 0.0274*** |
| | (-7.18) | (-6.65) | (-11.19) | (2.40) | (-7.81) | (6.93) |
| FCxRTW | -0.0149*** | -0.0313*** | -0.0493*** | -0.0106 | -0.0306*** | 0.00998*** |
| | (-4.88) | (-7.00) | (-4.40) | (-0.63) | (-2.99) | (3.38) |
| LogAsset | -0.00929*** | -0.00293* | -0.0873*** | -0.128*** | -0.0258*** | 0.0387*** |
| | (-13.28) | (-1.78) | (-15.26) | (-13.48) | (-12.65) | (20.09) |
| Tobin Q | 0.00352*** | 0.0108*** | 0.0131*** | -0.00247** | 0.00898*** | -0.00287*** |
| | (3.98) | (6.40) | (4.74) | (-2.19) | (3.59) | (-4.11) |
| Cashflow | -0.000269 | -0.00000764 | 0.000350 | 0.000416 | 0.00205 | |
| | (-1.15) | (-0.01) | (0.52) | (0.28) | (0.87) | |
| Leverage | -0.0327*** | -0.0809*** | -0.0986*** | -0.0833*** | -0.0375*** | |
| | (-9.33) | (-15.26) | (-7.56) | (-2.86) | (-5.43) | |
| GDP Growth | 0.128*** | 0.195*** | 0.305** | 0.0571 | 0.180*** | -0.0224 |
| | (3.78) | (4.80) | (2.61) | (0.54) | (3.29) | (-0.77) |
| Profitability | | | | | | -0.0810*** |
| | | | | | | (-5.27) |
| Tangiblity | | | | | | 0.0355*** |
| | | | | | | (3.06) |
| Firm FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Adjusted $R^2$ | 0.589 | 0.382 | 0.184 | 0.848 | 0.633 | 0.672 |

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

Table V: Spline Regression of Firm Accounting Variables on Leading and Lagged Right-to-Work Dummies: Full Panel

This table reports the coefficient estimates of the spline regressions on the following firm accounting variables: investment over assets, investment over property, plant, and equipment, employment growth rate, sales over assets, operating income over assets, and total debt over assets, in order from columns (1) to (6). The explanatory variables are dummies denoting each year in the eleven-year window around the Right-to-Work enactment plus two dummies denoting if a particular observation is more than five years before or after the enactment of the law. Observations in the one year immediately before the Right-to-Work law implementation do not have a RTW dummy and serve as the benchmark. All regressions include firm and year fixed effects while controlling for the same firm-level and aggregate variables as the panel regressions in Table III. Robust standard errors with double clustering at the firm- and year-level are used in reporting the t-statistics in parentheses. There are a total of 76,520 firm-year observations.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Inv/A | Inv/PPE | EmpGr | Sales/A | OI/A | Debt/A |
| RTW <-5 | 0.00627 | 0.00842 | -0.0138 | -0.0322 | -0.00192 | 0.0116 |
| | (1.18) | (1.38) | (-1.64) | (-1.47) | (-0.17) | (1.64) |
| RTW -5 | -0.00511* | -0.0150*** | -0.00586 | -0.0346 | -0.00705 | 0.00668 |
| | (-1.75) | (-3.56) | (-0.36) | (-1.42) | (-0.96) | (0.76) |
| RTW -4 | -0.00538 | -0.00447 | -0.0146 | -0.0694*** | -0.00680 | 0.00510 |
| | (-1.25) | (-0.82) | (-1.11) | (-5.59) | (-1.04) | (0.78) |
| RTW -3 | 0.00173 | -0.000692 | 0.0232** | -0.0429*** | -0.00366 | -0.00160 |
| | (0.49) | (-0.11) | (2.19) | (-3.21) | (-0.30) | (-0.35) |
| RTW -2 | 0.00498 | 0.0104** | -0.0102 | -0.0288** | -0.00940 | 0.00279 |
| | (1.49) | (2.12) | (-0.63) | (-2.43) | (-1.13) | (0.76) |
| RTW 0 | 0.00174 | 0.00223 | 0.00728 | -0.0289*** | 0.00782** | 0.000174 |
| | (0.98) | (0.63) | (0.47) | (-2.97) | (2.19) | (0.04) |
| RTW +1 | 0.00654 | 0.0121* | -0.00974 | -0.0175 | 0.000429 | -0.00175 |
| | (1.49) | (1.86) | (-0.83) | (-0.94) | (0.04) | (-0.70) |
| RTW +2 | 0.00274 | 0.00355 | 0.0181*** | -0.00541 | 0.0107 | 0.00600 |
| | (0.51) | (0.51) | (2.87) | (-0.31) | (1.21) | (1.21) |
| RTW +3 | 0.0136*** | 0.0116*** | 0.0159 | -0.00232 | 0.0229** | -0.00463 |
| | (3.99) | (3.08) | (1.51) | (-0.18) | (2.22) | (-1.12) |
| RTW +4 | 0.0215*** | 0.0281*** | 0.0218*** | -0.00646 | 0.0206** | -0.0107* |
| | (5.17) | (4.22) | (3.13) | (-0.48) | (2.21) | (-1.98) |
| RTW +5 | 0.0298*** | 0.0365*** | 0.00372 | -0.0102 | 0.0139** | -0.00173 |
| | (10.84) | (6.08) | (0.35) | (-0.38) | (2.10) | (-0.24) |
| RTW >+5 | 0.0165*** | 0.0224** | 0.00222 | 0.0772*** | 0.0216** | -0.0208** |
| | (3.45) | (2.72) | (0.32) | (3.15) | (2.10) | (-2.51) |
| Adjusted $R^2$ | 0.586 | 0.379 | 0.165 | 0.848 | 0.617 | 0.671 |

