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# The Short-Run Impact of Right-to-Work Policies

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# The Short-Run Impact of Right-to-Work Policies: Union Response and Heterogeneous Effects

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## Abstract

Right-to-Work (RTW) laws are a common state policy that outlaw union security agreements and thus decrease union funding. Their impact on labor markets may change significantly depending on the degree to which they reduce union presence or cause general equilibrium effects. Previous estimates of RTW laws influence on union density, wages, and employment have varied extremely widely. Using the early-to-mid 2010s implementation of RTW laws in five U.S. states, I estimate the short-run impacts of the policy on labor markets using a synthetic difference-in-differences estimation strategy. RTW laws lead to a 1 percentage point decrease in union membership overall and a 2 percentage point decrease in industries that are more highly unionized. They also contribute to a small but weakly significant increase in the employment to prime-age labor force ratio despite a decrease in people employed. This is due to a a 0.9 percentage point decrease in the labor force participation rate that is concentrated among older individuals. RTW laws do not lead to a statistically significant change in workers wages on aggregate, but do lead to a 30.0% increase in the union non-union wage gap and a 5.7% decrease in wages for Black workers. RTW also appears to increase firm concentration and lower business dynamism as the number of firms, establishments, new business applications, and new firm formations all decrease at statistically significant levels. Together this provides evidence that RTW laws decrease union density but do not increase labor demand by attracting firms in the short-run. In response to RTW, unions may change their negotiating focus to prioritize wages while Black workers tend to be disproportionately negatively impacted by the weakening of unions.

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# 1 Introduction

During the early-to-mid 2010s the United States experienced a resurgence in the popularity of Right-to-Work (RTW) laws. First passed by states during the mid-twentieth century, RTW laws outlaw 'union security clauses' in collective bargaining agreements between unions and firms. These clauses support organized labor by requiring non-union member workers to pay a fee to the union of a unionized workplace in exchange for working on a union-negotiated contract. By outlawing them, states may reduce the financial and negotiating strength of unions in workplaces. Between 2012 and 2017, Indiana, Michigan, Wisconsin, West Virginia, and Kentucky all passed RTW laws bringing the number of states with RTW policies on the books to twenty-seven.[1] Broadly, pro-RTW advocates argue that these security clauses are an unfair burden on workers and that RTW laws increase worker opportunities while making it less costly for firms to operate. Opponents argue that they hurt union bargaining power and incentivize free-riding which lead to lower compensation for workers.

What the renewed relevance of RTW laws to state policy has revealed is that the literature is not clear on how exactly these policies impact states. The most obvious channel through which RTW policies may impact labor markets is a reduction in unionization and union power. There are some competing claims as to whether they actually impact unionization and labor markets at all, as well as to what degree. Some work, such as Moore (1980), argues that the laws have little actual impact and are more a reflection of local public attitudes. Others, such as Ellwood and Fine (1987), find that RTW has a significant impact on the extent of unionization and labor markets. How this should be expected to impact wages and employment for both union and non-union workers varies significantly depending on choice of union model. Most, but not all, expect union wages and employment to fall, with non-union wages and employment extremely contingent on the researcher's market assumptions. Furthermore, if unions provide extra value to categories of workers who may

1. In 2023, Michigan passed SB-34 that just under a year later would repeal Michigan's RTW law, reducing the number of states under RTW today to twenty-six. See Figure 1 for map by RTW status.

face discrimination in the workplace, the effects for different workers within the union/non-union categories may vary. Finally, if pro-RTW advocates are correct, there may also be labor demand shifts caused by the pro-business environment of RTW laws or even shifts in labor supply from induced worker migration. The recent implementation of RTW laws provides a new opportunity to test their impact on a variety of these outcomes, as well as apply some of the modern applied econometric methods that were not yet in use when some of the early analysis on the topic was done.

This paper provides empirical evidence about the short-run labor market impacts of RTW policies. Using the new law implementations in the five aforementioned states, I use a synthetic difference-in-differences (SDID) estimation strategy to examine a number of labor market outcomes after the introduction of RTW. Given the difficulty in finding a natural control group to provide a plausible counterfactual for the treated states had they not adopted the new policies, the SDID estimator builds a "synthetic control" group to compare against. This synthetic control group is a weighted average of possible comparison states, with weights chosen such that the control group most closely mimics a treated states' pre-treatment outcome trend. Using a couple of the core assumptions of difference-in-differences estimation, it then provides an estimate of the effect of the treatment on the desired outcome variable. I examine aggregate outcomes such as average unionization rates, wages, employment, and firm dynamism in addition to examining how some of these outcomes vary across different subsets of workers.

There are a few advantages that make this paper's findings particularly salient for RTW laws today. To date, much of the evidence on RTW laws is focused on comparing RTW states vs non-RTW states over long periods of time such as in Austin and Lilley (2021), sometimes even without policy changes. Additionally, most of the literature examines states that enacted RTW laws in the twentieth century (or in 2001 in the case of Oklahoma). This paper is able to examine recent policy changes that provide rich data before and after their implementation. Measuring short-run impacts helps reduce the potential for omitted variable bias, as the longer the time period, typically the more difficult it is to isolate the effect of a single policy on a labor market. The recent RTW laws also have occurred in a very different organized labor environment than when most of the previous implementations happened. Unionization rates have been falling for decades leading up to these changes, and remain much lower today than when many of the earlier RTW policies were adopted.

Also of importance is that while part of the impetus for the new law changes was a political shift across the greater rustbelt region, the affected states are historically and presently still much more similar in their political attitudes and prevalence of unions to non-RTW states than their already RTW peers. Combined with the suddenness of the policy implementation, this enables me to attribute the impacts of the policy to RTW laws instead of public attitudes as long as I assume there wasn't a large and sudden shift in public opinions against unions that exactly correlated with the year RTW was implemented in each state. The affected states' union density also makes any findings more policy relevant. Given that the five new RTW states are more union-friendly than previous RTW states but slightly less union-friendly than some of the remaining non-RTW states, they also may be more likely to change their RTW status in the future (such as Michigan has in fact done). Findings about the impacts of RTW that differ from previous work about implementations in less union-friendly locations may be more applicable to future RTW changes given the likelihood that such changes happen where opinions of unions are more mixed than in older RTW states.

The other key difference relative to existing work is my estimation strategy. Two other papers have examined the most recent RTW implementations. Murphy (2023) looks at how union density has evolved since 2000 in the affected states while Fortin, Lemieux, and Lloyd (2023) look at union density and aggregate wages with a focus on quantifying the impact of a reduced union threat effect on wages. Both use event-study and two-way fixed effect estimation strategies to measure the impact of RTW. One of the main weaknesses of this strategy is that it relies on selecting a set of states to act as the control group to compare the treated states against. The selections chosen, such as non-RTW states and other rustbelt states, tend to be different from the treated states on a number of potentially important observable characteristics. While these can be controlled for, it signals a potential for other, unobserved differences between the treated and control states that may lead to a violation of the parallel trends assumption. Furthermore, there are a number of concerns about the way in which two-way fixed effects (difference-in-differences) estimates are calculated that I discuss later. Finally, I limit my analysis to private-sector employment due to the RTW laws in question governing only the private-sector and the impact of the Janus v. AFSCME supreme court case in 2018 that essentially implemented RTW for all public sector employment.

By using a synthetic difference-in-differences estimation strategy, I can reduce the concern of whether the selected comparison group actually provides a plausible counterfactual of the treated states that meets the parallel-trends assumption. While public and private-sector labor markets necessarily overlap significantly, by focusing on the private-sector I can target workers and firms that will be directly impacted by the law. This paper then will look at additional labor market outcomes of interest, such as employment and measures of migration and firm-entry that will signal whether there are important general equilibrium effects to consider. The final important addition this paper makes beyond those is to examine how these outcomes differ by gender and race. One may expect unions to provide different levels of benefits for workers of different demographic groups, particularly if some of those groups face obstacles or discrimination in certain workplaces.

I find that as might be expected, unionization rates decrease, particularly in industries where unions are more prevalent. While union membership declines, the proportion of workers not in a union but covered by a union contract does not decrease. This suggests a relative decrease in the ratio of union members to non-union workers on a union contract meaning an increase in the ratio of non-paying “free-riders" for unions. On aggregate, there is no statistically significant change in wages from the RTW implementations. However, there is a statistically significant increase in the union to non-union wage gap that cannot be explained by changes in union composition. I find small aggregate employment to prime-age labor force percentage increases, but these are coupled with a negative overall change in the number of individuals employed by firms. This appears likely to be due to changing participation in the labor force, as treated workers, particularly older ones, exhibit a lower labor force participation rate after the RTW policies are put into place. I can rule out this being due to a change in the percentage of those self-employed or those in the public sector, but I cannot identify why individuals leave the labor force entirely

The zero aggregate wage change result masks heterogeneity across workers. Black workers see their wages fall on average unlike White workers, while White workers experience employment increases that Black workers do not. This aligns with previous work such as Ashenfelter (1972), Kleykamp and Rosenfeld (2012), and Farber et al. (2021) that show unions can reduce White-Black wage inequality due to the protection they may provide against workplace discrimination. The aforementioned increase in the union to non-union wage gap could be explained by a few reasons, such as union composition, changes in non-pecuniary benefits, changes in union negotiating strategy, or market forces. I find no evidence of changes in union composition for skill-level or other observables that could to be driving this change, while suggestive evidence suggests the difference is more likely to be from higher union wages as opposed to lower non-union wages. While I am unable to rule out changes in non-pecuniary benefits, the widening union non-union wage gap may point to a change in union negotiating strategy under RTW laws. Alternatively, this result can be expected as compensation for the higher cost per member of unionizing after legislation like RTW that decreases union funding in a model of monopoly unions such as the one proposed by Lazear (1983). There is no statistically significant difference between female and male wages, in line with the findings of Blau and Kahn (2017) that the impact of unions on the gender wage gap has fallen to zero over time.

I find no substantial increase in migrations into the affected states or increases in the number of firms existing or starting there. There are no statistically significant increases in the number of individuals migrating into new RTW states, nor any decreases in out-migrations. Meanwhile, there are statistically significant decreases in the number of firms, business applications, and firm formations after RTW policies are introduced. This suggests that potential general equilibrium effects do not seem likely to impact the labor market in the short-run, outside of a rise in firm concentration and decrease in firm dynamism.

Together the evidence suggests that Right-to-Work laws lead to decreases in unionization and while large aggregate impacts to wages can be ruled out there are distributional impacts in the short-run. Whether the decrease in labor force participation is due to workers becoming discouraged, retiring earlier, or some other factor is not clear. It appears that unions may have larger benefits for Black workers than White workers in the affected states, as the new RTW laws hurt Black workers more than White workers on average. The policies did not lead to any significant changes in migration, and any firm benefits seem to have been realized through firm concentration at the expense of firm dynamism.

The rest of the paper is organized as follows. Section two provides context about RTW laws and unions, and situates the findings of this paper among them. Section three provides a description of the empirical strategy and methods used in estimation. Section four covers the data used, and section five provides the main results. Finally section six describes some of the work done for robustness, before the paper concludes in section seven.

## 2 Right-to-Work Laws and Unions

Right-to-Work laws first sprang up in the United States in the post-World War II period of the twentieth century. The National Labor Relations Act (commonly known as the Wagner Act) passed in 1935 and set the legal structure for workplace labor relations in the United States. It codified the right of all private sector workers to form a union, bargain with their employer, and strike or take other collective actions. Importantly, the Wagner Act specifies that a union elected by a majority of the employees in a workplace had the right to bargain as the exclusive negotiator on behalf of all employees in the workplace, including those who are not part of the union. Moreover, the act also required that firms must engage with any chosen representative of the workers in good faith. This led to the standard of non-union members at a unionized firm frequently having to pay fees to the union for negotiating their pay and benefits, with the fee often set by a “union security agreement" included in the contract with a firm. The Taft-Hartley Act, passed in 1947, amended the Wagner Act to allow states to outlaw union security agreements.[2] Immediately there was a large wave of RTW law implementations, primarily in the South and western Midwest regions.[3] A few more states added them over time, with Oklahoma in 2001 being the last state of what I will consider “previous" RTW implementations that had been in effect for a long time by the 2010s. These states tended to be ones that are today considered politically “conservative" in American politics. In the 2010s, a new wave of 5 states adopted RTW laws: Michigan, Indiana, Wisconsin, West Virginia, and Kentucky all implemented RTW laws, on the back of conservative electoral wins in those states.[4]

The research on Right-to-Work laws has generally presumed that the policy's effect on labor markets functions via it's impact on union membership. As outlined in Moore, Dunlevy, and Newman (1986b), this effect is usually theorized to fall in one of three categories. The “Bargaining-power hypothesis" postulates that RTW laws lower the negotiating power of unions directly and therefore decrease the wages unions can bargain for. The “Free-rider

2. Notably, it did not allow for the same bans at any local government level. A small number of counties/municipalities have tried implementing them in states without RTW laws. Both Seaford, Delaware, and Lincolnshire, Illinois passed RTW laws that were blocked or rendered moot in court. New Mexico had a small handful of counties (Chaves, Eddy, Lea, Lincoln, McKinley, Otero, Roosevelt, Sandoval, San Juan, and Sierra) that implemented local RTW laws at various points during 2018 while the legal status was not specified either way at the state-level. Only the New Mexico counties may have actually affected firm-union negotiations due to the legal ambiguity caused by those counties adopting RTW. The effect was likely small if not zero due to the legal ambiguity, that the counties that passed them tended to be small (none were among New Mexico's three biggest counties by population), and that in 2019 an anti-RTW state law was passed overriding the county laws. New Mexico additionally only occasionally is included in the comparison group with a non-zero weight, and the weight tends to be quite small. To the extent that RTW may have impacted New Mexican labor markets in 2018, it would likely lead to an minuscule underestimation of the effect of RTW in our estimates.

3. By 1948 there were 12 states with RTW; by 1963, 19.

4. Wisconsin, Indiana, and Michigan gained Republican trifectas starting in 2011. West Virginia's Republican legislature overrode the governor's veto in 2015, while Kentucky's Republicans gained a trifecta in 2017 and immediately passed RTW legislation

hypothesis" argues that because non-union members can still benefit from the negotiations of a union without actually paying in, the incentive to free-ride increases leading to a higher cost to organize workers for the union. This will lead to lower levels of unionization and therefore the amount of union services. The final theorized effect is the "Taste hypothesis" that states RTW laws only occur in places where there is already less pro-union sentiment among workers and the public at large. The presence of RTW is therefore endogenous to societal tastes with respect to unions, and does not so much influence labor markets itself but rather is a proxy for the public's preferences that may be impacting labor markets.

One new addition to this structure is made by Fortin, Lemieux, and Lloyd (2023), who argue RTW laws have the potential for spillover effects on the labor market as a whole, including in non-unionized workplaces, due to a reduced "threat effect." Just as the "Free-rider hypothesis" argues that the costs to organizing rise for unions and their services after RTW laws are implemented, the costs to forming a union will be higher because of the inability to compel all workers to pay the union. This lowers the probability a workplace will unionize, which in turn improves the negotiating position of a firm relative to its workers even in non-union workplaces. It fits in among the "Bargaining-power" and "Free-rider" hypotheses as ways in which RTW laws have a causal impact on the labor market. This is in contrast to the "Taste" hypothesis which suggests that the actual policy itself does not cause any changes to the labor market.

