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

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 incentivizing firms to form within or migrate into a state. 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. 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. The recent implementation of RTW laws provides a new opportunity to test their impact on a variety of 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. The most obvious channel through which RTW policies may impact labor markets is a reduction in unionization and union power. 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 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.

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 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. I am also able to use some of the recent applied econometric advances that were not available to researchers during some of the earlier work on the topic. Relative to those that have had the opportunity to use some of the modern econometric toolkit, this paper has the advantage of being able to look at five separate policy changes all together, making the result slightly more generalizable than work that focused on just one or two states.

I find that as might be expected, unionization rates decrease, particularly in industries where unions are more prevalent. However, workers covered by unions do not see a decrease in their wages, and may even see positive effects on their wages relative to non-union workers. On aggregate, there is no statistically significant change in wages from the RTW implementations. 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 it being due to a change in the percentage of those self-employed.

The zero aggregate wage change result masks heterogeneity across workers. Black workers see their wages fall on average relative to 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. There is no statistically significant difference between female and male wages, though there does appear to be an increase in the union wage gap relative to non-union workers after RTW is implemented. There are a few factors that might be driving this result, 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 education, age, or other observables that could to be driving this change, while suggestive

The rest of the paper is organized as follows. Section two provides context about RTW laws and unions, and situates the ndings 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 ve 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 rst 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 codi ed 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 speci es 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 rms must engage with any chosen representative of the workers in good faith. This led to the standard of non-union

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 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 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 does not distinguish which hypothesis best describes the e ect found, beyond some suggestive evidence. But it is interested in distinguishing 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 di erences 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 di erences across regions (such as geography or history). If one of the causal impact hypotheses is correct, regardless of which one, this could lead to signi cant di erences in the economies of even otherwise very similar areas if one of them has a RTW law and the other does not. Determining whether 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. 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 to 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 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.

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 just due to RTW laws, their border-pairs discontinuity design is more plausible a policy change. Most recently, Murphy (

di erences (SDID) estimator for panel data, provided by Arkhangelsky et al. (2021), es-

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 robustness, 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.[8] 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.

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

8. 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.

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 di cult 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 noti ed 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 re ects 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 ve 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 e ects entirely, I provide results for individuals exclusively in a low-union" industry. If RTW us primarily working divided by the total labor force between twenty- ve and fty- ve. 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 speci cally 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

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, whether wages fall, rise, or are una ected on aggregate varies substantially model to model. 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 e ect is zero due to a ow 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

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negative Thisimsugges,doad4a oo2-394(h(e)-394exonion)c2(ltionis)-394(of)-394manby modell, 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. The result is only significant at the ten percent level of significance, but as reported in column three of Table 5 RTW policies increase the union to non-union wage gap by 1.36 dollars on average. Among the high-union industry sample, the effect is larger but no longer statistically significant. This suggests that while there is quite a bit of variation in the union non-union wage gap, I find weak evidence that it actually grows when RTW is implemented. Looking back at the point-estimates in Table 4

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 rates increase by about 0.9% 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 employment 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 RTW ployment 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 rates 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 rates 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. Other research has suggested this may be occurring; this paper, however, cannot make such a claim.

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, rms, rm applications, and rm 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 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 whites 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 rates (1.1% points and 1.4% 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 bene t from unions, and therefore are disproportionately hurt by RTW laws.

6 Robustness

One of the advantages of examining ve separate policy changes together is that it reduces the probability that a spurious result or situation speci c omitted variable will accidentally create an e ect 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 speci cally of their mechanism being primarily through their impact on unions. I nd SDID estimates of the impact of RTW on unions, wages, and employment, for individuals in the four industries with the lowest unionization rates (omitting agriculture).[9] If RTW laws are impacting labor markets, but are particularly doing so through their impact on unions, e ects should be concentrated in industries with the highest union density. Finding signi cant 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 signi cantly through spillover threat e ects). Table 12 shows the resulting point-estimates. I nd no statistically signi cant 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 ndings. In Table A.3, the results of RTW on union membership rates is provided across a set of other popular di erence-in-di erences style estimators. These include a standard two-way xed e ects estimator, the Callaway and Sant'Anna (2021) estimator, the deChaisemartin and D'Haultfoeuille (2021)

9. 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.

estimator, and the Wooldridge (2021) estimator. AS these estimators require a chosen consignificantly. 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 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 allowing, future work should consider more granular units of examination and building plausible estimation frameworks for unit such as a county. 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.