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

Table VI: Geographic Discontinuity Study of Firm Accounting Variables on Right-to-Work and Financial Constraint: 150 Miles Filter

This table reports the coefficient estimates of discontinuity regressions by using firm-year observations in Right-to-Work counties and non-RTW counties that are located within 150 miles of one another. Distances between two counties are defined by the geographic centroids of the counties published by the U.S. Census. The *RTW* and the financial constraint indicator (*FC Dummy*) as well as the interaction ($FC \times RTW$) are the main explanatory variables. Six different firm characteristics are used as the dependent variables in both panels. Column (1) is for investment defined as capital expenditure (CAPX) over lagged assets. Column (2) is for investment defined as capital expenditure over lagged property, plant, and equipment (PPE). Column (3) is for the growth rate of number of employees. Column (4) is for sales over assets. Column (5) is for operating income (OIBDP) over lagged assets. Column (6) is for book leverage. All regressions include firm and year fixed effects. Robust standard errors with clustering at the firm level are used in reporting the t-statistics in parentheses. There are 27,083 firm-year observations.

| | (1) Inv/A | (2) Inv/PPE | (3) EmpGr | (4) Sales/A | (5) OI/A | (6) Debt/A |
|---|---|---|---|---|---|---|
| RTW | 0.0167** | 0.0180** | -0.00438 | 0.0478 | 0.0205** | -0.0522 |
| | (2.56) | (2.37) | (-0.15) | (0.61) | (2.05) | (-1.43) |
| FC Dummy | -0.0207*** | -0.0285*** | -0.178*** | 0.0123 | -0.0902*** | 0.0282*** |
| | (-4.14) | (-4.67) | (-12.28) | (0.51) | (-10.26) | (2.99) |
| FCxRTW | 0.00375 | 0.00121 | 0.00139 | 0.0632 | 0.0170* | -0.00406 |
| | (0.96) | (0.22) | (0.11) | (1.09) | (1.71) | (-0.30) |
| LogAsset | -0.0109*** | -0.00679*** | -0.0865*** | -0.143*** | -0.0239*** | 0.0416*** |
| | (-7.91) | (-2.88) | (-10.97) | (-8.72) | (-6.78) | (10.98) |
| Tobin Q | 0.00525*** | 0.0143*** | 0.0162*** | -0.00266 | 0.0141*** | -0.00377** |
| | (6.61) | (9.05) | (4.74) | (-0.71) | (4.84) | (-2.52) |
| Cashflow | -0.000467*** | -0.000414 | -0.0000774 | 0.000193 | 0.000679 | |
| | (-3.40) | (-1.30) | (-0.21) | (0.38) | (0.69) | |
| Leverage | -0.0387*** | -0.0779*** | -0.0855*** | -0.112*** | -0.0530*** | |
| | (-10.56) | (-13.21) | (-4.14) | (-2.90) | (-5.07) | |
| GDP Growth | 0.158*** | 0.201*** | 0.268* | 0.0663 | 0.137 | 0.0146 |
| | (3.21) | (2.96) | (1.92) | (0.44) | (1.57) | (0.31) |
| Profitability | | | | | | -0.100*** |
| | | | | | | (-5.35) |
| Tangiblity | | | | | | 0.0181 |
| | | | | | | (1.37) |
| Firm FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Adjusted $R^2$ | 0.587 | 0.392 | 0.156 | 0.854 | 0.606 | 0.681 |

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

Table VII: Geographic Discontinuity Study of Firm Accounting Variables on Right-to-Work and Financial Constraint: 100 Miles Filter

