The Effects of "Right to Work" Laws on Wages:

Evidence from the Taft-Hartley Act of 1947[*]

Kevin Rinz

University of Notre Dame, Department of Economics, 434 Flanner Hall, Notre Dame, IN 46556.

Email: krinz@nd.edu.

Abstract

This paper uses an historical setting in which the introduction of "right to work" laws was arguably exogenous - the period following the passage of the Taft-Hartley Act in 1947 - to produce credibly identified estimates of the effects of these laws on wages. I find no evidence that "right to work" laws reduce wages on average. In fact, my preferred estimates indicate that "right to work" laws increase wages by 3-4%, and these wage gains are enjoyed primarily by middle-skill workers and those outside of highly unionized industries.

1 Introduction

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. 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. 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. Before that, three states adopted RTW laws between 1960 and 2000, and seven had done so during the 1950s.[1]

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. Ellwood and Fine (1987) and Holmes (1998) find important effects of RTW laws on union organizing and manufacturing employment, respectively.[2] Empirically, results from previous studies of the effects of RTW laws 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 premia, Moore (1980) and Farber (1984) conclude that the effects of RTW laws on wages are minimal, and the laws are largely symbolic.

Estimating the effects of RTW laws on wages presents an important empirical challenge, which few previous studies have 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. Studies that use within-state before and after comparisons to estimate the effects of the RTW laws adopted after 1947 are vulnerable to this 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. Likewise, studies that compare states that have RTW laws at a point in time to those that do not could produce biased estimates if they do not address the possibility that some other, earlier

---

[1]Indiana adopted a RTW law in 1957, but repealed it in 1965.

[2]Holmes actually interprets his finding more cautiously, attributing the effects he finds to business-friendly regulatory environments, for which he uses RTW laws as a proxy.

factor could have determined both RTW status and wages.

To solve these problems, I use variation induced by the federal Taft-Hartley Act of 1947. 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 Taft-Hartley, 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. 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.

I make two contributions in this paper. First, I take advantage of the circumstances surrounding the passage of the Taft-Hartley Act to produce credibly identified estimates of the effects of RTW laws on wages, filling an important, policy-relevant gap in the literature on RTW laws. While Moore was among the first to give serious attention to the wage question and Farber's work considers important and interesting channels through which a wage effect might be realized, their empirical work does not overcome the identification challenges described above. Moore (1980) and Farber (1984) use cross-sectional variation in RTW status at a point in time but do not account for determinants of that status. Reed (2003) recognizes the importance of addressing the factors that determine RTW status, but his approach to doing so is somewhat arbitrary. Farber (2005) exploits the relatively recent adoption of RTW laws in Idaho and Oklahoma to make within-state wage comparisons before and after RTW adoption, but he does not provide any evidence in support of his claim that the introduction of these laws was arguably exogenous.

My second contribution is that I consider the possibility that the effects of RTW laws may vary across worker skill groups. There are good reasons to expect that this might be the case. RTW laws impose restrictions on unions. Previous research has shown that both union membership and the union wage premium vary across the distribution of wages (Card [1992]; Card, Lemieux, and Riddell [2003]; Frandsen [2012]). To the extent that RTW laws influence wages through their effects on unions, it is reasonable to expect these effects to vary across skill groups as well. Previous studies have primarily employed mean regression techniques to study the the effects of RTW laws on wages, and examinations of effects on subgroups within the labor force generally focus on groups like union members, non-members, or manufacturing workers. It is important, however, to allow the effects of RTW laws to vary across skill groups because, as Farber (2005) suggests, the sign of the effect may vary. If the effects are positive in one group (e.g. a group in which

union membership is high) and negative in another (e.g. where union membership is low), analysis based on mean regressions that does not allow effects to vary across groups could miss both effects altogether and erroneously conclude that RTW laws have no meaningful effects on wages. To address these issues, I use educational attainment as a proxy for skill and analyze RTW wage effects by education group. I also provide some suggestive evidence based on quantile regression estimates.

I find no evidence that RTW laws reduce wages on average and some evidence that they increase wages. My preferred estimates indicate that, on average, RTW laws increase wages by 3-4%. The positive wage effects appear to be concentrated among middle-skill workers, whose wages increase by as much as 5%. Industries with lower levels of unionization also experienced relatively large wage growth as a result of RTW laws. Triple difference estimation implies wage growth in highly unionized industries was up to 5% lower than it was outside of those industries, and wages grew most where unionization rates were lowest. Employment outcomes also improve most for middle skill workers and those outside of highly unionized industries.

While the wage effects I find here are within the the range of estimates provided in previous studies, the fact that mine are well identified makes them a new, credible answer to the question of what happens to wages when a RTW law is enacted. I also provide some evidence about how these effects are realized. Although there are many channels through which RTW laws may affect wages, the idea that they increase wages by increasing demand for labor - especially middle-skill labor, here - is most consistent with these data. There are other theories of how RTW laws affect wages that are not supported in the setting I consider; I discuss these more in section 4.4, after fully presenting wage and employment results.

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. Section four discusses results. Section five 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.

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." 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. Since federal law supercedes 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.[3] 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.

[Figure 1]

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

---

[3]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 superceded 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 jursidiction 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.

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.

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%. Outside of those states, only California (5%) 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 two (Indiana and Michigan) have ever done so. 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 (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, 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

The treatment group in my difference-in-differences estimation consists of states that had RTW laws go into effect in 1947 exogenously, as described in the previous section. I take as my control group the set of states that had not passed RTW laws by 1960. This allows me to estimate the effects of RTW laws in both the short- and long-run using census data from 1940-60 without changing the control group. This formulation excludes the seven states that adopted RTW laws during the 1950s from the analysis entirely, since the timing exogeneity argument does not apply to their laws, but their laws preclude them from serving as members of the control group when the analysis includes data from 1960.[4] A list of excluded states can be found in Figure 1.

The sample used in this analysis is drawn from census data for 1940, 1950, and 1960. Data were obtained from the Minnesota Population Center's Integrated Public Use Microdata Series (IPUMS) extracts. I perform my analysis at the individual level, using men between the ages of 18 and 54 who reported their age, education, race, marital status, student status, number of children, and sufficient information to calculate hourly wages (annual wage and salary income, weeks worked last year, and hours worked last week).[5] Additional details are provided in the data appendix.

[Table 1]

Table 1 presents sample means, changes in means over time, and differences in trends for a variety of potential determinants of wages for my treatment and control groups in both the short and long runs. While there are some statistically significant differences in the levels of some of these characteristics, the trends in individual-level characteristics across the treatment and control groups are not statistically distinguishable from each other at conventional significance levels in the short run. In the long run, only trends in age and children under five years of age are statistically different across groups at the 1% and 10% levels, respectively. These similarities in trends for factors that help determine wages are encouraging, because

---

[4]I conduct several robustness checks employing alternative control groups. They are discussed in section 4.4. They do not substantially alter my conclusions.

[5]Hourly wages are defined as

$$wage = \frac{\text{(Wage and salary income last year/Weeks worked last year)}}{\text{Hours worked last week}}.$$

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.

difference-in-differences analysis requires a control group that accurately represents the treatment group's trends in outcomes of interest. The ideal way to demonstrate the suitability of my control group would be to show that wage trends in the treatment and control groups did not differ leading up to the introduction of RTW laws. However, the census did not collect wage and salary information before 1940, so I can only construct hourly wages in one year before RTW laws were introduced and therefore cannot examine pre-trends in wages.

There are trend differences in two important state-level characteristics. The treatment group sees statistically significant decreases in union organization and increases in unemployment relative to the control group over both the short and long runs. However, since these outcomes are potentially endogenous to RTW status, these differences could reflect the effects of the treatment and should not be seen as invalidating difference-in-differences estimation in this case.

Table 2 reports unadjusted difference-in-differences estimates of the effects of RTW laws on log wages, employment, and labor force participation (LFP).[6] Estimates for employment outcomes are based on a larger sample of men who reported their employment status.[7] These results suggest large, positive effects on wages - adoption of a RTW law increases wages by over 16%. RTW laws also appear to have important effects on employment, decreasing both the employment rate and the employment/population ratio by more than three percentage points, results that are mechanically consistent with the finding that RTW laws do not affect the labor force participation rate.

[Table 2]

The fact that there are some substantial differences between the treatment and control groups in levels of characteristics like racial composition and educational attainment suggests that even though trends in these variables are conducive to difference-in-differences analysis, estimates derived from simple differences in means could suffer from omitted variables bias. To address this concern, I implement standard regression difference-in-differences estimation, considering the same outcomes shown in Table 2. The estimating equation is

$$y_{ist} = \left(RTW47_s \times After_t\right)\beta_1 + RTW47_s\beta_2 + After_t\beta_3 + X_i\gamma + Y_{st}\delta + \zeta_s + \zeta_t + \varepsilon_{ist}.$$

---

[6]Labor force participation is defined in the standard way. Any worker who is employed or unemployed but searching for a job is a labor force participant. I use two standard measures of employment: the employment rate (employed persons/labor force participants) and the employment/population ratio (employed persons/population). The labor force participation rate is defined as labor force participants/population.

[7]Estimates for the employment/population ratio and the employment rate differ in that the employment/population ratio effect is estimated using a sample that includes all men, including non-participants in the labor force, while estimates of the effect on the employment rate use a sample that excludes non-participants. Employment estimates use the census-provided person weights. Wage estimates use hours-adjusted person weights, defined as the product of the provided person weight and hours worked during the previous week. This weighting scheme for wage estimates follows the common practice in the wage inequality literature. For examples from that literature, see DiNardo, Fortin, and Lemieux (1996); Fortin and Lemieux (1997); and DiNardo and Lemieux (1997). Using person weights instead does not substantially alter the wage results.

In this equation, i indexes individuals, s indexes states, and t indexes years. When wages are the outcome of interest, they enter this equation in log form; employment outcomes enter as dummy variables. The variable $RTW47_s$ indicates that a state saw a RTW law go into effect in 1947, while $After_t$ indicates that an observation is made after the passage of Taft-Hartley in 1947. The coefficient on the interaction between these two variables, β_1, is the difference-in-differences estimate. This parameter can be interpreted as the percent change in wages (or the percentage point change in employment or labor force participation) that is attributable to the introduction of RTW laws.

The vector X_i contains the individual characteristics shown in Table 1, as well as census division and metropolitan statistical area (MSA) fixed effects (or metropolitan status fixed effects, when 1960 data are used).[8] Y_{st} contains time-varying state characteristics such as the unemployment rate, union organization rate, and the share of the two-party vote received by Democratic candidates in the most recent federal elections, as well as census division-year fixed effects.[9] The unemployment rate, union organization rate, and Democratic vote share may be important determinants of wages, but they could also be affected by RTW laws, so they are excluded from some specifications. ζ_s is a vector of state fixed effects, while ζ_t is a 1960 fixed effect.[10] ε_{ist} is an error term. In keeping with Bertrand, Duflo, and Mullainathan (2004), I cluster standard errors at the state level, and when I include 1960 data in my analysis, I collapse post-1947 observations into a single "after" period rather than estimating separate treatment effects for 1950 and 1960.

As discussed above, there are good reasons to believe the effects of RTW laws might vary across skill groups. I use mean regression to estimate the effects of RTW laws on wages for different skill groups defined according to educational attainment. I also produce quantile regression estimates for illustrative purposes, operating under the assumption that more highly skilled workers generally earn wages at higher points in the wage distribution. All tests of statistical significance, however, are based on mean regressions.[11]

Since RTW laws affect unions, they could have effects on wages that differ in sign or magnitude across

---

[8]The 1960 census data does not contain information on respondents' particular MSAs of residence. When my analysis includes 1960 data, I use fixed effects for metropolitan status (not in a metropolitan area; in a metropolitan area - central city; in a metropolitan area - not in central city; in a metropolitan area - central city status unknown) instead of MSA fixed effects. For pre-1960 analysis, I present results separate using MSA fixed effects and metropolitan status. They do not differ significantly.

[9]Unemployment rates are calculated at the state level from census data using the sample of all respondents, both male and female, with non-allocated employment status information. Union organizing and Democratic vote share variables come from other sources. See the Data Appendix for additional details.

[10]When only 1940 and 1950 data are used, year fixed effects are collinear with $After_t$ and ζ_t is not included.

[11]One might initially be inclined to evaluate the possibility that the effects of RTW laws vary with worker skill by estimating treatment effects at various points of the wage distribution using quantile regression. However, difficulties with this approach arise when performing inference on these treatment effect estimates. Two critical components of the empirical model come into conflict - state fixed effects are necessary in order to control for unobserved, time-invariant factors that determine wages and vary across states, and standard errors must be clustered at the state level to account for correlation in error terms among people affected by the same local conditions, including introduction of RTW laws. Since adding dummy variable "fixed effects" is not equivalent to de-meaning the data in the quantile regression model (as it is in OLS regressions), inference based on asymptotic theory cannot be performed when clustering is applied at the same level as a fixed effect, since there will always be at least as many parameters to estimate as there are clusters. For this reason, I base my analysis primarily on mean regressions for skill groups defined according to educational attainment.

industries depending on the extent of unionization present in those industries. In order to examine this possibility, I also estimate a triple difference (DDD) specification. The three differences are between treatment and control groups (using the same definitions as in the difference-in-differences analysis), between the periods before and after the introduction of RTW laws, and between workers in highly unionized industries and those in other industries. As of 1947, more than 67% of workers in the transportation, construction, and mining industries were unionized, as were roughly 40% of manufacturing workers. I classify these industries as highly unionized ($UnionHi_i = 1$).[12] At the other extreme, about 9% of workers in service industries were unionized, as were 12% of government workers. Historically, unionization rates in agricultural industries have also been very low. I implement triple difference estimation via the equation

$$
\begin{aligned}
y_{ist} \quad = \quad & (RTW47_s \times After_t \times UnionHi_i)\,\alpha_1 + (RTW47_s \times After_t)\,\alpha_2 + (UnionHi_i \times After_t)\,\alpha_3 + \\
& (UnionHi_i \times RTW47_s)\,\alpha_4 + UnionHi_i\alpha_5 + RTW47_s\alpha_6 + After_t\alpha_7 + X_i\gamma + Y_{st}\delta + \zeta_s + \zeta_t + \varepsilon_{ist}.
\end{aligned}
$$

As above, i indexes individuals, s indexes states, and t indexes years. The parameter of interest is α_1, which now gives the effect of RTW laws on the wages of workers in highly unionized industries relative to those of workers in other industries. Estimates of α_1 are unbiased in the DDD setting as long as no unobserved shocks differentially affect the relative wages of workers in highly unionized industries across treatment and control states. Additional interaction terms are included in the regression in order to produce the DDD estimate, and X_i, Y_{st}, ζ_s, and ζ_t are defined as in the difference-in-differences setting.