This paper is primarily concerned with determining whether there are short-run causal impacts of enacting RTW legislation. It can only provide suggestive evidence with regards to whether the effects found are due to a bargaining-power effect, free-rider effect, or threat effect (or some combination). But it can distinguish whether RTW laws should be thought of in terms of the "Taste" hypothesis, where their implementation has little to no real impact on labor markets, or one of the causal impact hypotheses. If the "Taste" hypothesis is correct, then RTW laws are just a proxy for the voting public's preferences. Actual differences between areas with RTW laws and those without them would likely represent a combination of individuals' preferences, other policies passed by governments that favor RTW policies, and perhaps other differences across regions (such as geography or history). If one of the causal impact hypotheses is correct, regardless of which one, this could lead to significant differences in the economies of even otherwise very similar areas if one of them has a RTW law and the other does not. Determining whether the policy has short-run causal impacts provides information relevant to public policy, and will help guide future research more interested in the particular avenue through which RTW laws impact labor markets.

Because RTW laws are expected to influence labor markets through their effect on unions, it is important to consider the large literature on the impact of unions on labor markets. Early work focused on theoretical models building off one of two basic conceptions of trade unions. The first was the monopoly model of unions, where a union uses its control over the labor supply to pick a wage maximizing point on a firm's demand curve (first proposed by Dunlop 1944). The competing hypothesis was that unions jointly maximized wages and employment as part of a utility function subject to a firm having a non-zero profit as first suggested by Ross (1948) (known as the efficient bargain model). A large variety of models combining, extending, or rejecting these followed in the seventies and eighties. Most models predict a positive relationship between union bargaining power and union wages. One model that diverges from this is the model of monopoly unionism in the context of a competitive market proposed by Lazear (1983). By focusing on firms and workers as profit-maximizing as opposed to the union and assuming costs for firms fighting unions as well as market clearing in the non-union labor-market, the model predicts that additional costs to running a union may lead to higher union wages. Thus, legislation that reduces the bargaining power of unions will decrease the number of union workers, but because it increases the per-member cost, union workers actually see the union to non-union wage gap rise to compensate as union wages rise and non-union wages remain steady or slightly fall. This model is of particular interest as it is well-designed to analyze legislation such as RTW and may explain the empirical findings here.

More recent union literature has tended to lean toward focusing on empirical results. A number of papers (Card 1996; Barth and Bryson 2022; Callaway and Collins 2018; Parolin 2021) have documented a wage premium for union workers, with the premium often being largest for lower-skill workers. Lemieux (1996) also finds unions increase wages and that the distribution of returns to skill is compressed in unionized workplaces. This fits common theoretical models of unions having larger wage premiums at the lower end of the income distribution, though it should be noted Hirsch and Schumacher (1998) find that the compressed skill returns distribution is actually less dispersion in unmeasured skills within unions which results from selection on unmeasured skill by firms and workers. Unions also tend to decrease overall wage inequality according to Card (2001). Card, Lemieux, and Riddell (2020) similarly find decreases in wage inequality, but that the effect is concentrated in public sector unions as opposed to private sector ones. This is important as the RTW laws this paper examines effect private sector employment, and as such, it may not be appropriate to generalize wage distribution findings from the public sector. As a whole, these results suggest that unions increase wages, even if some of it is due to who selects into them. Whether this means that the union to non-union wage gap is positively correlated with union bargaining power is unclear. Fortin, Lemieux, and Lloyd (2021) find that the threat effect from unions exceeds any crowding out effect and causes non-union wages to rise in the presence of higher union density as well. Weakening unions via RTW laws may lead to lower average wages for the entire labor market, but whether union wages or non-union wages fall more is not well-defined in the literature.

There's also evidence from Farber et al. (2021), Ashenfelter (1972), and Kleykamp and Rosenfeld (2012) that historically in the United States unions have caused wage gaps for Black workers to close. This paper will try and determine to what degree different demographic groups of workers, particularly those who have traditionally faced wage gaps or discrimination in the labor force, are impacted by the implementation of RTW laws. Traditionally, research has found that the male-female wage gap was smaller among union workers relative to non-union workers. That seems to no longer be the case, as Blau and Kahn (2017) find that the impact of unions on the gender wage gap has shrunk to zero over time.

Previous empirical research on RTW laws themselves has produced very mixed findings with regards to their effects. This is in part due to the long time frame across which some of the research has occurred, meaning early work had more limitations in the available data, empirical methods, and computational power available to them. It's also likely that given the much higher rate of unionization in the mid-twentieth century that the effects of RTW laws could be different relative to these more recent implementations with significantly lower unionization rates. At the same time, the first states to adopt RTW (and therefore be studied) tended to be the most politically hostile to unions. In contrast, the five states adopting RTW in this paper have traditionally been much more union-aligned with histories of strong union presences.

Early work tended to focus on determining whether RTW laws impacted unionization rates or whether they had no effect and were more likely just a reflection of the public's preferences. Some found the latter (Lumsden and Petersen 1975; Wessels 1981; Farber 1983; Koeller 1985; Moore, Dunlevy, and Newman 1986a) while others found a moderate to large negative effect on unionization (Warren and Strauss 1979; Ellwood and Fine 1987; Davis and Huston 1995; Ichniowski and Zax 1991; Hogler, Shulman, and Weiler 2004). The differences in results are wide and can mostly be attributed to the different methods used to account for simultaneity bias and omitted variable bias on what was mostly otherwise cross-sectional data. Different authors used different proxies for employer's willingness (or unwillingness) to work with unions and worker's opinions of organized labor. Notably, most of this work also only views states as RTW or non-RTW, and does not observe changing RTW status for a state over time. There is also a distinct lack of identification strategies or the use of tools such as time and state fixed effects in most of the work (the exceptions are Ellwood and Fine 1987, Ichniowski and Zax 1991 and Lumsden and Petersen 1975).

More recent work has also produced mixed results, but with a stronger lean towards RTW having some effect on the labor market. Dinlersoz and Hernandez-Murillo (2002) compare Idaho (RTW in 1986) before and after its implementation of RTW with neighboring states, and finds little change in unionization to their neighbors that can be attributed to RTW. Bruno et al. (2015) find that RTW doesn't impact the probability of being employed, but does decrease the probability that a worker is unionized and lowers wages slightly on average. Their no employment effect is the same in the manufacturing industry, which is at odds with Holmes (1998), Kalenkoski and Lacombe (2006), and Austin and Lilley (2021) who find an increase in the manufacturing share of employment. Eren and Ozbeklik (2016), on the other hand, find that while RTW laws lowered unionization rates, they did not boost employment or the manufacturing share of employment in a synthetic control analysis of Oklahoma's 2001 implementation. In addition to the increase in manufacturing employment result, Austin and Lilley (2021) also find that wages rise slightly on average over time (again in contrast to Bruno et al. 2015). While Austin and Lilley look at extremely long-run changes that may not plausibly be just due to RTW laws, their border-pairs discontinuity design is more plausible than Bruno et al.'s cross-sectional analysis of comparing RTW and non-RTW states without a policy change. Most recently, Murphy (2023) finds using a two-way fixed-effects estimator that union density decreased in RTW states since 2000.

One of the main challenges to the analysis of RTW laws is that union workers may receive different levels of non-pecuniary benefits relative to non-union workers. Non-pecuniary benefits, ranging from health-insurance to worker safety conditions, are often harder to measure or lack the same data as information on wages or even hours worked. The literature on these outcomes is sparser, but the existing work suggests that the non-pecuniary benefits received by unionized workers are larger in value than those received by non-union workers. As an example, Olson (2019) finds that the long-running gap in the proportion of workers with employer health insurance between union workers and non-union workers has only continued to widen in the United States over time. Sojourner and Pacas (2019) find that union members have a "net fiscal impact," meaning they contribute more to taxes and use fewer government services than their non-union counterparts, which may be due to the higher level of non-pecuniary benefits union members often receive. RTW laws may also reduce worker safety; both Zullo (2011) and Zoorob (2018) find that unions significantly decreases occupational fatalities and that RTW weakens their ability to do so. Gihleb, Giuntella, and Tan (2021) also use RTW laws to determine that after their implementation, the share of workers working more than forty-five hours a week rises, with the highest effects seen in high-unionization industries. One of the limitations in this paper is the inability to measure non-pecuniary benefits. While I will be able to rule out large changes in the probability of working full time, I will not be able to accurately measure other important benefits such as health insurance or worker safety.

With this in mind, this paper tries to understand more about the short-run effects of RTW laws and how they impact different outcomes for different groups of workers. Much of the literature, even more recent work, tends to have weaker causal identification strategies and instead relies on cross-sectional analysis without an identifiable policy change over time. The work that does examine policy changes within states tends to be limited to examining one state (Dinlersoz and Hernandez-Murillo 2002 looks at Idaho while Eren and Ozbeklik 2016 looks at Oklahoma) or one outcome (Murphy 2023). The results tend to be limited to aggregate average effects, with little extra detail about particular groups of affected individuals (Fortin, Lemieux, and Lloyd 2023). This paper will examine five separate policy changes, and do so using a policy change that is plausibly exogenous. The synthetic difference-in-differences model provides an average of the treatment effect for the "treated" states and will allow us to provide stronger evidence as to what the RTW laws actually do to labor markets across an entire state in the short-run. The effects will be broken down to examine how they differ across different subgroups of workers. Finally, I will be able to provide some evidence as to whether or not certain outcomes that might have general equilibrium effects on the labor market are appreciably changed by RTW policies.

# 3 Empirical Strategy

This paper will focus on reduced-form empirical results of the impact of implementing RTW policies in states. To measure these, ideally we would be able to observe outcomes in a state that is randomly assigned a RTW law and then compare them to outcomes in that same state but where the state had not received the random treatment. Any change in outcomes for the otherwise identical worlds could then be attributed to the effect of the RTW law. Since we are unable to observe both the world where a state is treated and the world where it is not, we are left to try and compare the states that implemented RTW laws to a chosen comparison group. If we can argue that the treated and comparison groups satisfy the equal trends assumption, we can estimate the treatment effect of RTW laws using a standard difference-in-differences framework where the effect is measured as any break in the parallel trends after treatment. To make this assumption, we must assume that our groups are equally affected by time and any selection effects into treatment are simply level differences across states. This will allow time and state fixed effects to account for outcome-level variability across states and across time (note this means the effects must be additively separable). Thus, if this holds, we would expect the difference in outcomes between treatment and control groups to not change over time in the absence of a treatment, suggesting the comparison group is a good “counterfactual" for our treatment group.

In this case, our treatment group will be the group of five states (Indiana, Michigan, Wisconsin, West Virginia, and Kentucky) that implemented RTW laws. Finding a reasonable comparison group to represent the counterfactual for our treatment ends up being difficult. A natural candidate would be the set of all non-RTW states in the country, as the treated group is going from being historically non-RTW states to RTW ones. Another reasonable candidate would be the set of non-RTW rustbelt states that have a high geographical proximity to our treated group. A quick comparison of these groups immediately raises some warning signs. Table 2 provides a t-test comparison of both potential comparison groups to the treated group along a number of observable characteristics. These groups are, on average, very different along a number of dimensions. This in particular raises concern that there are other, non-observable ways in which the states differ that would make the comparison group a poor choice of counterfactual to satisfy the equal trends assumption. Even if we can condition on many of the differences between the groups in our analysis, there are likely differences that we cannot condition on. Furthermore, these states do not have consistent pre-treatment differences relative to the treated group for a number of important outcomes (see Appendix Figure A.1-A3).[5] This suggests selecting a treatment group based on subjective characteristics like RTW history or geographical "closeness" will not likely satisfy the required assumptions of traditional difference-in-difference estimators.[6]

We therefore turn to a different method of choosing a comparison group that still fits in the difference-in-differences potential outcomes framework. The synthetic difference-in-differences (SDID) estimator for panel data, provided by Arkhangelsky et al. (2021), estimates a synthetic comparison group using a weighting scheme similar to a traditional synthetic control estimator. The outcome of the treated group is then compared to the outcome of the synthetic comparison group before and after treatment provide the difference-in-differences style estimate. More precisely, for an outcome variable $Y_{st}$ in a balanced panel of $N$ units (denoted with an $s$ for state) and $T$ time periods, the SDID estimator is calculated as:

$$(\hat{\tau}^{sdid}, \hat{\mu}, \hat{\alpha}, \hat{\beta}) = \underset{\tau,\mu,\alpha,\beta}{\arg\min} \left\{ \sum_{s=1}^{N} \sum_{t=1}^{T} (Y_{st} - \mu - \alpha_s - \beta_t - W_{st}\tau)^2 \hat{\omega}_s^{sdid} \hat{\lambda}_t^{sdid} \right\}$$

where $\alpha_s$ and $\beta_t$ are state and time fixed effects, $W_{st}$ is the binary indicator for treatment, $\hat{\omega}_s^{sdid}$ and $\hat{\lambda}_t^{sdid}$ are estimated unit and time weights, $\mu$ is the common mean, and $\tau$ is the estimated treatment effect. The unit weights are selected such that pre-treatment trends of

5. Figure A.1 provides a two-way fixed effects event-study graph for unionization rates, log wages, and employment rates. Figure A.2 provides the same but from using the Callaway & Sant'anna difference-in-differences estimator, and Figure A.3 provides the same but using the de Chaisemartin and de Haultefoeuille estimator.

6. Also included is a control group made up just of Missouri and Ohio. Both states passed RTW laws (in 2011 for Ohio and 2017 for Missouri) that were appealed by popular vote in a referendum. While that provides a potentially useful comparison group, the very small number of control units makes inference difficult due to the high potential for large standard errors.

the treated and comparison groups are the as close as possible ($\sum_{s=1}^{N_{co}} \hat{\omega}_s^{sdid} Y_{st} \approx N_{tr}^{-1} \sum_{N_{co}}^{N} Y_{st}$ where $N_{co}$ are the control units and $N_{tr}$ is $N - N_{co}$) for all time periods before treatment. The time weights are selected similarly such that pre-exposure time weights ar similar to post-exposure time weights.

This estimator therefore builds a counterfactual by choosing a comparison group by giving more weight to units that are similar to the treatment group in the pre-treatment period and more weight to time periods similar to the treated periods. It creates a more local version of the two-way fixed effect difference-in-differences estimator and by only including more similar units and time periods in the comparison group, it should also be more robust. Unlike the standard synthetic control procedure, unit weights are designed to prioritize making the weighted average comparison group have parallel trends to the average of the treated group instead of prioritizing an exact match. Time weights prioritize making the post-treatment outcomes for the comparison group differ by a constant from its weighted average in the pre-treatment time period (creating a linear trend). In doing so the SDID estimator develops the logic of a difference-in-differences estimator, but using a weighted average of all the potential control units for the comparison group. Similar to a two-way fixed effects model, the SDID estimator can be adjusted for covariates. For details on this or the exact algorithm for estimating weights, see Arkhangelsky et al. (2021). This allows for a more accurately developed counterfactual from which to estimate treatment effects. Inference is done by estimating standard errors using a clustered (block) bootstrap.

There are a number of outcomes this paper is interested in. The focus is limited to the private sector, as these RTW laws are specifically oriented towards private-sector work.[7] The first, and perhaps most important, is union membership rates (or unionization rates). Next I examine the log of the average inflation adjusted hourly wage[8] and the employment

7. The 2018 Janus vs AFSCME supreme court ruling effectively established RTW for all public-sector workers in the U.S. further complicating analysis of public sector employment during this time period.

8. Wages include hourly wages and non-hourly wages estimated using usual hours worked. When hours are not provided, they are imputed. Top-coded values are assigned an above the mean top-code using a log-normal approximation that differs by gender. All wages are adjusted to 2014 constant dollars using the CPS-U-RS. Other details follow the outline provided by Schmitt (2003).

to prime-age (25-55) labor force proportion. I also examine the log of the union relative to non-union wage gap, calculated as the log of the difference in average inflation adjusted hourly wage for union and non-union workers respectively. Other outcomes looked at with the SDID estimator include average migration of individuals in and out of states, the average number of firms, establishments, firm applications, firm creations, employees employed by firms, hours worked, and average proportion of self-employed individuals in the prime age labor force.

