# The Effects of "Right to Work" Laws on Wages: Evidence from the Taft-Hartley Act of 1947

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This paper uses an historical setting in which the introduction of "right to work" (RTW) laws was arguably exogenous - the period following passage of the Taft-Hartley Act - to produce credibly identified estimates of the effects of these laws on wages. The average effect of RTW laws on wages across all sectors of the economy is likely small and slightly negative. Some evidence indicates wage effects are more negative within the highly unionized sector.

# 1. Introduction

The last several years have seen a flurry of legislative and judicial activity related to unions' ability to collect dues from their members. In January 2012, Indiana became the 23rd state, and the first in the ``rust belt," to adopt a right to work (RTW) law, forbidding the use of union membership or dues payment as a condition of continuing employment.[1] During a lame-duck, December legislative session, Michigan followed suit. In the intervening months, Ohio and New Hampshire legislators debated RTW laws, and the Republican party added a plank to its platform calling for a national RTW law for the first time, a plank that remained in place in 2016. These actions marked a return to prominence for this issue. At the time, it had been more than ten years since the last RTW law had been adopted, in Oklahoma. Since then, the issue has shown no signs of receding in importance. In 2013, an activist, non-profit law firm filed a lawsuit in California aimed at overturning rules in that state that allow unions to require non-member workers for whom they bargain to pay for that representation. The case would reach the Supreme Court, which seemed poised, until Justice Scalia's death, to issue a ruling that could have imposed RTW-style rules on all public-sector unions. In March 2015, Wisconsin Governor Scott Walker, who touted his conflicts with unions and called for a national RTW law during his campaign for president, signed RTW legislation. Eleven months later, West Virginia became the 26th state to pass a RTW law.

Efforts to adopt RTW laws are often controversial. This could be due in part to the fact that their effects on important labor market outcomes such as wages are not well established. Theoretically, the sign of their effect on wages is ambiguous. If RTW laws weaken unions and reduce their bargaining power, they could lead to lower wages. Alternatively, RTW laws could increase wages by attracting new firms to a state, thereby increasing demand for labor and driving wages up.

Indeed, previous studies provide some support for each of these possibilities. RTW laws have been found to reduce rates of union organizing (Ellwood and Fine, 1987). Based on comparisons of bordering counties from different states, business-friendly regulatory environments (as proxied by RTW laws) are associated with greater manufacturing employment (Holmes, 1998). However, empirical studies of effects on wages have been mixed. Moore (1980) finds a negative, though not statistically significant, effect, while Reed (2003) finds a substantial positive effect. Farber (2005) finds negative effects on non-union wages and positive effects on union wages, but neither are statistically significant. Although they note that RTW laws are associated with larger union wage premiums, Moore (1980) and Farber (1984) conclude that the effects of RTW laws on wages are minimal, and the laws are largely symbolic.

While these inconclusive estimates could be due to countervailing positive and negative pressures on wages approximately canceling each other out on average, there is also an important identification challenge associated with estimating the effects of RTW laws on wages, one which previous studies have not overcome. States' decisions to adopt RTW laws could be correlated with unobserved local economic conditions that determine wages. If this endogeneity is not addressed, estimates of RTW laws' effects on wages will be biased, with correlated but secular changes in wages attributed to RTW laws.

Early estimates of the effects of RTW laws on wages (Moore, 1980 and Farber, 1984) were produced using single years of cross-sectional data and do not address local conditions surrounding the adoption of RTW. Studies of this kind, which compare states that have RTW laws at a point in time to those that do not, could produce biased estimates if some other, earlier, unobserved factor could have determined states' RTW status and wages. More recent studies address that concern by incorporating controls for local conditions at the time of RTW adoption (Reed, 2003) and performing difference-in-differences analysis of the adoption of more recent RTW laws in Idaho and Oklahoma (Farber, 2005). However, estimates based on within-state before and after comparisons surrounding RTW laws adopted after 1947 are also vulnerable to bias because lawmakers were free to enact those laws as they saw fit, and they may well have based their decisions on local economic conditions that were observable to them but not to econometricians. No previous study has used plausibly exogenous variation in state RTW status to identify the effects of RTW laws on wages.[2]

To address these issues, I use a strategy similar to those employed by Goldin and Katz (2002) and Bailey (2006) in the case of access to oral contraception, relying on policy variation created coincidentally when other laws were changed. Before 1947, states were precluded from passing RTW laws by the National Labor Relations Act, which had been passed in 1935. With the adoption of the federal Taft-Hartley Act of 1947, RTW laws went into effect in 12 states almost immediately - laws that had been passed prematurely and without federal permission became enforceable in five states, and seven additional states passed them before the end of 1947. The fact that these states adopted RTW laws as soon as they could (or sooner) suggests that they would have had the laws in place much earlier had federal law permitted. The change in federal law and the immediate adoption of state RTW laws during this period do not appear to have been caused by the same underlying factors, so the timing of the introduction of RTW laws in these states is arguably exogenous.[3] This fact, combined with the use of state and regional fixed effects in estimation, should address most concerns about the endogeneity of the introduction of RTW laws.

My results indicate that the effects of RTW laws on overall average wages across all sectors are small and likely slightly negative. Some evidence suggests the effects may be more negative within the highly unionized sector. I also find no effect on the level or sectoral composition of employment. The identification strategy I employ makes these estimates the first in this literature, to my knowledge, that are based on laws introduced with plausibly exogenous timing.