This table reports the coefficient estimates of discontinuity regressions by using firm-year observations in Right-to-Work counties and non-RTW counties that are located within 100 miles of one another. Distances between two counties are defined by the geographic centroids of the counties published by the U.S. Census. The *RTW* and the financial constraint indicator (*FC Dummy*) as well as the interaction ($FC \times RTW$) are the main explanatory variables. Six different firm characteristics are used as the dependent variables in both panels. Column (1) is for investment defined as capital expenditure (CAPX) over lagged assets. Column (2) is for investment defined as capital expenditure over lagged property, plant, and equipment (PPE). Column (3) is for the growth rate of number of employees. Column (4) is for sales over assets. Column (5) is for operating income (OIBDP) over lagged assets. Column (6) is for book leverage. All regressions include firm and year fixed effects. Robust standard errors with clustering at the firm level are used in reporting the t-statistics in parentheses. There are 11,842 firm-year observations.

| | (1) Inv/A | (2) Inv/PPE | (3) EmpGr | (4) Sales/A | (5) OI/A | (6) Debt/A |
|---|---|---|---|---|---|---|
| RTW | 0.0307*** | 0.0124* | 0.00591 | 0.110 | 0.0347*** | -0.0635*** |
| | (4.67) | (1.76) | (0.18) | (0.85) | (2.96) | (-6.96) |
| FC Dummy | -0.0177*** | -0.0134*** | -0.160*** | 0.0649 | -0.0788*** | 0.0480*** |
| | (-3.45) | (-2.79) | (-5.39) | (0.92) | (-7.15) | (2.85) |
| FCxRTW | -0.000367 | -0.0118* | -0.0245 | -0.0207 | -0.00152 | -0.0295 |
| | (-0.06) | (-1.70) | (-0.78) | (-0.21) | (-0.10) | (-1.67) |
| LogAsset | -0.0129*** | -0.00981*** | -0.0927*** | -0.166*** | -0.0301*** | 0.0410*** |
| | (-8.71) | (-3.35) | (-8.79) | (-6.87) | (-6.29) | (7.95) |
| Tobin Q | 0.00497*** | 0.0127*** | 0.0130*** | 0.000487 | 0.0124*** | -0.00418* |
| | (4.84) | (5.00) | (3.47) | (0.08) | (4.00) | (-1.95) |
| Cashflow | -0.000566*** | -0.000615*** | -0.000269 | -0.000137 | 0.000147 | |
| | (-8.36) | (-5.19) | (-0.98) | (-0.61) | (0.39) | |
| Leverage | -0.0427*** | -0.0803*** | -0.0710** | -0.128 | -0.0530*** | |
| | (-8.65) | (-6.86) | (-2.18) | (-1.58) | (-4.17) | |
| GDP Growth | 0.0852* | 0.103* | 0.0608 | 0.174 | 0.0883* | -0.0430 |
| | (1.86) | (1.94) | (0.57) | (0.92) | (1.80) | (-0.60) |
| Profitability | | | | | | -0.0877*** |
| | | | | | | (-4.06) |
| Tangiblity | | | | | | 0.0216 |
| | | | | | | (1.39) |
| Firm FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Adjusted $R^2$ | 0.579 | 0.412 | 0.159 | 0.842 | 0.638 | 0.674 |

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

Table VIII: State Diagnostics of Firm Investment and Profitability on Right-to-Work and Financial Constraint

This table reports the coefficient estimates of panel regressions by pooling all firm-year observations from 1966 to 2014 while only keeping one Right-to-Work state at a time. Regressions use $RTW$ and the financial constraint indicator ($FC\ Dummy$) and the interaction ($FC \times RTW$) as explanatory variables. Firm investment-to-asset and operating income-to-asset ratios are used as the dependent variables in the regressions. Column (1) and Column (2) report results of the regression by keeping only the state of Louisiana. Column (3) and Column (4) report results of the regression by keeping only the state of Oklahoma. Column (5) and Column (6) report results of the regression by keeping only the state of Texas. All regressions include firm and year fixed effects. Robust standard errors with clustering at the firm level are used in reporting the t-statistics in parentheses.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Louisiana | | Oklahoma | | Texas | |
| | Inv/A | OI/A | Inv/A | OI/A | Inv/A | OI/A |
| RTW | 0.0201** | 0.00733 | 0.0486*** | 0.0705*** | 0.0197*** | 0.0293*** |
| | (2.50) | (1.16) | (26.83) | (6.71) | (5.81) | (6.84) |
| FC Dummy | -0.0175*** | -0.0953*** | -0.0178*** | -0.0957*** | -0.0194*** | -0.0942*** |
| | (-8.68) | (-6.76) | (-8.04) | (-6.83) | (-7.19) | (-7.51) |
| FCxRTW | -0.0209*** | -0.00950 | -0.0419*** | 0.00219 | -0.0208*** | -0.0475*** |
| | (-3.24) | (-0.56) | (-12.85) | (0.20) | (-5.55) | (-6.43) |
| LogAsset | -0.00868*** | -0.0241*** | -0.00869*** | -0.0245*** | -0.00852*** | -0.0218*** |
| | (-9.67) | (-5.42) | (-9.71) | (-5.90) | (-10.65) | (-6.21) |
| Tobin Q | 0.00317*** | 0.00798*** | 0.00313*** | 0.00794*** | 0.00354*** | 0.00913*** |
| | (4.99) | (3.48) | (5.74) | (3.74) | (4.85) | (4.24) |
| Cashflow | 0.00905*** | 0.0853*** | 0.00884** | 0.0855*** | -0.000379 | 0.00130 |
| | (2.82) | (4.92) | (2.58) | (4.91) | (-1.68) | (0.75) |
| Leverage | -0.0307*** | -0.0219 | -0.0319*** | -0.0222 | -0.0326*** | -0.0425*** |
| | (-6.39) | (-1.41) | (-6.35) | (-1.40) | (-7.70) | (-4.17) |
| GDP Growth | 0.0665*** | 0.0180 | 0.0861*** | 0.0377 | 0.151*** | 0.186* |
| | (2.90) | (0.38) | (2.97) | (0.72) | (3.02) | (1.86) |
| Firm FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 41409 | 41409 | 41701 | 41701 | 47031 | 47031 |
| Adjusted $R^2$ | 0.551 | 0.669 | 0.563 | 0.669 | 0.585 | 0.641 |