The main unit of analysis will be the state. RTW laws are state level policies, and given their binding nature on collective bargaining agreements negotiated within the given state, effects should present themselves in the affected state. It is possible that there will be some spillover effects into neighboring geographic areas. The most plausible spillover would be migrations of individuals or firms across states. We can test the former using migration data, and while the latter cannot be directly tested, it seems likely any expected migration would be of firms into an affected RTW state to take advantage of the potentially enhanced negotiated position of the firm relative to labor. By comparing firm and establishment numbers over time, we can potentially rule out such a change. Another possible spillover would be of a change in the labor force drawing or reducing the number of people commuting across a state border for work (in a non-RTW state to a RTW state). While these individuals would not show up in migration data, the likelihood of such a population changing enough to impact overall state outcomes is small, and therefore ignored.

Using a more granular unit of analysis could also provide interesting results. I stick to the state as opposed to counties, “PUMAs," (Public Use Microdata Area) or other areas for data limitation reasons. First, some of the data I will be using is only available at the state level. Even for outcomes that do come from the Current Population Survey (CPS) or other dataset with more granular information, there is often not enough data to create a consistent and balanced panel over the desired time period. Without a complete panel, it would be difficult to attribute effects to a state labor market as a whole. For example, it would be possible to have a limited number of counties in a balanced panel. The CPS is not evenly drawn from every county, and thus the counties in the panel might tend to have certain characteristics (urban counties for example would be more likely to be included than rural ones). An effect might be attributed to a state based on the labor market outcomes of just its densest counties that might not be a good representation of the whole state's labor market. There may be within-state migration or spillover effects that the limited panel could not capture. While this heterogeneity might be interesting on its own, it is not the focus of this paper. Furthermore, the number of control variables in any such estimation would likely have to be greatly reduced. I thus stick to state-level data, with variables from more granular data aggregated among the labor force appropriately.

It should be noted that because the policy is implemented over multiple states, and at different time periods, the analysis has the potential for heterogeneous treatment effects across time and across states. The SDID estimator handles this by estimating each treatment period separately by dividing the treatment matrix into multiple separate matrices, each with one treatment period. The estimator is calculated for each treatment period relative to the possible control units individually, and then is averaged across the treatment periods weighted by the number of post-treatment periods in each estimation. Because the five states adopting RTW all do so in different periods, each has their own SDID estimator calculated and then the effect is averaged with the larger weights going towards the earlier adopters. Because there are no time periods where multiple states implement RTW, we do not have to worry about within specific adoption time periods heterogeneity in effects across states. This method of calculating the overall average effect among the treated states also avoids the issue of having to compare any treated states against already treated states within the sample (avoiding the potential "negative weighting" problem).

This should alleviate some of the concerns that come from two-way fixed effects style estimation. In a difference-in-differences framework for estimation, there may be concerns about the estimated treatment effect that have been well-documented in the applied econometrics literature. In particular there are worries that with staggered treatment, some treated units are also control units and therefore appear both on the positive and negative side of the differencing done in the "difference-in-differences" estimate. These is not an issue due to the way SDID estimates treatment effects, as none of the treated units show up among the possible control units for the other treated units (any unit treated in a different time period is completely excluded from the treatment matrix of the other treatments).

Another worry is about heterogeneous treatment effects, particularly if there may be a time-element affecting treatment size. This is more of a concern, particularly given the relatively close geographical proximity of the treated states. I argue that this is not a large problem, as RTW laws effects for the whole state should swamp any spillover effects that might impact the size of an effect in a neighboring state. Furthermore, if there were heterogeneous effects, it would likely be due to a characteristic of the states. The treated states in our sample are somewhat similar, and I do not find evidence for significant general equilibrium effects that might pull workers or firms out of a going to be treated later state that might significantly change the labor market conditions of each state. Finally, excluding the other treated states from the treatment matrix of each individual state means that worries about a non-parallel trend from an already-treated state are not a concern, as that already treated unit is not part of the potential comparison group. For completeness, however, I include a few of the more popular two-way fixed effects estimators that aim to correct for these issues in the appendix as a comparison.[9] Because these estimators require chosen control groups, I use all non-RTW states, then all non-RTW rustbelt states, and finally only states that have passed RTW laws that were repealed via referendum (Ohio and Missouri) as comparison groups for these estimators. As is evident in the results, the estimates vary significantly with the choice of comparison group and method, reinforcing my skepticism of this as an estimation strategy.

9. Included are: Callaway and Sant'Anna (2021), deChaisemartin and D'Haultfoeuille (2021), Wooldridge (2021), and a standard two-way fixed effect model estimated at the individual level for specifications using CPS data and aggregated and estimated at the state level for all others. See Table A.3 through 8.

One final concern with the estimator may be the use of a vector of controls. Like typical two-way fixed effects estimators, the SDID estimator easily accommodates a vector of covariates such that the estimator is calculated from the residuals of a regression of the outcome on the covariates. This is notably different from the conception provided for controls in the synthetic control method, where the estimator tries to then match control variables along with the outcome variable between the treated and synthetic control group. The SDID estimator removes variation in the outcome variable that is due to changes in the control variables first, and then moves on to calculating the estimator on the remaining variation in the outcome. Details can be found in Clarke et al. (2023). The main potential issue is that if the covariates include variables that are endogenous or closely related to the outcomes, the relationship between the covariates and the outcome may change differentially over time in the treated versus comparison groups. For example, if RTW policies reduce union power, unionization rates, and thus decrease the amount of wage compression in a labor market, the wage returns to education might increase in the treated state's labor market on average relative to non-treated labor markets. If education is a control in the SDID estimate of the effect of RTW on wages, then the estimate might be biased as the relationship between education and the outcome wages has diverged in RTW states relative to non-RTW states. The variation that was controlled out of the wage variable by the education control may no longer fully capture all the variation in wage that might be due to education, and then any correlation between RTW and education will bias the estimate of RTW impacts on wages. To deal with this concern, the SDID estimator is specified to use a regression adjustment from parameters estimated from an only control units regression of the outcome on covariates and fixed effects. Further details can again be found in Clarke et al. (2023).

The main specifications in this paper use a vector of covariates to account for potential important differences across states that might change over time. These include average values for education in years, experience, education interacted with experience, education squared, experience squared, proportions of White, Black, Hispanic, Asian, and other racial groups, proportions of male or female individuals, proportion of married individuals, proportion of individuals living in a metropolitan area, proportion of the workforce working in one of eleven different broad industry categories, and a measure of business attractiveness from the Beacon Hill Institute. These should help control for any trends in states that are not captured in the specifications fixed effects. One of the main challenges to the interpretation of the SDID estimator as a treatment effect is the potential endogeneity of treatment. The Taste-based hypothesis of RTW laws argues that the laws themselves have little actual effect on labor markets, and that differences in outcomes such as unionization rates reflect differences among the populations attitudes towards organized labor. Given that these laws were passed after conservative gains at the state political level, one might be worried that there may be other legislation being passed around the same time impacting labor market outcomes.

I include the control for the measure of business attractiveness as a way of controlling for some of these potential confounders. The measure is built on a large number of factors, but prominently featured are fiscal policy and government regulation. Importantly, RTW laws are not part of this measure. If there are a number of other pieces of legislation being passed along with RTW laws, this variable should help capture that. Furthermore, I argue that unlike many of the early-adopting RTW states, these new adopters are much more similar to non-RTW states than their already-RTW counterparts. For example, while not quite as high as non-RTW states, the newly treated states have unionization rates closer to non-RTW states than states that previously had RTW (see Figure 2). Moreover, it is difficult to argue that these states suddenly changed the entire public perception of organized labor all at once. It seems more likely that the populace has slowly evolved to be slightly less pro-union, but likely does not have the same level of anti-union sentiment that is common in many RTW states.

Further supporting this point is the way in which these laws were passed. Despite all coming on the back of conservative state-level political sweeps, the RTW laws were passed very fast and somewhat secretly. Notably, they frequently bypassed committees and public input, were often fast-tracked to be passed in a matter of days, and in the case of Michigan, only notified the media of the process after the law was signed (Murphy 2023). As summarized by Hertel-Fernandez (2019, p. 174-176) about these state political gains, Republican candidates campaigned on an “anodyne" economic agenda featuring tax cuts and smaller government, but once in power, also turned to less popular attacks on organized labor. RTW laws were not necessarily popular with the public in these states, and the legislative process reflects as much. Further underscoring this is that Michigan in particular has since repealed their RTW law under a less conservative state-government. While this might not make the implementation of RTW perfectly exogenous, it suggests the laws were not necessarily desired nor expected. This makes the “treatment" of these states a somewhat more plausible interpretation, as well as likely a better estimate for what might occur to any future state that adopts RTW (given that the current states without RTW are closer to these five new RTW states than the old RTW states in attitudes towards unions).

If as expected, the impact of RTW laws is working through their impact on unionization rates, we should expect the results to be most concentrated in industries where there are more unions. Throughout estimation, I provide results for individuals in any industry, and for individuals exclusively working in a “high-union" industry. This is any individual working in an industry category where at least twenty-percent of workers are unionized in the treatment states. Of the twelve included industry categories (armed forces employment and public administration are excluded for being public-sector work), this includes construction, education and health services, and transportation and utilities work (see Figure 3). These results may also highlight results that are too small or obscured in the whole sample. Notably, manufacturing, one of the traditionally union-heavy industries, does not have a high enough unionization rate to be included in the high-union category. Then, as a robustness check against other mechanisms or other factors driving observed effects entirely, I provide results for individuals exclusively in a “low-union" industry. If RTW us primarily working through unions and their impact on the labor market, we should expect to see little to no significant changes in the low-union industry.

# 4 Data

To analyze Right-to-Work policies, I look at data from 2007-2019. This gives the earliest adopter (Indiana) five pre-treatment periods and cuts off the analysis before the COVID-19 pandemic can influence it. The primary data on labor market outcomes for individuals comes from the Current Population Survey (CPS). The CPS provides monthly survey estimates of US households and tracks both demographic and economic outcomes. Importantly, households are in the survey for four consecutive months, then after an eight month break, another four months. In the final month of each four block period, they are asked additional questions as part of the Outgoing Rotation Group (ORG). These additional questions include information about union status, among other things, and as such provide the basis of our key outcomes.

I use the measure of union membership to measure unionization rates. Also available is a measure of union coverage, though that is clearly less likely to capture declines in union membership. Wages are calculated as previously described by combining measures of hourly wages and non-hourly work. Employment is measured as the number of employed individuals divided by the total labor force between twenty-five and fifty-five. The data from the CPS is then aggregated using the survey's earnings weights to the state-level to build the state-level panel for analysis. The state-level includes every state as well as Washington D.C.

To examine sub-groups of workers, I limit the sample to individuals in that group. The data is then aggregated specifically for that group of individuals, and estimates are taking from estimation on the resulting panel. In a few cases, particularly when looking at a certain race of worker, there aren't enough workers of that race that are also in a union for a given state. Thus, for estimation of Black and Hispanic workers in particular, a couple states are dropped from the panel due to not having 10 individuals with union status in the data.

Migration data comes from the IRS using its migration flow data based on tax returns. This data can be somewhat noisy and so the results are confirmed using the Census Bureau's intercensal estimates of population flows as a robustness check. The IRS data provides both individual and household movements across borders. It's measured as a change in location from the previous year's tax filing address, such that for some individuals, if the move happened in the first three months of the year, it may not appear in the data until the following year.

Firm data comes from a few different sources. The number of establishments, a measure of a workplace (such as a factory, store, etc.), comes from the Quarterly Census of Employment and Wages (QCEW). The QCEW also provides the number of individuals employed. This measure of employment notably excludes unincorporated self-employed individuals, proprietors, unpaid family members, and certain farm and domestic workers. Also excluded are railroad employees covered by the railroad unemployment insurance system. The number of firms, firm applications, and firm formations comes from Business Formation Statistics (BFS) data, which itself is built from data from the employer identification IRS filings, the Census Bureau's Business Register, and the Longitudinal Business Database.

# 5 Results

The first stage of estimating the impact of Right-to-Work laws is to examine whether they impact union density. Table 3 shows the results from the SDID estimate of the effect of RTW on unionization rates (trend graphs are provided in Figure 4). Columns one and two show the estimated treatment coefficient with and without a vector of controls for the entire prime age labor force. Columns three and four then do the same but for the high-union industries sample of the labor force. The states treated with RTW laws see a 1.22 percentage point decrease in the proportion of their workers in unions relative to the comparison group, which grows to a 2 percentage point relative decrease in the sample of only highly unionized industries. While slightly more moderate than some other estimates of RTW (particularly older ones), this result is statistically significant at the 1% significance level. Given the low rates of unionization in the U.S. currently, a 1-2 percentage point decrease in unionization is quite a large percentage decrease. Given that union membership is about 10% on average in the treated states before treatment, a greater than one percentage point decrease is an over 10% decrease in unionization. Given this context, the relative size of the effect is quite large.

If the identifying assumptions of the estimation strategy are believed, this refutes the taste-based hypothesis of RTW laws as lacking any impact on their own. To argue for it in light of the estimate, one would have to argue that public preferences shifted dramatically in the year of treatment in each state enough to cause an over 10% decrease in unionization rates. That, or there was some other factor that conveniently aligned across the multiple states that caused them to see significant decreases in unionization rates starting right when their RTW law was implemented. The estimates are not driven by one particular state either; four of the five treated states have statistically significant (at the 5% significance level) results on their own in the sample across all industries (Indiana being the only one that is not). For the high-union industries it is the same (except Indiana is now significant at a 10% significance level as well); see Table A.1 for more details.

There is also suggestive evidence for the free-rider hypothesis of RTW laws. In panel 2 of Table 3 the results of the same estimation method but with the proportion of non-union workers who are covered by a union contract as the outcome is displayed. There are no statistically significant results, and the coefficients are extremely small for both the whole sample and the high-union sample. This suggests that while the proportion of union members was falling after RTW was implemented, there was no change in the proportion of individuals who worked on a union contract without being in the union. This means there was a relative decrease in the union to non-union ratio of workers working on union contracts, a result that is consistent with the free-rider hypothesis. As the relative cost of being a union-member rises, workers in unionized workplaces are more likely to choose not to join the union and work on the union contract without paying any union dues.

Next I turn to estimating the effect of RTW laws on the log of wages. The prediction for this result is not clear ex ante. Previous work has found both positive and negative results for aggregate wages, and while most models of organized labor predict union wages will fall, other such as the Lazear (1983) model predict a rise in union wages. Some models predict the loss of bargaining power hurts unions and non-union workers, and all wages fall on aggregate. Others predict that as union wages fall, non-union wages will rise such that the net effect is zero due to a flow of non-union workers into jobs that were previously union jobs (reducing the supply for previously non-union jobs and increasing it for formerly union jobs). Then there are models that suggest wages will rise on average across all workers. This could be due to increased investment in previously more heavily unionized industries leading to higher productivity. Other possibilities include a weakening of wage compression at the top end of incomes being larger than the weakening of wage compression at the bottom end of incomes (though empirical evidence does not support this), a shift towards a larger portion of worker compensation being paid in wages as opposed to non-pecuniary benefits, or particular to the RTW case, more elastic supply as workers are do not have to worry as much about wait times or high fees to join a union or unionized workplace.

Table 4 shows the results of the estimate of RTW treatment on the log of wages (trend figures provided in Figure 5). There is no statistically significant effect across the whole population of workers, either in all industries or in high-union industries only. When the sample is split into workers covered by a union contract and those who are not, there is one coefficient that is statistically significantly different than zero (at the 5% significance level): The estimate of RTW implementation on union wages, across all industries, with controls included is a 4.19% increase in wages. This, however, is a unique result in that none of the other estimated coefficients are statistically significant. Interestingly, the coefficients for the workers covered by a union contract are all positive, while the non-union groups have all negative coefficients. This may suggest, paradoxically to the expectations of many models, that the union to non-union wage gap grows with the implementation of RTW.