The rest of this paper is organized as follows. Section two discusses potential endogeneity issues related to the adoption of RTW laws in general settings, provides additional historical background, and argues for the exogeneity of the laws that went into effect during the period considered here. Section three details my estimation strategy and results. Section four discusses and concludes.

## 2. Background: Exogeneity of Right to Work Laws

When evaluating the effects of RTW laws on labor market outcomes, including wages, researchers face the same primary endogeneity issue that complicates analysis of other state-level policies: lawmakers may adopt policies in response to local trends or conditions that influence the outcomes of interest and are unobserved to econometricians. If policy adoption is correlated with these unobserved factors, it can be difficult to disentangle the effects of the policy on outcomes from secular changes in outcomes, and estimates that do not address this issue will be biased. In this paper, I take advantage of a change in federal law that limited the possibility of endogenous timing of policy adoption for the first RTW laws.

The National Labor Relations Act (NLRA) of 1935 established much of the legal framework within which employers and employees interact. The NLRA explicitly permitted employers to make agreements with unions requiring all new hires to be union members at the time of their hiring, a type of union security agreement known as a “closed shop.”[4] In June of 1947, the Taft-Hartley Act amended the NLRA to ban closed shops. It also gave states the right to take the additional step of passing RTW laws, which prohibit employers and unions from establishing a ``union shop,'' another type of union security agreement that requires all employees to join the union after being hired. Most RTW laws also ban the “agency shop” arrangement, which requires workers to pay union dues or equivalent fees, even if they do not join the union. Although the Taft-Hartley Act permitted these regulations of union security agreements, its primary purpose was to address problems related to widespread strikes, as I will discuss further below.

This change in federal law allowed RTW laws to go into effect in several states that likely would have adopted them sooner if they could have. In fact, five states had already passed RTW laws, even though they technically were not permitted to do so. Figure 1 provides a timeline of these and other events relevant to this study. Arkansas and Florida passed RTW laws in 1944; Arizona, Nebraska, and South Dakota joined them in 1946.[5] Since federal law supersedes state law, however, these state laws faced legal challenges from local labor organizations and should not have been enforceable prior to 1947, but they would have become valid when the federal law changed.[6] Seven other states enacted RTW laws in 1947. The fact that Georgia, Iowa, North Carolina, North Dakota, Tennessee, Texas, and Virginia immediately took advantage when granted the right to pass RTW laws suggests that policymakers in those states would have done so sooner if such laws had been permitted. The passage of the Taft-Hartley Act, then, caused RTW laws to take effect in 12 states in 1947 instead of prior to that year, which likely would have been the case had those states not been constrained by federal law.

The single largest factor contributing to the passage of the Taft-Hartley Act appears to have been a wave of strikes that took place across the nation in 1945-46. Strikes had been illegal during World War II (WWII), but in the 18 months following its conclusion, seven million workers participated in thousands of strikes at a cost of 144 million lost days of work, according to Metzgar (2009). Contemporary observers and historians have attributed both the Republican party's ability to retake control of the House of Representatives with large gains in the 1946 midterm elections and the subsequent passage of the Taft-Hartley Act to a popular desire to address the problems created by these strikes. Indeed, many legislators whose comments were recorded in the legislative history of the Taft-Hartley Act mention a need to reduce the frequency with which strikes occurred (U.S. GPO, 1974).

If the states that quickly adopted RTW laws had been especially hard-hit by strikes, one might be concerned that strikes caused the adoption of both the Taft-Hartley Act and RTW laws, and that the timing with which RTW laws went into effect was therefore endogenous. This, however, does not appear to be the case. Of all the days of work lost to strikes during the 18 month period following World War II, the vast majority was lost in the Northeast and Great Lakes regions. Pennsylvania, New York, Ohio, Michigan, and Illinois combined to account for 50\% of the lost time, and Massachusetts, Connecticut, New Jersey, West Virginia, Indiana, and Wisconsin accounted for another 25 percent. Outside of those states, only California (5 percent) experienced a loss of work time that represented a significant share of the national total. None of these states, all hit hard by strikes, immediately adopted a RTW law, and only four (Indiana, Michigan, West Virginia, and Wisconsin) have done so since. In fact, the two primary sponsors of the Taft-Hartley Act, Senator Robert Taft (Ohio) and Representative Fred Hartley, Jr. (New Jersey), represented states that were hit hard by strikes and have not adopted RTW laws. In light of these facts, it seems unlikely that the same strikes that led to the adoption of the Taft-Hartley Act, a federal law, were also the underlying force behind the passage of RTW laws in particular states.

Additionally, the Taft-Hartley Act received overwhelming support in both houses of Congress and ultimately became law over President Harry S. Truman's veto. The law could not have been passed with support from only those states that quickly passed RTW laws. In fact, when the House of Representatives first voted to pass the Taft-Hartley Act, only 69 of the 308 votes in favor of passage came from states that passed RTW laws by the end of 1947, indicating that the vast majority of the law's support came from other states.[7] Indeed, enough representatives from other states voted in favor of the Taft-Hartley Act that it could have passed the House without any support from early RTW states. Finally, although each group could certainly observe and respond to the same local conditions, federal legislators passed the Taft-Hartley Act, while state legislators passed RTW laws, and neither group participates directly in the activities of the other. Together, these factors suggest that, from the states' perspective, the timing of the RTW laws that took effect in 1947 was exogenously determined.