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

Table IX: Impact of Right-to-Work Introduction on County-Level Average Annual Payroll

This table reports the coefficient estimates of the panel regression of county-level average annual payroll on the *RTW* dummy. Columns (1) and (2) are from regressions using all county-year observations from 1986 to 2013. Columns (3) and (4) are from regressions where observations in states that passed RTW legislation prior to 1986 are dropped. Furthermore, columns (2) and (4) are from regressions that control for state-level real GDP growth. All regressions include year fixed effects. Robust standard errors with double clustering at the state- and year-level are used in reporting the t-statistics in parentheses. Data sources is the County Business Patterns data from the U.S. Census Bureau. Measure of unit is in thousands of dollars.

| | (1) County Average Annual Payroll | (2) County Average Annual Payroll | (3) County Average Annual Payroll | (4) County Average Annual Payroll |
|---|---|---|---|---|
| RTW | -2.152*** | -2.185*** | -2.109** | -2.158*** |
| | (-3.11) | (-3.17) | (-2.75) | (-2.85) |
| GDP Growth | | 4.375 | | 2.625 |
| | | (1.26) | | (0.75) |
| Year FE | ✓ | ✓ | ✓ | ✓ |
| Observations | 87366 | 87366 | 47495 | 47495 |
| Adjusted $R^2$ | 0.522 | 0.522 | 0.484 | 0.485 |

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

Table X: Full Panel Study of Firm Accounting Variables on Right-to-Work and Employment Volatility

This table reports the coefficient estimates of panel regressions by pooling all firm-year observations from 1966 to 2014. Regressions use the *RTW* dummy and the high employment volatility indicator variable (*Emp Vol Dummy*) and the interaction ($EmpVol \times RTW$) as the main explanatory variables. Six different firm policies are used as the dependent variables across the six columns. Column (1) is for investment defined as capital expenditure (CAPX) over lagged assets. Column (2) is for investment defined as capital expenditure over lagged property, plant, and equipment (PPE). Column (3) is for the growth rate of number of employees. Column (4) is for sales over assets. Column (5) is for operating income (OIBDP) over lagged assets. Column (6) is for book leverage. All regressions include firm and year fixed effects. Robust standard errors with clustering at the firm level are used in reporting the t-statistics in parentheses. There are 40,675 firm-year observations.

| | (1) Inv/A | (2) Inv/PPE | (3) EmpGr | (4) Sales/A | (5) OI/A | (6) Debt/A |
|---|---|---|---|---|---|---|
| RTW | 0.0179*** | 0.0198*** | 0.0197* | 0.102*** | 0.0246*** | -0.0144** |
| | (4.73) | (3.03) | (1.92) | (4.38) | (6.08) | (-2.18) |
| Emp Vol Dummy | -0.000719 | -0.00647*** | -0.0298*** | 0.0400*** | -0.00619* | 0.00596* |
| | (-0.58) | (-3.14) | (-4.78) | (4.04) | (-1.89) | (1.81) |
| EmpVolxRTW | -0.0112** | -0.00980 | -0.0149 | -0.0635*** | -0.0107*** | -0.0162*** |
| | (-2.21) | (-1.64) | (-1.54) | (-5.91) | (-2.82) | (-2.77) |
| LogAsset | -0.00645*** | 0.00593** | -0.0469*** | -0.163*** | -0.0175*** | 0.0405*** |
| | (-7.68) | (2.68) | (-8.51) | (-9.10) | (-5.76) | (12.17) |
| Tobin Q | 0.00631*** | 0.0154*** | 0.0202*** | 0.00151 | 0.0193*** | -0.00433*** |
| | (5.68) | (7.96) | (11.69) | (0.50) | (6.66) | (-3.14) |
| Cashflow | 0.0192 | 0.0447* | 0.0311 | 0.0211 | 0.105** | |
| | (1.53) | (1.83) | (1.10) | (1.42) | (2.15) | |
| Leverage | -0.0442*** | -0.0843*** | -0.136*** | -0.0764* | -0.0392*** | |
| | (-12.90) | (-11.93) | (-10.17) | (-1.89) | (-4.79) | |
| GDP Growth | 0.122*** | 0.180*** | 0.342** | 0.293 | 0.184*** | -0.0448 |
| | (4.39) | (5.15) | (2.71) | (1.66) | (3.02) | (-1.42) |
| Profitability | | | | | | -0.144*** |
| | | | | | | (-9.17) |
| Tangiblity | | | | | | 0.0265** |
| | | | | | | (2.50) |
| Firm FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Adjusted $R^2$ | 0.607 | 0.397 | 0.139 | 0.859 | 0.612 | 0.685 |