This hypothesis is examined by estimating the treatment effect of RTW on the union to non-union wage gap. Across all industries, there is a statistically significant widening of the wage gap between union and non-union workers after treatment. As reported in column three of Table 5 RTW policies increase the union to non-union wage gap by 0.91 dollars (30.01%) on average even after controlling for observable differences between union and non-union workers. Among the high-union industry sample, the effect is similar but no longer statistically significant due to larger standard errors. This suggests that while there may be quite a bit of variation in the union non-union wage gap, I find evidence that it actually grows when RTW is implemented. Looking back at the point-estimates in Table 4, it looks more likely a function of increasing union wages as opposed to decreasing non-union wages. This is a somewhat surprising result, that while on net wages are not rising, it appears that union wages are rising (at least relative to non-union wages) after the implementation of RTW laws.

There are a couple of plausible explanations for this. First, this is one of the predictions of the competitive theory of monopoly unions model as proposed by Lazear (1983). By prioritizing non-union labor market clearing, firm profit optimization, and worker utility optimization, the model argues that the relative payoff from a union (as expressed in the union to non-union wage gap) needs to be worth it to entice workers into a union. Legislation such as RTW that will hurt union finances will increase the costs of running a union per person, as it develops a free-rider incentive that will strip the union of funding. Unions will need to actually raise wages to continue to entice workers now that the cost of running the union has risen. This will be at the cost of significantly lower union employment. Another possible explanation is that, as unions see their bargaining power decreased, they shift their priorities in negotiation to more clearly distinguish the potential benefits of a union and further prioritize wages at the expense of other benefits. Thus, while overall union compensation might still be falling, there could actually be an increase in union wages as unions emphasize those at the expense of other benefits in labor negotiations with firms. This would certainly fit with the expected findings of decreased non-pecuniary benefits based on previous research on the topic.

The final main labor market aggregate outcome to be measured is employment. It is clear that the percentage of workers who are in a union has declined in the treated states. Whether or not the total number of workers as a share of the prime-age labor force will increase or decrease on net may depend on a couple of factors. First, does the relative decline in unions mean that more jobs are available overall? This would be expected if one of the ways that firms react to unions negotiating higher overall compensation is by decreasing employment. Another possible reaction to union negotiations would be a shift by firms to investing more heavily in capital or some other input into production as labor becomes more expensive. RTW laws might be able to reverse that effect, even if it is likely a longer-term decision. Finally, its possible that the removal of RTW allows workers to be more flexible in changing jobs, allowing individuals to more easily change into opportunities outside of their current work.

I find that employment to prime-age labor force ratio increases by about 0.9 percentage points after the implementation of RTW (Table 6 and Figure 6). This result is only statistically significant in the all industries sample (though the point estimates are similar in the high-union industry sample). Given that average percentage of the prime-age labor force that is employed is a little under 92% in the treated states prior to the implementation of RTW, this is a roughly 1% increase in employment. Notably, however, when the log total count of individuals employed is regressed on RTW, there is a roughly 1.5% decrease (column five of Table 6). This presents a couple of possibilities. Employment counts are used from the QCEW as they are expected to be more accurate than from a survey such as the CPS. The QCEW does omit a couple categories of workers, including business proprietors, certain agricultural workers, and unincorporated self-employed individuals. I rule out that it is due to an increase in the proportion of self-employed workers relative to the appropriate comparison groups. Column 6 shows that new RTW states see a decline in the proportion of their workforce that is self-employed relative to the comparison group. I also test whether part-time work is increasing, which can also be ruled out (column 7).

Other likely explanations for these findings regarding the employment counts and employment rates have to do with the composition of the labor force. Table 7 provides SDID estimation results on the labor force composition of treated states. I can rule out significant decreases in the relative population of RTW states. Similarly, it does not appear that workers are moving into the public sector as a result of the policies on average. It appears, however, that there is an increase in the number of people not in the labor force. A decrease in the labor force would explain both slightly increasing employment to prime-age labor force ratios and a decrease in the actual number of people employed. There appears to be a 0.9% point reduction in the proportion of individuals out of the labor force for the entire population of adults between the ages of 18 and 65 (column 3) relative to the non-treated comparison group. This result suggests that RTW policies may push some people out of the labor force, such that while employment to prime-age labor force ratios rise, total employment decreases. It is not obviously clear based on the presented evidence why this is. One possible explanation, however, is that as unionization rates decrease, individuals may perceive their respective bargaining position and total compensation as decreasing. This may cause some workers to no longer consider working worthwhile and leave the labor force. This would be a particularly convincing argument if this paper provided evidence on non-pecuniary benefits and could better measure total compensation. Another potential explanation is that the decreasing union membership may be due to the union choosing to decrease employment in order to increase wages as they deal with higher operating costs. Older union members may be offered incentives to retire by the union in order to achieve this.

One of the more common arguments given by RTW proponents is that the laws will help attract individuals to their state and create a more business-friendly environment that will drive firm dynamism. I test the first claim by estimating the impact of RTW treatment on individual and household migration. I find no significant increase in net-migration into a state at either the individual or household level (see Table 8). I find no significant result when net in-migration is adjusted to be at a per capita level either. While I cannot determine if there is a difference in the composition or type of movers, I can rule out that there is a significant increase of individuals and particularly one large enough to greatly change the supply of workers in the labor market.

I then test the whether the number of establishments, firms, firm applications, and firm formations change after the implementation of a RTW law relative to the comparison group. I find that the log of the number of establishments, firms, applications, and formations all fall after the implementation of RTW policies. Table 9 shows the results from the SDID estimation. RTW implementation is associated with a 4.15% decrease in the number of establishments (though the result is no longer statistically significant in the high-union industry sample) and 2.23% decrease in the number of firms. The data on establishments comes from the QCEW, which allows for breakdowns by industry, but the data on firms comes from the BFS which cannot be similarly broken down by industry. Furthermore, the number of applications fell 7.11% and the number of new firm formations fell 5%. Together these suggest RTW laws lead to increased concentration among firms, and a decrease in dynamism as measured by new firm creation. This provides evidence against the argument that RTW laws significantly increase the business dynamism of a state, if anything suggesting the opposite.

Determining what is driving this effect likely requires additional data. While industry specific data is not-available across the number of firms, applications, and formations, I can break down the change in establishments by industry. Unions tend to be more common in larger workplaces and some of the high-union industries have significant fixed costs. While the result is not significant due to a larger standard error, the decrease in establishments has a larger point estimate in the high-union category, suggesting that the firm concentration could be concentrated there. Meanwhile, the point estimate in column 3 shows that the result is significantly smaller in low-union industries. This provides suggestive evidence that the change in number of establishments is concentrated in union-dense industries. It may be that the firms benefiting from reduced union presence are large and in industries with fixed costs, causing the firm gains to accrue to large established firms at the expense of smaller or younger firms. This would explain why the reduction in union density actually decreases the number of firms and new entrants. Alternatively, if union workers are in fact getting higher wages at the expense of lower employment as was proposed earlier, that could be forcing firms with unions to cut back on employment and establishments as a result.

One component of that the previous literature on RTW laws has not touched is how their impacts may affect different workers differently. This is an important consideration for public policy, particularly given what the previous research has suggested about the role in unions for workers who may face discrimination or other obstacles in the labor market. I examine how unionization rates, wages, and employment change for different types of workers. I first compare male and female workers across these outcomes (results in Table 10). I find that male workers see more sizable and statistically significant decreases in unionization rates in high-union industries relative to female workers (columns 3 and 4). It should also be noted that men have much higher unionization rates on average in the private sector than women, so the point estimate of a 2.7 percentage point decrease in unionization among high-union workers for men is roughly a 16.6% percentage decrease. This is actually lower than the associated decrease for female workers in the high-union sample, where the 1.66 percentage point decrease is a 19.8% decrease in unionization. However, the result for the female sample is not statistically significantly different than zero. None of the estimated effects of RTW on log wages or employment are significant for either sub-sample, though the point-estimates for employment are consistently much larger for male workers than female workers. It should be noted that using a placebo method of calculating p-values and significance, the employment results for columns 1-3 become statistically significant. Results are shown in Table A.2.

The evidence for differential impacts of RTW laws on different sub-groups of workers is slightly stronger when examining race. Table 11 reports estimates for the impact of RTW on unionization rates, wages, and employment to prime-age labor force rates for White, Black, and Hispanic workers separately. While only the White high union industry sample produces a significant result, individuals of all three races see significant point-estimate declines in unionization rate. Wages for White workers essentially do not change, but Black workers see a statistically significant 5.7% decrease in wages after the implementation of RTW. Hispanic workers see as similarly large but statistically insignificant result. The estimates of RTW on employment are somewhat noisier - for both Black and Hispanic workers, the size of the point-estimate is quite different in magnitude between the all industry sample and the high union only industry sample. None of the results are statistically significant, in contrast to the White samples which both see a statistically significant increase in employment to prime-age labor force rates (1.1 and 1.4 percentage points for all workers and workers in high union industries respectively). Even when only considering statistically significant results, White workers have better wage and employment outcomes than Black workers. Furthermore, the point-estimates hint the same could be true with White and Hispanic workers, but the standard errors are quite large making the evidence at best suggestive. Regardless, the divergent outcomes for White and Black workers may reflect that unions can provide extra benefits for workers who are underrepresented or potentially face discrimination in an industry. Whether it's due to unions extracting stronger protections against discrimination in negotiations or simply the presence of a powerful body to support workers who face discrimination, it is not difficult to imagine how workers who may face discrimination due to their race would benefit from unions, and therefore are disproportionately hurt by RTW laws.

# 6 Robustness

One of the advantages of examining five separate policy changes together is that it reduces the probability that a spurious result or situation specific omitted variable will accidentally create an effect that is not due to RTW laws. It is still possible, however, for other factors to unintentionally create a false RTW result. To help test for this, I apply a robustness test of the impact of RTW laws on markets, and specifically of their mechanism being primarily through their impact on unions. I find SDID estimates of the impact of RTW on unions, wages, and employment to prime-age labor force ratios, for individuals in the four industries with the lowest unionization rates (omitting agriculture).[10] If RTW laws are impacting labor markets, but are particularly doing so through their impact on unions, effects should be concentrated in industries with the highest union density. Finding significant labor market impacts in low-union density industries would suggest that either something other than RTW is driving the measured impacts, or that RTW is operating through other channels (perhaps significantly through spillover threat effects). Table 12 shows the resulting point-estimates. I find no statistically significant impact from RTW on aggregate measures of unionization, wages, or employment. This is also true for the White sub-sample as well as the Black sub-sample. The results suggest that RTW laws are not impacting this part of the labor force, adding evidence to the primary mechanism of RTW laws being their impact on unions.

I also provide results using estimators outside of the SDID findings. In Table A.3, the results of RTW on union membership rates is provided across a set of other popular difference-in-differences style estimators. These include a standard two-way fixed effects estimator, the Callaway and Sant'Anna (2021) estimator, the deChaisemartin and D'Haultfoeuille (2021) estimator, and the Wooldridge (2021) estimator. As these estimators require a chosen control group, they are shown first using all non-RTW states as the comparison group, then just rustbelt non-RTW states, and finally with just Missouri and Ohio (the two almost RTW

10. This includes Financial Activities (1.96% unionized), Professional and Business Services (2.64 % unionized), Leisure and Hospitality (unionized 2.79%), and Other Services (2.80%unionized). Agriculture is excluded to to the particular nature of the industry - results are robust to including it.

states) making up the comparison group. While estimates vary for each method, generally they roughly follow the pattern of the SDID estimator (provided for comparison). Appendix tables 4-8 provide further results with these alternative estimators. Crucially, results tend to change significantly depending on the control group selected, underlining just how important that choice is in estimation. Overall, the totality of the evidence from these seems to mirror the SDID estimator with regards to labor market outcomes: Union membership rates are down, wages do not seem to change, and employment as a proportion of the prime-age labor force slightly increases. The White and Black worker divergence also remains clear, suggesting the result is a robust outcome of RTW laws.

# 7 Conclusion

Despite long being a prominent and controversial policy, there has been no consensus about how exactly RTW laws impact states and labor markets. Part of this is likely due to the policy being strongly connected to political partisanship. Nearly every conservative dominated state has a RTW law in place, while essentially no democratic dominated state has one. Because of this, there is necessarily endogeneity with respect to the effects of the actual laws themselves and the labor markets they are present in. This paper works to provide clearer estimates of the reduced form effect of RTW laws on private sector labor markets. Using the recent set of five states that implemented RTW laws, I utilize a synthetic difference-in-differences estimator to provide a more plausible counterfactual for the treated states. The more union friendly nature of the afflicted states and the method with which the legislation was passed provide a more plausible identification framework for considering the implementation of the law as a quasi-exogenous treatment. I find that RTW laws do decrease union density significantly. Their impact on wages, however, does not appear to be statistically significant.

RTW laws do seem to have increased the union non-union wage gap, which seems best explained either by a model of unions focused on free-riding and additional costs, or as a change in negotiating strategy where unions shift compensation towards wages and away from other benefits to emphasize their worth and attract members. RTW laws also seem to slightly increase employment to prime-age population rates, but the effect appears to be from individuals leaving the work force as opposed to any increase in jobs. Furthermore, the potential general equilibrium effects on the labor market sometimes cited by RTW proponents do not seem to occur: There is no statistically significant change in net in-migration, and there's actually a decrease in the number of establishments, firms, and new firm activity.

There also appears to be important heterogeneity across different types of workers. While differences between male and female workers are small and statistically insignificant, there are clearer differences for workers of different races. Black workers fare significantly worse than their White counterparts in terms of both wages and employment after the implementation of RTW. Hispanic workers have similar estimated results but there's too much variance in the estimation to make a claim of a statistically significant result. This result fits squarely within the theory that unions can provide extra benefits for workers that may face discrimination in the workplace over demographic features, and as such the weakening of unions hurts those workers the most.

This paper is not able to make conclusive claims about total compensation, as data about non-pecuniary benefits is limited. Furthermore there is the potential for heterogeneity across different spaces within a state that this paper does not address. I am able to provide results both for the labor force across all private sector industries and just for the labor force within highly unionized industries. While the results tend to strengthen in magnitude in high union industries, often the lost precision means it the result is often less statistically significant and makes identifying a non-zero result more difficult.

As becomes evident when comparing the SDID results to other empirical estimators, the choice of estimation and comparison group matters significantly for the results. Rather than having to make subjective claims about the inclusion or exclusion of specific states or groups of states, the SDID estimator instead selects a comparison group based on a reasonable weighting algorithm to best match the outcome trends for each treated unit. While this does not guarantee satisfaction of the assumptions required in a difference-in-differences causal estimation framework, it provides a more flexible and observably similar group to compare a treated state against. Data permitting, future work should consider more granular units of examination and building plausible estimation frameworks for units such as a county or commuting zone. Assuming this provides more total units, it would enhance the ability of the SDID estimator to accurately build a counterfactual. It would also be important to determine whether there are distinct differences between how rural and urban areas of a state are impacted by RTW laws, or to what degree living near the border of a state might ameliorate or intensify the effects of the policy.

More broadly, this paper suggests that policymakers should be careful to reckon with how certain populations may fare worse under a RTW law. It is also unclear to what extent the apparent concentration among firms impacts the labor market or other outcomes. Broadly speaking, if RTW is being used with the purpose of developing a more entrepreneur-friendly environment, the evidence provided here suggests that they should reconsider. Further work should explore the degree to which there is any shift in the share of employment by industry and whether it targets or moves away from higher-union industries after RTW. It is also possible that the impact of RTW is highly dependent on the structure (industry composition) of a state's economy. This paper controls for broad industry category employment share as well as other characteristics of the economy (such as typical worker characteristics) but it is entirely possible that using RTW as a policy tool will have different effects in differently structured economies. Future work should emphasize these differences and to what degree the differential impacts on different workers do or do not depend on them.

