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Exploring the Effects of Right-to-Work Laws on Private Wages

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Abstract

The empirical literature on the wage effect of 'right-to-work' (RTW) legislation remains fairly ambiguous with studies producing contrasting results. In this article, we address this issue by exploring the impact of RTW on wage heterogeneity between socio-demographic and occupational subpopulations in the U.S. Using data from the 2012-2014 Current Population Survey, we employ two analytical techniques for estimating the wage effect of RTW legislation. First, we utilize multi-level regression to observe the effects of state-level RTW legislation on individual hourly earnings and wage differences while controlling for socio-demographic and occupational characteristics of individual workers as well as developmental and regulatory characteristics of states. Second, we utilize propensity-score matching (PSM) to observe the wage difference between similar workers located in RTW and non-RTW states. According to estimates from the fully specified multi-level regression models, private workers in RTW states earn 1 percent less than workers in non-RTW states. However, according to estimates from the PSM approach, private workers in RTW states earn about 6 percent less than similar workers in non-RTW states. The results show RTW legislation reduces hourly wages, but the magnitude of the wage effect is dependent on the specification of regression models and the statistical approach used to estimate the treatment effect of RTW legislation. Additionally, we also find RTW both suppresses and exacerbates existing wage inequalities between socio-demographic subpopulations.

Introduction

The 1947 Taft-Hartley amendments of the National Labor Relations Act formally affirmed the unambiguous right for states to implement “right-to-work” (RTW) laws prohibiting unions and employers from entering into contracts that require employees to pay dues as a condition of their employment. The purpose of these laws was to ensure that workers are afforded the opportunity of employment without forcing them to become a member of labor organization. Currently, twenty-five states have enacted RTW legislation through constitutional amendments and state legislation. The majority of these states enacted RTW laws during the latter part of the 1940s and 1950s. However, since 2012, three states have implemented RTW legislation with several others currently debating over the possibility of introducing new RTW legislation. This renewed political contention over RTW laws among state legislators has increase the demand for research on the impact of these laws.

More recently, a coalition of conservative advocacy groups has pushed for local RTW ordinances to circumvent the partisan politics of passing state-level RTW legislation. This recent campaign for local ordinances has raised questions on the economic impact of RTW laws on the livelihood of workers (e.g. Bruno 2015; Gould and Kimball 2015; Sherk and Kloster 2014; Sherk 2015). Despite a renewed interest in RTW among policy-makers, the empirical research on the wage impact of RTW legislation is fairly ambiguous about whether these laws reduce or increase private earnings (Reed 2003). As a result, the contentious politics over RTW legislation continues without any definitive evidence on how RTW laws may affect the livelihood of workers.

Proponents of RTW legislation contend compulsory union dues hinder economic development and suppress wage growth (Shrek 2015; Sherk and Kloster 2014). From this perspective, enacting RTW legislation improves the status of workers by attracting business investment and jobs through ensuring employers are not subjected to the collective demands of labor organizations. This policy narrative has gained particular traction in states struggling with economic restructuring, such as recent RTW adoptees like Indiana, Michigan, and Wisconsin and battle sites like Kentucky, Illinois, and Missouri. In contrast, opponents of this legislation argue that RTW laws suppress wages by empowering employers and accelerating the decline of private unions (Gould and Kimball 2015; Gould and Sheirholz 2011). In RTW states, employers are better able to negotiate more favorable agreements with employees without the interference of unions since RTW legislation directly restricts membership in labor organizations. In fact, research shows the implementation of RTW legislation has contributed to the decline of union density – the average union density in RTW states is 8.8 percent lower than non-RTW states (Holger, Shulman, and Weiler 2004).

In this article, we address the ambiguity in the empirical literature over the wage effect by analyzing the impact of RTW legislation on hourly earnings in U.S. states between 2012 and 2014 using micro data from the Current Population Survey. Further, we implement parametric and non-parametric statistical techniques estimating the wage effect of RTW legislation in observational data. In the first stage of the analyses, we estimate the effect of RTW legislation on individual earnings while controlling for socio-demographic, occupational, and state-level characteristics using multi-level regression. Additionally, we also explore the effect of RTW

legislation on union membership to illustrate differences in the unionization rate across socio-demographic subpopulations. While most studies have primarily focused on explaining differences in the *average* wage rate across states, few studies examine whether state RTW laws accounts for individual-level wage variation across states (e.g. Moore 1980; Farber 1984; Gould and Kimball 2015). This is an important issue in the literature because ignoring wage important variation amongst private workers which may explain why prior studies have produced the contrasting findings on the wage impact of RTW legislation.