8 References

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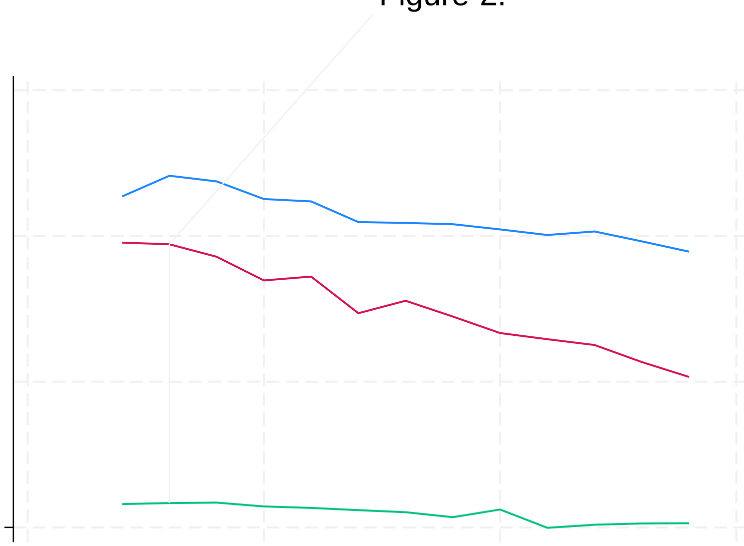
9 Figures

Figures are displayed below in order of reference.

Figure 1:

[redacted]

Figure 2:



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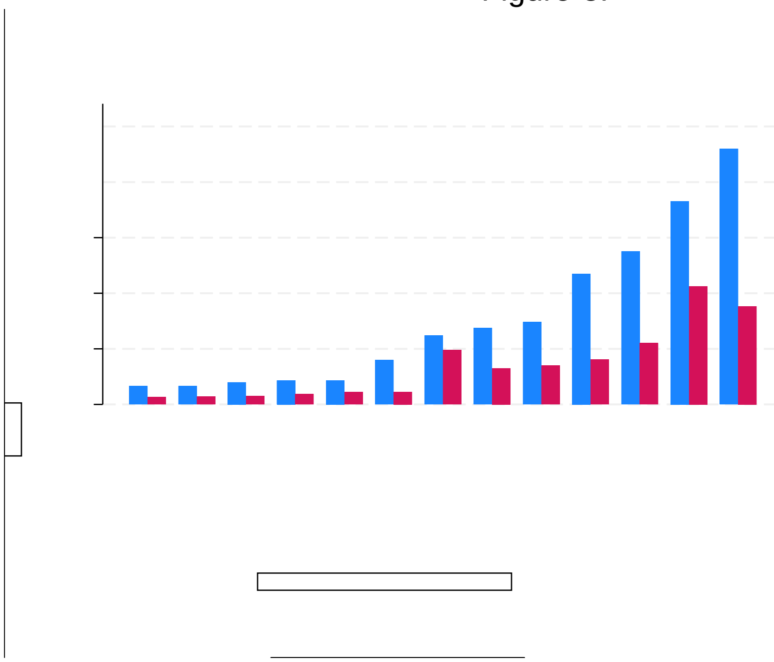


Figure 4:

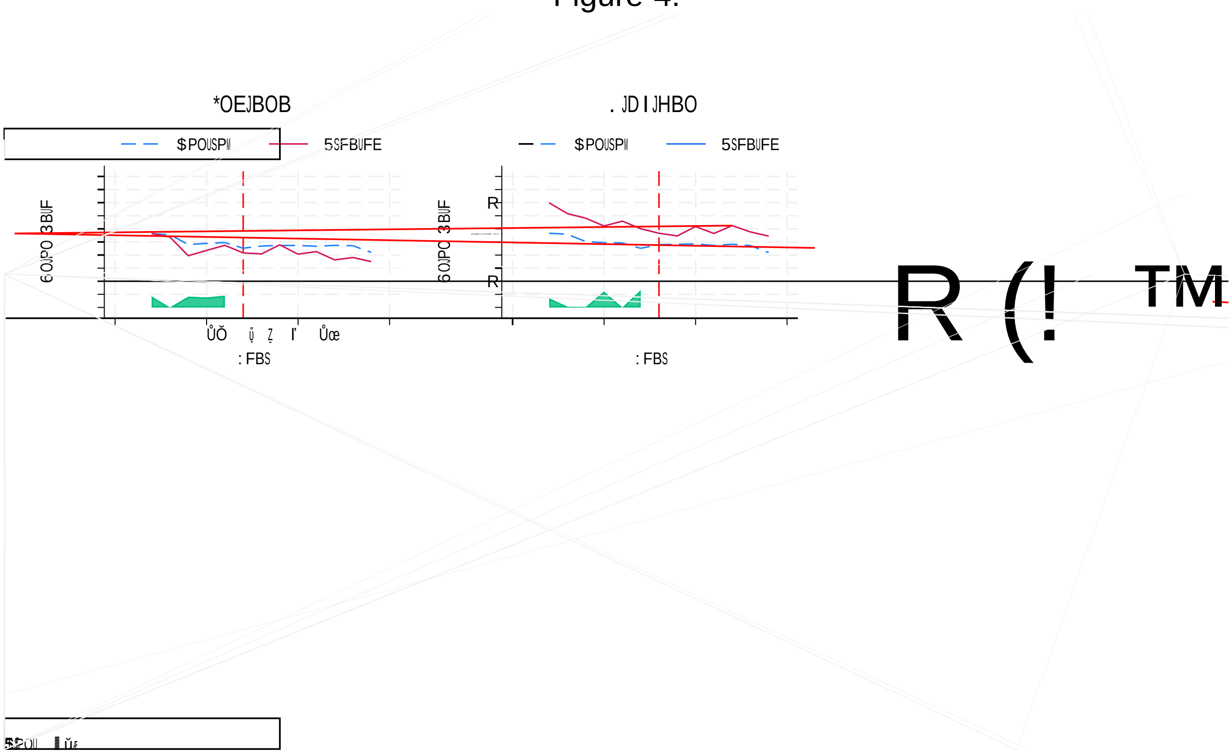


Figure 5:

Figure 6:

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	0.1902	0.00						

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**	1.3138*	1.3619*	1.3954	1.4585	1.5308
	(0.5827)	(0.7265)	(0.7276)	(1.1291)	(1.2199)	(1.2065)
avg	25.7095	25.7095	25.7095	26.1553	26.1553	26.1553
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

Table 7: SDID Estimate on Labor Force

Control

Table 9: SDID Estimate on Firms

All Firms	(1) Log Establishments	(2) Log Establishments High Union	(3) Log Firms	(4) Log Applications	(5) Log Formations
RTW	-0.0416***	-0.0633	-0.0223***	-0.0711**	-0.0500***
	(0.0113)	(0.0408)	(0.0068)	(0.0318)	(0.0120)
avg	1.84e+05	7.39e+04	1.05e+05	5.49e+04	5717.0905
Controls	Yes	Yes	Yes	Yes	Yes
FE	Yes	Yes	Yes	Yes	Yes
obs	663	663	663	663	663

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)

11 Appendix

11.1 Figures

Figure A.1:

Figure A.2:

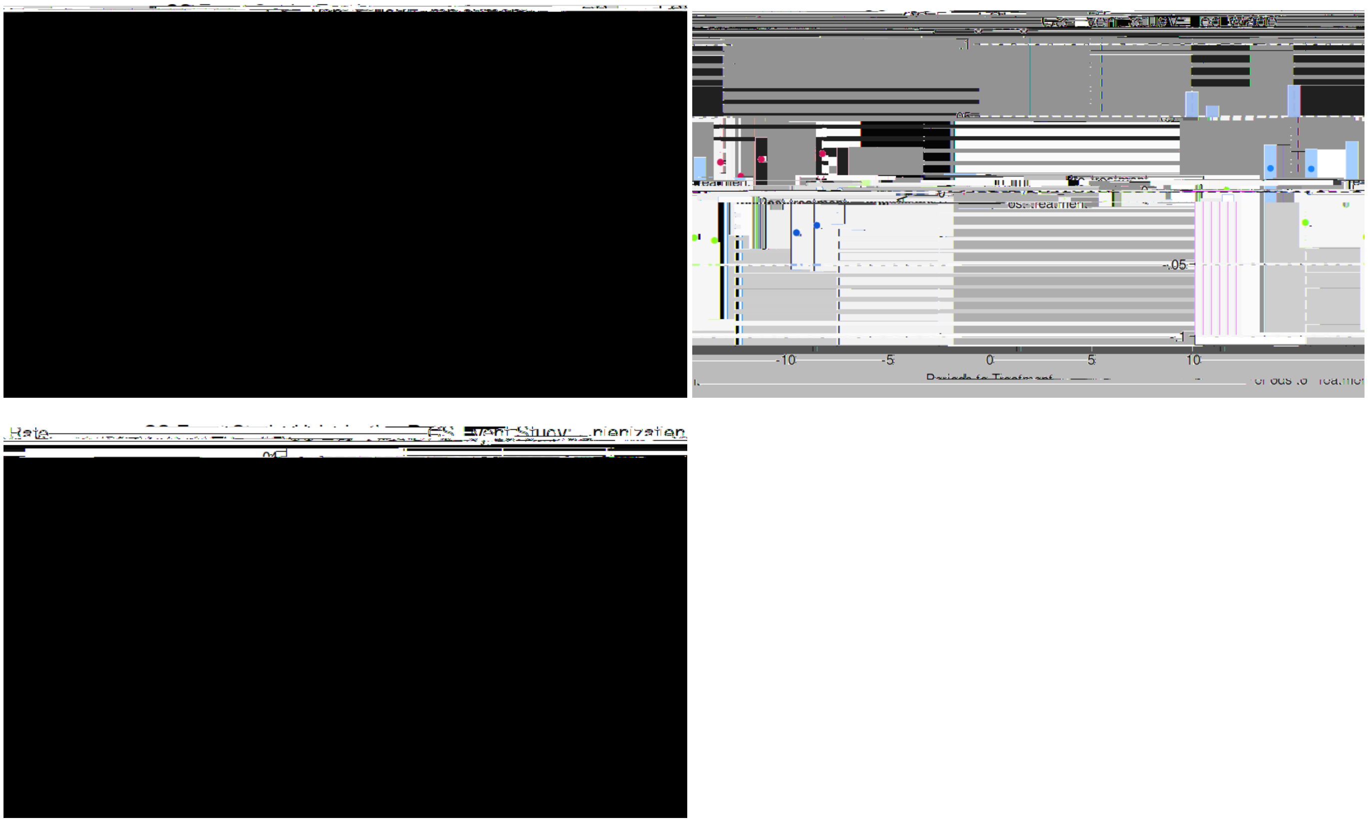
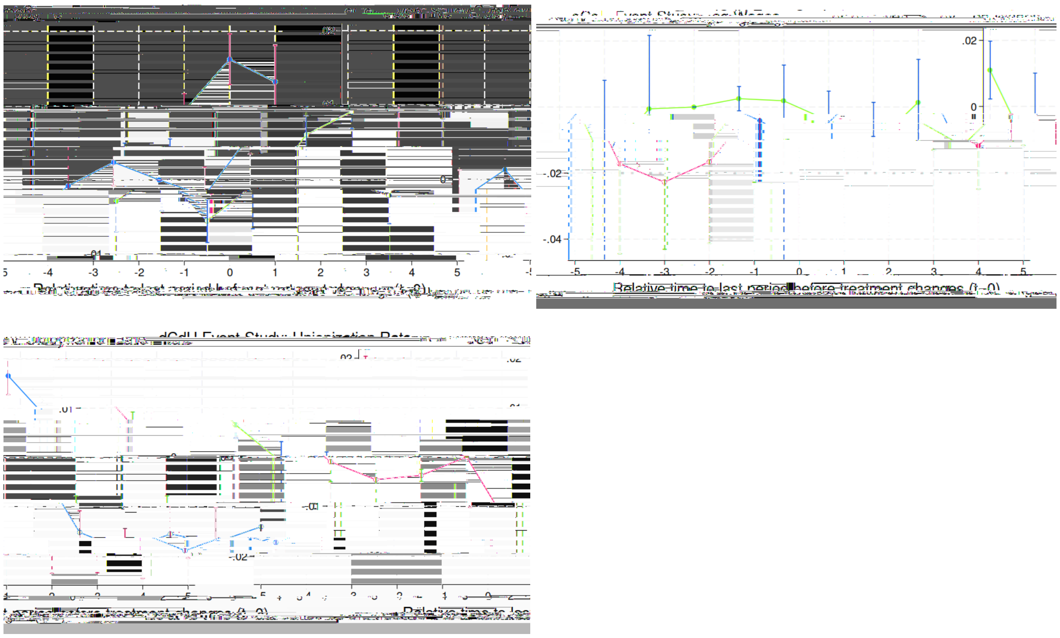


Figure A.3:



11.2 Tables

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

	SDID estimate	Std. Error	Treatment
Indiana	-.0050648	.0034523	2012

Table A.3: Alternative Estimates on Union Membership

Table A.4: Alternative Estimates on Wages

[illegible]	(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

[illegible]	(1) TWFE	(2) CS	(3) dCdH	(4) Wooldridge	(5) SDID

Table A.5: Alternative Estimates on Employment

[illegible]	(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

[illegible]	(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

[illegible]	(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

[illegible]	(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

[illegible]	(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

[illegible]	(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

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

A	(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

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