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# 9 Figures

Figures are displayed below in order of reference.

Figure 1:

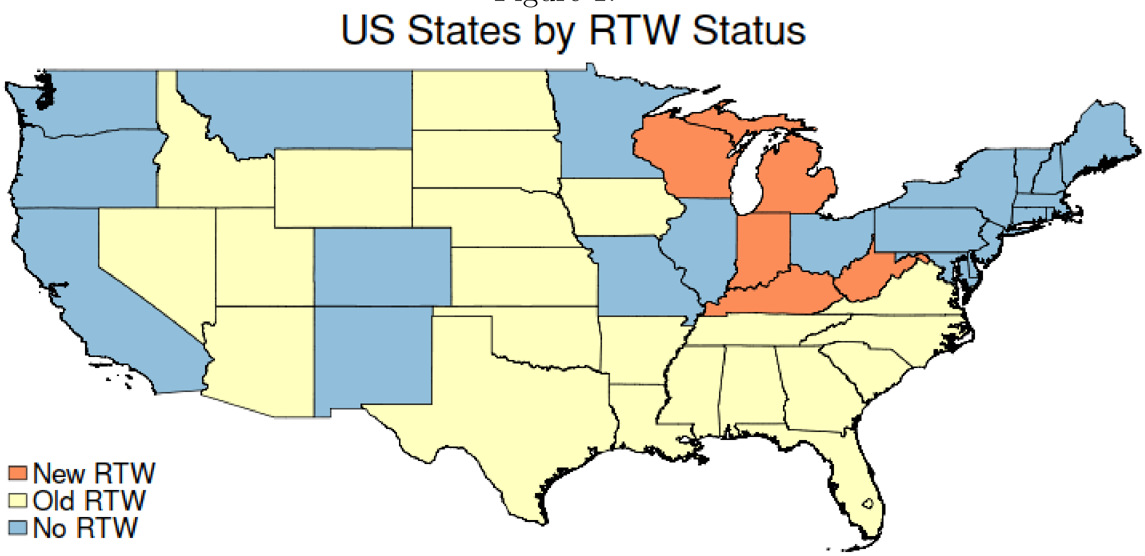



Figure 2:

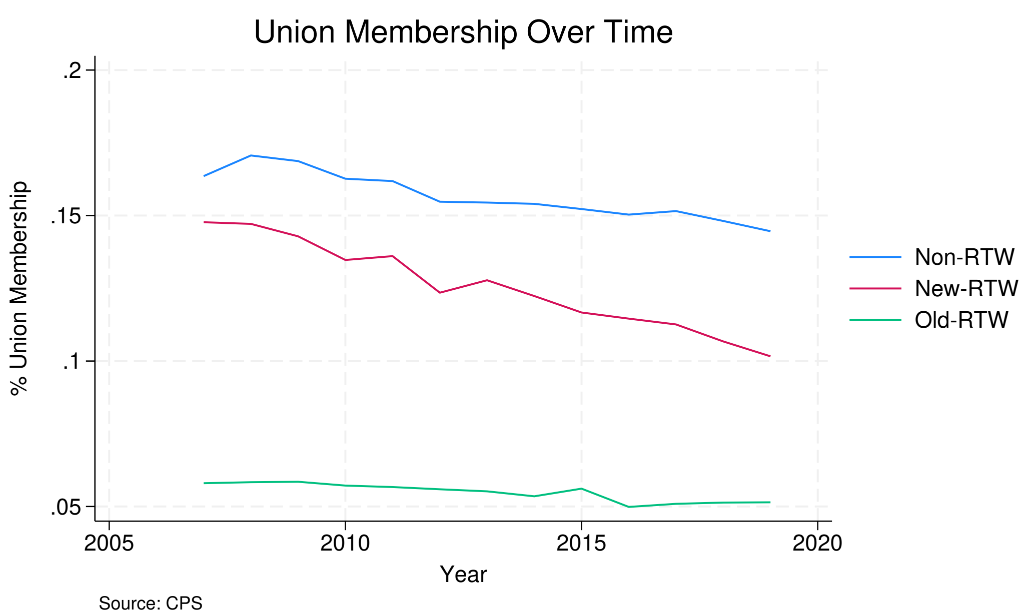


Figure 3:

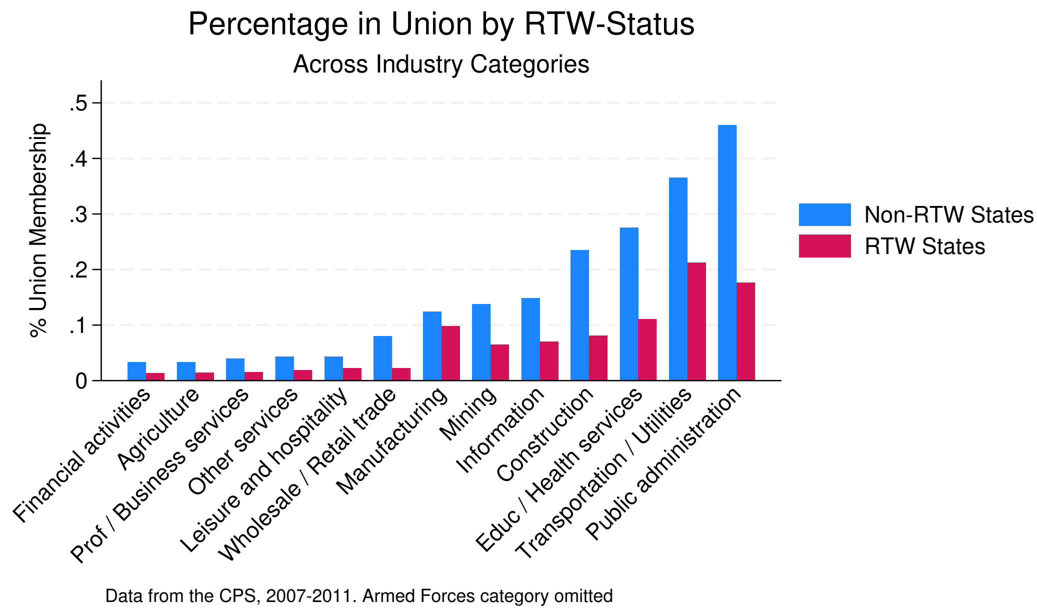



Figure 4:

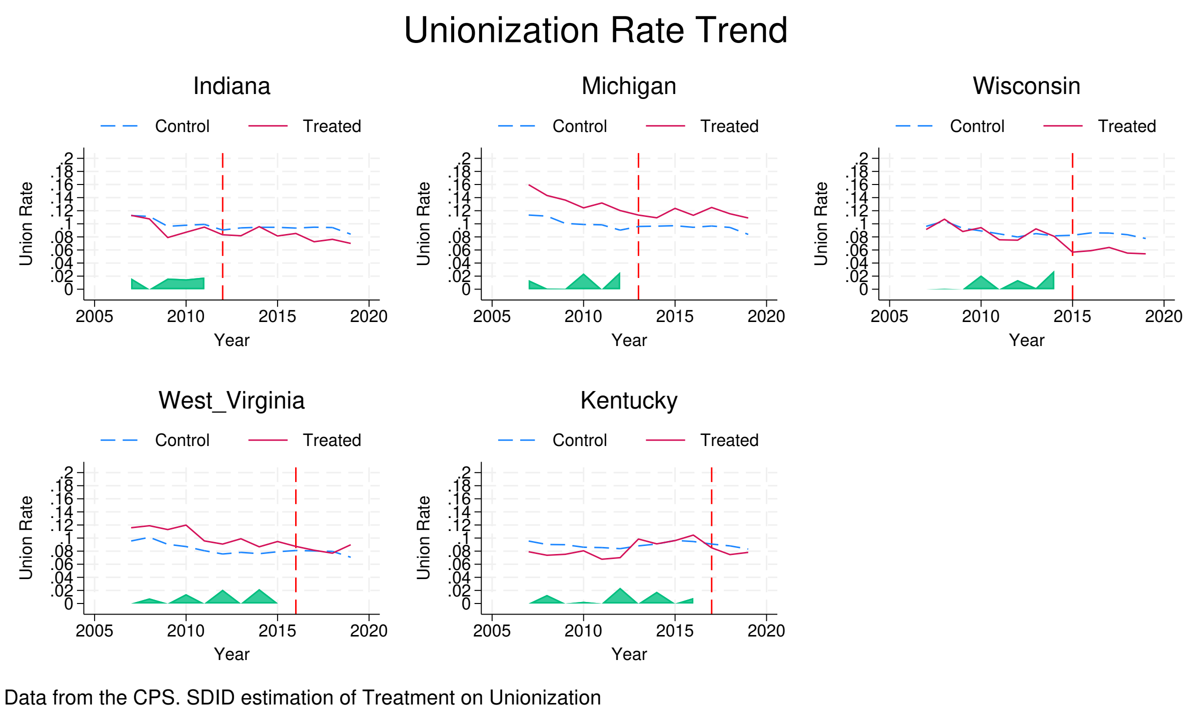
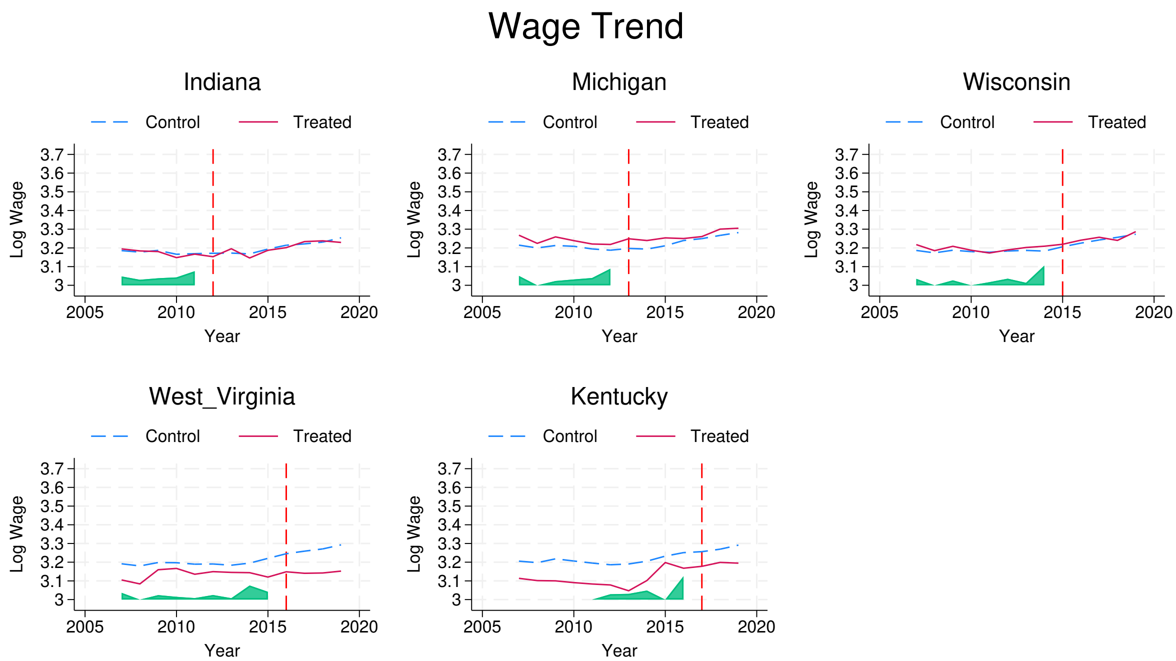
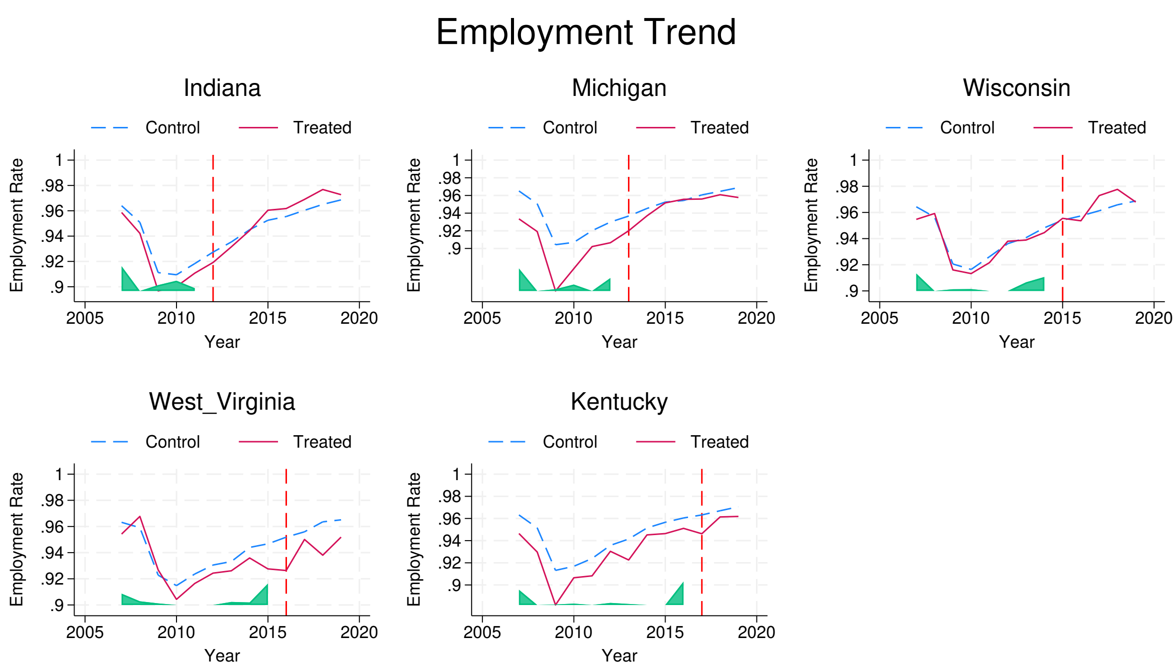


Figure 5:




Data from the CPS. SDID estimation of Treatment on Log Wages.

Figure 6:




Data from the CPS. SDID estimation of Treatment on Employment Rate.

# 10 Tables

Tables are displayed below in order of reference.