In the second stage of the analyses, we utilize propensity-score matching to simulate experimental control and treatment groups for comparing wages between workers in RTW states and non-RTW states. This approach estimates a single factor score from a set of relevant covariates at the individual- and state-level to match workers in RTW states to similar workers in non-RTW states. Overall, results from both sets of analyses show RTW legislation effectively reduces wages and union membership, but the magnitude of the wage effect is dependent on the specification of the regression models and the statistical approach used to estimate the treatment effect of RTW legislation.

In the third stage of the analyses, we also extend on previous research by exploring the effect of RTW legislation on gender and racial wage differences. The purpose of this analysis is to determine whether RTW legislation exacerbates or suppresses wage inequalities through accelerating the decline of private unions. According to the results, RTW legislation reduces the rate of unionization while exerting contrasting effects on wage differences between whites, African Americans, and Hispanics. Moreover, we also find RTW legislation expands the wage difference between men and women. This research provides a platform to disentangle the wage effect of RTW legislation while also contributing to extent research on wage inequality by examining the institutional conditions behind gender and racial gaps.

Right-to-Work Legislation and Wages in the U.S.

Figure 1. Right-to-Work States and Year of Enactment

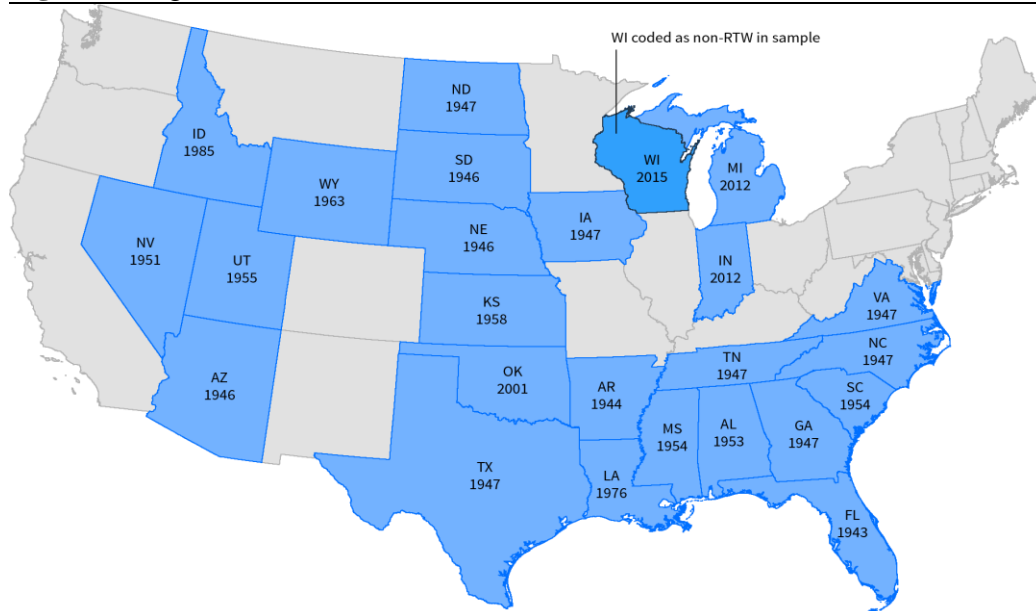


Figure 1 shows the number of states with RTW laws in the United States and the year of enactment. During the early part of the 1940s, business organizations began to mobilize against an expanding and increasingly ambitious labor movement by designing state policies aimed at curbing the growth of labor organizations. The Christian American Association was one of the main architects of RTW legislation and vigorously campaigned for the legislation during postwar period. Major business organizations, such as the Chamber of Commerce and the National Association of Manufacturers, were also instrumental to the spread of RTW laws on both the federal and state levels. Roughly a decade after the passage of the Taft-Hartley amendment in 1947, the majority of Southern and Western states had enacted RTW legislation.

Between 1960 and the 2012, only a handful of states adopted RTW. More recently, states in the “Rust Belt,” where unions have historically possessed a strong foundation, have experienced a renewed push for RTW laws. While proponents have crafted studies showing positive impacts of RTW (e.g. Sherk and Kloster 2014), trade unions and liberal think tanks continue to argue that RTW legislation harms workers by effectively lowering the average wage rate (e.g. Gould and Kimball 2015). These conflicting claims over RTW’s impacts echo the ambiguities of RTW’s impact on wages, both through the purported theoretical mechanism linking RTW laws to the wage rate in local labor markets and across empirical studies on the average wage difference between private workers in RTW and non-RTW states.