Since the timing of these RTW laws was plausibly exogenous in this setting, observable differences between treatment and control states (detailed in Table 1) are easily controlled for, and a set of state and regional fixed effects can address endogeneity concerns related to unobserved, time-invariant differences between states, a simple difference-in-differences framework can be used to estimate the effects of RTW laws on wages during the period surrounding the passage of the Taft-Hartley Act. Assuming that the changes in wages over this period in a control group of states that did not pass RTW laws accurately represent the changes that would have taken place in RTW states had they not passed RTW laws, the difference between the observed wage changes in RTW states and those in non-RTW states can be causally attributed to RTW laws.

## 3. Estimation and Results

The baseline specification compares the treatment group discussed above to a control group composed of all other states. The estimating equation is

$$y_{ist} = (RTW47_s \times After_t)\beta_1 + RTW47_s\beta_2 + After_t\beta_3 + RTW_{st}\beta_4 + X_i\gamma + \delta_s + \zeta_t + \xi_{dt} + \varepsilon_{isdt}.$$

It includes state ($\delta_s$), year ($\zeta_t$), and census division by year ($\xi_{dt}$) fixed effects, as well as controls for age, race, education, family structure, and metropolitan status (found in $X_i$). Since seven states in the control group passed RTW laws between 1950 and 1960, the specification also includes a dummy variable ($RTW_{st}$) that is equal to one in all state-years in which a RTW law was in effect. The sample, which is drawn from the Minnesota Population Center's IPUMS extracts of the 1940, 1950, and 1960 censuses, is restricted to men aged 25-54. Those with allocated values for outcomes of interest or missing values for demographic controls are excluded.[8] Controls for potentially endogenous state and individual characteristics (unemployment rate, Democratic vote share, union organization, industry, occupation) are not included in the baseline specification.[9]

In this equation, $i$ indexes individuals, $s$ indexes states, $d$ indexes census divisions, and $t$ indexes years. When hourly wages are the outcome of interest, they enter this equation in log form, while employment outcomes enter as dummy variables.[10] $RTW47_s$ is equal to one for states that saw a RTW law go into effect in 1947. $After_t$ is equal to one for observations from the 1950 and 1960 censuses, after the passage of the Taft-Hartley Act. The coefficient on the interaction between those two variables, $\beta_1$, is the difference-in-differences estimate. It can be interpreted as the percent change in wages (or the percentage point change in employment outcomes) that is attributable to the introduction of RTW laws. $\varepsilon_{isdt}$ is an error term. In keeping with Bertrand, Duflo, and Mullainathan (2004), standard errors are clustered at the state level, and effects are estimated for a single ``after'' period that includes observations from 1950 and 1960. All regressions employ sample weights.[11]

Estimates from the baseline specification suggest that the aggregate effects of RTW laws on wages are small and unlikely to be meaningfully positive. As reported in Table 2, the point estimate of $\beta_1$ in the equation above indicates that wages in treatment states fell 1.88 percent relative to control states after the introduction of the initial RTW laws, though that estimate is not statistically significant. It is, however, sufficiently negative to rule out an increase in wages of 1.94 percent or more at the 95 percent confidence level. Since proponents of RTW laws advance an improved business climate as an important consequence of the laws and a contributor to wage growth, I also examine employment outcomes. Effects on employment are similarly small. The introduction of the first RTW laws in the baseline specification reduces the probability that an individual is employed by 0.15 percentage points, an effect that is neither statistically nor economically significant, as it represents just 0.16 percent of the pre-treatment mean in the treatment states (in 1940, 89.8 percent of men in sample in treatment states were employed).

These estimates are consistent with previous research that has found RTW laws to have little impact on wage and employment outcomes. However, null estimates for effects on the economy as a whole could be the result of meaningful but countervailing effects on different parts of the economy. For example, effects on wages or employment in highly unionized industries could offset effects in lightly unionized industries, leading to incorrect zero estimates for the economy as a whole.

As of 1947, more than 67 percent of workers in the transportation, construction, and mining industries were unionized. At the other extreme, workers in agricultural industries historically have had very low levels of unionization, as did workers in service industries (9 percent unionized in 1947) and government (12 percent unionized) when the first RTW laws took effect. When the difference-in-differences approach described above is applied just to highly or lightly unionized industries (i.e. one that makes cross-state wage comparisons within the group of highly or lightly unionized workers), estimates suggest little impact of wages in either sector.[12]

However, a triple difference specification comparing wages both between highly and lightly unionized industries and across states, employing all the same controls as the baseline difference-in-differences specification, shows differential wage growth across sectors. For the purposes of the triple difference estimation, workers in transportation, construction, and mining constitute the highly unionized group. Relative to all other industries, wage growth in highly unionized industries was 10.9 percent lower in the initial RTW states than in other states, an effect that is economically substantial and statistically significant at the 99 percent level. If industries with intermediate levels of unionization are excluded from the analysis, and highly unionized industries are compared only to lightly unionized industries (i.e. agricultural, service, and government workers), the effect becomes slightly larger in magnitude. Relative to workers in lightly unionized industries, wage growth in highly unionized industries was 13.3 percent lower in treatment states than in control states, an effect that is also highly statistically significant.

Adding state-by-year fixed effects to the triple-difference specification does not alter these conclusions.

This large reduction in wage growth within the highly unionized sector could be mitigated if RTW laws also affected the sized of the highly unionized sector. If RTW laws reduced the relative size of the highly unionized sector, where they also reduced wage growth, thereby increasing the size of the less unionized sector, where wage growth was relatively high, the magnitude of statewide estimates of the effect of RTW laws would be reduced. However, the 1947 RTW laws did not meaningfully alter the size of the highly unionized sector. As shown in the bottom panel of Table 2, the adoption of the first RTW laws had only a minimal, not statistically significant effect on the probability that a man would be employed in a highly unionized industry (-0.3 percentage point, off a 1940 base of 17.7 percent of all men in sample in treatment states). Changes in the relative size of the highly unionized sector are unlikely to have mitigated the reduction in wage growth in that sector due to RTW laws.