Table 1: Treated State Timeline

| | Date Law is Effective | Date Law is Enacted |
|---|---|---|
| Indiana | March 15th, 2012 | February 1st, 2012 |
| Michigan | March 28th, 2013 | December 12th, 2012 |
| Wisconsin | March 11th, 2015 | March 9th, 2015 |
| West Virginia | July 1st, 2016 | February 12th, 2016 |
| Kentucky | January 9th, 2017 | January 9th, 2017 |

Table 2: T-Test: Control vs Treated

| | Never RTW - All | | | Never RTW - Rustbelt | | | Almost RTW | | |
|---|---|---|---|---|---|---|---|---|---|
| | Control | Treated | P | Control | Treated | P | Control | Treated | P |
| union | 0.10 | 0.11 | 0.08 | 0.12 | 0.11 | 0.26 | 0.10 | 0.11 | 0.40 |
| wage | 27.12 | 23.79 | 0.00*** | 25.96 | 23.79 | 0.00*** | 24.11 | 23.79 | 0.48 |
| employed | 0.93 | 0.92 | 0.03* | 0.93 | 0.92 | 0.12 | 0.92 | 0.92 | 0.67 |
| education | 13.69 | 13.36 | 0.00*** | 13.64 | 13.36 | 0.00*** | 13.43 | 13.36 | 0.35 |
| experience | 20.43 | 20.89 | 0.01** | 20.50 | 20.89 | 0.00*** | 20.80 | 20.89 | 0.46 |
| black | 0.09 | 0.07 | 0.38 | 0.10 | 0.07 | 0.00*** | 0.11 | 0.07 | 0.00*** |
| hispanic | 0.11 | 0.04 | 0.00*** | 0.08 | 0.04 | 0.00** | 0.03 | 0.04 | 0.12 |
| asian | 0.08 | 0.02 | 0.03* | 0.04 | 0.02 | 0.00*** | 0.02 | 0.02 | 0.94 |
| other | 0.02 | 0.01 | 0.06 | 0.01 | 0.01 | 0.36 | 0.01 | 0.01 | 0.40 |
| female | 0.45 | 0.45 | 0.02* | 0.45 | 0.45 | 0.00** | 0.46 | 0.45 | 0.00** |
| married | 0.60 | 0.64 | 0.00** | 0.61 | 0.64 | 0.00*** | 0.62 | 0.64 | 0.05 |
| metro | 0.82 | 0.71 | 0.01** | 0.83 | 0.71 | 0.00*** | 0.80 | 0.71 | 0.01* |
| industry1 | 0.02 | 0.02 | 0.98 | 0.01 | 0.02 | 0.00** | 0.01 | 0.02 | 0.05* |
| industry2 | 0.01 | 0.02 | 0.00*** | 0.00 | 0.02 | 0.00** | 0.00 | 0.02 | 0.07 |
| industry3 | 0.09 | 0.08 | 0.02* | 0.08 | 0.08 | 0.32 | 0.09 | 0.08 | 0.56 |
| industry4 | 0.12 | 0.19 | 0.00*** | 0.15 | 0.19 | 0.01** | 0.18 | 0.19 | 0.66 |
| industry5 | 0.15 | 0.15 | 0.39 | 0.15 | 0.15 | 0.11 | 0.15 | 0.15 | 0.42 |
| industry6 | 0.05 | 0.05 | 0.07 | 0.05 | 0.05 | 0.91 | 0.05 | 0.05 | 0.97 |
| industry7 | 0.03 | 0.02 | 0.00*** | 0.03 | 0.02 | 0.00** | 0.02 | 0.02 | 0.43 |
| industry8 | 0.08 | 0.07 | 0.00*** | 0.09 | 0.07 | 0.00*** | 0.08 | 0.07 | 0.00** |
| industry9 | 0.14 | 0.10 | 0.00*** | 0.12 | 0.10 | 0.00*** | 0.11 | 0.10 | 0.02* |
| industry10 | 0.18 | 0.18 | 0.77 | 0.19 | 0.18 | 0.05* | 0.19 | 0.18 | 0.57 |
| industry11 | 0.09 | 0.07 | 0.01** | 0.08 | 0.07 | 0.45 | 0.07 | 0.07 | 0.37 |
| Observations | 145 | | | 55 | | | 35 | | |

Data is taken from 2007-2011 CPS. Never RTW - All includes all states that have never had a RTW law in the control group. Never RTW - Rustbelt limits the control sample to Minnesota, Iowa, Illinois, Ohio, Pennsylvania, New York (minus New York City). Almost RTW limits the control sample to Missouri and Ohio.

Table 3: SDID Estimate on Union Density

| Union Membership | (1) Unionization | (2) Unionziation Controls | (3) Unionization High-Union | (4) Unionization Controls High-Union |
|---|---|---|---|---|
| RTW | -0.0111*** | -0.0122*** | -0.0175*** | -0.0200*** |
| | (0.0040) | (0.0042) | (0.0049) | (0.0060) |
| avg | 0.0746 | 0.0746 | 0.1206 | 0.1206 |
| Controls | No | Yes | No | Yes |
| FE | Yes | Yes | Yes | Yes |
| obs | 663 | 663 | 663 | 663 |

| Union Coverage | (1) Union Coverage | (2) Union Coverage Controls | (3) Union Coverage High-Union | (4) Union Coverage Controls High-Union |
|---|---|---|---|---|
| RTW | 0.0013 | 0.0015 | 0.0010 | 0.0007 |
| | (0.0026) | (0.0027) | (0.0042) | (0.0045) |
| avg | 0.0086 | 0.0086 | 0.0131 | 0.0131 |
| Controls | No | Yes | No | Yes |
| FE | Yes | Yes | Yes | Yes |
| obs | 663 | 663 | 663 | 663 |

Data from the CPS aggregated to the state-level. SDID estimate of the effect of RTW on Unionization rates. Panel 1 provides estimates with union membership proportion of the prime-age labor force as the outcome variable. Panel 2 provides estimates with non-union members covered by a union contract proportion of the prime-age labor force as the outcome variable. Standard errors are calculated using a clustered bootstrap method. Significance levels: * - 10%; ** - 5%; *** - 1%

Table 4: SDID Estimate on Log-Wages

| All | (1) Log Wages | (2) Log Wages Controls | (3) Log Wages High-Union | (4) Log Wages Controls High-Union |
|---|---|---|---|---|
| RTW | -0.0062 | -0.0041 | -0.0102 | -0.0037 |
| | (0.0071) | (0.0074) | (0.0138) | (0.0091) |
| avg | 25.7095 | 25.7095 | 26.1553 | 26.1553 |
| Controls | No | Yes | No | Yes |
| FE | Yes | Yes | Yes | Yes |
| obs | 663 | 663 | 663 | 663 |

| Union Covered | (1) Log Wages | (2) Log Wages Controls | (3) Log Wages High-Union | (4) Log Wages Controls High-Union |
|---|---|---|---|---|
| RTW | 0.0231 | 0.0419** | 0.0287 | 0.0337 |
| | (0.0197) | (0.0208) | (0.0370) | (0.0347) |
| avg | 28.3242 | 28.3242 | 30.6221 | 30.6221 |
| Controls | No | Yes | No | Yes |
| FE | Yes | Yes | Yes | Yes |
| obs | 663 | 663 | 663 | 663 |

| Non-Union | (1) Log Wages | (2) Log Wages Controls | (3) Log Wages High-Union | (4) Log Wages Controls High-Union |
|---|---|---|---|---|
| RTW | -0.0053 | -0.0051 | -0.0107 | -0.0061 |
| | (0.0078) | (0.0084) | (0.0125) | (0.0089) |
| avg | 25.5182 | 25.5182 | 25.5598 | 25.5598 |
| Controls | No | Yes | No | Yes |
| FE | Yes | Yes | Yes | Yes |
| obs | 663 | 663 | 663 | 663 |

Data from the CPS aggregated to the state-level. SDID estimate of the effect of RTW on logged wages. Standard errors are calculated using a clustered bootstrap method. Significance levels: * - 10%; ** - 5%; *** - 1%

Table 5: SDID Estimate on Union Non-Union Wage Gap

| | (1) All Industries | (2) All Industries | (3) All Industries | (4) High-Union Industries | (5) High-Union Industries | (6) High-Union Industries |
|---|---|---|---|---|---|---|
| RTW | 1.1840** | 0.8771* | 0.9107** | 1.3954 | 0.9244 | 0.8328 |
| | (0.5827) | (0.5254) | (0.4578) | (1.1291) | (0.6721) | (0.8413) |
| avg | 3.0289 | 3.0289 | 3.0289 | 4.2571 | 4.2571 | 4.2571 |
| Education | No | No | Yes | No | No | Yes |
| Controls | No | Yes | Yes | No | Yes | Yes |
| FE | Yes | Yes | Yes | Yes | Yes | Yes |
| obs | 663 | 663 | 663 | 663 | 663 | 663 |

Data from the CPS aggregated to the state-level. SDID estimate of the effect of RTW on union non-union wage gap. The average value is a measure of the average union to non-union wage gap in the five treated-states during pre-treatment years. Controls include both the state levels of the main specification control variables and the gap in the levels of those control variables between union and non-union workers. Standard errors are calculated using a clustered bootstrap method. Significance levels: * - 10%; ** - 5%; *** - 1%

Table 6: SDID Estimate on Employment

| | (1) Employment | (2) Employment Controls | (3) Employment High-Union | (4) Employment Controls High-Union | (5) QCEW Emp Count | (6) Self-Emp | (7) Part-Time |
|---|---|---|---|---|---|---|---|
| RTW | 0.0098** | 0.0094* | 0.0105 | 0.0104 | -0.0152* | 0.0006 | -0.0037 |
| | (0.0044) | (0.0052) | (0.0064) | (0.0076) | (0.0086) | (0.0034) | (0.0029) |
| avg | 0.9448 | 0.9448 | 0.9423 | 0.9423 | 2.68e+06 | 0.1160 | 0.1423 |
| Controls | No | Yes | No | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| obs | 663 | 663 | 663 | 663 | 663 | 663 | 663 |

Data for columns 1-4 and 6-7 come from the CPS. Data for column 5 comes from the QCEW and the CPS. SDID estimate of the effect of RTW on a variety of employment metrics. Employment is measured as the number of employed individuals divided by the number of individuals in the prime-age (25-55) labor force. Part-time work is measured as the proportion of individuals working between 1-34 hours a week on average. Standard errors are calculated using a clustered bootstrap method. Significance levels: * - 10%; ** - 5%; *** - 1%

Table 7: SDID Estimate on Labor Force

| Control Group | (1) Population | (2) Public Sector | (3) In LF | (4) In LF: 18-25 | (5) In LF 25-55 | (6) In LF 55-65 |
|---|---|---|---|---|---|---|
| RTW | 4699.4660 | -0.0008 | -0.0091*** | -0.0206 | -0.0129*** | -0.0135** |
| | (85482.5400) | (0.0035) | (0.0034) | (0.0141) | (0.0036) | (0.0056) |
| avg | 6.19e+06 | 0.1492 | 0.6474 | 0.6834 | 0.8011 | 0.5924 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes | Yes | Yes |
| obs | 663 | 663 | 663 | 663 | 663 | 663 |

Data for column 1 comes from the Census Bureau intercensal population estimates and the CPS. Data for columns 2-6 come from the CPS. SDID estimate of the effect of RTW on various measure of labor force composition. The proportion in the labor force is measured as the number of adults in the labor force divided by the total number of adults. Public Sector is a measure is a measure of the proportion of the labor force working in the public sector. Standard errors are calculated using a clustered bootstrap method. Significance levels: * - 10%; ** - 5%; *** - 1%

Table 8: SDID Estimate on Migration

| Raw Number | (1) Net Migration Indiv | (2) Net Migration Indiv | (3) Net Migration Household | (4) Net Migration Household |
|---|---|---|---|---|
| RTW | 2735.7650 | 3197.2940 | 4570.3500 | 3575.9900 |
| | (4740.8110) | (7535.5300) | (2971.1350) | (3894.8970) |
| avg | -9.0302 | -9.0302 | 403.5716 | 403.5716 |
| Controls | No | Yes | No | Yes |
| FE | Yes | Yes | Yes | Yes |
| obs | 663 | 663 | 663 | 663 |
| **Per Person** | **(1) Net Migration Indiv** | **(2) Net Migration Indiv** | **(3) Net Migration Household** | **(4) Net Migration Household** |
| RTW | 0.0011 | 0.0004 | 0.0007 | 0.0002 |
| | (0.0009) | (0.0010) | (0.0005) | (0.0005) |
| avg | 0.0002 | 0.0002 | 0.0002 | 0.0002 |
| Controls | No | Yes | No | Yes |
| FE | Yes | Yes | Yes | Yes |
| obs | 663 | 663 | 663 | 663 |

Data comes from the IRS migration data and the CPS. SDID estimate of the effect of RTW on migration measures. Standard errors are calculated using a clustered bootstrap method. Significance levels: * - 10%; ** - 5%; *** - 1%

Table 9: SDID Estimate on Firms

| All Firms | (1) Log Establishments | (2) Log Establishments High Union | (3) Log Establishments Low Union | (4) Log Firms | (5) Log Applications | (6) Log Formations |
|---|---|---|---|---|---|---|
| RTW | -0.0416*** | -0.0641 | 0.0035 | -0.0223*** | -0.0711** | -0.0500*** |
| | (0.0113) | (0.0405) | (0.0167) | (0.0068) | (0.0318) | (0.0120) |
| avg | 1.84e+05 | 7.33e+04 | 7.51e+04 | 1.05e+05 | 5.49e+04 | 5717.0905 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes | Yes | Yes |
| obs | 663 | 663 | 663 | 663 | 663 | 663 |

Data for columns 1-3 comes from the QCEW and the CPS. Data for columns 4-6 comes from the BFS and the CPS. SDID estimate of the effect of RTW on firm outcomes. Standard errors are calculated using a clustered bootstrap method. Significance levels: * - 10%; ** - 5%; *** - 1%

Table 10: SDID Male vs Female

| Unionization | (1) Male | (2) Female | (3) Male High-Union | (4) Female High-Union |
|---|---|---|---|---|
| RTW | -0.0112* | -0.0123** | -0.0270*** | -0.0166 |
| | (0.0063) | (0.0061) | (0.0103) | (0.0113) |
| avg | 0.0926 | 0.0536 | 0.1627 | 0.0839 |
| Controls | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes |
| obs | 663 | 663 | 663 | 663 |

| Log Wages | (1) Male | (2) Female | (3) Male High-Union | (4) Female High-Union |
|---|---|---|---|---|
| RTW | 0.0005 | 0.0009 | 0.0067 | -0.0227 |
| | (0.0091) | (0.0091) | (0.0156) | (0.0170) |
| avg | 28.2473 | 22.7276 | 28.2445 | 24.3429 |
| Controls | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes |
| obs | 663 | 663 | 663 | 663 |

| Employment | (1) Male | (2) Female | (3) Male High-Union | (4) Female High-Union |
|---|---|---|---|---|
| RTW | 0.0117 | 0.0058 | 0.0191 | 0.0028 |
| | (0.0077) | (0.0047) | (0.0120) | (0.0070) |
| avg | 0.9434 | 0.9466 | 0.9294 | 0.9553 |
| Controls | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes |
| obs | 663 | 663 | 663 | 663 |

Data comes from the CPS. SDID estimate of the effect of RTW on labor market outcomes, by split samples. Standard errors are calculated using a clustered bootstrap method. Significance levels: * - 10%; ** - 5%; *** - 1%

Table 11: SDID by Race

| Unionization | (1) White | (2) Black | (3) Hispanic | (4) White High-Union | (5) Black High-Union | (6) Hispanic High-Union |
|---|---|---|---|---|---|---|
| RTW | -0.0078 | -0.0155 | -0.0193 | -0.0182** | -0.0120 | -0.0458 |
| | (0.0066) | (0.0119) | (0.0169) | (0.0083) | (0.0148) | (0.0283) |
| avg | 0.0725 | 0.0957 | 0.0678 | 0.1222 | 0.1221 | 0.0961 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes | Yes | Yes |
| obs | 663 | 611 | 663 | 663 | 611 | 624 |

| Log Wages | (1) White | (2) Black | (3) Hispanic | (4) White High-Union | (5) Black High-Union | (6) Hispanic High-Union |
|---|---|---|---|---|---|---|
| RTW | 0.0043 | -0.0570** | -0.0699 | 0.0057 | -0.0587 | -0.0470 |
| | (0.0089) | (0.0237) | (0.0454) | (0.0119) | (0.0390) | (0.0551) |
| avg | 27.8137 | 20.6766 | 19.6931 | 28.0958 | 21.2355 | 21.2480 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes | Yes | Yes |
| obs | 663 | 611 | 663 | 663 | 611 | 624 |

| Employment | (1) White | (2) Black | (3) Hispanic | (4) White High-Union | (5) Black High-Union | (6) Hispanic High-Union |
|---|---|---|---|---|---|---|
| RTW | 0.0106*** | 0.0161 | -0.0015 | 0.0139** | 0.0022 | 0.0237 |
| | (0.0041) | (0.0207) | (0.0127) | (0.0070) | (0.0247) | (0.0175) |
| avg | 0.9541 | 0.8981 | 0.9348 | 0.9522 | 0.9090 | 0.9248 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes | Yes | Yes |
| obs | 663 | 611 | 663 | 663 | 611 | 624 |