In theory, RTW legislation should affect the average wage level of state labor markets, but the net direction and size of the effect depends on the interplay among three components: (1) changes in union wage premium; (2) changes in union density; and (3) changes in the wage rate in non-union sectors (Reed 2003: 714). Individually, each of these component effects could work to either inflate or suppress wages. Together, if each component effect balances out the others, the cumulative impact of RTW rules is likely to be small, whether positive or negative. To suggest a minimal net effect of RTW on average wages, however, does not deny the possibility of heterogeneous effects across different sub-groups of workers, who may unevenly bear the burden of labor market adjustments induced by RTW laws.

One of the main mechanisms affecting the union and non-union wage component involves the “free-rider” problem induced by RTW laws. Non-compulsory union laws may encourage non-union workers to seek work in unionized sectors, since most employees in these sectors would benefit from union bargained contracts but are not required to pay union dues. With fewer resources and membership, unions may not be able to pressure employers into greater wage concessions, which could effectively lower the long-term wage rates in unionized sectors. Indeed, previous studies show, the number of “free riders” in RTW states increased from 13.3 percent in the 1970s to about 17.3 percent during the 1989-1991 period (Katz 1985; Davis and Huston 1993; Sobel 1995). Without any offsetting gains from “free riders” seeking jobs in relatively unionized sectors, the wage premium in union sectors would be expected to decline with weaker bargaining contracts and increasing labor supply, which should reduce the average wage in RTW states.

Another important mechanism is the potential for aggressive recruiting behavior of unions in RTW states. By undermining membership incentives, RTW legislation may induce unions to aggressively secure greater benefits, including higher wages, in order to boost recruitment. As a

result, the union wage premium may expand with more favorable wage contracts with employers. However, this may also lead employers in unionized sectors to decrease employment, which would force the migration of additional workers into non-union sectors, where the wage rate would decrease (Reed 2003; Moore 1998). The growth of labor supply in non-union sectors would suppress wages in non-union sectors despite the persistence of a relatively high union wage premium which may also exacerbate wage inequality between union and non-union sectors. Alternatively, employers in non-unionized sectors may counteract the aggressive recruiting of unions by voluntarily increasing the wages of non-members (Lewis 1963; Farber 2005). As a result of this bidding war, the average wage level in a state would increase as both employers in non-union sectors and collective wage contracts boost wages.

The wage outcome of these market mechanisms is likely to be influenced by RTW's effect on union membership. Given the "free rider" problem created by RTW laws, the decline of private unions has been accelerated in RTW states. Moreover, employers may amplify the secular trend of declining union membership by relocating operations to states with RTW laws (Moore 1998: 450). Indeed, prior studies show RTW legislation effectively reduced union density by 3 to 5 percent and individuals were 8.2 percent less likely to belong to a union in RTW states (Moore and Newman 1985; Davis and Huston 1995). Overall, we contend this decline in union membership represents an indirect pathway to lower wages since unions are less effective in gaining wage concessions from employers and labor in unionized sectors is forced to seek employment in non-unionized sectors with lower wages.

This overview of the purported theoretical mechanisms linking RTW laws to average wage rates shows the potential for mitigating and offsetting wage effects with the enactment of RTW legislation. Consequently, it is not surprising that the empirical literature on the wage effect of RTW legislation remains fairly mixed and unclear. Conflicting studies report negative (Mishel 2001; Farber 2005; Gould and Shierholz 2011; Gould and Kimball 2015), positive (Reed 2003; Sherk 2015), or null effects (Moore 1998). The status of the empirical evidence warrants a renewed investigation that attempts to disentangle these components by applying analytical techniques designed to determine whether RTW status may simply correlate with other state conditions or individual characteristics that account for wage differentials between private workers.

The Status of Empirical Research on RTW Legislation and Wages

In earlier empirical studies, Farber (1984) finds lower average wages among individual workers in RTW states compared to their non-RTW counterparts, while Moore (1980) found RTW legislation showed no impact on the wages of private workers. Other early studies narrowed the scope of the analysis to investigating the adverse effect of RTW legislation on the wage rate of private production workers in manufacturing sectors. While Carroll (1983) showed that RTW legislation reduced the earnings of private manufacturing workers, Wessels (1981) found the wages of manufacturing workers to be unaffected by RTW legislation.