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

Table XI: Full Panel Study of Firm Accounting Variables on Right-to-Work and Unionization Rates

This table reports the coefficient estimates of panel regressions by pooling all firm-year observations from 1966 to 2014. The *RTW* dummy, the high unionization membership indicator variable (*Union Dummy*), and the interaction (*Union* × *RTW*) are the main explanatory variables. Six different firm characteristics are used as the independent variables in both panels. Column (1) is for investment defined as capital expenditure (CAPX) over lagged assets. Column (2) is for investment defined as capital expenditure over lagged property, plant, and equipment (PPE). Column (3) is for the growth rate of number of employees. Column (4) is for sales over assets. Column (5) is for operating income (OIBDP) over lagged assets. Column (6) is for book leverage. All regressions include firm and year fixed effects. Robust standard errors with clustering at the firm level are used in reporting the t-statistics in parentheses. There are 51,314 firm-year observations.

| | (1) Inv/A | (2) Inv/PPE | (3) EmpGr | (4) Sales/A | (5) OI/A | (6) Debt/A |
|---|---|---|---|---|---|---|
| RTW | 0.0185*** | 0.0219*** | 0.0207*** | 0.0417* | 0.0248*** | -0.00840 |
| | (5.34) | (3.84) | (2.84) | (1.86) | (6.99) | (-1.41) |
| Union Dummy | 0.00287** | 0.00374* | 0.00757* | 0.0123 | 0.00565* | 0.00380 |
| | (2.27) | (1.98) | (1.81) | (1.14) | (1.70) | (1.63) |
| UnionxRTW | -0.00827*** | -0.00131 | -0.00616 | 0.0228 | -0.000516 | -0.00990** |
| | (-3.42) | (-0.45) | (-0.43) | (1.08) | (-0.09) | (-2.61) |
| LogAsset | -0.00869*** | 0.00114 | -0.0843*** | -0.124*** | -0.0190*** | 0.0364*** |
| | (-11.71) | (0.54) | (-12.57) | (-11.51) | (-5.92) | (15.24) |
| Tobin Q | 0.00331*** | 0.0100*** | 0.0144*** | -0.00152 | 0.00772*** | -0.00232*** |
| | (3.95) | (5.66) | (4.62) | (-1.51) | (3.11) | (-4.12) |
| Cashflow | -0.000369** | -0.000229 | 0.000262 | 0.0000931 | 0.00131 | |
| | (-2.05) | (-0.52) | (0.36) | (0.10) | (0.84) | |
| Leverage | -0.0406*** | -0.0914*** | -0.150*** | -0.0363 | -0.0479*** | |
| | (-8.15) | (-13.75) | (-11.65) | (-1.15) | (-6.31) | |
| GDP Growth | 0.0915*** | 0.141*** | 0.208 | 0.108 | 0.176*** | -0.0910** |
| | (3.64) | (4.11) | (1.44) | (0.65) | (3.44) | (-2.11) |
| Profitability | | | | | | -0.0755*** |
| | | | | | | (-4.75) |
| Tangiblity | | | | | | 0.0337*** |
| | | | | | | (2.84) |
| Firm FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Adjusted $R^2$ | 0.604 | 0.406 | 0.180 | 0.855 | 0.649 | 0.699 |

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

Table XII: Spline Regression of Firm Accounting Variables on Leading and Lagged Right-to-Work Dummies: Low Union Membership Rate Firms

This table reports the coefficient estimates of the spline regressions on the following accounting variables of firms with low union membership rates: investment over assets, investment over property, plant, and equipment, employment growth rate, sales over assets, operating income over assets, and total debt over assets, in order from columns (1) to (6). The explanatory variables are dummies denoting each year in the eleven-year window around the Right-to-Work enactment plus two dummies denoting if a particular observation is more than five years before or after the enactment of the law. Observations in the one year immediately before the Right-to-Work law implementation do not have a RTW dummy and serve as the benchmark. All regressions include firm and year fixed effects while controlling for the same firm-level and aggregate variables as the panel regressions in Table III. Robust standard errors with double clustering at the firm- and year-level are used in reporting the t-statistics in parentheses. There are a total of 25,836 firm-year observations.