4 Results

I begin by estimating the effects of RTW laws on wages and employment via regressions that include the individual characteristics listed in Table 1 and geographic fixed effects (MSA/metro status, state, census division, census division-year) but not the potentially endogenous state characteristics (unemployment rate, Democratic vote share, union organization). This is my preferred specification. Since RTW laws could affect wages through these channels, controlling for them in regressions could lead one to misattribute some portion of the effect of RTW laws on wages to exogenous changes in these variables.

---

[12]I use the 1950 census industry classification system to categorize industries. IPUMS provides a variable that codes industries observed in 1940 and 1960 under the 1950 system. I treat as highly unionized all industries that fall within the single-digit classifications for mining and construction (coded 2), manufacturing (3), and transportation (5). The transportation category also includes communications industries and other utilities.

4.1 Wage Effects

Table 3 reports regression difference-in-differences estimates of the effects of RTW laws on wages for the full sample and by education group. Within each group, I present two estimates for the short run (1940-50), one that controls for MSA of residence and another that controls for metropolitan status, and one estimate for the long run (1940-60), since MSA of residence is not reported in the 1960 census. Since the more specific MSA control is preferable to the broader metropolitan status control, and the control group is more likely to be valid in the short run (since only one decade of outside factors have had the chance to differentially affect the two groups rather than two), the short-run estimates that control for MSA of residence are most credibly identified. I provide the short-run estimates that control for metropolitan status to facilitate comparisons between the short- and long-run estimates that are based on identical specifications.[13]

[Table 3]

In the full sample, RTW laws increase wages rather substantially in the short run, with enactment of a RTW law increasing wages by 4.4%. This estimate is statistically significant at the 1% level. This regression estimate is much smaller in magnitude than the mean comparison estimate in Table 2, indicating substantial omitted variables bias in the mean comparison estimates. The regression estimate is also much more precise.

Looking across education groups reveals that this positive effect is concentrated among low- and middle-skill workers.[14] Men who failed to complete high school experienced a statistically significant wage increase of 4.7%. Workers who completed high school saw their wages increase by 2.8% (statistically significant at the 5% level), while the point estimate indicates a 5.0% increase in wages for those who attended some college, although this estimate is not statistically significant due to the much smaller sample size in this group. When workers who finished high school and those who attended some college are grouped together as middle-skill workers, RTW laws are estimated to increase their wages by 3.2%, and this estimate is statistically significant at the 10% level.[15] High-skill workers (those who completed college) experience small, imprecisely estimated wage gains.

Controlling instead for metropolitan status in the short run reduces the magnitude of the estimates of the wage effects of RTW laws in some education groups. Estimates for the full sample and for workers who did not complete high school (LTHS) are virtually unchanged, but the estimate for high school graduates

---

[13]Long-run wage estimates also suffer from additional measurement error (beyond any in the short-run estimates) associated with imputing the outcome variable from data reported in ranges. Due to these data issues, I focus primarily on drawing conclusions from short-run estimates but check the long run to see if it reveals any dramatic reversals from the short run.

[14]Throughout this paper, "low-skill" refers to workers who did not complete high school, "middle-skill" refers to those who completed high school or attended some college, and "high-skill" refers to those who completed college.

[15]Table A1 reports combined estimates of effects on both wages and employment for middle-skill workers, combining the high school and some college samples. These results are very similar in general to the results for high school graduates. They are altered slightly and in some cases made less precise by the addition of the relatively small group of workers who attended some college. When I aggregate the two groups in this paper, I do so for simplicity, since the "some college" group is a natural fit for combination with high school graduates both intuitively and in terms of the effects they experience from the introduction of RTW laws.

falls by 0.7 percentage points and those for workers in the some college and college graduate groups fall by more than a full percentage point. Only the full sample and LTHS estimates remain statistically significant.

Adding data from 1960 causes estimates to decline further. The wage effect for the full sample is reduced to an increase of 3.3%, while RTW laws increase wages by 3.7% for LTHS workers. These estimates are statistically significant at the 10% level. No other group experiences an effect larger than 1.5%, and none of those estimates is statistically significant.

Educational attainment is likely a fairly crude proxy for skill, especially during this period when nearly two-thirds of the short run sample did not complete high school. If the educational attainment proxy is taken completely seriously, "low skill" workers will occupy a large portion of the middle of the wage distribution simply by virtue of there being so many of them. Estimating the effects of RTW laws on wages using quantile regression can illustrate how these effects varied across the distribution of wages.

[Figure 2]

Figure 2 plots quantile regression estimates of the effects of RTW laws on wages for select percentiles of the full-sample wage distribution from each specification in Table 3, as well as lines depicting the estimates from mean regressions, supplied for reference. Each plot shows effects on wages that are uniformly positive and largest in the middle of the wage distribution. Short run estimates peak at the median, above 7%. Consistent with Table 3, long-run estimates are smaller than short-run estimates, surpassing 4% between the 30th and 60th percentiles of the wage distribution. While the quantile regression results are not definitive, they do suggest that the effects of RTW laws on wages might best be described as largest for middle-skill workers.

[Table 4]

Table 4 reports DDD estimates of the effects of RTW laws on the wages of workers in highly unionized industries relative to those of workers in other industries. Across all specifications, the effects are negative and statistically significant at the 10% level or higher. The addition of state-year fixed effects to the DDD specification does not alter the estimates in any meaningful way, nor does moving from the short run to the long run. In the short run, wages of workers in highly unionized industries fall by at least 4.7% relative to workers in all other industries when RTW laws go into effect, a decline that becomes nearly twice as large when the comparison group is limited to only workers in lightly unionized industries (service industries, government, and agriculture). The full-sample DDD effect, which is derived from variation in wages across industries within states, is nearly identical in magnitude to the positive wage effect shown in the full sample difference-in-differences analysis, which is based on cross-state variation. Together, these estimates indicate that even as RTW laws increase wages in the state economy as a whole, the workers most likely to be directly affected by RTW laws, those working in highly unionized industries, are not sharing substantially in those

gains.

4.2 Employment Effects

RTW laws also affect employment outcomes. Table 5 shows regression-adjusted estimates of the effects of RTW laws on employment outcomes. Like the unadjusted estimates, the full sample regression estimates show RTW laws causing a statistically significant decrease in the employment rate in both the short and long runs. The regression estimate is slightly smaller in magnitude, indicating a two percentage point decline in the employment rate as opposed to the 2.7 percentage point unadjusted estimate. Both the adjusted and unadjusted estimates remain essentially unchanged when 1960 data are added.

[Table 5]

Although adding regression controls alters the unadjusted point estimate of the effect of RTW on the employment rate only slightly, doing so leads to a substantially different interpretation of why the employment rate falls. The employment rate can fall if the number of people who have jobs decreases or if the number of people who want jobs increases. The unadjusted estimates show essentially no change in the labor force participation rate and a decline in the employment rate and employment/population ratios. Taken together, these three results suggest that RTW laws cause disemployment, since the same share of workers want to work, but fewer are employed.

In the regression estimates, on the other hand, the effects on the employment/population ratio are still negative, but they are not statistically significant, and there are positive and statistically significant effects on the labor force participation rate in the short run. The employment rate falls in part because RTW laws induce more workers to seek jobs. In fact, within each education group, the estimate of the effect of RTW laws on the employment rate is closely approximated by subtracting the labor force participation estimate from the employment/population estimate, as one should expect.

The importance of the the labor force participation effect varies across skill groups. Among low-skill workers, the magnitude of the employment/population ratio effect is much larger than the LFP effect (though it is only statistically significant in the short run MSA specification), and the two effects combine to produce negative employment rate effects that are statistically significant in all three specifications and driven primarily by disemployment.

Among middle-skill workers, positive effects of RTW laws on LFP are the dominant cause of the reduction in the employment rate. RTW laws increase LFP among middle-skill workers by about 1.8-3.6 percentage points in the short run (statistically significant at the 5% level or higher) and by 1.3-1.6 percentage points in the long run (significant at the 1% level for workers who completed high school) while having essentially no

effect on the employment/population ratio in any specification for high school graduates and a somewhat sizable positive effect for workers who attended some college (statistically significant at the ten percent level).[16] For both groups of these workers, estimates show statistically significant decreases in the employment rate, even though the employment/population ratio increased among workers with some college.[17]

Point estimates for high-skill workers indicate that RTW laws decrease both LFP and the employment/population ratio by similar magnitudes (about two percentage points), thereby leaving the employment rate unchanged. These estimates, however, are only statistically significant in the long run. These results imply that RTW laws induce some previously employed high-skill workers to drop out of the labor force. It is difficult to rationalize this effect on high-skill workers, and the statistical significance of this result in the long run may simply be attributable to random sampling error.

While disemployment is generally seen as making workers worse off, increased labor force participation could be a sign of improved economic conditions. In this sense, the results presented here suggest negative employment effects for low- and high-skill workers, but potentially positive effects for middle-skill workers. Although the wage results in the previous section suggest that some low-skill workers may have benefitted from RTW laws, if RTW laws make it easier for firms to fire low-productivity workers (perhaps by weakening unions), disemployment among low-skill workers could mechanically contribute to the increase in wages for employed low-skill workers by selecting those with the lowest wages out of the sample (since I only observe wages for workers employed at the time of the census), biasing my estimate of this wage effect.[18] I will return to this possibility below.

RTW laws could also affect the skill composition of different sectors of the economy. The ability to work without joining or financially supporting a union could make jobs in the highly unionized sector more (or less) appealing to workers in a way that varies with skill. Alternatively, employers in either the highly or lightly unionized sectors could adjust the types of workers they hire in light of the ways RTW laws changed their expected future relationships with unions.

[Table 6]

Table 6 provides estimates of the effects of RTW laws on employment in the highly and lightly unionized sectors by skill level in the short run and the long run.[19] RTW laws have no statistically significant effects on

---

[16]The lower employment and labor force participation rates among workers who completed some college are due to the fact that a substantial share of these men (roughly 27%) were still enrolled in college at the time. Since employment is an alternative to education and outside employment options Figure 1nto decisions about educational attainment, it is appropriate to include these workers in the sample used to estimate employment effects. In wage estimates, only those who report wage and salary income are included in the analysis.

[17]Estimates in Table A1 show positive effects on LFP and essentially no effects on the employment/population ratio, leading to a decline in the employment rate in the combined sample of middle-skill workers.

[18]One potential way to address this type of selection bias would be to jointly estimate the effects of RTW laws on employment and wages. However, it is difficult to find an excluded variable that would identify those estimates based on something other than functional form, especially in data collected more than 50 years ago.

[19]Table 6 presents results based on a sample that includes labor force non-participants. When they are instead excluded,

employment in highly unionized industries for any skill group in either the short or long run. Employment in lightly unionized industries, however, may be affected, at least among low- and middle-skill workers, though the statistical significance of these results varies over time. In the short run, RTW laws make low-skill workers about 1.9 percentage points less likely to be employed in lightly unionized industries, an effect equal to approximately 6% of employment in those industries for that group. In the long run, though, this effect shrinks to 0.4 percentage points and is no longer statistically significant.

Short-run point estimates for middle-skill workers, on the other hand, imply that RTW laws made middle-skill workers between 2.3 and 3.5 percentage points more likely to work in lightly unionized industries (8.5%-13% of the mean), depending on specification, though this effect is not statistically significant. In the long run, however, the magnitude of this effect remains large at three percentage points and becomes statistically significant.

These sector-skill employment results shed additional light on the wage results presented above. The difference-in-differences wage estimation shows that, on average, RTW laws increase wages, and that low- and middle-skill workers experience the largest increases. The DDD analysis suggests that these wage gains are enjoyed primarily by workers outside of highly unionized industries. The sector-skill employment results are consistent with these findings because they show that RTW laws increase employment of a group of workers who experience wage gains (middle-skill workers) in the sector where wages increase (lightly unionized industries, or more broadly, outside of highly unionized industries).

These results also address the possibility that the increase in low-skill wages reported in the previous section is due to firms disemploying their lowest-skill workers, thereby selecting them out of my wage analysis sample. If this type of selection were responsible for the estimated increase in low-skill wages, one would expect to see a negative effect on employment in highly unionized industries among low-skill workers here, since workers without wage information still appear in the employment analysis sample. The fact that no such disemployment is observed suggests that some other factors are likely responsible for much of the positive effect on low-skill wages reported above.

4.3   Controlling for Labor Market Conditions

Although my main results are derived from a specification that does not control for labor market conditions, it would be inappropriate to ignore those conditions entirely. While RTW laws may affect labor market conditions like the unemployment rate or the share of workers covered by unions, these conditions can and do fluctuate over time for a variety of other reasons (e.g. business cycle fluctuations) and are important

---

magnitudes of point estimates differ slightly, but their signs remain the same and only one statistical significance result changes (the long run effect on employment in lightly unionized industries loses statistical significance).

determinants of wages and employment status for individuals. Ignoring these factors entirely may introduce significant omitted variables bias if they are correlated with RTW status. For this reason, it is of interest to estimate the effects of RTW laws on wages and employment while controlling for macroeconomic conditions, even though doing so shuts down some (but not all) channels through which those effects could be realized.[20] In comparison to the previous subsections, this section could be seen as presenting more conservative estimates of the effects of RTW laws on wages and employment.