Recent studies extend on these earlier studies by accounting for differences in prices and economic development between RTW and non-RTW states. For example, Mishel (2001) examined the wage effect of RTW legislation after controlling for regional differences in cost-of-

living given the concentration of RTW legislation in less-expensive states located in the South and Midwest. According to his study, the average wage rate in RTW states is about 4 percent lower than non-RTW states even after controlling for price differences across regions. In contrast, Reed (2003) shows average wages are higher in RTW states after controlling for the initial economic conditions of states during the period of legislative enactment. According to this research, states that were earlier adopters of RTW legislation were also relatively poorer and experienced less economic development than other states. Thus, ignoring this initial condition may omit an important structural factor that could explain the lower wage rates of RTW states.

Other studies have concentrated on developing analytical strategies to establish the causal link between the enactment of RTW laws and wages. For example, Farber (2005) departs from prior studies by observing the changes in non-union wages in states that recently enacted RTW legislation. Using data from the Current Population Survey, Farber measures the wage difference between pre- and post-RTW periods in Oklahoma and Idaho.¹ He finds the implementation of RTW legislation lowered non-union wages in Idaho by 4.2 percent, but RTW legislation showed no significant effect in Oklahoma.² Stevans (2009) accounts for the potential endogeneity in the relationship between RTW laws and wages (e.g. higher wages may induce states to adopt RTW legislation) by using a three-stage least squares regression to estimate the wage effect of RTW laws across U.S. states. According to the findings, after correcting for potential endogeneity, RTW legislation reduced wages and salaries of workers while expanding the income of owners and investors.

More recently, researchers at partisan think tanks have returned to examining the wage rate of individual workers in RTW states using nationally-representative micro-data. For example, Gould and Shierholz (2011) utilized the 2009 Current Population Survey to estimate the wage effect of RTW legislation. According to their findings, private workers in states with RTW laws earn 3.2 percent less than workers in non-RTW states, which accounts for a \$1,500 difference in annual earnings. Sherk (2015) challenges these findings by arguing Gould and Shierholz incorrectly specified their regression models and failed to account for regional differences in cost-of-living. After addressing for these issues, Shrek finds RTW legislation exerts little to no effect on wages. In response to this criticism, Gould and Kimball (2015) update the analysis of Gould and Shierholz (2011) by examining the wage effect of RTW legislation using 2010-2012 data from the Current Population Survey. After accounting for the various critiques of Sherk (2015), Gould and Kimball show RTW legislation reduces wages by 3.1 percent even when using different regression model specifications and controlling for cost-of-living differences. Nonetheless, these recent studies highlight the continued empirical ambiguity over the wage effect of RTW legislation.

¹ Oklahoma enacted RTW laws through a constitutional amendment in 2001 while Idaho passed an amend to the state labor statue in 1985.

² We argue the null effect in Oklahoma may be explained by the short period of observation in Farber (2005), which would not allow for the expiration of union contracts established prior to 2001. We conducted a similar analyses as Farber (2005) for Oklahoma with a longer period of observation. In this analysis, we measure the wage difference between private workers over a fourteen year period (2000-2014) using propensity score matching to pair workers with similar socio-demographic and occupational characteristics across both periods (2000 & 2014). Unlike Farber, we find the cumulative wage effect of RTW legislation may have reduced wages by as much as 22 percent. Subsequent research needs to examine whether this observed wage difference is attributable to the enactment of RTW and not secular changes to the state economy.

Less ambiguity marks evidence on the non-wage outcomes of RTW legislation. One of the only consistent findings in the extant literature is the negative impact of RTW legislation on the unionization rate of private workers (Reed 2003; Moore 1998). In the earlier empirical literature on RTW legislation, several studies confirm the negative effect of RTW laws on union membership and participation in private sectors (Dickens and Leonard 1985; Farber 1984; Freeman 1988; Lazear, 1988; Reder, 1988). According to this research, the implementation of RTW laws reduce the incentive to participate in unions since workers receive the benefits of collective bargaining without being required to pay dues and join (Katz 1985; Davis and Hutson 1993; Sobel 1995). In turn, reduce funding from membership dues hinders the capacity for unions to actively recruit new members and to engage employers in contract negotiations. We contend the decline of unions in RTW states contributed to the downward pressure on the average wage rate among private workers. Accordingly, we hypothesize the following:

H₁: The hourly wage of private workers is lower in RTW states.