| | (1) Inv/A | (2) Inv/PPE | (3) EmpGr | (4) Sales/A | (5) OI/A | (6) Debt/A |
|---|---|---|---|---|---|---|
| RTW <-5 | -0.000279 | -0.00911 | -0.0510*** | -0.00415 | -0.0176 | 0.0148 |
| | (-0.04) | (-1.19) | (-2.95) | (-0.09) | (-0.95) | (1.09) |
| RTW -5 | -0.00924 | -0.0261** | -0.0324* | 0.0117 | -0.00827 | 0.00454 |
| | (-1.30) | (-2.07) | (-1.77) | (0.21) | (-1.14) | (0.27) |
| RTW -4 | -0.00638 | -0.00537 | 0.000230 | -0.0104 | 0.00739 | 0.00611 |
| | (-0.87) | (-0.46) | (0.01) | (-0.24) | (0.83) | (0.57) |
| RTW -3 | 0.00979 | 0.00481 | 0.0113 | 0.00295 | 0.00896 | 0.00368 |
| | (1.57) | (0.69) | (0.49) | (0.11) | (0.42) | (0.33) |
| RTW -2 | 0.00248 | 0.00666 | -0.00755 | -0.0268 | 0.00651 | 0.00968** |
| | (0.57) | (1.51) | (-0.36) | (-0.92) | (0.58) | (2.16) |
| RTW 0 | 0.000968 | -0.000897 | -0.0291* | -0.00983 | 0.00595 | 0.00335 |
| | (0.36) | (-0.20) | (-1.94) | (-0.55) | (0.97) | (0.30) |
| RTW +1 | 0.00818 | 0.0177** | -0.00933 | 0.00357 | 0.00191 | 0.0107 |
| | (1.06) | (2.67) | (-0.68) | (0.12) | (0.11) | (1.19) |
| RTW +2 | 0.0129** | 0.0172* | 0.0146 | -0.00932 | 0.0174 | 0.0156** |
| | (2.68) | (2.01) | (0.96) | (-0.57) | (0.99) | (2.25) |
| RTW +3 | 0.0234*** | 0.0210** | 0.0244** | -0.0370*** | 0.0283 | 0.00533 |
| | (4.50) | (2.50) | (2.23) | (-3.07) | (1.36) | (0.96) |
| RTW +4 | 0.0348*** | 0.0417*** | 0.0213** | 0.00636 | 0.0342** | 0.00411 |
| | (5.32) | (5.67) | (2.15) | (0.36) | (2.21) | (0.45) |
| RTW +5 | 0.0481*** | 0.0541*** | -0.00180 | -0.0364 | 0.00847 | 0.0203** |
| | (13.83) | (7.80) | (-0.11) | (-1.45) | (1.15) | (2.38) |
| RTW >+5 | 0.0268*** | 0.0342*** | 0.00322 | 0.0594* | 0.0215 | -0.00112 |
| | (4.87) | (4.67) | (0.36) | (2.00) | (1.36) | (-0.10) |
| Adjusted $R^2$ | 0.649 | 0.384 | 0.193 | 0.875 | 0.683 | 0.710 |

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

Table XIII: Spline Regression of Firm Accounting Variables on Leading and Lagged Right-to-Work Dummies: High Union Membership Rate Firms

This table reports the coefficient estimates of the spline regressions on the following accounting variables of firms with high union membership rates: investment over assets, investment over property, plant, and equipment, employment growth rate, sales over assets, operating income over assets, and total debt over assets, in order from columns (1) to (6). The explanatory variables are dummies denoting each year in the eleven-year window around the Right-to-Work enactment plus two dummies denoting if a particular observation is more than five years before or after the enactment of the law. Observations in the one year immediately before the Right-to-Work law implementation do not have a RTW dummy and serve as the benchmark. All regressions include firm and year fixed effects while controlling for the same firm-level and aggregate variables as the panel regressions in Table III. Robust standard errors with double clustering at the firm- and year-level are used in reporting the t-statistics in parentheses. There are a total of 25,478 firm-year observations.