I add three state-level characteristics to the specifications in the previous section: the unemployment rate, the share of the workforce organized by unions, and the share of the two-party vote received by Democratic candidates in the most recent federal election.[21] Unemployment and union organizing are obviously potential determinants of wages and employment. Democratic two-party vote share is included as a proxy for states' political environments. Variation in this variable could capture variation in other state-level labor market policies that affect wages and employment. I also include individual-level, single-digit industry and occupation fixed effects in wage regressions in this section.[22]

[Table 7]

Table 7 presents estimates of the RTW wage effect for the full sample and by education group from models that include these labor market characteristics. There are a few important differences between these estimates (which I will refer to as the labor market controls estimates) and those that do not control for labor market characteristics (which I will refer to as the main estimates). First, when controls for labor market characteristics are added, short run estimates become smaller. The full sample wage effect estimate shrinks from 4.4% to 1.3% and is no longer statistically significant when these additional controls are included. Among educational groups, only the estimate for high school graduates remains statistically significant (at the 5% level), while the estimate for workers who did not complete high school falls from 4.7% to essentially zero.

The LTHS and high school graduate estimates also illustrate the second important difference between the main and labor market controls estimates: the short-run wage gains are more concentrated among middle-skill workers in the labor market controls estimates. Neither low- nor high-skill workers experience appreciable changes to their wages in the short run as a result of RTW laws in the labor market controls estimates, but middle-skill workers (again defined as those who completed high school or attended some college) see their wages rise by 3.7%, an effect that is statistically significant at the 10% level.

---

[20]For example, RTW laws could affect wages even with macroeconomic conditions held constant by changing the relative bargaining power of firms and unions.

[21]See the Data Appendix for more details on the construction of these variables.

[22]I do not include industry and occupation fixed effects in the employment regressions because these regressions are estimated using a sample that includes men who are unemployed or out of the labor force, and those fixed effects are only applicable to men who are employed.

Finally, in the labor market controls estimates, moving from the short run to the long run increases the magnitude of the effect on wages in all three skill groups. The full sample estimate increases from 0.8% to 4.9%, driven largely by the increase in the magnitude of the effect for low-skill workers, which is 5.2% in the long run rather than -0.1% in the short run.[23] In the main estimates, these estimates all became smaller in magnitude when 1960 data were included in the analysis.

[Figure 3]

Figure 3 plots quantile regression estimates of the effects of RTW laws on wages for the labor market controls specification. Like the main estimates, they show the largest effects in the middle of the distribution. As is the case with the mean estimates, the labor market controls estimates are smaller than the main estimates in the short run, increase in magnitude when 1960 data are included, and are larger than the main estimates in the long run. The largest quantile regression estimate is at the 70th percentile in the short run (3.6% with MSA fixed effects, 2.8% with metro status fixed effects) and at the 60th percentile in the long run (6.5%). Controlling for labor market characteristics does not substantially alter the suggestion from the main quantile regression estimates that middle-skill workers are the biggest beneficiaries of RTW laws.

The most important similarity between these two sets of estimates is that neither shows any evidence of a large negative effect on average wages for the economy as a whole. Based on the short-run labor market controls estimate for the full sample, I can reject the hypothesis that RTW laws reduced mean wages by 2.5% or more at the 5% level (p-value 0.0345).[24] This evidence weighs against the claims made by opponents of RTW laws that the laws will substantially reduce wages in states that adopt them.

Another noteworthy similarity between the main and labor market controls estimates is that both show larger long-run effects on workers who did not complete high school than they do for any other educational group. This could be due in part to the imperfect nature of educational attainment as a proxy for skill. Even in 1960, workers who did not complete high school make up nearly 50% of my estimation sample in wage regressions. There is likely to be substantial variation in skill within a group that large. Although these workers have the lowest levels of education, those who benefit from RTW laws most could still be in the middle of the skill distribution. If this is the case, one might expect to see larger quantile regression estimates of the RTW wage effect near the top of the wage distribution among men who did not complete high school. Indeed, the largest quantile regression results for men without a high school diploma are at the

---

[23]These comparisons between short run and long run estimates are based on short run specifications that use metropolitan status fixed effects.

[24]This calculation is based on the specification that includes MSA fixed effects rather than metro status fixed effects because the specification with MSA fixed effects is intuitively more appealing since it does not discard available information. As stated in the text, the short-run specification using metro status fixed effects is provided primarily to facilitate direct comparison between the short run and the long run effects. However, if I were to use the short-run, full sample estimate from the metro status specification, which produces a smaller treatment effect estimate, I would be able to reject the hypothesis that RTW laws reduce mean wages by 2.5% or more at the 10% level (p-value 0.085).

top of the wage distribution, with point estimates peaking at the 80th percentile (results not reported here).

Like the main DDD results, the labor market controls DDD estimates (not reported) show that RTW laws cause wages in highly unionized industries to decline relative to wages in other industries. Estimates that compare highly unionized industries to all other industries are not statistically significant, but suggest reductions in relative wages of about 2%-3%. When highly unionized industries are compared only to lightly unionized industries, the negative effect on relative wages is larger (wages fall by about 5%-7%) and statistically significant at the 5% level or higher in all specifications.

Although the magnitudes of the estimates differ, the labor market controls wage estimates confirm the primary result from the main estimates - there is no evidence that RTW laws reduce wages on average. The two sets of estimates are also in agreement that RTW laws do cause wages in highly unionized industries to decline relative to wages in other industries. In other words, any wage growth caused by RTW laws in experienced primarily outside of highly unionized industries. Though only suggestive, all quantile regressions indicate that the effects of RTW laws are largest for middle-skill workers, and the short-run labor market controls estimates from mean regressions support that interpretation as well.

Interpreting employment results from specifications that include labor market controls can be difficult. When the state-level unemployment rate is included as a covariate, estimates of the effects of RTW laws on the employment rate are near zero for mechanical reasons.[25] Intepretation of the effects on the employment/population ratio and labor force participation rate are complicated by the fact that these outcomes are quite similar to the employment rate, and with controls for labor market conditions included, it is not clear what meaning an estimate of the residual effect of RTW laws on these outcomes has. For this reason, I will discuss these results briefly but will not report coefficients.

Estimates of the effects on the labor force participation rate is perhaps least affected by these considerations since its numerator includes both employed and unemployed people. One could interpret the effects on the employment/population ratio as a translation of the effects on labor force participation through an employment rate that is held constant. I will focus on labor force participation results.

Strikingly, the effects on labor force participation are very similar to the main estimates. The largest effects are again experienced by middle-skill workers, who are at least two percentage points more likely to participate in the labor force in the short run due to RTW laws, an effect that is statistically significant at the 5% level for high school graduates. At 1.9 percentage points, the long-run labor market controls estimate

---

[25]One might think that all coefficients in regressions of the employment rate on the unemployment rate and other variables would be zero except the coefficient on the unemployment rate. That is not the case in this setting for two reasons. First, I use state unemployment rates as the independent variable in my regressions, and the dependent variable is the national employment rate. Second, the state unemployment rates I use are calculated without imposing age or gender restrictions, and my estimation sample consists exclusively of prime age males. While the unemployment rate is an important independent variable in these regressions, including it does not force estimates of the effects of other variables on the employment rate to be zero.

for high school graduates is about 0.6 percentage points larger than the corresponding main estimate.[26] The long-run estimates for the full sample and low-skill effects have become statistically significant in the labor market controls specification, but their magnitudes remain fairly small at about 0.9 percentage points each (on a base of nearly 92% labor force participation in both cases). It is, however, interesting that these increases coincide with increases in the magnitude of the wage effects for these groups.

The sector-skill employment results from the labor market controls specification should also be relatively less affected by the inclusion of labor market controls, since sectoral changes could still occur with state-level economic conditions held constant. As with labor force participation, adding labor market controls does not change these estimates very much. As before, they show no change in the skill composition of the highly unionized workforce. The increases in middle-skill employment in lightly-unionized industries, about three percentage points in both the short and long runs, are also close the the main estimates, though they are now statistically significant in both cases (at the 10% level in the short run and at the 5% level in the long run) rather than only in the long run.

Overall, the main and labor market characteristics estimates support three major conclusions about the effects of RTW laws on wages and employment. First, RTW laws do not decrease wages on average. Second, RTW laws reduce wages in highly unionized industries relative to wages in other industries, especially lightly unionized industries. Finally, there is some evidence that RTW laws have positive wage and employment-related effects for at least some workers, and middle-skill workers appear to be their primary beneficiaries.

4.4   Possible Channels for Wage Effects

A priori, RTW laws could affect wages through a variety of channels. Since individual-level information on union membership is not available in the census and I can find only aggregated data from other sources, I cannot definitively address any of the possibilities. However, the pattern of results observed in the previous subsections suggests that some channels are more likely than others.

As Lemieux (1998) notes, union membership is the result of a two-sided selection process. Given that collective bargaining agreements (CBAs) often determine wages simultaneously for groups of positions based on factors other than individual worker productivity (such as broadly defined job responsibilities or seniority), highly productive workers will be the most appealing to firms looking to fill positions covered by a CBA (since the CBA could end up requiring that they be paid less than their marginal product), and workers with low productivity will be the least appealing (since the CBA could end up requiring that they be paid more

---

[26]In the combined middle-skill sample (high school graduates and those who attended some college) used in Table A1, the increase in LFP is about 2.4 percentage points, and it is statistically significant at the 5% level. The long run estimate for middle-skill workers also indicates a 1.9 percentage point increase in LFP and is about 0.5 percentage points larger than the corresponding main estimate.

than their marginal product). For the same reasons, positions covered by a CBA will be most appealing to workers with the lowest productivity and least appealing to workers with the highest productivity.

Since wages and job security are often subjects of CBAs, RTW laws could increase average wages observed in the census (in which hourly wages are only computable for the currently employed) if they made it easier for union employers to select a more productive workforce by firing their lowest productivity workers (i.e. those with the lowest wages), thereby causing them not to be observed. However, if this were a major contributing factor to the wage effects I find, one would expect to see reductions in employment in highly unionized industries among low-skill workers, and I find no such reduction.

If RTW laws affected unions' bargaining power in a way that made union jobs more appealing to high-skill workers (employers may want to tie pay more closely to productivity in order to attract more productive workers), wages could rise due to movement of these workers into the sector that pays higher wages on average (as the highly unionized sector did at this time). Two pieces of evidence suggest that this type of employee-side selection did not produce the wage effect estimated here. First, I observe no increase in employment in highly unionized industries among high-skill workers (or even middle-skill workers). Second, the DDD analysis shows that the positive overall wage effect is driven by increases in wages outside of the highly unionized industries. These two pieces of evidence, together with the fact that I observe no disemployment of low-skill workers in highly unionized industries, suggest that changes in selection into union jobs based on new circumstances surrounding collective bargaining are probably not responsible for the wage effects I find here.

While I observe essentially no changes in the level of employment or skill-composition of the workforce in highly unionized industries attributable to RTW laws, I do find increased employment of middle-skill workers in lightly unionized industries, providing more evidence that the positive effects of RTW laws are experienced largely by workers who do not belong to unions. I also find some signs of increased wages and labor force participation for middle-skill workers in general. Since the introduction of RTW laws affected both existing and future unions, the laws could plausibly be responsible for changes in wages and employment outside the highly unionized industries. In particular, RTW laws could increase wages by increasing demand for labor, specifically middle-skill labor.

Suppose there are potential firms whose activities rely heavily on middle-skill labor. There is some evidence that middle-skill workers are more likely to be members of unions.[27] RTW laws eliminate the power of unions to compel membership and financial support, which could make it more difficult to form

---

[27]Card, Lemieux, and Riddell (2003) consider union membership across the wage distribution during a later time period beginning in 1973-74 and show a distinctly hump-shaped pattern of union membership rates among men that peaks in the middle of the distribution. While I cannot address union membership in my analysis sample, I create a proxy for it (employment in a non-managerial, non-professional occupation within a highly unionized industry) and plot the mean of this measure by wage decile for the period 1940-50 in Figure A1. This plot also peaks in the middle of the wage distribution.

new unions, as Ellwood and Fine (1987) suggest. In that case, states that pass RTW laws become more attractive to potential firms that rely on middle-skill labor because the laws decrease the likelihood and expected costs of dealing with a unionized workforce in the future. As these potential firms become actual firms in RTW states, demand for middle-skill labor increases, driving up wages for these workers.

It remains to be explained why these effects would be realized primarily in the lightly-unionized sector. One might expect a regulation of unions to primarily affect industries in which unions are prominent. However, in 1947, unionization rates in highly unionized industries were very high, in some cases exceeding 80% (e.g. mining, construction). At those levels of unionization, it may have been difficult for new firms to avoid unionization, even with the help of RTW laws, since the vast majority of potential employees would already have experience in unionized environments. On the other hand, RTW laws could have created a substantial barrier to unionization in industries without large, pre-existing bases of union members. If that barrier to future unionization were more substantial than the weakening of existing unions, RTW states could be more attractive to potential firms in lightly unionized industries than in highly unionized industries.

While speculative, this illustrates that RTW laws could plausibly have increased demand for middle-skill labor and thereby middle-skill wages in the manner observed. In addition to the selection mechanisms discussed above, the alternative channels through which RTW laws could affect wages are either inconsistent with these data or untesTable 1n this setting. For example, Moore (1980) suggests that unions subject to RTW laws must earn their members support (since members are permitted to free-ride), so they exert greater effort in bargaining and provide higher wages. However, if this were the driving force behind the wage effect observed here, one would expect wages in highly unionized industries to rise relative to those in other industries. Instead, they fall, rendering this mechanism inconsistent with these data.

Alternatively, if anti-union sentiment is strong among workers in RTW states, the adoption of RTW laws could alleviate a friction in the labor market (compulsory union membership and financial support) and lead to better matches between workers and firms, producing higher wages. This proposition is not testable with the available data.

Although the evidence is not nearly definitive, the hypothesis that RTW laws increased wages for middle-skill workers by increasing demand for their labor is the one most supported by these data.

4.5 Robustness Checks

The pattern of wage results described above is robust to a variety of specifications. Table 8 reports short-run estimates by education groups for several alternative specifications that vary the definition of the control group for both the main and labor market controls specifications. The preferred estimates are reproduced for

reference. In this table, each coefficient is from a separate regression. Rows show results limiting the sample to particular education groups. Columns present alternate specifications. Nearly every point estimate is positive, many are positive and statistically significant, and none are negative and statistically significant. Looking down each column, the pattern of finding larger (more positive) wage effects among middle-skill workers holds in many of these alternative specifications. Long run estimates, which are not reported here, follow the same pattern.