### 3.1. Adding Labor Market Controls

The baseline specification does not include controls for state labor market and political characteristics because they could be endogenous to a state's RTW status. If RTW laws affect wages by affecting the unemployment rate, for example, or the share of the labor forced organized by unions, including controls for those factors would bias estimates of the effects of RTW laws. However, if treatment states were experiencing differential secular trends in economic or political variables that may contribute to the outcomes of interest around the introduction of RTW laws, failing to control for those factors could also lead to biased estimates of the effects of those laws.

To examine the extent to which such bias affects the baseline estimates, I add controls for labor market and political factors to the baseline specification. The state-level controls added here include the unemployment rate, the share of the workforce organized by unions, and the share of the two-party vote received by Democratic party candidates in the most recent federal elections.[13] At the individual level, controls for workers' industries and occupations are also added when wages are the outcome of interest. With these additional controls included, the difference-in-differences estimate of the effect of RTW laws on wages is little changed from the baseline estimate, -2.08 percent here vs. -1.88 percent at baseline. With respect to the employment, including these additional controls leads to a similarly small change in the magnitude of the estimate of the effect of RTW laws, increasing it from -0.15 percentage points to -0.22 percentage points. The employment effect remains economically small and not statistically significant.

The triple difference estimates, shown in Table 3, are also robust to the inclusion of the additional labor market and political controls. When highly unionized industries are compared to all other industries, the inclusion of these additional controls reduces the magnitude of the point estimate of the effect on wages from -10.9 percent to -8.95 percent. When highly unionized industries are compared only to lightly unionized industries, the additional controls reduce the magnitude of the point estimate of the wage effect from 13.3 percent to 10.4 percent. In both cases, the wage effect remains highly statistically significant.

Adding labor market and political controls to the baseline specification reveals that the baseline estimates likely do not suffer from significant bias due to their exclusion. In a sense, this should not be especially surprising. The baseline estimates indicated that RTW laws had little effect on the probability of a worker being employed and little effect on the sector in which he is employed. It makes sense, then, that controlling for factors similar to those would lead to little change in the estimate of the effect of RTW laws on wages.

### 3.2. Other Identification Issues

In difference-in-differences estimation, the identifying assumption is that the control group appropriately represents the counterfactual trend in outcomes that the treatment group would have experienced in the absence of treatment. If that assumption does not hold, difference-in-differences estimates are not causal. The selection of the control group, then, is critically important.

One might worry that the group of all non-treatment states may not be sufficiently similar to the group of treatment states to accurately represent its counterfactual trends in wages and employment. To address this concern, I estimate wage and employment effects again using alternative control groups, constructed to be similar to the group of treatment states with respect to a variety of criteria. Table 5 summarizes the results discussed below.

Data availability presents an important challenge to this analysis. The 1940 census was the first to collect enough information to calculate workers' hourly wages. Without an alternative data source, wages are observed in just one pre-treatment year, making it is impossible to compare pre-treatment wage trends in the treatment group to those in potential control groups. Table 4 reports 1940 averages of key outcomes for treatment and control groups. Comparing the average 1940 wage in the treatment states to the average wage in all other states reveals a substantial difference in levels. In treatment states, the average hourly wage in 1940 was $1.06 in 1960 dollars. In the baseline control group, the average hourly wage was $1.49. A difference in outcome levels between treatment and control groups does not doom difference-in-differences analysis, but in the absence of other evidence, such a difference may lead to concern about a difference in trends. In order to address that concern, I re-estimate wage and employment effects using as the control group the 20 states in which the average wage in 1940 was closest to the treatment group average. The average 1940 wage in this control group is $1.22, much closer to the treatment group average. Using this control group instead of the baseline control group has little impact on the estimated wage and employment effects. In this specification, RTW laws reduce wages by 2.4 percent and increase employment by 0.13 percentage points. Neither effect is statistically significant. The triple difference estimate of the wage effect in highly unionized industries is cut roughly in half, to -4.89 percent when compared to all other industries. Unlike in the baseline specification, this triple difference estimate is not statistically significant.[14]

One might also wonder what, if any, impact World War II has on the baseline estimates. Though none of the census data used here was collected during the United States' direct involvement in the war, states could have experienced persistent, structural changes as the American economy retooled to supply the war effort, changes which could have altered the path of wage growth in subsequent years. If producers of war supplies made decisions about where to locate production based on, for example, local unionization rates or attitudes toward unions, persistent structural differences due to World War II could be correlated with treatment status here.

To the extent that the factors that lead to differential growth in per capita personal income during World War II also contributed to differential growth in wages, failing to control for these factors could bias the baseline estimates. One potential way to address this concern is to add controls for wartime growth in per capita personal income directly to the baseline specification. I do that here by calculating the difference between the log of per capita personal income in 1945 and 1940 for each state and adding that measure, interacted with the $After_t$ dummy, to the baseline specification.[15] The inclusion of this additional control has essentially no effect on the baseline estimates.