| | (1) Inv/A | (2) Inv/PPE | (3) EmpGr | (4) Sales/A | (5) OI/A | (6) Debt/A |
|---|---|---|---|---|---|---|
| RTW <-5 | 0.00199 | 0.00632 | 0.0157 | -0.0213 | -0.000223 | -0.00671 |
| | (0.44) | (0.72) | (0.89) | (-0.99) | (-0.02) | (-0.46) |
| RTW -5 | -0.00393 | -0.0150** | 0.0316 | -0.0574 | -0.0135 | 0.0000721 |
| | (-0.85) | (-2.23) | (0.89) | (-1.40) | (-1.49) | (0.01) |
| RTW -4 | -0.00362 | -0.00357 | -0.00785 | -0.114*** | -0.0277** | -0.00444 |
| | (-1.38) | (-0.54) | (-0.40) | (-5.93) | (-2.73) | (-0.35) |
| RTW -3 | -0.00553** | -0.00452 | 0.0312 | -0.0570 | -0.0145 | -0.0147 |
| | (-2.25) | (-0.78) | (1.10) | (-1.58) | (-1.29) | (-1.22) |
| RTW -2 | 0.00851** | 0.0211*** | -0.00238 | -0.0233* | -0.0231*** | -0.00724 |
| | (2.59) | (2.82) | (-0.08) | (-1.79) | (-2.98) | (-1.09) |
| RTW 0 | 0.00257 | 0.00420 | -0.00896 | -0.0256 | 0.00243 | -0.00608 |
| | (0.57) | (0.56) | (-0.62) | (-1.21) | (0.31) | (-0.64) |
| RTW +1 | 0.00531 | 0.00898 | -0.0196 | -0.0304 | 0.00495 | -0.0170 |
| | (1.15) | (1.07) | (-0.95) | (-1.02) | (0.43) | (-1.58) |
| RTW +2 | -0.00194 | -0.00413 | 0.0270 | -0.00316 | 0.00123 | -0.00543 |
| | (-0.57) | (-0.84) | (1.25) | (-0.18) | (0.12) | (-0.61) |
| RTW +3 | 0.00192 | 0.00444 | -0.00891 | 0.0287** | 0.00445 | -0.0195 |
| | (0.63) | (1.30) | (-0.46) | (2.34) | (0.52) | (-1.41) |
| RTW +4 | 0.00959** | 0.0253*** | 0.0477*** | 0.000656 | 0.00653 | -0.0294** |
| | (2.05) | (3.61) | (2.90) | (0.03) | (0.89) | (-2.44) |
| RTW +5 | 0.0114** | 0.0268*** | -0.00245 | -0.0239 | 0.0122 | -0.0256** |
| | (2.67) | (3.53) | (-0.13) | (-0.75) | (1.57) | (-2.06) |
| RTW >+5 | 0.00968*** | 0.0217*** | 0.0302* | 0.0742** | 0.0224*** | -0.0401*** |
| | (3.14) | (3.50) | (1.94) | (2.33) | (3.06) | (-3.01) |
| Adjusted $R^2$ | 0.553 | 0.442 | 0.188 | 0.848 | 0.609 | 0.699 |

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

Table XIV: Extended Sample from 1952 to 2014 of Firm Accounting Variables on Right-to-Work and Financial Constraints: Excluding Texas

This table reports the coefficient estimates of panel regressions by pooling all firm-year observations from 1952 to 2014 while excluding all observations from the state of Texas. Each year, the top decile of firms by total asset are also dropped. Regressions use the *RTW* dummy ONLY in years 3 to 5 after Right-to-Work introduction and the financial constraint indicator (*FC Dummy*) and the interaction ($FC \times RTW$) as the main explanatory variables. Six different firm characteristics are used as the dependent variables in both panels. Column (1) is for investment defined as capital expenditure (CAPX) over lagged assets. Column (2) is for investment defined as capital expenditure over lagged property, plant, and equipment (PPE). Column (3) is for the growth rate of number of employees. Column (4) is for sales over assets. Column (5) is for operating income (OIBDP) over lagged assets. Column (6) is for book leverage. All regressions include firm and year fixed effects. Robust standard errors with clustering at the firm level are used in reporting the t-statistics in parentheses. There are 73,521 firm-year observations.

| | (1) Inv/A | (2) Inv/PPE | (3) EmpGr | (4) Sales/A | (5) OI/A | (6) Debt/A |
|---|---|---|---|---|---|---|
| Years 3-5 post-RTW | 0.0500** | 0.0522*** | 0.0366 | 0.129* | 0.0524 | 0.0139 |
| | (2.47) | (2.87) | (1.34) | (1.95) | (1.68) | (1.14) |
| FC Dummy | -0.0201*** | -0.0349*** | -0.198*** | 0.0386*** | -0.0996*** | 0.0460*** |
| | (-12.69) | (-10.03) | (-12.38) | (3.31) | (-8.73) | (11.23) |
| FCxRTW | -0.0471* | -0.0423** | 0.00406 | -0.0955* | -0.0483 | 0.0287 |
| | (-1.77) | (-2.52) | (0.13) | (-1.80) | (-0.86) | (0.68) |
| LogAsset | -0.0103*** | -0.00305 | -0.0959*** | -0.126*** | -0.0127*** | 0.0420*** |
| | (-12.39) | (-1.25) | (-22.36) | (-13.38) | (-2.94) | (16.58) |
| Tobin Q | 0.00267*** | 0.00767*** | 0.00937*** | -0.00448*** | -0.0000248 | -0.000557 |
| | (6.12) | (7.26) | (5.79) | (-2.80) | (-0.02) | (-0.90) |
| Leverage | -0.0240*** | -0.0693*** | -0.0867*** | 0.0447** | -0.0461*** | |
| | (-6.75) | (-11.47) | (-11.32) | (2.36) | (-6.35) | |
| Profitability | | | | | | -0.0915*** |
| | | | | | | (-5.71) |
| Tangiblity | | | | | | 0.0528*** |
| | | | | | | (4.51) |
| Firm FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Adjusted $R^2$ | 0.557 | 0.368 | 0.185 | 0.845 | 0.696 | 0.657 |