[Table 8]

Among the alternative control groups presented here, two (never RTW, not RTW by 1960 and not bordering a RTW state) exhibit differential trends between treatment and control groups in at least one observable, individual-level characteristic, which makes them less appealing than the control group I use in my primary analysis.

However, a third alternative (not RTW by 1960 and bordering a RTW state) exhibits no such differential trends and may have some intuitive appeal. States that are located near RTW states could be the ones most likely to accurately represent counterfactual outcome trends. Results from this specification are qualitatively similar to those from the main and labor market controls specifications. The one notable departure from those results is that the point estimate for college graduates is large and negative, though not statistically significant. That, along with the substantially larger samples sizes available when the border restriction is not imposed lead me to prefer my primary control group, the set of all states that had not passed RTW laws by 1960.

These tables also address a more specific threat to my identification strategy. The strikes that lead to the adoption of the Taft-Hartley Act were concentrated in states that did not pass RTW laws. If the non-RTW provisions of Taft-Hartley affected wages in a way that was correlated with the strikes or the conditions that caused them (or if the strikes themselves affected wages), those affects would be negatively correlated with RTW status and concentrated in the control group. This could bias my difference-in-differences estimates.

Historical evidence suggests that this is not a major concern. According to Metzgar (2009), the strikes during this period led to a wage increase of around 18.5 cents per hour that was fairly consistent across the nation, due in part to engagement on the part of the Truman administration, as well as coordination among unions that were members of the Congress of Industrial Organizations. Also, this wage increase was largely eroded within a few years in real terms by higher post-war ination that followed the removal of price controls that had been in place during the war.

Columns 5, 6, 11, and 12 of Table 8 also address this issue. Columns 5 and 11 exclude the five states hit hardest by strikes following World War II (Illinois, Michigan, New York, Ohio, and Pennsylvania) from the main and labor market controls specifications, respectively, and columns 6 and 12 exclude those states as

well as California, Connecticut, Massachusetts, New Jersey, West Virginia, and Wisconsin.[28] Though these results differ in magnitude from the preferred estimates from each specification and are not statistically significant, the pattern of point estimates across education groups is similar. Since the states whose strikes brought about the Taft-Hartley Act play no role in this analysis, the similarity between these results and those from the preferred control group suggests that the preferred results are not likely to be driven by effects of non-RTW components of the Taft-Hartley Act on wages within the control group.

Although the trends in observable characteristics do not differ substantially between my treatment and control groups over the analysis period, one might be concerned that the two groups are not sufficiently similar in levels of those characteristics for the control group to accurately represent counterfactual outcomes. Mean wages, for example, are considerably higher in my control group than in the treatment group. Since economic outcomes, including wages, have converged across regions over time (see Barro and Sala-i-Martin [1991] and Blanchard and Katz [1992]), the wage effect I find could simply be a product of wages in lower-wage treatment states converging on those in higher-wage control states for reasons unrelated to RTW laws.

To address this issue, I reestimate my main and labor market controls specifications using the 20 states with mean wages closest to the pooled mean wage for the treatment group in 1940 as the control group.[29] Estimates are reported in columns 7 and 14 of Table 8. These results follow essentially the same pattern as the results based on my preferred control group, as well as those based on the other alternative control groups presented in Table 8. RTW laws have positive and statistically significant effects on wages for workers with a high school diploma or less. When labor market controls are included, the effect for LTHS workers shrinks, some estimates become negative, but the wage effect for workers who completed high school remains positive and statistically significant. Only the labor market controls estimate for college graduates is negative and statistically significant. This suggests that wage convergence is not primarily responsible for my findings.[30]

4.6   Controlling for World War II

Figure 4 plots the log of mean state personal income (SPI) per capita in my treatment and control groups over the period I analyze. Although the trends are roughly parallel in the years immediately preceding and succeeding American involvement in World War II, there is some divergence during the war. This raises the

---

[28]Indiana, also among the states hit hard by strikes following the war, was already excluded from the control group because it adopted a RTW law in 1957.

[29]I use 20 states in order to maintain a sufficiently large number of clusters for performing inference. Mean wages are calculated using hours-adjusted person weights. I exclude states that adopted RTW laws during the 1950s in order to avoid comparing conditions that followed the arguably exogenous introduction of RTW laws in my treatment states to conditions that preceded the endogenous adoption of RTW laws in other states during that decade.

[30]If I use the 20 states with mean wages closest to my treatment group without regard for subsequent RTW status, my main estimates are positive (except for high school graduates) and only the LTHS estimate is statistically significant. The labor market controls estimates are negative but small, and none are statistically significant. Results are available upon request. Although these results do not show wage gains for middle-skill workers, they do support the conclusion that RTW laws do not substantially reduce wages.

question of how WWII affected wage growth during this period. Involvement in a war of that magnitude increases demand for many goods, some of which are widely produced during peacetime, but many of which are not. Since the census data used here are from 1940, 1950, and 1960, estimates will not be directly affected by the production of these goods. However, as the American economy re-tooled to supply the war effort, local economies could have experienced structural changes that persisted after the war had concluded, leading to higher or lower wages in those areas in the following years. Structural changes could be correlated with RTW status. Producers of war supplies could have decided where to locate their plants based on local unionization rates or attitudes toward unions. The states that would go on to pass RTW laws had, on average, lower unionization rates. If changes in state economies during the war were correlated with immediate adoption of RTW laws, it is important to try to control for those changes. Figure 4 suggests that to the extent that SPI per capita and wages are positively correlated, omitting controls for these changes could positively bias my difference-in-differences estimates.

[Figure [IV]

Ideally, I would address this concern by controlling for the underlying factors that lead to divergence in this measure between my treatment and control groups by including variables describing war-time production levels or changes in various industries. Since I lack such detailed data, I simply use the percent change in SPI per capita in each state between 1940 and 1945 as a proxy for local economic changes related to World War II. I include this measure in my regressions interacted with a dummy variable that is equal to one if an observation is from after 1947, an approach analogous to the one employed by Acemoglu, Autor, and Lyle (2004) to address changes in wages due to World War II.[31]

Table 9 reports estimates of the effects of RTW laws on wages based on specifications that control for changes in per capita SPI during World War II. For both the main and labor market controls specifications, controlling for World War II makes nearly every point estimate less positive. However, the vast majority of these WWII estimates remain positive, and most that were statistically significant in the baseline estimates (seven out of twelve overall and six out of nine when ignoring the short run metro status estimates) remain so, albeit at lower significance levels. In the short run, the main estimates (with MSA controls) remain statistically significant at the 10% level or higher for the full sample and for those who did not complete high school, as does the labor market controls estimate for high school graduates. In the long run, the labor market controls estimates are significant at the 10% level for the full sample, those who did not complete high school, and high school graduates when I control for WWII.

---

[31] Acemoglu, Autor and Lyle (2004), which exploits variation in male mobilization rates across states to estimate the elasticity of demand for female and male labor, suggests an alternative control for structural changes during World War II: male mobilization rates. Using mobilization rates (or other measures employed in the AAL analysis, including German heritage and industrial and occupational structure) instead of changes in per capita SPI to control for World War II does not substantially alter the estimates produced.

[Table 9]

Although the WWII specifications provide weaker evidence of positive wage effects than the baseline specifications do, they still offer no evidence of negative wage effects. They also largely preserve the pattern of point estimates across education groups over both the short and long runs. Figures 5 and 6 plot quantile regression estimates comparing the WWII specifications to the baseline specifications and show that the effects of RTW laws on wages remain largest in the middle of the distribution when WWII controls are included. Finally, as Table 4 indicates, controlling for WWII does not alter the conclusion that the positive wage effects of RTW laws were primarily experienced outside of highly unionized industries. The conclusions drawn from analysis of the baseline specifications survive inclusion of controls for WWII.

[Figure 5]

[Figure 6]

5   Conclusion

RTW laws often incite contentious debates among politicians and the public, despite the fact that previous research on these laws has not definitively established if or how they affect wages. No consensus exists in large part because studies of this effect face important challenges to identification. The sign of the effect of RTW laws on wages is theoretically ambiguous, both in general and across the distribution of wages. Additionally, adoption of a RTW law is often endogenous to the economic circumstances policymakers faced at the time they considered the law. This paper addresses both of these challenges. I consider an historical setting in which RTW laws took effect almost simultaneously in 12 states due to the passage of the Taft-Hartley Act. Critically, I argue that the circumstances that brought about that change in federal law were uncorrelated with conditions in the states that adopted RTW laws, so the timing with which RTW went into effect was exogenous to those states. I address distributional issues by estimating effects separately for different skill groups (proxied by educational attainment), which are likely to occupy different regions of the wage distribution.

I find no evidence that RTW laws decrease wages on average. Across the many specifications I consider, the vast majority of my full sample estimates are positive, and none are negative and statistically significant. My main results indicate that RTW laws increase wages by 3-4%. These wage gains are enjoyed largely by middle-skill workers and those outside of highly unionized industries. While there are several potential channels through which RTW laws could affect wages, the idea that they increase wages by increasing demand for labor (in particular middle-skill labor here) is most consistent with these data. These results are robust across a variety of specifications.

Since the effects of RTW laws on wages appear to depend on the extent of unionization, one might wonder how applicable these estimates are to laws adopted in the present era, in which union membership rates are much lower than they were during the period I analyze and differences between unionization rates in highly and lightly unionized industries are much smaller. Additional research to determine the exact nature of the dependency of RTW wage effects on unionization levels is necessary.

6 Appendix

[Table A1]

[Figure A1]

6.1 Data Appendix

I assess the effects of RTW laws on the distribution of wages using data from the 1940 and 1950 censuses, obtained from the Minnesota Population Center's Integrated Public Use Microdata Series (IPUMS). In each of these surveys, respondents are asked how much they earned in wage and salary income over the previous year, how many weeks they worked in that year, and how many hours they worked last week. Using responses to these questions, I construct a measure of hourly wages by dividing annual wage and salary income by the total number of hours worked in that year, a figure calculated by multiplying hours worked last week by weeks worked in the previous year.[32] Since individuals report wage and salary income from all jobs combined, the hourly wage measure calculated here is the average hourly wage for each individual across all jobs held in the preceding year. I exclude all people for whom values of any variable used to construct my wage measure were allocated.[33] Additionally, I analyze only the wages of so-called "prime-age" males, those between the ages of 18 and 54, inclusive.

Annual wage and salary income is topcoded at $5,000 in the 1940 census, at $10,000 in 1950, and at $25,000 in 1960. I adjust topcoded observations by multiplying them by 1.5, following the technique employed by Autor, Katz, and Kearney (2008), among others. After making these adjustments, I also exclude the top and bottom 2.5% of hourly wage observations in order to limit the influence of outliers on my analysis.

---

[32]This method of calculating workers' hourly wages implicitly assumes that "last week" was a typical work week in terms of the number of hours worked. It also limits the sample used for wage analysis to those workers who were on the job during the reference week, since hourly wages cannot be calculated for workers who report zero hours worked in the previous week. Analyzing weekly wages instead of hourly wages would address both of these issues, but in that case it would be impossible to distinguish changes in weekly wages due to changes in the return to a unit of labor from those due to changes in hours worked. Since my primary interest here is in how RTW laws affect the returns to units of labor, I focus on hourly wages. If a measure of usual hours worked were available for these years, it would be preferable, since it would make the wage measure less sensitive to atypical work weeks and allow for the inclusion of workers who happened to be away from work during the reference week while also preventing changes in hours worked from influencing the analysis.

[33]When I consider effects on employment and labor force participation, I exclude people with allocated values for those outcomes.

Wages are adjusted for inflation using the Consumer Price Index to 1960 dollars.

The census also provides many of the variables I use as controls in my analysis. Age, race, education, marital status, student status, number of children, number of children under five years of age, industry, and occupation are all reported consistently in the IPUMS data across the censuses I use, as are state and census division.[34] Age, number of children, and number of children under five enter my analysis continuously, and I use dummy variables for educational attainment, married, student, single-digit industry, and single-digit occupation. The various fixed effects I include also enter as dummy variables.

I calculate the unemployment rate in each state in each year based on reported employment status for respondents of all ages and both genders.

Two important variables, however, are not contained in the census data. First among these is the extent to which workers in each state and each year are covered by unions. Ideally, I would like to know which workers in my sample were members of unions. The census, however, did not (and does not) ask respondents whether they are members of labor unions. However, it may still be important to control for the extent of union coverage at the state level. Troy and Sheflin (1985) provide union coverage rates by state for select years, beginning with 1939. Since the years I use in my analysis are not among the ones reported, I impute values of union coverage using linear interpolation.

Second, I control for political attitudes in each state in each year. As a measure of these attitudes, I use the share of the two-party vote received by Democratic candidates in federal elections. My measure of democratic two-party vote share is constructed in the typical way, that is, the number of votes in each state received by Democratic candidates in federal elections divided by the number of votes received by Democratic and Republican candidates combined. I construct this measure from vote totals reported by the Clerk of the House of Representatives. The Clerk's office provides, on its website, a report completed after each federal election that gives the number of votes received by each candidate in each race for the House, the Senate, and the presidency, as well as each candidate's party affiliation. Beginning in 1942, these reports also give the total number of votes received by all candidates of each party. In 1940, I create Democratic two-party vote share by adding up votes myself, and in 1950, I use the totals provided in the report. For races in which a candidate received votes as both a representative of one of the two major parties (Democratic and Republican) and as a representative of one or more minor parties, I count only those votes he received as a major-party candidate. In Minnesota in 1950, I count votes cast for members of the Democratic-Farmer-Labor (DFL) party as votes for Democrats, since the DFL is affiliated with the national Democratic party. I construct a Democratic two-party vote share measure based only on House races and one that includes

---

[34]Where variable coding procedures change over time (e.g. industry and occupation variables), IPUMS creates and provides standardized versions in which responses are recoded using a single coding procedure for all years. I use these standardized variables.

votes cast in House, Senate, and presidential races.