Concerns about World War II can also be addressed through the use of alternative control groups. One might think that states that are geographically proximate to treatment states would be likely to experience the war in a similar way and therefore represent counterfactual wage trends in the treatment states more accurately than more distant states. When wage and employment effects are re-estimated using only states that border treatment states to construct the control group, point estimates are not significantly different from baseline estimates and remain consistent with the conclusions drawn from them. Alternatively, states with similar attitudes and politics surrounding unions might best reflect what would have happened in treatment states in the absence of RTW laws. Again, limiting the control group to states that adopted RTW laws shortly after the treatment states (i.e. states adopting RTW laws during the 1950s) does little to alter the baseline estimates.[16]

The strikes that contributed to the passage of the Taft-Hartley Act could also complicate the baseline analysis. None of the states in which strikes were especially prevalent saw RTW laws take effect in 1947. If intense strikes had long-lasting effects on state economies, those effects would be concentrated completely within the baseline control group, which could bias difference-in-differences estimates based on it. However, when states hit hard by strikes prior to the adoption of the Taft-Hartley Act are excluded from the control group, wage and employment estimates are little changed. This is the case when only the group of the five states mentioned above that experienced 50 percent of lost days of work are excluded and when that group is broadened to include seven additional states and account for 80 percent of lost days of work.

In addition to constructing control groups manually to address specific concerns, the control group can be constructed algorithmically using synthetic control methodology (Abadie and Gardeazabal, 2003; Abadie, Diamond, and Hainmueller, 2010; Abadie, Diamond, and Hainmueller, 2014).[17] The synthetic control algorithm creates an artificial comparison group by re-weighting non-treatment states, with greater weight given to states in which the relationship between state characteristics and the outcome of interest in the pre-treatment period is most similar to that relationship within the treatment group. For this analysis, I use log per capita personal income as the outcome, since it is available on an annual basis. As Figure 2 shows, synthetic control estimation produces no evidence that the initial RTW laws had an important effect on per capita personal income. The difference between per capita personal income in the treatment group and its synthetic control group is well within the range of placebo estimates generated by assigning treatment status to each non-treatment state. When the states that receive positive weight in the synthetic control analysis are used as the control group for estimating the effects of RTW laws with census data, the wage effect is little changed.[18] The employment effect is positive and marginally statistically significant, but remains economically small.

Finally, one might be concerned that early-adopting RTW states could be especially highly selected into treatment. Some of these states passed RTW laws before they were permitted under federal law, possibly incurring some cost to do so. The rest passed RTW laws as soon as they were permitted. States so eager to adopt these policies that they would act in conflict with federal law may also differ from other states on unobservable dimensions that affect wages and employment. In this case, estimates could be more confounded than those in the prior literature.[19] However, to the extent that these differences are time-invariant, the state fixed effects included in the baseline specification will prevent them from biasing my estimates.

Moreover, the fact that the wage and employment estimates differ little from baseline when the control group is limited states that adopted RTW laws shortly after those in the treatment group (which would likely be most similar to the treatment states in terms of unobserved factors leading to adoption of RTW laws) suggests that any selection of this kind that may exist is not driving the baseline estimates.

## 4. Discussion and Conclusion

While their importance to workers is paramount, one might think of the wage effects of RTW laws as secondary to other more direct effects. Reductions in union organizing as in Ellwood and Fine (1987) likely put downward pressure of wages. On the other hand, increases in manufacturing employment as in Holmes (1998) would tend to push wages up. If multiple, countervailing direct effects are realized, the sign and magnitude of the wage effects depends on their relative importance.

The evidence presented here indicates that on balance, the average effects of RTW laws on wages across all sectors are small and likely slightly negative. Though most point estimates presented are not statistically different from zero, the baseline estimates can rule out wage increases of 1.94 percent or more due to the adoption of RTW laws at the 95 percent confidence level. Negative effects of RTW laws on wages are likely larger within the highly unionized sector. The baseline triple difference estimates show a statistically significant 10.9 percent reduction in wages in highly unionized industries relative to other, less unionized industries, though that effect becomes smaller in magnitude and loses statistical significance in some alternative specifications.

Although the estimates of the overall average wage effect across sectors presented here are within the range found in the previous literature, these estimates advance that literature because they are derived from a setting in which the timing of the adoption of RTW laws was arguably exogenous, a feature not found in other studies. The passage of the federal Taft-Hartley Act in 1947 led RTW laws to take effect in 12 states that year for reasons not related to local conditions in those states. Within this framework, my estimates of the effect of RTW laws on wages are robust to the inclusion of additional controls and estimation using alternative control groups constructed to match the treatment group on a variety of dimensions.

Though a complete accounting of the possible channels through which RTW laws may affect wages is beyond the scope of this paper, the results above can be interpreted with potential channels in mind. First, triple difference estimates show RTW laws reduce wages in highly unionized industries relative to other industries, especially those with little union presence. That the most adverse wage effects are found where unionization rates are highest suggests that weakened unions are an important consequence of RTW laws. Second, estimates of employment effects show little change in overall employment and little change in the sectoral composition of employment. Based on these estimates it seems unlikely that RTW laws lead to meaningful increases in demand for labor, or the attendant upward pressure on wages.