Data comes from the CPS. SDID estimate of the effect of RTW on labor market outcomes, by split samples. Standard errors are calculated using a clustered bootstrap method. Significance levels: * - 10%; ** - 5%; *** - 1%

Table 12: SDID Low-Union

| All Low-Union | (1) Unionization | (2) Log Wages | (3) Employment |
|---|---|---|---|
| RTW | -0.0013 | -0.0074 | 0.0076 |
| | (0.0029) | (0.0150) | (0.0046) |
| avg | 0.0285 | 25.6467 | 0.9439 |
| Controls | Yes | Yes | Yes |
| FE | Yes | Yes | Yes |
| obs | 663 | 663 | 663 |

| White Low-Union | (1) Unionization | (2) Log Wages | (3) Employment |
|---|---|---|---|
| RTW | -0.0005 | -0.0085 | 0.0074 |
| | (0.0042) | (0.0164) | (0.0048) |
| avg | 0.0246 | 27.9218 | 0.9538 |
| Controls | Yes | Yes | Yes |
| FE | Yes | Yes | Yes |
| obs | 663 | 663 | 663 |

| Black Low-Union | (1) Unionization | (2) Log Wages | (3) Employment |
|---|---|---|---|
| RTW | -0.0010 | -0.0405 | 0.0350 |
| | (0.0104) | (0.0597) | (0.0263) |
| avg | 0.0405 | 20.4148 | 0.8832 |
| Controls | Yes | Yes | Yes |
| FE | Yes | Yes | Yes |
| obs | 624 | 624 | 624 |

Data comes from the CPS. SDID estimate of the effect of RTW on labor market outcomes, for low union industries. Standard errors are calculated using a clustered bootstrap method. Significance levels: * - 10%; ** - 5%; *** - 1%

# 11 Appendix

## 11.1 Figures

Figure A.1:

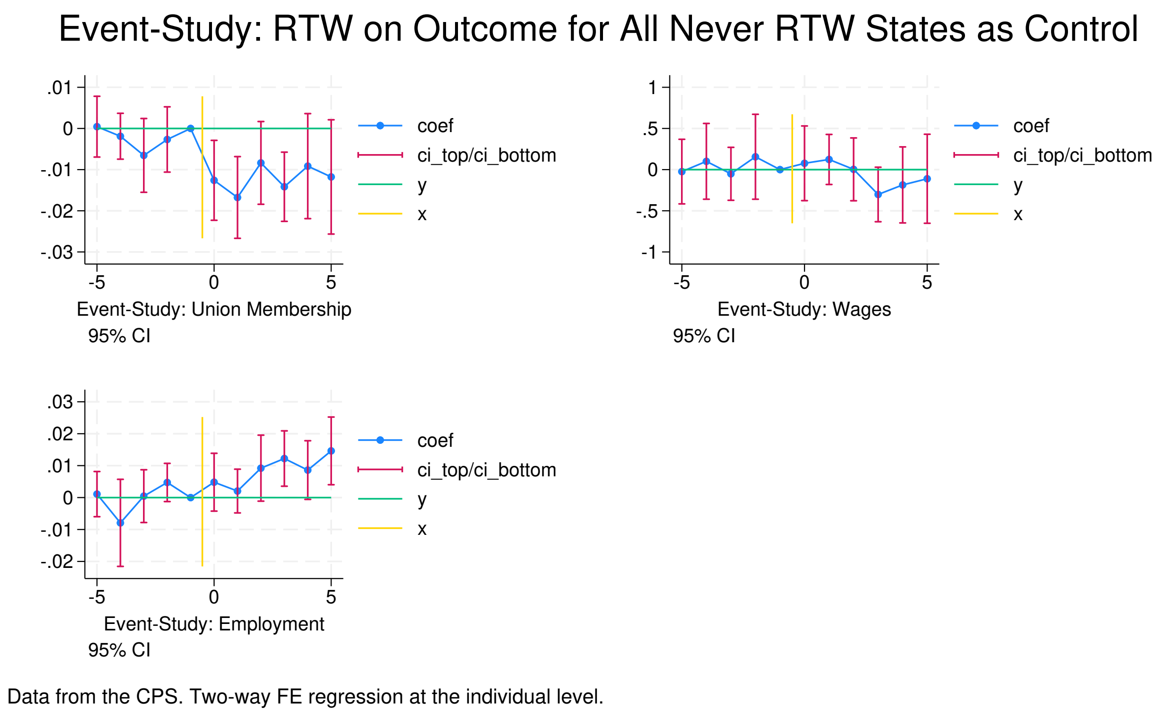
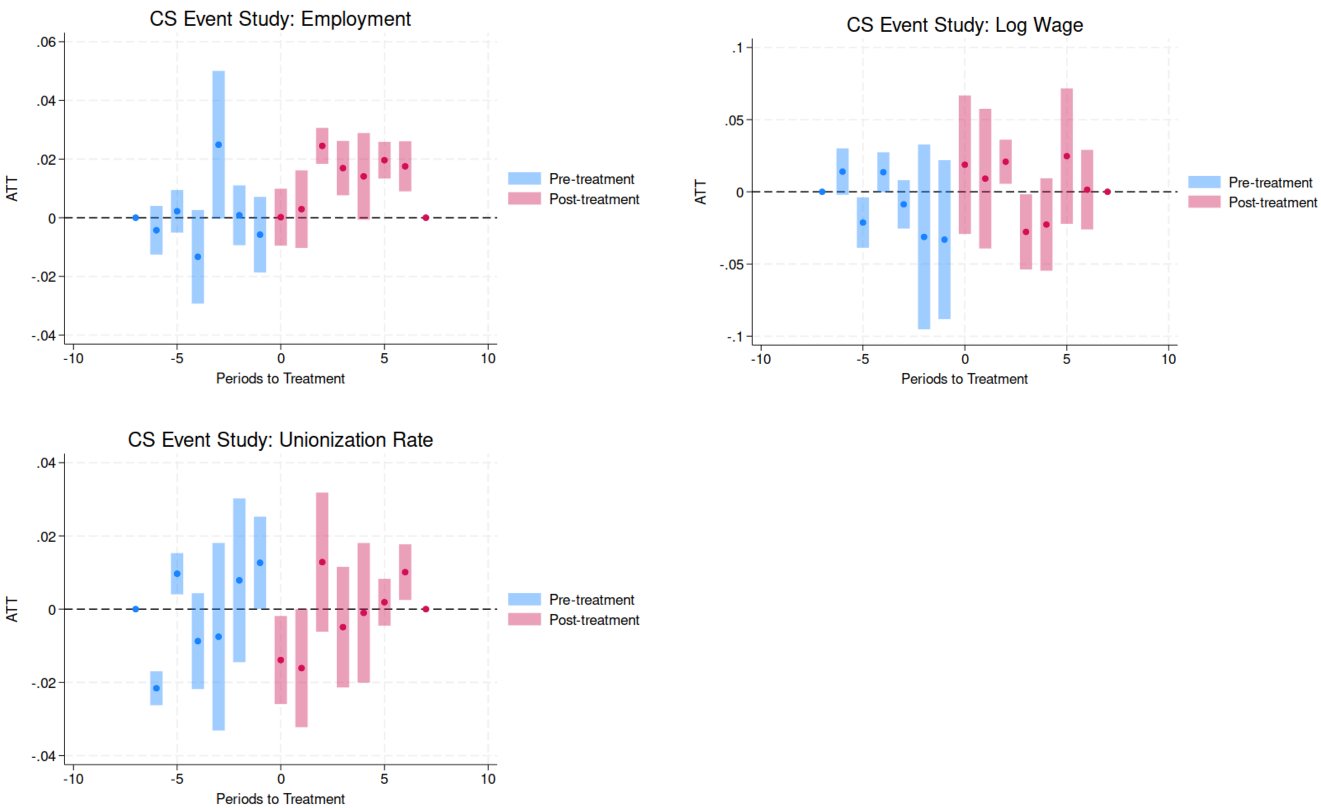
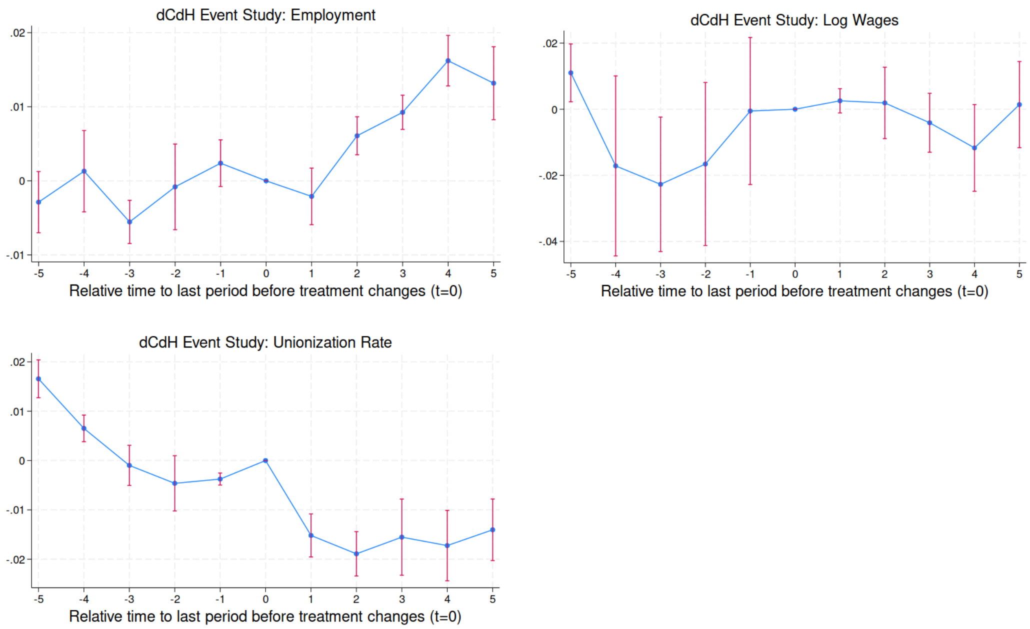


Figure A.2:



CS Event Study: Employment
.06
.04
.02
0
-.02
-.04
ATT
-10
-5
0
5
10
Periods to Treatment
Pre-treatment
Post-treatment
CS Event Study: Log Wage
.1
.05
0
-.05
-.1
ATT
-10
-5
0
5
10
Periods to Treatment
Pre-treatment
Post-treatment
CS Event Study: Unionization Rate
.04
.02
0
-.02
-.04
ATT
-10
-5
0
5
10
Periods to Treatment
Pre-treatment
Post-treatment

Figure A.3:


dCdH Event Study: Employment
.02
.01
0
-.01
-5
-4
-3
-2
-1
0
1
2
3
4
5
Relative time to last period before treatment changes (t=0)
dCdH Event Study: Log Wages
.02
0
-.02
-.04
Relative time to last period before treatment changes (t=0)
dCdH Event Study: Unionization Rate
.02
.01
0
-.01
-.02
Relative time to last period before treatment changes (t=0)

## 11.2 Tables

Table A.1: SDID Estimate on Union Membership - by State

| | SDID estimate | Std. Error | Treatment |
|---|---|---|---|
| Indiana | -.0050648 | .0034523 | 2012 |
| Michigan | -.0116459 | .0033458 | 2013 |
| Wisconsin | -.0252204 | .003791 | 2015 |
| West-Virginia | -.0128238 | .0039442 | 2016 |
| Kentucky | -.0096415 | .0047136 | 2017 |

| | SDID estimate | Std. Error | Treatment |
|---|---|---|---|
| Indiana | -.0095619 | .0052777 | 2012 |
| Michigan | -.0204541 | .0033561 | 2013 |
| Wisconsin | -.0369851 | .0072555 | 2015 |
| West-Virginia | -.019029 | .008095 | 2016 |
| Kentucky | -.0200966 | .0050529 | 2017 |

Individual state results from SDID estimation. Data from the CPS. Standard errors calculated using a clustered bootstrap. Significance levels: * - 10%; ** - 5%; *** - 1%

Table A.2: SDID Male vs Female

| Unionization | (1) Male | (2) Female | (3) Male High-Union | (4) Female High-Union |
|---|---|---|---|---|
| RTW | -0.0112** | -0.0123** | -0.0270** | -0.0166** |
| | (0.0055) | (0.0048) | (0.0116) | (0.0074) |
| avg | 0.0926 | 0.0536 | 0.1627 | 0.0839 |
| Controls | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes |
| obs | 663 | 663 | 663 | 663 |

| Log Wages | (1) Male | (2) Female | (3) Male High-Union | (4) Female High-Union |
|---|---|---|---|---|
| RTW | 0.0005 | 0.0009 | 0.0067 | -0.0227 |
| | (0.0113) | (0.0117) | (0.0204) | (0.0149) |
| avg | 28.2473 | 22.7276 | 28.2445 | 24.3429 |
| Controls | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes |
| obs | 663 | 663 | 663 | 663 |

| Employment | (1) Male | (2) Female | (3) Male High-Union | (4) Female High-Union |
|---|---|---|---|---|
| RTW | 0.0117** | 0.0058* | 0.0191*** | 0.0028 |
| | (0.0053) | (0.0032) | (0.0060) | (0.0042) |
| avg | 0.9434 | 0.9466 | 0.9294 | 0.9553 |
| Controls | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes |
| obs | 663 | 663 | 663 | 663 |

Data comes from the CPS. SDID estimate of the effect of RTW on labor market outcomes, by split samples. Standard errors are calculated using a placebo method. The estimate is re-run with each potential control state used as the “treatment" state to create placebos. The outcomes are saved and compared, with the proportion of placebos that the real treatment is more extreme than used as the p-value. Extremeness is measured using root mean squared prediction error ratios before and after the treatment period. Significance levels: * - 10%; ** - 5%; *** - 1%

Table A.3: Alternative Estimates on Union Membership

| **All** | (1) TWFE | (2) CS | (3) dCdH | (4) Wooldridge | (5) SDID |
|---|---|---|---|---|---|
| RTW | -0.0110*** | -0.0043 | -0.0163*** | -0.0136** | -0.0109** |
| | (0.0033) | (0.0072) | (0.0029) | (0.0045) | (0.0046) |
| avg | 0.0969 | 0.0934 | 0.0934 | 0.0934 | 0.0934 |
| Controls | Yes | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes | Yes |
| obs | 696,860 | 367 | 227 | 377 | 377 |

| **Rustbelt** | (1) TWFE | (2) CS | (3) dCdH | (4) Wooldridge | (5) SDID |
|---|---|---|---|---|---|
| RTW | -0.0087** | 0.0024*** | -0.0160*** | -0.0087 | -0.0005 |
| | (0.0039) | (0.0000) | (0.0048) | (0.0070) | (0.0066) |
| avg | 0.0976 | 0.1056 | 0.1056 | 0.1056 | 0.1056 |
| Controls | Yes | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes | Yes |
| obs | 256,931 | 126 | 83 | 143 | 143 |

| **Almost RTW** | (1) TWFE | (2) CS | (3) dCdH | (4) Wooldridge | (5) SDID |
|---|---|---|---|---|---|
| RTW | -0.0109*** | 0.0032 | -0.0159*** | -0.0081 | -0.0949 |
| | (0.0039) | (0.0103) | (0.0055) | (0.0083) | (0.0822) |
| avg | 0.0937 | 0.0990 | 0.0990 | 0.0990 | 0.0990 |
| Controls | Yes | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes | Yes |
| obs | 146,540 | 91 | 51 | 91 | 91 |