I do not create Senate-only or President-only measures because about one-third of states do not elect a Senator in each federal election, and a President is elected in every other federal election, so these measures would be undefined for certain state-years. Since terms in the House of Representatives are two years long, the House-only measure is defined for every state in every year in which a federal election was held. As an indicator of state-level preferences over political parties, the House-only measure may be preferred, since it is, in general, created by aggregating many local election results and therefore less susceptible to being influenced in a major way by a single, particularly good or particularly bad candidate winning or losing a statewide race by a wide margin, with vote totals from that race overwhelming results from more evenly contested elections. However, since elections for Senate seats and the presidency can have important economic consequences, and the federal policies that are proposed and enacted depend very heavily on the officials in these offices, the all-races measure may be more accurately representative of preferences over policies and the superior measure to include in regression analysis. The estimated effects of RTW laws on wages do not vary substantially depending on which of these measures is used.

Observations with missing wage values or missing values for any of the individual-level covariates I use are excluded from my analysis. In all wage analyses, regressions are weighted according to the product of each respondent's provided sample weight and the number of hours that person worked the previous week, following the common practice in the wage inequality literature (see DiNardo, Fortin, and Lemieux [1996]; DiNardo and Lemieux [1997]; and Fortin and Lemieux [1997]). Employment regressions use the provided sample weights without adjustment.

References

Acemoglu, Daron, David H. Autor, and David Lyle. 2004. Women, War, and Wages: The Effect of Female Labor Supply on the Wage Structure at Midcentury. *Journal of Political Economy* 112, no. 3: 497-551.

Autor, David, Lawrence F. Katz, and Melissa Kearney. 2008. Trends in U.S. Wage Inequality: Revising the Revisionists. *Review of Economics and Statistics* 90, no. 2: 300-323.

Barro, Robert J. and Xavier Sala-i-Martin. 1991. Convergence across States and Regions. *Brookings Papers on Economic Activity* 1991, no. 1: 107-182.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. How Much Should We Trust Differneces-in-Differences Estimates? *Quarterly Journal of Economics* 119, no. 1: 249-275.

Blanchard, Olivier Jean and Lawrence F. Katz. 1992. Regional Evolutions. *Brookings Papers on Economic Activity* 1992, no. 1: 1-75.

Card, David. 1992. The Effect of Unions on the Distribution of Wages: Redistribution or Relabelling? Working Paper no. 4195, National Bureau of Economic Research, Cambridge, MA.

Card, David, Thomas Lemieux and W. Craig Riddell. 2003. Unionization and Wage Inequality: A Comparative Study of the U.S., the U.K., and Canada. Working Paper no. 9473, National Bureau of Economic Research, Cambridge, MA.

DiNardo, John, Nicole M. Fortin and Thomas Lemieux. 1996. Labor Market Institutions and the Distribution of Wages, 1973-1992: A Semiparametric Approach. *Econometrica* 64, no. 5: 1001-1044.

DiNardo, John and Thomas Lemieux. 1997. Diverging Male Wage Inequality in the United States and Canada, 1981-1988: Do Institutions Explain the Difference? *Industrial and Labor Relations Review* 50, no. 4: 629-651.

Ellwood, David T. and Glenn Fine. 1987. The Impact of Right-to-Work Laws on Union Organizing. *Journal of Political Economy* 95, no. 2: 250-273.

Farber, Henry S. 1984. Right-to-Work Laws and the Extent of Unionization. *Journal of Labor Economics* 2, no. 3: 319-352.

——. 2005. Nonunion Wage Rates and the Threat of Unionization. *Industrial and Labor Relations Review* 58, no. 3: 335-353.

Fortin, Nicole M. and Thomas Lemieux. 1997. Institutional Chnages and Rising Wage Inequality: Is there a Linkage? *Journal of Economic Perspectives* 11, no. 2: 75-96.

Frandsen, Brigham R. 2012. Why Unions Still Matter: The Effects of Unionization on the Distribution of Employee Earnings. Working Paper, Massachusetts Institute of Technology, Cambridge, MA. Updated 30 January 2012. http://economics.mit.edu/files/6950

Gall, Gilbert J. 1988. *The Politics of Right to Work: The Labor Federations as Special Interests, 1943-1979.* Westport, Connecticut: Greenwood Press, Inc.

Holmes, Thomas J. 1998. The Effect of State Policies on the Location of Manufacturing: Evidence from State Borders. *Journal of Political Economy* 106, no. 4: 667-705.

Lemieux, Thomas. 1998. Estimating the Effects of Unions on Wage Inequality in a Panel Data

Model with Comparative Advantage and Nonrandom Selection. *Journal of Labor Economics* 16, no. 2: 261-291.

Metzgar, Jack. 2009. The 1945-1946 Striek Wave. In *The Encyclopedia of Strikes in American Histroy*, ed. Aaron Brenner, Benjamin Day, and Immanuel Ness. New York: M.E. Sharpe, Inc.

Moore, William J. 1980. Membership and Wage Impact of Right-to-Work Laws. *Journal of Labor Research* 1, no. 2: 349-368.

National Right to Work Legal Defense Foundation, Inc. Right to Work States. http://www.nrtw.org/rtws.htm (accessed February 29, 2012).

Office of the Clerk, United States House of Representatives. Statistics of the Congressional Election of November 7, 1950. http://clerk.house.gov/member_info/electionInfo/1950election.pdf (accessed July 25, 2012).

Office of the Clerk, United States House of Representatives. Statistics of the Presidential and Congressional Election of November 5, 1940. http://clerk.house.gov/member_info/electionInfo/1940election.pdf (accessed July 25, 2012).

Office of the Clerk, United States House of Representatives. Statistics of the Presidential and Congressional Election of November 8, 1960. http://clerk.house.gov/member_info/electionInfo/1960election.pdf (accessed July 25, 2012).

Reed, W. Robert. 2003. How Right to Work Laws Affect Wages. *Journal of Labor Research* 24, no. 4: 713-730.

Steven Ruggles, J. Trent Alexander, Katie Genadek, Ronald Goeken, Matthew B. Schroeder, and Matthew Sobek. Integrated Public Use Microdata Series: Version 5.0 [Machine-readable database]. Minneapolis: University of Minnesota, 2010.

Tauberer, Joshua. HR 3020 Passage – GovTrack.us. http://www.govtrack.us/congress/votes/80-1947/h27 (accessed July 14, 2012).

Troy, Leo and Neil Sheflin. *1985. U.S. Union Sourcebook: Membership, Finances, Structure, Directory.* 1st ed. West Orange, New Jersey: Industrial Relations Data and Information Services.

U.S. Government Printing Office. Legislative History of the Labor Management Relations Act, 1947. http://hdl.handle.net/2027/mdp.39015077922303 (accessed 21 July 2012).

Table 1: Simple Difference-in-Differences Estimates and Covariate Means for Treatment and Control Groups, by Year

	1940			1950			1950-60 Pooled			Difference-in-Differences (1940-50)			Difference-in-Differences (1940-60)		
	Treatment	Control	Difference	Treatment	Control	Difference	Treatment	Control	Difference	ΔTreatment	ΔControl	Difference	ΔTreatment	ΔControl	Difference
Age	33.4043 (0.1698)	35.0056 (0.0907)	-1.6013 (0.1925)	34.6338 (0.1634)	35.9083 (0.1291)	-1.2745 (0.2082)	35.1822 (0.1061)	36.1898 (0.1072)	-1.0077 (0.1508)	1.2295 (0.2356)	0.9028 (0.1578)	0.3268 (0.2836)	1.7778 (0.2002)	1.1842 (0.1404)	0.5936 (0.2446)
Less than High School	0.7093 (0.0191)	0.6755 (0.0139)	0.0338 (0.0236)	0.6467 (0.0209)	0.5665 (0.0135)	0.0802 (0.0249)	0.5960 (0.0193)	0.5185 (0.0131)	0.0776 (0.0234)	-0.0625 (0.0283)	-0.1090 (0.0194)	0.0465 (0.0343)	-0.1132 (0.0272)	-0.1570 (0.0191)	0.0438 (0.0332)
High School	0.1709 (0.0125)	0.2045 (0.0088)	-0.0336 (0.0153)	0.1876 (0.0145)	0.2570 (0.0068)	-0.0694 (0.0160)	0.2123 (0.0119)	0.2702 (0.0057)	-0.0579 (0.0132)	0.0167 (0.0191)	0.0524 (0.0112)	-0.0358 (0.0221)	0.0413 (0.0173)	0.0656 (0.0105)	-0.0243 (0.0202)
Some College	0.0670 (0.0055)	0.0619 (0.0048)	0.0051 (0.0073)	0.0965 (0.0068)	0.0985 (0.0077)	-0.0020 (0.0103)	0.1081 (0.0080)	0.1157 (0.0092)	-0.0076 (0.0122)	0.0294 (0.0087)	0.0365 (0.0091)	-0.0071 (0.0126)	0.0411 (0.0097)	0.0537 (0.0104)	-0.0127 (0.0142)
College	0.0528 (0.0028)	0.0580 (0.0023)	-0.0052 (0.0037)	0.0692 (0.0035)	0.0780 (0.0025)	-0.0088 (0.0043)	0.0836 (0.0042)	0.0957 (0.0033)	-0.0121 (0.0054)	0.0164 (0.0045)	0.0200 (0.0034)	-0.0036 (0.0056)	0.0308 (0.0051)	0.0377 (0.0041)	-0.0068 (0.0065)
White	0.8184 (0.0279)	0.9511 (0.0076)	-0.1327 (0.0289)	0.8447 (0.0235)	0.9285 (0.0071)	-0.0837 (0.0246)	0.8537 (0.0208)	0.9285 (0.0069)	-0.0748 (0.0219)	0.0264 (0.0365)	-0.0226 (0.0104)	0.0490 (0.0379)	0.0354 (0.0348)	-0.0225 (0.0102)	0.0579 (0.0363)
Black	0.1798 (0.0279)	0.0450 (0.0079)	0.1348 (0.0290)	0.1533 (0.0235)	0.0656 (0.0074)	0.0878 (0.0246)	0.1438 (0.0209)	0.0653 (0.0073)	0.0785 (0.0221)	-0.0265 (0.0365)	0.0205 (0.0108)	-0.0470 (0.0381)	-0.0360 (0.0349)	0.0203 (0.0107)	-0.0563 (0.0365)
Native American	0.0013 (0.0007)	0.0008 (0.0003)	0.0006 (0.0008)	0.0016 (0.0012)	0.0009 (0.0004)	0.0007 (0.0012)	0.0020 (0.0012)	0.0012 (0.0004)	0.0008 (0.0012)	0.0003 (0.0014)	0.0001 (0.0005)	0.0001 (0.0014)	0.0006 (0.0014)	0.0004 (0.0005)	0.0002 (0.0015)
Asian/Pacific Islander	0.0005 (0.0002)	0.0032 (0.0020)	-0.0027 (0.0020)	0.0002 (0.0001)	0.0051 (0.0029)	-0.0049 (0.0029)	0.0004 (0.0001)	0.0048 (0.0027)	-0.0044 (0.0027)	-0.0003 (0.0002)	0.0019 (0.0035)	-0.0023 (0.0035)	-0.0001 (0.0002)	0.0017 (0.0033)	-0.0018 (0.0033)
Other Race	0.0000 (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)	0.0002 (0.0002)	0.0000 (0.0000)	0.0002 (0.0002)	0.0002 (0.0001)	0.0002 (0.0000)	0.0000 (0.0001)	0.0002 (0.0002)	0.0000 (0.0000)	0.0002 (0.0002)	0.0002 (0.0001)	0.0002 (0.0000)	0.0000 (0.0001)
Married	0.7238 (0.0063)	0.7029 (0.0065)	0.0208 (0.0090)	0.7672 (0.0063)	0.7375 (0.0080)	0.0297 (0.0102)	0.8021 (0.0044)	0.7758 (0.0065)	0.0263 (0.0079)	0.0435 (0.0089)	0.0346 (0.0103)	0.0089 (0.0136)	0.0783 (0.0077)	0.0728 (0.0092)	0.0055 (0.0120)
Student	0.0087 (0.0006)	0.0128 (0.0009)	-0.0041 (0.0011)	0.0341 (0.0024)	0.0353 (0.0015)	-0.0013 (0.0029)	0.0373 (0.0024)	0.0397 (0.0020)	-0.0024 (0.0032)	0.0254 (0.0025)	0.0225 (0.0018)	0.0029 (0.0031)	0.0286 (0.0025)	0.0269 (0.0022)	0.0017 (0.0034)
Number of Children	1.1246 (0.0325)	1.0522 (0.0391)	0.0724 (0.0508)	0.8089 (0.0247)	0.7627 (0.0225)	0.0462 (0.0335)	1.2237 (0.0145)	1.1615 (0.0252)	0.0623 (0.0291)	-0.3156 (0.0408)	-0.2895 (0.0451)	-0.0262 (0.0608)	0.0092 (0.0356)	0.1093 (0.0465)	-0.0101 (0.0585)
Number of Children under 5	0.3181 (0.0082)	0.2604 (0.0102)	0.0577 (0.0131)	0.3231 (0.0093)	0.2922 (0.0085)	0.0309 (0.0126)	0.4290 (0.0059)	0.4018 (0.0092)	0.0272 (0.0109)	0.0050 (0.0124)	0.0319 (0.0133)	-0.0268 (0.0182)	0.1109 (0.0101)	0.1415 (0.0138)	-0.0305 (0.0171)
Union Organization %	11.9401 (1.2636)	24.6315 (1.2005)	-12.6914 (1.7430)	16.4031 (1.3183)	33.7328 (1.1286)	-17.3296 (1.7354)	15.4370 (1.4181)	33.4327 (1.3491)	-17.9956 (1.9573)	4.4630 (1.8261)	9.1013 (1.6477)	-4.6382 (2.4596)	3.4969 (1.8994)	8.8011 (1.8059)	-5.3042 (2.6208)
Democratic Vote Share	77.9289 (4.9372)	52.3801 (1.3116)	25.5488 (5.1085)	77.4376 (5.6357)	48.3970 (1.2840)	29.0406 (5.7801)	69.6300 (4.0372)	49.9105 (0.9491)	19.7195 (4.1472)	-0.4913 (7.4925)	-3.9831 (1.8355)	3.4918 (7.7140)	-8.2989 (6.3777)	-2.4696 (1.6190)	-5.8293 (6.5800)
Unemployment Rate	6.5205 (0.4151)	10.4628 (0.6816)	-3.9422 (0.7981)	2.7859 (0.2019)	4.2687 (0.3840)	-1.4828 (0.4339)	3.4425 (0.1535)	4.6233 (0.2567)	-1.1808 (0.2991)	-3.7346 (0.4616)	-6.1941 (0.7824)	2.4595 (0.9084)	-3.0780 (0.4426)	-5.8395 (0.7284)	2.7615 (0.8523)
N	32,848	134,457		12,966	47,973		65,492	238,065							

Note: Sample includes men, ages 18-54, and has been cleaned for wage analysis as described in the text. Estimates use provided sample weights, which are scaled to sum to one within each year. Standard errors are clustered at the state level. Differences within group over time are taken by subtracting the value for the earlier period from the value for the later period (i.e. 1950/60-1940). Differences across groups are taken by subtracting the value for the control group from the value for the treatment group (i.e. treatment - control).