Estimated employment effects in this setting could be small because RTW laws never have important effects on employment, but the relatively small magnitude of the estimates presented here could also be tied to macroeconomic conditions during my analysis period. For example, roughly 90 percent of prime-age men were employed at the beginning of the period covered by this analysis, and the overall unemployment rate averaged 4.65 percent over 1948-1960. An economy that sustained that level of employment may have had less to gain from regulating unions than states might today. Alternatively, employment effects for men in particular could have been small during this period if regulating unions made firms more likely to hire women. However, while separate estimates for women (not reported here) generally show positive effects on employment, they are not statistically significant and similar to the estimates for men in their small magnitude, suggesting that effects on women's employment did not play a major role in limiting effects on men's employment.[20]

Given the substantial decline in unionization rates in the past half century, this suggestive evidence that the wage effects of RTW laws may be concentrated in the highly unionized sector raises the question of how a newly enacted RTW law might affect wages today. With the notable exception of the public sector, most industries have lower unionization rates today than they did in the 1940s. Even within today's highly unionized industries, union members constitute a fairly small minority of workers. Might these differences simply mitigate whatever factors lead to larger declines in wages in highly unionized industries after the adoption of the first RTW laws? Could lower levels of unionization change the dynamic by which wage effects are realized? Further research on the precise channels through which RTW laws influence wages is needed to address these questions.

## Endnotes

[1] Indiana had previously adopted a RTW law in 1957, but repealed it in 1965.
[2] Farber (2005) asserts that the adoption of RTW laws in Idaho and Oklahoma represented an arguably exogenous change in the threat of unionization, but does not provide a supporting argument.
[3] In the case of access to oral contraception, state policy was federally driven in that many states took action to align their age of majority (which had widely been set at 21 years of age) with the federally determined age at which men could be drafted into military service during the Vietnam War (18 years) and then with the federal voting age (18 years after the adoption of the 26th amendment in 1971). Coincidentally, these changes gave women ages 18-20 access to oral contraception. See Bailey (2006).
[4] Prior to the NLRA, the Railway Labor Act (1926) explicitly permitted union security agreements in all states, precluding states from passing RTW laws. The NLRA maintained that status quo.
[5] Sources vary on the year of enactment in these early-adopting states. Collins (2014), for example, lists only Florida as having adopted a RTW law prior to 1947 (in 1943).
[6] All five states that adopted RTW laws before 1947 saw the legislation first introduced by ballot initiative. Voters approved the laws after sufficient petition signatures had been collected to put the issue on the ballot. Subsequent action by legislators was taken in response to this initiative. Labor organizations, including the American Federation of Labor, challenged the laws in court after they were adopted, claiming they were superseded by federal law. Although a lawsuit challenging Florida's law reached the Supreme Court in late 1945, the issues involved had not yet been resolved when the Taft-Hartley Act was passed, ceding jurisdiction over the matter to states and rendering legal challenges moot. See Gall, 1988. Empirically, I find no evidence that the states that passed RTW laws prematurely differ in important ways from those that passed their laws in 1947. In regressions not reported here, I find adoption of RTW before 1947 had no effect on wages compared to adoption in 1947.
[7] See http://www.govtrack.us/congress/votes/80-1947/h27.
[8] In 1960, observations from Alaska and Hawaii are excluded, since Alaska and Hawaii were not states at the times of the previous censuses.
[9] One might argue that RTW status is also an endogenous state characteristic that affects outcomes and including an additional control for it, $RTW_{st}$, is inappropriate here. Conducting all the analysis presented here with the $RTW_{st}$ control excluded produces the same qualitative results.

[10] Hourly wages are defined as $Wage = \frac{Wage\ and\ salary\ income\ last\ year / Weeks\ worked\ last\ year}{Hours\ worked\ last\ week}$. Usual hours worked would have been preferable for this calculation, but that measure is not available in these data. The 1960 census reported ranges of values for wage and salary income, weeks worked last year, and hours worked last week rather than particular values. As such, in order to calculate wages for the 1960 data, I use the midpoint of each range of values. Topcoded annual earnings values are multiplied by 1.5. Individuals with hourly wages less than $1.00 or greater than $100 in 2013 dollars are excluded.

[11] When wages are the outcome of interest, the provided person weights are multiplied by the number of hours worked last week to create an hours-adjusted person weight. When employment is the outcome of interest, person weights are used without adjustment. Using unadjusted person weights in wage regressions has little effect on estimates.
[12] These results are not reported here but are available upon request.
[13] State unemployment rates are calculated from census data using both male and female labor force participants of all ages. Union organization rates are taken from Troy and Sheflin (1985) when available and imputed from those data by linear interpolation for years not provided. Democratic vote share is constructed from vote totals reported by the Clerk of the House of Representatives.
[14] Constructing the control group using the 12 or 15 states with average wages closes to the treatment group average produces similar results.
[15] This approach mimics a strategy used in Acemoglu, Autor, and Lyle 2004 to study the effect of male mobilization for World War II on women's wages.
[16] Using all future RTW states as the control group has a similarly small effect of wage and employment estimates.
[17] Eren and Ozbeklik (2016) also use this approach to study RTW laws.
[18] The states receiving positive weight in the synthetic control analysis are Alabama, Delaware, the District of Columbia, Missouri, Nevada, New Jersey, South Carolina, Utah, and Wyoming. The state characteristics used to identify the control group are state averages of the covariates included in the baseline specification, calculated in census years and linearly interpolated for intercensal years. When considering regressions using these states as the control group, note that different states may have been selected if it were possible to perform synthetic control analysis using hourly wages rather than per capita personal income.
[19] Thanks to an anonymous referee for helpful comments on this point.
[20] Estimates for women also show more negative but less precise effects on wages in the difference-in-differences analysis. Wage effects in the triple difference analysis were smaller and less precise than the corresponding effects for men, though the small number of women working in highly unionized industries during this period may limit the value of these estimates. Full results for women are available upon request.