Data comes from the CPS. Presented are alternative difference-in-differences style estimators of the effect of RTW on union membership. Column 1 presents a classic two-way fixed effect model at the individual (as opposed to state) level. Column 2 is the Callaway and Sant'anna estimator. Column 3 is the de Chaisemartin and D'Haultefoeuille estimator. Column 4 is the Wooldridge imputation two-way fixed effect estimator. Column 5 is the SDID estimator. The 3 panels represent different comparison groups. 1st is all non-RTW states. Then all rustbelt non-RTW states. Finally, the control group is limited to Missouri and Ohio. In this panel, the Callaway and Sant'anna estimator does not estimate due to not having enough control units. Significance levels: * - 10%; ** - 5%; *** - 1%

Table A.4: Alternative Estimates on Wages

| **All Non-RTW** | (1) TWFE | (2) CS | (3) dCdH | (4) Wooldridge | (5) SDID |
|---|---|---|---|---|---|
| RTW | -0.0004 | 0.0002 | -0.0019 | -0.0020 | -0.0041 |
| | (0.0052) | (0.0148) | (0.0033) | (0.0080) | (0.0074) |
| avg | 27.7904 | 26.9201 | 26.9201 | 26.9201 | 25.7095 |
| Controls | Yes | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes | Yes |
| obs | 668,526 | 367 | 227 | 377 | 663 |

| **Rustbelt** | (1) TWFE | (2) CS | (3) dCdH | (4) Wooldridge | (5) SDID |
|---|---|---|---|---|---|
| RTW | -0.0032 | -0.0002*** | -0.0028 | -0.0143** | -0.0264 |
| | (0.0056) | (0.0000) | (0.0084) | (0.0058) | (0.0227) |
| avg | 25.8877 | 25.5068 | 25.5068 | 25.5068 | 25.5068 |
| Controls | Yes | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes | Yes |
| obs | 247,414 | 126 | 83 | 143 | 143 |

| **Almost RTW** | (1) TWFE | (2) CS | (3) dCdH | (4) Wooldridge | (5) SDID |
|---|---|---|---|---|---|
| RTW | -0.0039 | -0.0129 | -0.0075 | -0.0178 | -0.0353 |
| | (0.0051) | (0.0211) | (0.0103) | (0.0100) | (0.0580) |
| avg | 24.8478 | 24.3400 | 24.3400 | 24.3400 | 24.3400 |
| Controls | Yes | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes | Yes |
| obs | 141,206 | 91 | 51 | 91 | 91 |

Data comes from the CPS. Presented are alternative difference-in-differences style estimators of the effect of RTW on log wages. Column 1 presents a classic two-way fixed effect model at the individual (as opposed to state) level. Column 2 is the Callaway and Sant'anna estimator. Column 3 is the de Chaisemartin and D'Haultefoeuille estimator. Column 4 is the Wooldridge imputation two-way fixed effect estimator. Column 5 is the SDID estimator. The 3 panels represent different comparison groups. 1st is all non-RTW states. Then all rustbelt non-RTW states. Finally, the control group is limited to Missouri and Ohio. In this panel, the Callaway and Sant'anna estimator does not estimate due to not having enough control units. Significance levels: * - 10%; ** - 5%; *** - 1%

Table A.5: Alternative Estimates on Employment

| **All Non-RTW** | (1) TWFE | (2) CS | (3) dCdH | (4) Wooldridge | (5) SDID |
|---|---|---|---|---|---|
| RTW | 0.0107** | 0.0118** | 0.0078*** | 0.0085** | 0.0078 |
| | (0.0048) | (0.0054) | (0.0011) | (0.0029) | (0.0068) |
| avg | 0.9397 | 0.9426 | 0.9426 | 0.9426 | 0.9426 |
| Controls | Yes | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes | Yes |
| obs | 807,571 | 367 | 227 | 377 | 377 |

| **Rustbelt** | (1) TWFE | (2) CS | (3) dCdH | (4) Wooldridge | (5) SDID |
|---|---|---|---|---|---|
| RTW | 0.0126* | 0.0233*** | 0.0075** | 0.0098** | 0.0069 |
| | (0.0067) | (0.0000) | (0.0031) | (0.0041) | (0.0129) |
| avg | 0.9370 | 0.9403 | 0.9403 | 0.9403 | 0.9403 |
| Controls | Yes | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes | Yes |
| obs | 261,966 | 126 | 83 | 143 | 143 |

| **Almost RTW** | (1) TWFE | (2) CS | (3) dCdH | (4) Wooldridge | (5) SDID |
|---|---|---|---|---|---|
| RTW | 0.0082 | 0.0172** | 0.0084* | 0.0061 | -0.0200 |
| | (0.0055) | (0.0087) | (0.0045) | (0.0051) | (0.0242) |
| avg | 0.9379 | 0.9381 | 0.9381 | 0.9381 | 0.9381 |
| Controls | Yes | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes | Yes |
| obs | 167,614 | 91 | 51 | 91 | 91 |

Data comes from the CPS. Presented are alternative difference-in-differences style estimators of the effect of RTW on employment rates. Column 1 presents a classic two-way fixed effect model at the individual (as opposed to state) level. Column 2 is the Callaway and Sant'anna estimator. Column 3 is the de Chaisemartin and D'Haultefoeuille estimator. Column 4 is the Wooldridge imputation two-way fixed effect estimator. Column 5 is the SDID estimator. The 3 panels represent different comparison groups. 1st is all non-RTW states. Then all rustbelt non-RTW states. Finally, the control group is limited to Missouri and Ohio. In this panel, the Callaway and Sant'anna estimator does not estimate due to not having enough control units. Significance levels: * - 10%; ** - 5%; *** - 1%

Table A.6: Alternative Estimates on Unionization for White vs Black

| **All** | (1) TWFE White | (2) TWFE Black | (3) CS White | (4) CS Black | (5) dCdH White | (6) dCdH Black | (7) Wooldridge White | (8) Wooldridge Black |
|---|---|---|---|---|---|---|---|---|
| RTW | -0.0109*** | -0.0079 | -0.0053 | -0.0558* | -0.0131*** | -0.0438*** | -0.0157*** | -0.0101 |
| | (0.0033) | (0.0086) | (0.0051) | (0.0286) | (0.0013) | (0.0094) | (0.0036) | (0.0070) |
| avg | 0.0944 | 0.1410 | 0.0827 | 0.1051 | 0.0827 | 0.1051 | 0.0827 | 0.1051 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| obs | 481,039 | 54,037 | 369 | 376 | 227 | 226 | 377 | 376 |

| **Rustbelt** | (1) TWFE White | (2) TWFE Black | (3) CS White | (4) CS Black | (5) dCdH White | (6) dCdH Black | (7) Wooldridge White | (8) Wooldridge Black |
|---|---|---|---|---|---|---|---|---|
| RTW | -0.0088** | -0.0073 | -0.0008 | -0.0050 | -0.0132*** | -0.0018 | -0.0100* | 0.0102 |
| | (0.0041) | (0.0096) | (0.0020) | (0.0047) | (0.0032) | (0.0361) | (0.0047) | (0.0077) |
| avg | 0.0963 | 0.1308 | 0.0956 | 0.1269 | 0.0956 | 0.1269 | 0.0956 | 0.1269 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| obs | 210,265 | 20,719 | 115 | 124 | 83 | 83 | 143 | 143 |

| **Almost RTW** | (1) TWFE White | (2) TWFE Black | (3) CS White | (4) CS Black | (5) dCdH White | (6) dCdH Black | (7) Wooldridge White | (8) Wooldridge Black |
|---|---|---|---|---|---|---|---|---|
| RTW | -0.0114** | -0.0030 | 0.0061*** | 0.0000 | -0.0106* | -0.0117 | -0.0155 | 0.0167* |
| | (0.0046) | (0.0128) | (0.0000) | (.) | (0.0057) | (0.0188) | (0.0100) | (0.0082) |
| avg | 0.0924 | 0.1211 | 0.0906 | 0.1089 | 0.0906 | 0.1089 | 0.0906 | 0.1089 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| obs | 124,553 | 11,537 | 38 | 31 | 51 | 51 | 91 | 91 |

Data comes from the CPS. Presented are alternative difference-in-differences style estimators of the effect of RTW on unionization rates. Columns 1 & 2 presents a classic two-way fixed effect model at the individual (as opposed to state) level. Columns 3 & 4 are the Callaway and Sant'anna estimator. Columns 5 & 6 are the de Chaisemartin and D'Haultefoeuille estimator. Columns 7 & 8 are the Wooldridge imputation two-way fixed effect estimator. The 3 panels represent different comparison groups. 1st is all non-RTW states. Then all rustbelt non-RTW states. Finally, the control group is limited to Missouri and Ohio. In this panel, the Callaway and Sant'anna estimator does not estimate for the Black subsample due to not having enough of a sample. Similarly, the Wooldridge imputation estimator was unable to calculate a standard error in panel 1 column 7. Significance levels: * - 10%; ** - 5%; *** - 1%

Table A.7: Alternative Estimates on Log Wages for White vs Black

| **All** | (1) TWFE White | (2) TWFE Black | (3) CS White | (4) CS Black | (5) dCdH White | (6) dCdH Black | (7) Wooldridge White | (8) Wooldridge Black |
|---|---|---|---|---|---|---|---|---|
| RTW | 0.0079* | -0.0114 | 0.0015 | -0.0797* | 0.0162* | -0.0779*** | 0.0031 | -0.0510 |
| | (0.0044) | (0.0113) | (0.0142) | (0.0475) | (0.0097) | (0.0172) | (0.0088) | |
| avg | 29.6659 | 22.4176 | 29.0460 | 21.8786 | 29.0460 | 21.8786 | 29.0460 | 21.8786 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| obs | 479,621 | 53,864 | 369 | 376 | 227 | 226 | 377 | 376 |

| **Rustbelt** | (1) TWFE White | (2) TWFE Black | (3) CS White | (4) CS Black | (5) dCdH White | (6) dCdH Black | (7) Wooldridge White | (8) Wooldridge Black |
|---|---|---|---|---|---|---|---|---|
| RTW | 0.0026 | -0.0192 | -0.0025 | -0.0222** | 0.0165 | 0.0060 | 0.0007 | -0.0033 |
| | (0.0055) | (0.0126) | (0.0111) | (0.0097) | (0.0184) | (0.0698) | (0.0108) | (0.0258) |
| avg | 26.9064 | 20.3536 | 26.7091 | 20.1544 | 26.7091 | 20.1544 | 26.7091 | 20.1544 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| obs | 209,725 | 20,668 | 115 | 124 | 83 | 83 | 143 | 143 |

| **Almost RTW** | (1) TWFE White | (2) TWFE Black | (3) CS White | (4) CS Black | (5) dCdH White | (6) dCdH Black | (7) Wooldridge White | (8) Wooldridge Black |
|---|---|---|---|---|---|---|---|---|
| RTW | 0.0021 | -0.0321*** | 0.0221*** | 0.0000 | 0.0080 | -0.0530* | -0.0130 | -0.0115 |
| | (0.0045) | (0.0113) | (0.0000) | (.) | (0.0088) | (0.0311) | (0.0141) | (0.0319) |
| avg | 25.6509 | 19.6708 | 25.1114 | 19.3778 | 25.1114 | 19.3778 | 25.1114 | 19.3778 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| obs | 124,270 | 11,513 | 38 | 31 | 51 | 51 | 91 | 91 |

Data comes from the CPS. Presented are alternative difference-in-differences style estimators of the effect of RTW on log wages. Columns 1 & 2 presents a classic two-way fixed effect model at the individual (as opposed to state) level. Columns 3 & 4 are the Callaway and Sant'anna estimator. Columns 5 & 6 are the de Chaisemartin and D'Haultefoeuille estimator. Columns 7 & 8 are the Wooldridge imputation two-way fixed effect estimator. The 3 panels represent different comparison groups. 1st is all non-RTW states. Then all rustbelt non-RTW states. Finally, the control group is limited to Missouri and Ohio. In this panel, the Callaway and Sant'anna estimator does not estimate for the Black subsample due to not having enough of a sample. Similarly, the Wooldridge imputation estimator was unable to calculate a standard error in panel 1 column 8. Significance levels: * - 10%; ** - 5%; *** - 1%

Table A.8: Alternative Estimates on Employment for White vs Black

| **All** | (1)<br>TWFE<br>White | (2)<br>TWFE<br>Black | (3)<br>CS<br>White | (4)<br>CS<br>Black | (5)<br>dCdH<br>White | (6)<br>dCdH<br>Black | (7)<br>Wooldridge<br>White | (8)<br>Wooldridge<br>Black |
|---|---|---|---|---|---|---|---|---|
| RTW | 0.0112*** | 0.0228** | 0.0129*** | 0.0458** | 0.0095*** | 0.0164** | 0.0112*** | 0.0058 |
| | (0.0040) | (0.0101) | (0.0014) | (0.0208) | (0.0020) | (0.0079) | (0.0034) | (0.0160) |
| avg | 0.9486 | 0.8914 | 0.9512 | 0.8940 | 0.9512 | 0.8940 | 0.9512 | 0.8940 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| obs | 582,539 | 65,528 | 369 | 377 | 227 | 227 | 377 | 377 |
| **Rustbelt** | (1)<br>TWFE<br>White | (2)<br>TWFE<br>Black | (3)<br>CS<br>White | (4)<br>CS<br>Black | (5)<br>dCdH<br>White | (6)<br>dCdH<br>Black | (7)<br>Wooldridge<br>White | (8)<br>Wooldridge<br>Black |
| RTW | 0.0116*** | 0.0253** | 0.0101*** | 0.0317*** | 0.0070* | -0.0047 | 0.0050 | -0.0030 |
| | (0.0041) | (0.0120) | (0.0016) | (0.0067) | (0.0038) | (0.0261) | (0.0050) | (0.0122) |
| avg | 0.9478 | 0.8819 | 0.9478 | 0.8815 | 0.9478 | 0.8815 | 0.9478 | 0.8815 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| obs | 248,774 | 25,071 | 115 | 124 | 83 | 83 | 143 | 143 |
| **Almost RTW** | (1)<br>TWFE<br>White | (2)<br>TWFE<br>Black | (3)<br>CS<br>White | (4)<br>CS<br>Black | (5)<br>dCdH<br>White | (6)<br>dCdH<br>Black | (7)<br>Wooldridge<br>White | (8)<br>Wooldridge<br>Black |
| RTW | 0.0078 | 0.0196 | -0.0001*** | 0.0000 | 0.0052 | -0.0007 | 0.0070 | -0.0014 |
| | (0.0049) | (0.0138) | (0.0000) | (.) | (0.0057) | (0.0215) | (0.0046) | |
| avg | 0.9449 | 0.8813 | 0.9444 | 0.8800 | 0.9444 | 0.8800 | 0.9444 | 0.8800 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| obs | 147,437 | 14,011 | 38 | 31 | 51 | 51 | 91 | 91 |

Data comes from the CPS. Presented are alternative difference-in-differences style estimators of the effect of RTW on employment rates. Columns 1 & 2 presents a classic two-way fixed effect model at the individual (as opposed to state) level. Columns 3 & 4 are the Callaway and Sant'anna estimator. Columns 5 & 6 are the de Chaisemartin and D'Haultefoeuille estimator. Columns 7 & 8 are the Wooldridge imputation two-way fixed effect estimator. The 3 panels represent different comparison groups. 1st is all non-RTW states. Then all rustbelt non-RTW states. Finally, the control group is limited to Missouri and Ohio. In this panel, the Callaway and Sant'anna estimator does not estimate for the Black subsample due to not having enough of a sample. Similarly, the Wooldridge imputation estimator was unable to calculate a standard error in panel 3 column 8. Significance levels: * - 10%; ** - 5%; *** - 1%