Tables

Table 2: Unadjusted Difference-in-Differences Estimates of the Effects of RTW Laws on Wages and Employment Outcomes

Outcome Levels and Treatment-Control Differences

	1940			1950			1950-60 Pooled		
	Treatment	Control	Difference	Treatment	Control	Difference	Treatment	Control	Difference
log(wage)	-0.3096	0.1279	-0.4375	0.1780	0.4553	-0.2772	0.3754	0.6449	-0.2695
	(0.0262)	(0.0251)	(0.0363)	(0.0252)	(0.0177)	(0.0308)	(0.0226)	(0.0182)	(0.0290)
N	32,848	134,457		12,966	47,973		65,492	238,065	
Employment/Population Ratio	0.8643	0.8220	0.0423	0.8847	0.8666	0.0181	0.8829	0.8744	0.0085
	(0.0040)	(0.0066)	(0.0077)	(0.0042)	(0.0047)	(0.0063)	(0.0038)	(0.0037)	(0.0053)
N	73,439	252,823		98,661	309,539		184,192	582,710	
Labor Force Participation Rate	0.9212	0.9195	0.0017	0.9084	0.9062	0.0022	0.9116	0.9158	-0.0042
	(0.0017)	(0.0019)	(0.0026)	(0.0030)	(0.0031)	(0.0043)	(0.0033)	(0.0028)	(0.0043)
N	73,439	252,823		98,661	309,539		184,192	582,710	
Employment Rate	0.9382	0.8940	0.0442	0.9739	0.9563	0.0176	0.9685	0.9548	0.0137
	(0.0047)	(0.0071)	(0.0085)	(0.0022)	(0.0032)	(0.0039)	(0.0017)	(0.0022)	(0.0028)
N	67,644	232,429		90,905	283,846		169,147	536,661	

Unadjusted Difference-in-Differences Estimates

	Difference-in-Differences, 1940-50			Difference-in-Differences, 1940-60		
	ΔTreatment	ΔControl	Difference	ΔTreatment	ΔControl	Difference
log(wage)	0.4877	0.3274	0.1602	0.6850	0.5170	0.1680
	(0.0363)	(0.0307)	(0.0476)	(0.0346)	(0.0310)	(0.0464)
Employment/Population Ratio	0.0204	0.0446	-0.0242	0.0186	0.0524	-0.0338
	(0.0058)	(0.0081)	(0.0099)	(0.0055)	(0.0075)	(0.0093)
Labor Force Participation Rate	-0.0128	-0.0133	0.0005	-0.0096	-0.0037	-0.0059
	(0.0035)	(0.0036)	(0.0050)	(0.0037)	(0.0034)	(0.0050)
Employment Rate	0.0357	0.0623	-0.0266	0.0303	0.0608	-0.0305
	(0.0052)	(0.0078)	(0.0093)	(0.0050)	(0.0074)	(0.0090)

Note: Sample includes men, ages 18-54, and has been cleaned for analysis as described in the text. The size differs across outcomes because fewer respondents report all the information required to impute hourly wages than report employment status. Wage estimates use hours-adjusted sample weights, and employment estimates use unadjusted sample weights. In both cases, weights are scaled to sum to one within each year. Standard errors are clustered at the state level. Differences within group over time are taken by subtracting the value for the earlier period from the value for the later period (i.e. 1950/60-1940). Differences across groups are taken by subtracting the value for the control group from the value for the treatment group (i.e. treatment - control).

Table 3: Effects of RTW Laws on Wages, by Education Group (Main Estimates)

	All	Less Than High School	High School	Some College	College
1940-50, MSA Controls					
RTW*After	0.0438	0.0473	0.0284	0.0495	0.0146
	(0.0156)	(0.0168)	(0.0136)	(0.0341)	(0.0458)
RTW	-0.0883	-0.0500	-0.2186	-0.1279	-0.1269
	(0.0097)	(0.0105)	(0.0061)	(0.0171)	(0.0284)
After	0.2332	0.2753	0.1918	0.1295	0.1653
	(0.0128)	(0.0184)	(0.0087)	(0.0197)	(0.0410)
N	228244	150922	47813	15901	13608
1940-50, Metro Status Controls					
RTW*After	0.0415	0.0470	0.0213	0.0373	0.0030
	(0.0161)	(0.0173)	(0.0140)	(0.0319)	(0.0460)
RTW	-0.2053	-0.1880	-0.1909	0.0403	-0.1152
	(0.0138)	(0.0153)	(0.0122)	(0.0200)	(0.0296)
After	0.2295	0.2690	0.1926	0.1316	0.1594
	(0.0132)	(0.0196)	(0.0091)	(0.0170)	(0.0425)
N	228244	150922	47813	15901	13608
1940-60, Metro Status Controls					
RTW*After	0.0329	0.0366	0.0146	0.0122	-0.0090
	(0.0195)	(0.0208)	(0.0185)	(0.0272)	(0.0314)
RTW	-0.1467	-0.1652	-0.2253	-0.1342	-0.0658
	(0.0152)	(0.0147)	(0.0224)	(0.0168)	(0.0312)
After	0.2308	0.2682	0.1946	0.1317	0.1599
	(0.0132)	(0.0197)	(0.0084)	(0.0170)	(0.0421)
N	470862	269004	114106	47451	40301

Notes: All regressions are estimated using the set of individual-level covariates listed in Table 1 (not including unemployment rate, union coverage, or Democratic vote share, which are state-level covariates), as well as census division, state, MSA/metropolitan status, and census division-year fixed effects. Regressions that use 1940-60 data also include year fixed effects. Standard errors are clustered at the state level. Regressions use hours-adjusted sample weights, which are scaled to sum to one within each year. Columns vary the education groups included in the sample. Panels vary the years and metropolitan area/status controls included in regressions.

Table 4: DDD Estimates of the Effects of RTW Laws on Wages (Main Estimates)

Comparison group	Non-highly unionized				Lightly unionized			
Years	1940-50	1940-50	1940-60	1940-60	1940-50	1940-50	1940-60	1940-60
MSA controls	Yes	No	No	No	Yes	No	No	No
Metro status controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
State-year dummies	No	No	No	No	No	No	No	No
WWII control	No	No	No	Yes	No	No	No	Yes
RTW*After*Highly unionized	-0.0472	-0.0510	-0.0529	-0.0519	-0.0862	-0.0932	-0.0857	-0.0839
	(0.0253)	(0.0249)	(0.0292)	(0.0291)	(0.0297)	(0.0295)	(0.0301)	(0.0301)
N	228244	228244	470862	470862	148390	148390	310859	310859

Comparison group	Non-highly unionized				Lightly unionized			
Years	1940-50	1940-50	1940-60	1940-60	1940-50	1940-50	1940-60	1940-60
MSA controls	Yes	No	No	No	Yes	No	No	No
Metro status controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
State-year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
WWII control	No	No	No	Yes	No	No	No	Yes
RTW*After*Highly unionized	-0.0483	-0.0520	-0.0552	-0.0552	-0.0876	-0.0952	-0.0881	-0.0881
	(0.0252)	(0.0248)	(0.0287)	(0.0287)	(0.0299)	(0.0298)	(0.0303)	(0.0303)
N	228244	228244	470862	470862	148390	148390	310859	310859

Note: See note for Table 3.

Table 5: Effects of RTW Laws on Employment and Labor Force Participation, by Education Group (Main Estimates)

Outcome	All			Less Than High School			High School			Some College			College		
	EPOP	ER	LFP	EPOP	ER	LFP	EPOP	ER	LFP	EPOP	ER	LFP	EPOP	ER	LFP
1940-50, MSA Controls															
RTW*After	-0.0103	-0.0198	0.0077	-0.0152	-0.0220	0.0045	-0.0054	-0.0239	0.0175	0.0248	-0.0143	0.0361	-0.0210	-0.0034	-0.0181
	(0.0075)	(0.0057)	(0.0037)	(0.0089)	(0.0069)	(0.0040)	(0.0083)	(0.0064)	(0.0062)	(0.0147)	(0.0070)	(0.0164)	(0.0147)	(0.0066)	(0.0131)
N	734462	674824	734462	598601	551795	598601	78959	74323	78959	31594	25113	31594	25308	23593	25308
Outcome Mean	0.8511	0.9321	0.9133	0.8524	0.9270	0.9197	0.8787	0.9374	0.9374	0.7160	0.9537	0.7516	0.8924	0.9752	0.9153
1940-50, Metro Status Controls															
RTW*After	-0.0099	-0.0190	0.0076	-0.0155	-0.0213	0.0037	-0.0001	-0.0210	0.0206	0.0243	-0.0140	0.0354	-0.0206	-0.0012	-0.0200
	(0.0080)	(0.0060)	(0.0039)	(0.0094)	(0.0073)	(0.0041)	(0.0085)	(0.0066)	(0.0063)	(0.0135)	(0.0068)	(0.0154)	(0.0149)	(0.0065)	(0.0134)
N	734462	674824	734462	598601	551795	598601	78959	74323	78959	31594	25113	31594	25308	23593	25308
Outcome Mean	0.8511	0.9321	0.9133	0.8524	0.9270	0.9197	0.8787	0.9374	0.9374	0.7160	0.9537	0.7516	0.8924	0.9752	0.9153
1940-60, Metro Status Controls															
RTW*After	-0.0109	-0.0158	0.0035	-0.0089	-0.0145	0.0046	-0.0072	-0.0206	0.0126	0.0097	-0.0089	0.0164	-0.0192	-0.0014	-0.0183
	(0.0078)	(0.0057)	(0.0040)	(0.0082)	(0.0067)	(0.0037)	(0.0069)	(0.0061)	(0.0036)	(0.0105)	(0.0053)	(0.0106)	(0.0096)	(0.0049)	(0.0079)
N	1090747	1003623	1090747	776453	713291	776453	170502	163060	170502	80749	67361	80749	63043	59911	63043
Outcome Mean	0.8614	0.9400	0.9165	0.8518	0.9303	0.9158	0.8986	0.9476	0.9481	0.7559	0.9599	0.7876	0.9124	0.9800	0.9310

Note: Estimates are derived from a linear probability model. See note for Table 3. Outcomes are abbreviated as follows: employment/population ratio (EPOP), employment rate (ER), labor force participation rate (LFP). Panels vary the years and metropolitan area/status controls included in regressions. Sets of columns vary the education groups included in the sample.

Table 6: Effects of RTW Laws on Employment in Highly and Lightly Unionized Industries, by Skill Level (Main Estimates)

Employment Outcome:	Employed in Highly Unionized Industry				Employed in Lightly Unionized Industry			
Skill Level:	All	Low	Middle	High	All	Low	Middle	High
1940-50, MSA Controls								
RTW*After	0.0006	0.0054	-0.0011	0.0006	-0.0053	-0.0185	0.0229	-0.0073
	(0.0063)	(0.0081)	(0.0107)	(0.0108)	(0.0080)	(0.0087)	(0.0167)	(0.0261)
N	734462	598601	110553	25308	734462	598601	110553	25308
Outcome Mean	0.3276	0.3501	0.2651	0.1739	0.3012	0.3017	0.2691	0.4734
1940-50, Metro Status Controls								
RTW*After	0.0021	0.0068	-0.0022	0.0090	-0.0047	-0.0192	0.0352	-0.0134
	(0.0065)	(0.0081)	(0.0122)	(0.0116)	(0.0091)	(0.0097)	(0.0201)	(0.0257)
N	734462	598601	110553	25308	734462	598601	110553	25308
Outcome Mean	0.3276	0.3501	0.2651	0.1739	0.3012	0.3017	0.2691	0.4734
1940-60, Metro Status Controls								
RTW*After	-0.0023	-0.0040	-0.0075	-0.0053	0.0003	-0.0041	0.0304	0.0023
	(0.0074)	(0.0086)	(0.0103)	(0.0108)	(0.0115)	(0.0116)	(0.0163)	(0.0187)
N	1090747	776453	251251	63043	1090747	776453	251251	63043
Outcome Mean	0.3443	0.3756	0.2967	0.1873	0.2998	0.2855	0.2779	0.4857

Note: Estimates are derived from a linear probability model. See note for Table 3.

Table 7: Effects of RTW Laws on Wages, by Education Group (Labor Market Controls Estimates)

	All	Less Than High School	High School	Some College	College
1940-50, MSA Controls					
RTW*After	0.0130	0.0009	0.0378	0.0377	-0.0023
	(0.0174)	(0.0141)	(0.0173)	(0.0411)	(0.0576)
RTW	-0.2164	-0.1422	0.0224	-0.1470	-0.0698
	(0.0641)	(0.0313)	(0.0485)	(0.1263)	(0.1178)
After	0.3276	0.4012	0.1686	0.2600	0.3016
	(0.0434)	(0.0466)	(0.0379)	(0.1016)	(0.1266)
N	228244	150922	47813	15901	13608
1940-50, Metro Status Controls					
RTW*After	0.0080	-0.0011	0.0312	0.0185	-0.0189
	(0.0187)	(0.0143)	(0.0185)	(0.0403)	(0.0590)
RTW	-0.1083	-0.2218	0.0298	-0.0165	-0.0550
	(0.0697)	(0.0341)	(0.0342)	(0.0707)	(0.1111)
After	0.3371	0.4090	0.1728	0.2758	0.3030
	(0.0441)	(0.0459)	(0.0388)	(0.0954)	(0.1304)
N	228244	150922	47813	15901	13608
1940-60, Metro Status Controls					
RTW*After	0.0489	0.0521	0.0356	0.0375	0.0188
	(0.0186)	(0.0203)	(0.0167)	(0.0273)	(0.0360)
RTW	-0.4058	-0.4470	-0.2748	-0.0065	-0.3305
	(0.0371)	(0.0526)	(0.0262)	(0.0289)	(0.0806)
After	0.1610	0.1812	0.1216	0.1295	0.1199
	(0.0275)	(0.0354)	(0.0243)	(0.0309)	(0.0583)
N	470862	269004	114106	47451	40301

Notes: All regressions are estimated using the set of covariates listed in Table 1, including unemployment rate, union coverage, or Democratic vote share, as well as census division, state, MSA/metropolitan status, census division-year, and single-digit industry and occupation fixed effects. Regressions that use 1940-60 data also include year fixed effects. Standard errors are clustered at the state level. Regressions use hours-adjusted sample weights, which are scaled to sum to one within each year. Columns vary the education groups included in the sample. Panels vary the years and metropolitan area/status controls included in regressions.