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# Tables

Table 1: Characteristics of Treatment and Control Groups

| | Treatment | Control | Difference |
|---|---|---|---|
| Age | 38.36 | 38.77 | -0.410 |
| | (0.107) | (0.061) | (0.123) |
| Education: Less than high school | 0.744 | 0.698 | 0.046 |
| | (0.016) | (0.012) | (0.020) |
| Education: High School | 0.131 | 0.160 | -0.029 |
| | (0.009) | (0.005) | (0.010) |
| Education: Some College | 0.0650 | 0.0689 | -0.004 |
| | (0.005) | (0.005) | (0.007) |
| Education: College | 0.0597 | 0.0732 | -0.014 |
| | (0.004) | (0.003) | (0.005) |
| White | 0.845 | 0.919 | -0.074 |
| | (0.023) | (0.010) | (0.026) |
| Black | 0.151 | 0.0749 | 0.076 |
| | (0.024) | (0.011) | (0.026) |
| Married | 0.840 | 0.815 | 0.025 |
| | (0.004) | (0.005) | (0.007) |
| Student | 0.0118 | 0.0137 | -0.002 |
| | (0.001) | (0.001) | (0.001) |
| Number of children | 1.604 | 1.449 | 0.155 |
| | (0.037) | (0.038) | (0.053) |
| Number of children under 5 years old | 0.448 | 0.406 | 0.042 |
| | (0.007) | (0.011) | (0.013) |
| N | 200,154 | 751,975 | |

Note: Control group consists of all non-treatment states. Control group mean is subtracted from the treatment group mean to produce the difference. Standard errors are clustered at the state level.

Table 2: Effects of RTW Laws on Wages and Employment

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Wages | | | | |
| DD Estimate | -0.0188 | -0.0187 | -0.0208 | -0.0210 |
| | (0.0228) | (0.0233) | (0.0183) | (0.0197) |
| Sample Size | 443,739 | 443,739 | 436,472 | 436,472 |
| R-squared | 0.400 | 0.400 | 0.523 | 0.523 |
| WWII Controls | No | Yes | No | Yes |
| Labor Market Controls | No | No | Yes | Yes |
| | | | | |
| Employment | | | | |
| DD Estimate | -0.00149 | -0.000784 | -0.00219 | -0.00209 |
| | (0.00597) | (0.00601) | (0.00241) | (0.00239) |
| Sample Size | 952,129 | 952,129 | 898,647 | 898,647 |
| R-squared | 0.069 | 0.069 | 0.036 | 0.036 |
| WWII Controls | No | Yes | No | Yes |
| Labor Market Controls | No | No | Yes | Yes |
| | | | | |
| Employment in Highly Unionized Industries | | | | |
| DD Estimate | -0.00302 | -0.00321 | -0.00555 | -0.00619 |
| | (0.00818) | (0.00808) | (0.00867) | (0.00840) |
| Sample Size | 952,129 | 952,129 | 898,647 | 898,647 |
| R-squared | 0.023 | 0.023 | 0.025 | 0.025 |
| WWII Controls | No | Yes | No | Yes |
| Labor Market Controls | No | No | Yes | Yes |

*** p<0.01, ** p<0.05, * p<0.1

Note: Control group consists of all non-treatment states. Regressions include individual-level demographic controls, plus World War II and labor market controls as specified. Standard errors are clustered at the state level.

Table 3: Triple Difference Estimate of the Effect of RTW Laws on Wages

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Including intermediate unionization industries | | | | |
| DDD Estimate | -0.109*** | -0.109*** | -0.0895*** | -0.0881*** |
| | (0.0378) | (0.0374) | (0.0288) | (0.0288) |
| Sample Size | 443,739 | 443,739 | 436,472 | 436,472 |
| R-squared | 0.409 | 0.409 | 0.523 | 0.523 |
| WWII Controls | No | Yes | No | Yes |
| Labor Market Controls | No | No | Yes | Yes |
| Excluding intermediate unionization industries | | | | |
| DDD Estimate | -0.134*** | -0.133*** | -0.105*** | -0.104*** |
| | (0.0385) | (0.0380) | (0.0282) | (0.0283) |
| Sample Size | 182,512 | 182,512 | 181,423 | 181,423 |
| R-squared | 0.441 | 0.441 | 0.542 | 0.542 |
| WWII Controls | No | Yes | No | Yes |
| Labor Market Controls | No | No | Yes | Yes |

*** p<0.01, ** p<0.05, * p<0.1

Note: Control group consists of all non-treatment states. Regressions include individual-level demographic controls, plus World War II and labor market controls as specified. Highly unionized industries are transportation, construction, and mining. Lightly unionized industries are agriculture, services, and government. Industries with intermediate rates of unionization are excluded as indicated. Standard errors are clustered at the state level.