Right-to-Work Legislation and Wage Inequalities

As stated above, prior studies on the wage effect of RTW legislation have primarily concentrated on explaining the average wage rates rather than the wages of individual workers (Reed 2003; Stevans 2007). The problem with this approach is analyzing state-level or sector-level wage averages assumes a homogenous effect of RTW laws across groups of workers which may not be tenable since the impact of unions on wages varies across sectors, occupations, and demographic groups (Schnabel and Wagner 2003). While other studies employ micro-data for estimating the wage effect of RTW laws, very few studies examines whether RTW legislation exerts a heterogeneous effect on different worker groups (e.g. Gould and Shierholz 2011). Given observed differences in the unionization rates between genders and races, we expect RTW laws may help explain the persistence of wage inequalities.

Several recent studies incorporate individual-level controls when estimating the impact of RTW legislation to isolate the effect of residency in RTW states on hourly earnings (Gould and Shierholz 2011; Gould and Kimball 2015; Sherk 2015). However, Gould and Shierholz (2011) also decompose their samples by demographic and educational groups to analyze whether the effect of residency in a RTW state on individual wages varies across these groups. According to their findings, RTW legislation affects most socio-demographic subpopulations, albeit at different rates. Specifically, Gould and Sheirholz find that RTW legislation adversely impacts the wages of women more than men. Moreover, they find that Hispanic and African American workers experienced a higher wage loss from RTW legislation than white workers. They also show that most educational groups are adversely affected at the same rate. We extend on this recent research by examining how RTW legislation affects racial, gender, and educational *wage inequality* among private workers. Given the heterogeneous effect of RTW legislation, we would expect RTW legislation amplifies persistent inequalities between socio-demographic and occupational groups.

An extensive literature has documented the gender and racial wage gaps over the last few decades (e.g. Blau and Kahn 2007; Fortin and Lemieux 1998; Gornick and Jacobs 1998; Roth

2003; Tomaskovic-Devey 1993; Tomaskovic-Devey et al. 2005). The dominant perspective in this research frames race- and gender-based wages inequalities with theories of human capital by attributing group differences to inequalities in achievement and ascription (Leicht 2008). For example, differences in human capital account for roughly half of the observed wage gaps between men and women as well as among racial and ethnic groups (Card and Lemieux 1994; McCall 2001a; 2001b; Carrington, McCue, and Pierce 1996; Blau and Kahn 2000). This strand of research, while useful for tracking the dynamics of wage inequality, focuses too much on the motives and behaviors of individuals instead of the organizational or institutional mechanisms of inequality (Reskin 2003).

The difficulty of observing individual motivation obscures the underlying processes that generate inequality. Moreover, ignoring the organizational or institutional context may omit important factors linking the ascribed status of individuals to the allocative mechanisms driving inequalities. In context of RTW legislation, prior researchers have primarily ignored the impact of this legislation on racial and gender-based inequalities (Hanely 2010). This is even more surprising considering the role of unionization in determining wages. For example, Even and Macpherson (1993) show that the decline of private sector unionization among male workers contributed to the slight decline in the gender pay gap during the 1970s and 80s. However, subsequent research has ignored both the institutional context behind union decline and the role of unionization in wage inequalities.

A persistent observation in the extant literature on unionization concerns the variation in the demographic and occupational make up of membership. The gender, education, age and race of private workers remain significant predictors of union membership across most developed countries (Schnabel and Wagner 2003). In terms of gender, the relatively higher rate of union membership among men reflects their greater attachment to the labor force and wage compensation (Antos et al. 1980; Schmidt and Strauss 1976; Guest and Dewe 1988; Hirsch and Berger 1984; Lorenz and Wagner 1991; Van den Berg and Groot 1992). Unlike findings on gender, the results for age remain fairly mixed as a number of studies have little-to-no significant association (Riley 1997). However, in most studies, while the association may not be statistically significant, the relationship between age and union membership appears to be concave with higher propensity among younger workers and a lower propensity among older workers (Schnabel and Wagner 2003). In terms of race, minority workers have increasingly sought participation in unions as a means of protection from economic inequality and racial discrimination (Rosenfeld and Kleykamp 2012; 2009; Minchin 1999).