Table 8: Robustness of Wage Results to Inclusion of Various Covariates and Estimation Using Different Control Groups, Short Run Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
States included in control group:	Not RTW by 1960	Never RTW	Not RTW by 1960, not bordering a RTW state	Not RTW by 1960, does border a RTW state	Not RTW by 1960, not among hardest hit by strikes	Not RTW by 1960, not hard hit by strikes	Most similar 1940 wages, not RTW by 1960	Not RTW by 1960	Never RTW	Not RTW by 1960, not bordering a RTW state	Not RTW by 1960, does border a RTW state	Not RTW by 1960, not among hardest hit by strikes	Not RTW by 1960, not hard hit by strikes	Most similar 1940 wages, not RTW by 1960
Specification:	Main	Main	Main	Main	Main	Main	Main	LMC	LMC	LMC	LMC	LMC	LMC	LMC
Outcome: log(wage)														
A. Full Sample														
RTW*After	0.0438	0.0538	0.0040	0.0444	0.0427	0.0385	0.0348	0.0130	0.0313	-0.0279	0.0072	-0.0025	-0.0010	-0.0155
	(0.0156)	(0.0168)	(0.0226)	(0.0156)	(0.0155)	(0.0156)	(0.0162)	(0.0174)	(0.0172)	(0.0126)	(0.0190)	(0.0175)	(0.0207)	(0.0143)
N	228244	208386	162171	131745	136562	89147	102062	228244	219204	161624	120476	136591	89625	102062
B. Less Than High School														
RTW*After	0.0473	0.0544	0.0089	0.0488	0.0462	0.0386	0.0424	0.0009	0.0113	-0.0219	-0.0008	-0.0140	-0.0056	-0.0126
	(0.0168)	(0.0199)	(0.0238)	(0.0167)	(0.0168)	(0.0167)	(0.0165)	(0.0141)	(0.0154)	(0.0180)	(0.0143)	(0.0148)	(0.0178)	(0.0190)
N	150922	137666	109337	86998	89737	60511	69130	150922	144958	109217	80001	90255	61421	69130
C. High School														
RTW*After	0.0284	0.0397	-0.0018	0.0265	0.0270	0.0298	0.0280	0.0378	0.0508	-0.0034	0.0467	0.0325	0.0358	0.0229
	(0.0136)	(0.0165)	(0.0095)	(0.0137)	(0.0136)	(0.0133)	(0.0136)	(0.0173)	(0.0192)	(0.0098)	(0.0212)	(0.0170)	(0.0174)	(0.0120)
N	47813	43724	33058	26722	28193	16709	19854	47813	46182	32933	24088	28099	16672	19854
D. Some College														
RTW*After	0.0495	0.0357	0.0413	0.0510	0.0494	0.0569	0.0525	0.0377	0.0377	-0.0056	0.0375	0.0177	0.0168	-0.0004
	(0.0341)	(0.0465)	(0.0349)	(0.0346)	(0.0342)	(0.0357)	(0.0386)	(0.0411)	(0.0548)	(0.0415)	(0.0454)	(0.0436)	(0.0439)	(0.0451)
N	15901	14443	10353	10281	10374	6676	7242	15901	15115	10260	9419	10255	6544	7242
E. College														
RTW*After	0.0146	0.0687	-0.1142	0.0178	0.0145	0.0066	0.0009	-0.0023	0.0684	-0.1444	-0.0864	-0.0580	-0.0969	-0.1092
	(0.0458)	(0.0426)	(0.0596)	(0.0463)	(0.0454)	(0.0471)	(0.0577)	(0.0576)	(0.0489)	(0.0478)	(0.0701)	(0.0632)	(0.0801)	(0.0629)
N	13608	12553	9423	7744	8258	5251	5836	13608	12949	9214	6968	7982	4988	5836

Note: All regressions are estimated using the set of individual-level covariates listed in Table 1, as well as MSA, census division, state, and census division-year fixed effects. Labor Market Controls (LMC) regressions include the state unemployment rate, Democratic vote share, union organization rate, and industry and occupation fixed effects. Standard errors are clustered at the state level. Regressions use hours-adjusted sample weights, which are scaled to sum to one within each year. For columns 5, 6, 11, and 12, the states that were hit hard by strikes during the period after World War II are described in the text. The states excluded from the control group in columns 5 and 11 are Illinois, Michigan, New York, Ohio, and Pennsylvania. Those five states, as well as California, Connecticut, Massachusetts, New Jersey, West Virginia, and Wisconsin are excluded from the control group in columns 6 and 12. In columns 7 and 14, the control group includes the 20 states that had not passed RTW laws by 1960 with the smallest differences in 1940 mean wages from the pooled treatment group, computed using hours-adjusted person weights.

38

Table 9: Effects of RTW Laws on Wages when Controlling for World War II, by Education Group

	Reduced Form Estimates					Labor Market Controls Estimates				
	All	Less Than High School	High School	Some College	College	All	Less Than High School	High School	Some College	College
1940-50, MSA Controls										
Baseline	0.0438	0.0473	0.0284	0.0495	0.0146	0.0130	0.0009	0.0378	0.0377	-0.0023
	(0.0156)	(0.0168)	(0.0136)	(0.0341)	(0.0458)	(0.0174)	(0.0141)	(0.0173)	(0.0411)	(0.0576)
WWII Controls	0.0283	0.0351	0.0146	0.0517	0.0054	0.0024	-0.0051	0.0267	0.0384	-0.0310
	(0.0153)	(0.0164)	(0.0157)	(0.0370)	(0.0536)	(0.0155)	(0.0162)	(0.0146)	(0.0449)	(0.0624)
1940-50, Metro Status Controls										
Baseline	0.0415	0.0470	0.0213	0.0373	0.0030	0.0080	-0.0011	0.0312	0.0185	-0.0189
	(0.0161)	(0.0173)	(0.0140)	(0.0319)	(0.0460)	(0.0187)	(0.0143)	(0.0185)	(0.0403)	(0.0590)
WWII Controls	0.0253	0.0332	0.0075	0.0363	-0.0057	-0.0033	-0.0081	0.0206	0.0119	-0.0483
	(0.0167)	(0.0177)	(0.0164)	(0.0351)	(0.0545)	(0.0164)	(0.0163)	(0.0156)	(0.0412)	(0.0641)
1940-60, Metro Status Controls										
Baseline	0.0329	0.0366	0.0146	0.0122	-0.0090	0.0489	0.0521	0.0356	0.0375	0.0188
	(0.0195)	(0.0208)	(0.0185)	(0.0272)	(0.0314)	(0.0186)	(0.0203)	(0.0167)	(0.0273)	(0.0360)
WWII Controls	0.0244	0.0296	0.0056	0.0208	-0.0075	0.0406	0.0452	0.0317	0.0412	0.0053
	(0.0202)	(0.0226)	(0.0206)	(0.0268)	(0.0391)	(0.0210)	(0.0233)	(0.0172)	(0.0285)	(0.0423)

Note: Baseline estimates are the same as those reported in Tables 3 and 7. The WWII Controls estimates come from regressions that are identical to the ones that produced the baseline estimates with which they are paired in all respects except that they contain an additional term that is equal to the percent change in state personal income between 1940 and 1945 interacted with a dummy variable that is equal to one if the observation is from after 1947, as described in the text. Regressions use hours-adjusted sample weights, which are scaled to sum to one within each year.

Table A1: Effects of RTW Laws on Wages and Employment, Combined Middle-Skill Sample

Specification:	Main Estimates				Labor Market Controls Estimates			
Outcome:	ln(wages)	EPOP	ER	LFP	ln(wages)	EPOP	ER	LFP
1940-50, MSA Controls								
RTW*After	0.0319	0.0045	-0.0216	0.0246	0.0371	0.0151	-0.0074	0.0225
	(0.0172)	(0.0096)	(0.0047)	(0.0085)	(0.0202)	(0.0096)	(0.0053)	(0.0088)
N	63714	110553	99436	110553	63714	110553	99436	110553
1940-50, Metro Status Control								
RTW*After	0.0242	0.0075	-0.0196	0.0263	0.0267	0.0218	-0.0044	0.0273
	(0.0172)	(0.0101)	(0.0048)	(0.0088)	(0.0220)	(0.0100)	(0.0055)	(0.0098)
N	63714	110553	99436	110553	63714	110553	99436	110553
1940-60, Metro Status Controls								
RTW*After	0.0131	-0.0025	-0.0177	0.0139	0.0362	0.0113	-0.0080	0.0190
	(0.0198)	(0.0079)	(0.0046)	(0.0059)	(0.0178)	(0.0058)	(0.0041)	(0.0057)
N	161557	251251	230421	251251	161557	251251	230421	251251

Note: Estimates are based on the combined middle-skill sample, which consists of workers who completed high school or attended some college. All regressions include the individual-level covariates listed in Table 1, and the labor market controls estimates also include the state-level covariates from that table. Wage regressions use hours-adjusted sample weights, and employment regressions use unadjusted sample weights. In both cases, weights are scaled to sum to one within each year. Outcomes are abbreviated as follows: employment/population ratio (EPOP), employment rate (ER), labor force participation rate (LFP).

40

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

These states are excluded from the analysis presented here.