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

Table XV: Spline Regression from 1952 to 2014 of Firm Accounting Variables on Leading and Lagged Right-to-Work Dummies: Excluding Texas

This table reports the coefficient estimates of the spline regressions on the following firm accounting variables: investment over assets, investment over property, plant, and equipment, employment growth rate, sales over assets, operating income over assets, and total debt over assets, in order from columns (1) to (6). The explanatory variables are dummies denoting each year in the eleven-year window around the Right-to-Work enactment plus two dummies denoting if a particular observation is more than five years before or after the enactment of the law. Observations in the one year immediately before the Right-to-Work law implementation do not have a RTW dummy and serve as the benchmark. All regressions include firm and year fixed effects while controlling for the same firm-level and aggregate variables as the panel regressions in Table III, except cashflow and state-level GDP growth. Robust standard errors with double clustering at the firm- and year-level are used in reporting the t-statistics in parentheses. There are a total of 73,521 firm-year observations.

| | (1) Inv/A | (2) Inv/PPE | (3) EmpGr | (4) Sales/A | (5) OI/A | (6) Debt/A |
|---|---|---|---|---|---|---|
| RTW <-5 | -0.00341 | -0.000488 | -0.0131 | -0.0420 | 0.0104 | 0.00361 |
| | (-0.54) | (-0.09) | (-0.59) | (-1.01) | (0.35) | (0.39) |
| RTW -5 | -0.00175 | -0.00711 | -0.0274 | -0.0831** | 0.0186 | -0.00751 |
| | (-0.43) | (-0.58) | (-1.01) | (-2.12) | (1.13) | (-0.40) |
| RTW -4 | 0.000904 | 0.00214 | -0.0252 | -0.110** | 0.00588 | -0.00905 |
| | (0.16) | (0.15) | (-0.85) | (-2.70) | (0.43) | (-0.56) |
| RTW -3 | -0.00537 | -0.0123 | -0.00619 | -0.0984 | -0.0231 | -0.0117 |
| | (-0.66) | (-0.96) | (-0.22) | (-1.60) | (-0.75) | (-1.35) |
| RTW -2 | -0.00126 | 0.00493 | 0.000861 | -0.0451 | -0.0121 | -0.00625 |
| | (-0.16) | (0.36) | (0.02) | (-1.16) | (-0.95) | (-0.91) |
| RTW 0 | 0.00107 | -0.000473 | 0.0142 | -0.0539 | 0.0200** | 0.000581 |
| | (0.19) | (-0.03) | (1.13) | (-1.56) | (2.62) | (0.09) |
| RTW +1 | -0.00332 | 0.000601 | -0.0127 | -0.0561 | 0.00385 | -0.00687 |
| | (-0.55) | (0.07) | (-0.24) | (-0.92) | (0.22) | (-0.92) |
| RTW +2 | -0.00843 | -0.0152* | 0.0182 | -0.0391 | 0.00779 | -0.00648 |
| | (-0.75) | (-1.89) | (0.45) | (-0.74) | (0.27) | (-0.45) |
| RTW +3 | 0.0242 | 0.0158 | 0.0226 | 0.0315 | 0.0159 | 0.0149 |
| | (1.27) | (0.83) | (0.62) | (0.42) | (0.77) | (0.80) |
| RTW +4 | 0.0131* | 0.0114 | -0.0139 | 0.0415 | 0.0219 | -0.00948 |
| | (1.87) | (0.90) | (-0.74) | (0.68) | (1.10) | (-0.62) |
| RTW +5 | 0.0329** | 0.0278* | 0.0251 | 0.107 | 0.0460** | -0.00723 |
| | (2.47) | (1.88) | (0.73) | (1.07) | (2.32) | (-0.37) |
| RTW >+5 | 0.00244 | -0.00181 | -0.0110 | 0.0270 | 0.0182 | -0.0290 |
| | (0.35) | (-0.30) | (-0.35) | (0.53) | (0.79) | (-1.37) |
| Adjusted $R^2$ | 0.552 | 0.363 | 0.160 | 0.844 | 0.684 | 0.654 |

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