Table 4: Average 1940 Outcomes for Treatment and Various Control Groups

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | | Control Groups | | | | | | |
| | Treatment Group | Baseline | Closest Wages | RTW During 1950s | Border States | Excluding 5 Strike States | Excluding 12 Strike States | Synthetic Control States |
| Average Hourly Wage | 1.062 | 1.491 | 1.224 | 1.158 | 1.379 | 1.395 | 1.249 | 1.390 |
| | (0.0304) | (0.0355) | (0.0469) | (0.115) | (0.0682) | (0.0498) | (0.0455) | (0.137) |
| Share Employed | 0.898 | 0.871 | 0.886 | 0.900 | 0.876 | 0.877 | 0.881 | 0.892 |
| | (0.00328) | (0.00610) | (0.00418) | (0.00370) | (0.00616) | (0.00523) | (0.00404) | (0.00427) |
| Share of Employed in Highly Unionized Industries | 0.179 | 0.196 | 0.198 | 0.184 | 0.209 | 0.198 | 0.203 | 0.191 |
| | (0.0111) | (0.00990) | (0.0111) | (0.0133) | (0.0118) | (0.00991) | (0.00915) | (0.00982) |
| Share of Employed in Lightly Unionized Industries | 0.478 | 0.309 | 0.423 | 0.441 | 0.388 | 0.362 | 0.428 | 0.355 |
| | (0.0162) | (0.0200) | (0.0239) | (0.0587) | (0.0295) | (0.0234) | (0.0203) | (0.0567) |

Note: Within each specificied group, average hourly wage is calculated from the sample of prime-age men with non-allocated, non-outlier wage observations and non-missing demographic information. Share employed is calculated using such men with non-allocated employment information. Shares of employed workers in highly and lightly unionized indsutries are calculated using all employed men in the sample used to calculate share employed, including those with missing industry information.

Table 5: Effects of RTW Laws on Wages and Employment, Using Various Control Groups

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | Baseline | Closest Wages | RTW During 1950s | Border States | Excluding 5 Strike States | Excluding 12 Strike States | Synthetic Control States |
| Wages | | | | | | | |
| DD Estimate | -0.0188 | -0.0240 | -0.00423 | -0.00275 | -0.00629 | -0.00891 | -0.00519 |
| | (0.0228) | (0.0237) | (0.0290) | (0.0215) | (0.0216) | (0.0230) | (0.0200) |
| Sample Size | 443,739 | 178,681 | 118,724 | 241,683 | 285,434 | 182,788 | 130,776 |
| R-squared | 0.400 | 0.412 | 0.425 | 0.422 | 0.415 | 0.411 | 0.433 |
| DDD Estimate 1 | -0.109*** | -0.0489 | -0.0642 | -0.0661 | -0.0777* | -0.0479 | -0.107* |
| | (0.0378) | (0.0413) | (0.0540) | (0.0416) | (0.0392) | (0.0393) | (0.0519) |
| Sample Size | 443,739 | 178,681 | 118,724 | 241,683 | 285,434 | 182,788 | 130,776 |
| R-squared | 0.409 | 0.427 | 0.439 | 0.436 | 0.426 | 0.425 | 0.444 |
| DDD Estimate 2 | -0.134*** | -0.0537 | -0.0757 | -0.0905* | -0.1000** | -0.0673 | -0.129** |
| | (0.0385) | (0.0391) | (0.0521) | (0.0451) | (0.0411) | (0.0414) | (0.0615) |
| Sample Size | 182,512 | 83,561 | 55,417 | 112,799 | 127,303 | 88,183 | 60,187 |
| R-squared | 0.441 | 0.470 | 0.481 | 0.468 | 0.461 | 0.468 | 0.480 |
| Employment | | | | | | | |
| DD Estimate | -0.00149 | 0.00133 | 0.00695 | -0.000908 | -0.000730 | -0.000512 | 0.00694* |
| | (0.00597) | (0.00603) | (0.00446) | (0.00584) | (0.00563) | (0.00611) | (0.00385) |
| Sample Size | 952,129 | 421,771 | 283,547 | 548,089 | 636,172 | 432,380 | 301,982 |
| R-squared | 0.069 | 0.068 | 0.066 | 0.068 | 0.067 | 0.067 | 0.064 |
| Employment in Highly Unionized Industries | | | | | | | |
| DD Estimate | -0.00302 | -0.00311 | 0.00261 | -0.00842 | -0.00255 | -0.00840 | 0.00834 |
| | (0.00818) | (0.00869) | (0.0106) | (0.0103) | (0.00864) | (0.0108) | (0.00849) |
| Sample Size | 952,129 | 421,771 | 283,547 | 548,089 | 636,172 | 432,380 | 301,982 |
| R-squared | 0.023 | 0.021 | 0.020 | 0.025 | 0.025 | 0.021 | 0.020 |

*** p<0.01, ** p<0.05, * p<0.1

Note: All regressions include demographic controls, as well as state, year, census division by year, and metropolitan status fixed effects. World War II and labor market controls are not included. The first set of triple difference estimates compares highly unionized industries to all other industries. The second set excludes industries with intermediate unionization rates from the control group.

# Figures

Figure 1: Timeline of Important Events

1935: National Labor Relations Act becomes law, establishing basic framework for labor-management relations, including rules about collective bargaining, unfair labor practices, and strikes
    -States may not pass RTW laws, none are in effect

1940: First decennial census to collect information on wage and salary income is conducted

1944: Arkansas and Florida pass RTW laws, despite not being legally permitted to do so; laws not enforceable

1946: Arizona, Nebraska, and South Dakota pass RTW laws; again, laws not enforceable

1947: Taft-Hartley Act becomes law, amending the NLRA and altering rules for labor-management relations
    -States may pass RTW laws, and existing laws become enforceable
    -Georgia, Iowa, North Carolina, North Dakota, Tennessee, Texas, and Virginia adopt RTW laws

1950: Decennial census collects wage and salary information

1951: Nevada passes a RTW law

1953: Alabama passes a RTW law

1954: Mississippi and South Carolina pass RTW laws

1955: Utah passes a RTW law

1957: Indiana passes a RTW law (which it repealed in 1965)

1958: Kansas passes a RTW law

1960: Decennial census collects wage and salary information