1960: Decennial census collects wage and salary information

Figure 2: Quantile Regression Estimates of the Effects of RTW Laws on Wages (Main Estimates)
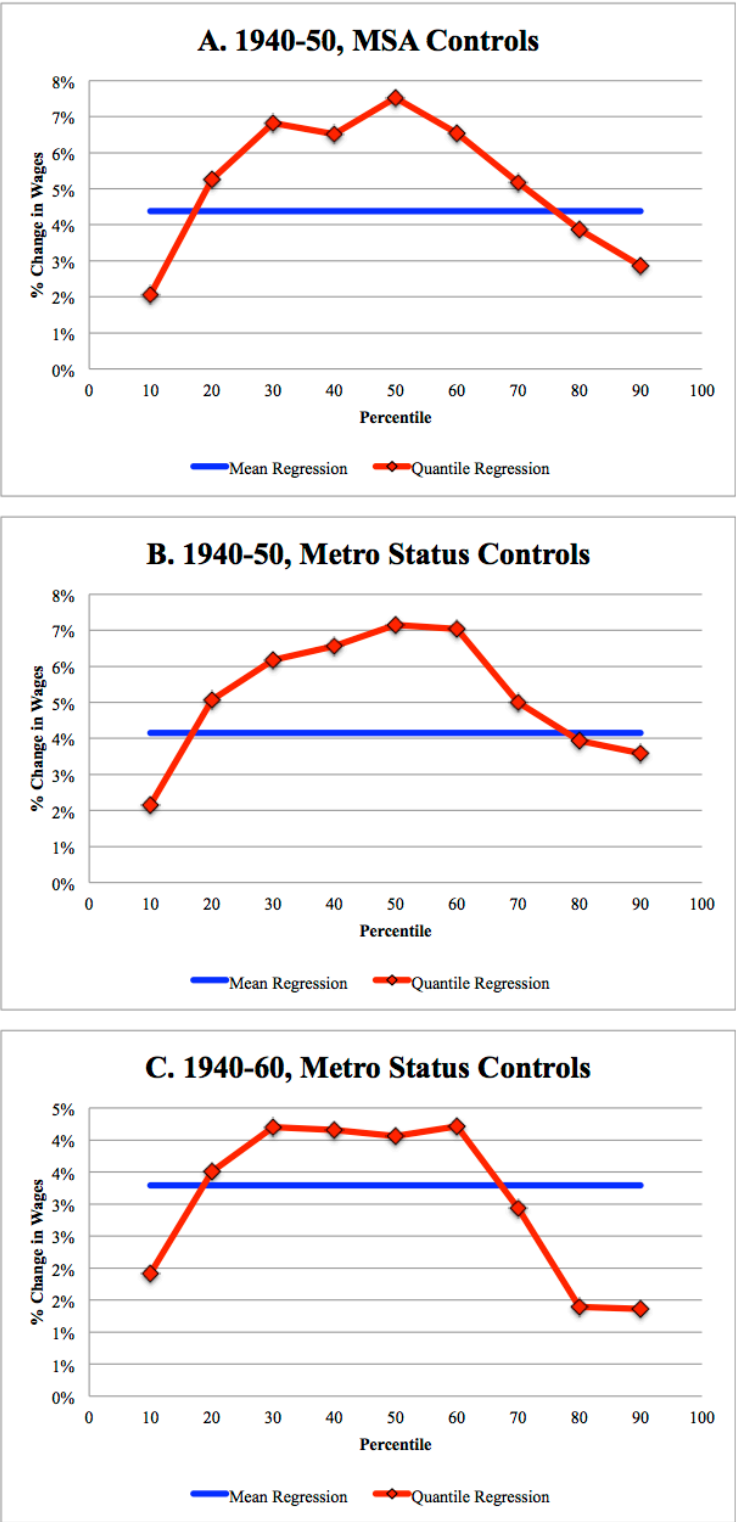
Figure 3: Quantile Regression Estimates of the Effects of RTW Laws on Wages (Labor Market Controls Estimates)
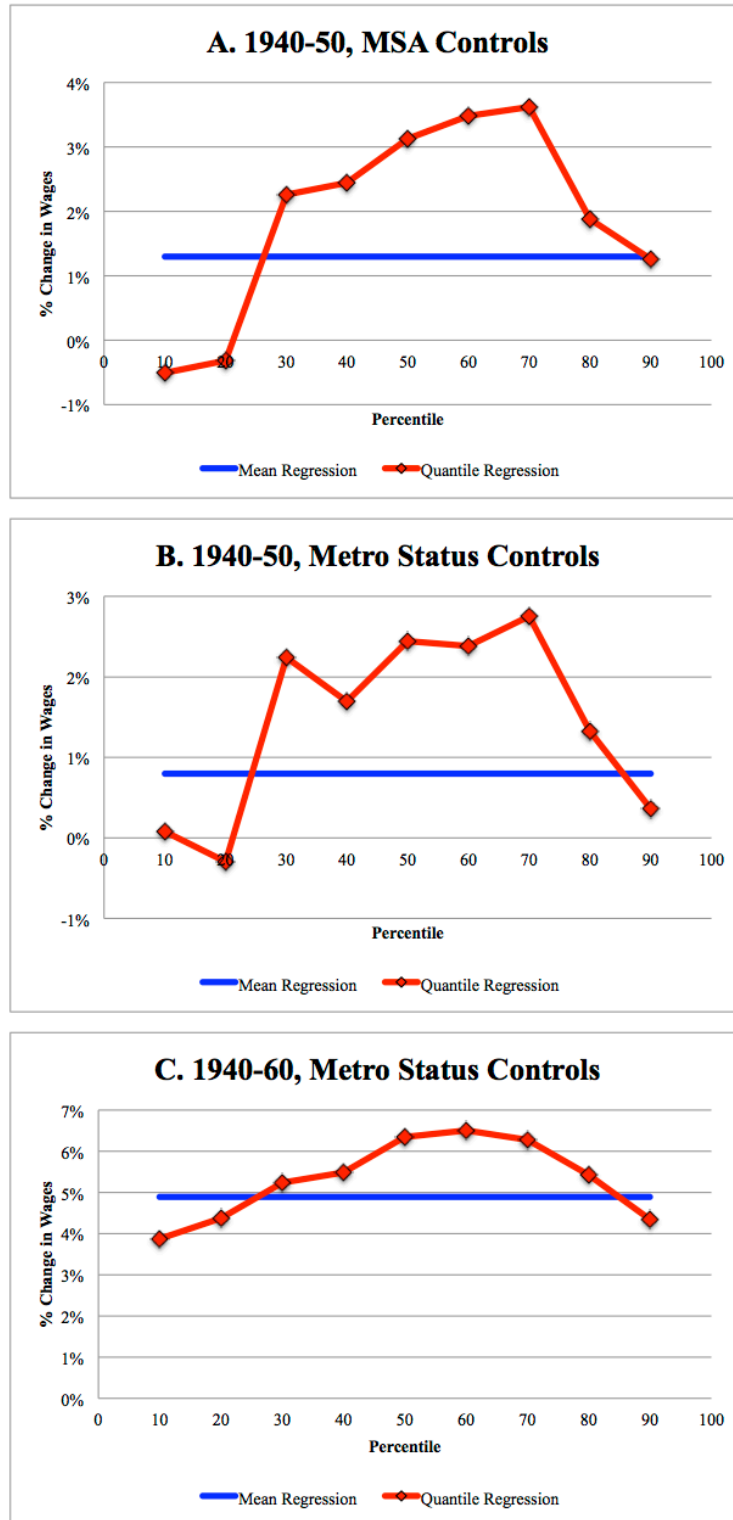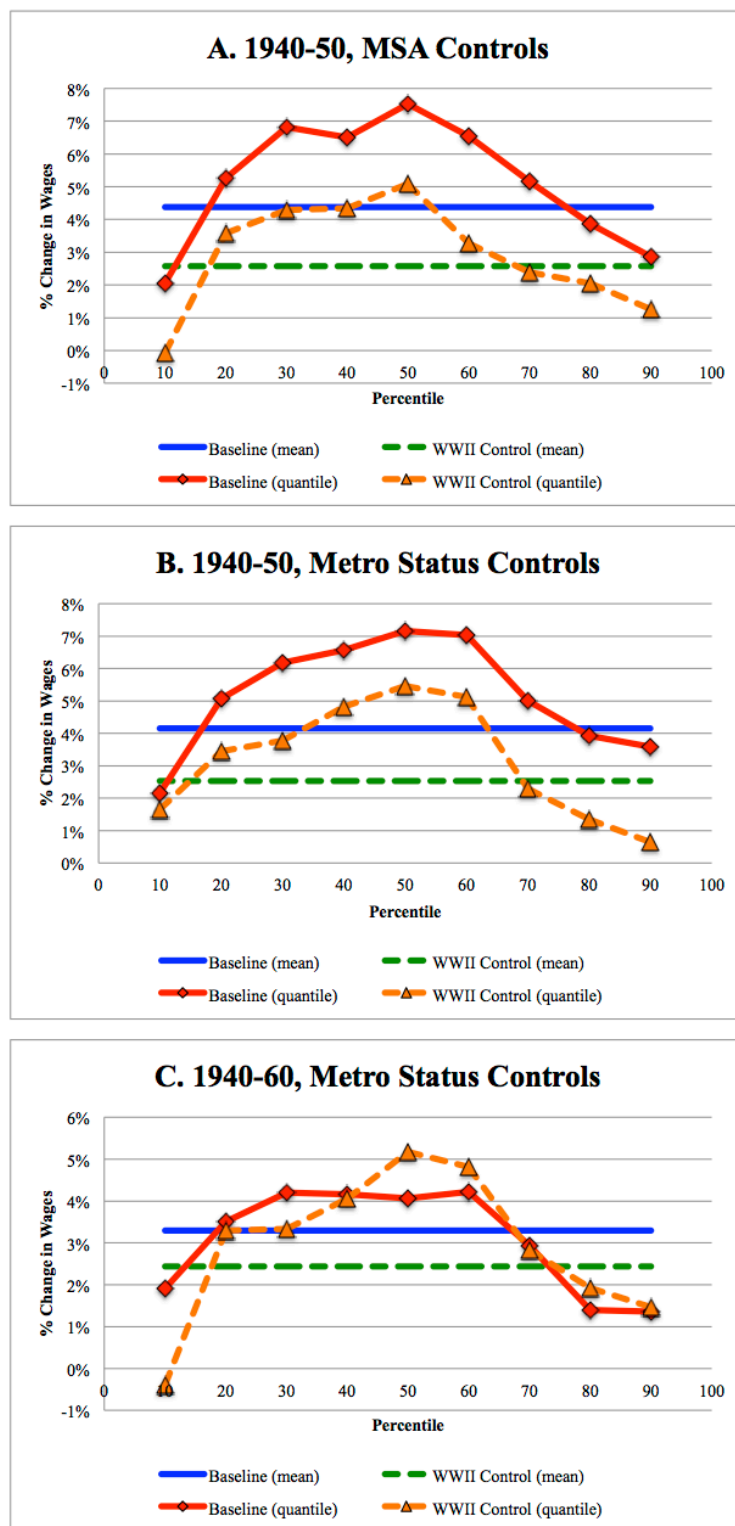
Figure 4: Trends in State Personal Income Per Capita

Note: States are weighted according to population. Vertical line indicates passage of the Taft-Hartley Act
in 1947.

Figure 5: Quantile Regression Estimates of the Effects of RTW Laws on Wages, Controlling for World War II (Main Estimates)

Figure 6: Quantile Regression Estimates of the Effects of RTW Laws on Wages, Controlling for World War II (Labor Market Controls Estimates)
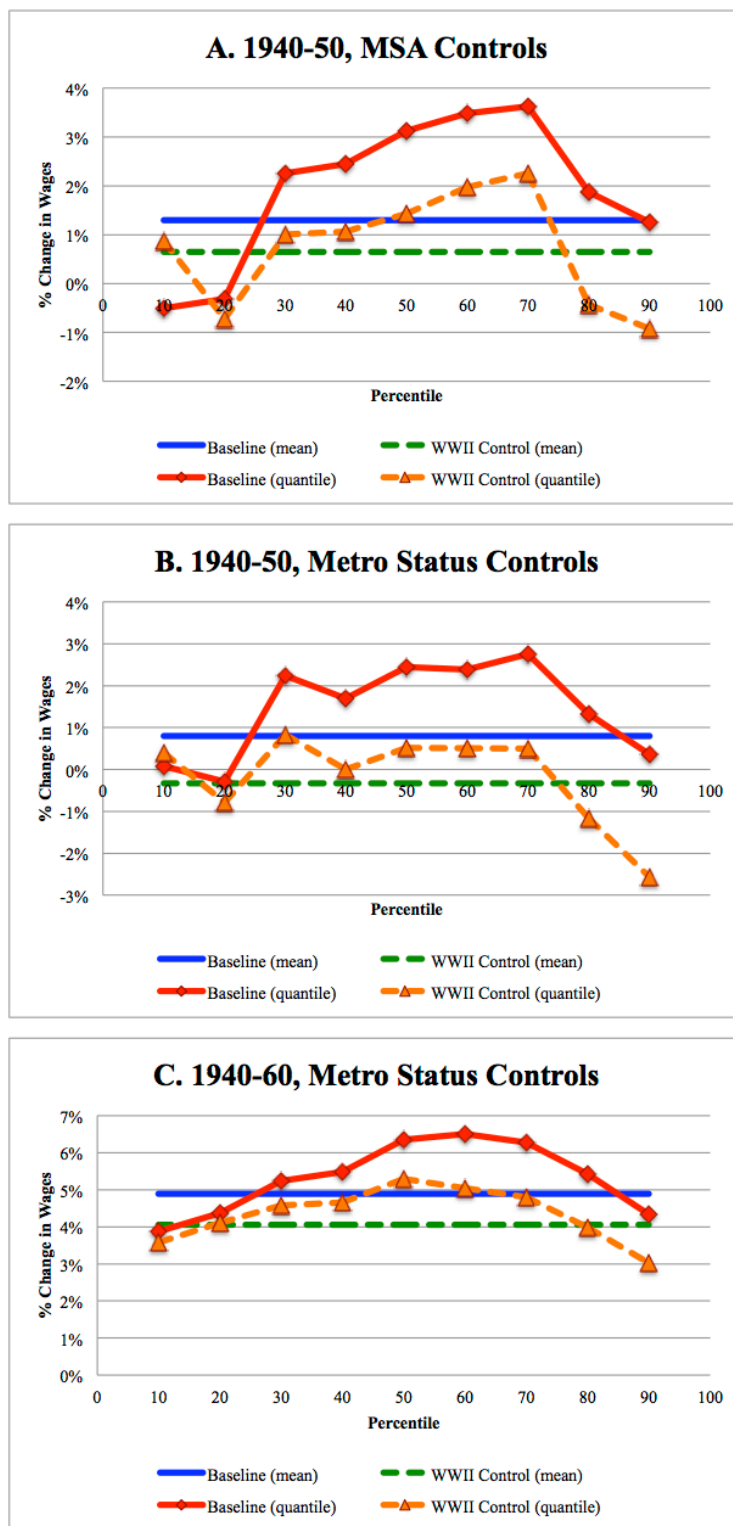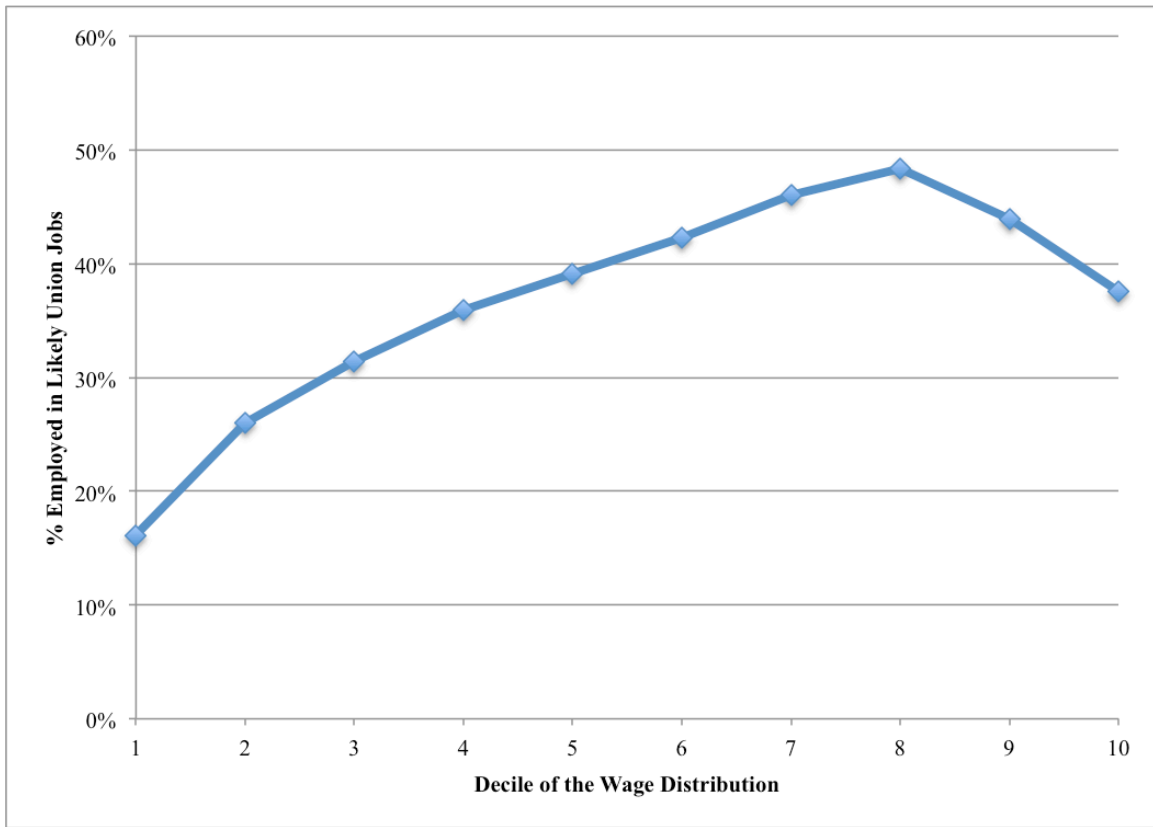
Figure A1: Approximation of Union Membership Rates by Wage Decile

Note: Likely union jobs are defined as non-management, non-professional jobs in highly unionized industries.