Table 1 shows the conditional probabilities of union membership by RTW residency, race, gender, age, education, and occupational characteristics. On average, a private worker in the United States possesses 5.52 percent chance of belonging to a union, which is fairly consistent with the private sector union density rate of 6.6 percent in 2014 (BLS 2015). Consistent with previous studies (Fiorito and Greer 1982; Stepina and Fiorito 1986; Freeman 1988), private workers in RTW states have a 3.84 percent chance of union membership while workers in non-RTW states show a 7.66 percent probability of union membership. Moreover, African American workers are more likely to belong to a union compared to other racial and ethnic groups (a probability of 8.56 percent). Similarly, both males and workers with a high school or associates degree show an above average probability of belonging to a union. Finally, both hourly workers

and workers in production-based occupations have a high probability of union membership. Overall, Table 2 illustrates the substantial variation of union membership across different demographic, occupational, and state groups.

Table 1. Conditional Odds & Probability of Union Membership			
	Odds	Probability	Z-Score
Unconditional	.058	5.52%	-34.12
Worker in RTW State	.040	3.84%	-33.96
Worker in Non-RTW State	.083	7.66%	-27.63
<i>Race & Ethnicity</i>			
Black	.094	8.56%	-26.35
Hispanic	.052	4.98%	-32.93
Asian	.044	4.24%	-33.22
Other	.051	4.89%	-28.97
White	.057	5.37%	-33.11
<i>Gender & Age</i>			
Male	.075	6.95%	-30.80
Female	.041	3.97%	-37.61
Age = 40	.029	2.83%	-46.95
<i>Education</i>			
Some High School	.045	4.32%	-34.68
High School	.077	7.12%	-30.16
Some College	.059	5.55%	-32.94
Associates Degree	.075	6.97%	-29.85
Bachelors Degree	.039	3.77%	-37.46
Advanced Degree	.040	3.81%	-36.16
<i>Occupation</i>			
Hourly Worker	.074	6.89%	-30.97
Production-Based Occupation	.124	11.05%	-23.96
Metropolitan Worker	.058	5.51%	-34.06
Note: Estimated derived from multi-level logistic models (see Appendix 1).			

The observed differences in the probabilities of union membership and the extensive documentation of wage inequalities between socio-demographic groups illustrate the role of unions in mitigating economic inequalities. Accordingly, the accelerated decline of unions in RTW states may contribute to the intensification of wage inequality. For women, a reduction in the rate of unionization may reduce the average wages of male workers given their overrepresentation in unions and unionized sectors (Freeman and Leonard 1987). However, gender differences in union membership have been declining since the 1970s especially in male-dominated sectors of the economy. As a result, the gender pay gap has declined by as much as 14 percent especially in sectors with the highest union wage premium (Even and Macpherson 1993: 295). Since RTW laws reduce the incentive to participate and the ability of unions to recruit women, we should expect the following:

H₂: RTW legislation should increase the wage difference between male and female workers.

Traditionally, unions were hostile toward minority workers and erected institutional barriers to their participation (Bonacich 1976; Beck 1980; Cohen 2001). However, by the end of the 1970s, nearly 25 percent of female and about 34 percent of male African American workers were union members (Rosenfeld and Kleykamp 2012: 1465). Similarly, union participation among Hispanic citizens is higher than white citizens between 1973 and 2007 (Rosenfeld and Kleykamp 2009). While the unionization rate has steadily declined over the last forty years, African American workers are more unionized than their white counterparts. In fact, absent union decline, the wage difference between African American and white females would have declined by nearly 28 percent while the wage difference between men would have declined by 4 percent. Since RTW laws accelerate union decline, we should expect the following:

H₃: RTW legislation should increase the wage difference between white and minority workers.

Data

Data on worker age, gender, race/ethnicity, education, marital status, employment status, and union membership are drawn from the 2012-2014 versions of the Current Population Survey Merged Outgoing Rotation Group (CPS-MORG) compiled and harmonized by the National Bureau of Economic Research (NBER).³ We observe the most recent years of the CPS data to ensure the estimates are based on the newest survey information available and none of the states in the sample changed RTW status. For example, even though Wisconsin passed RTW legislation in 2015, the state is coded as non-RTW in the sample. Moreover, we pool CPS-MORG data for 2012-2014 to control for periodic short-term economic conditions using year fixed-effects in the model.

Starting in 1979, the CPS asked 25 percent of respondents to answer a set of detailed questions about their earnings. However, the design of the CPS survey makes it difficult to create a consistent and valid measure of hourly earnings from the raw data. Schmitt (2003) describes a set of procedures for resolving this issue. First, the CPS top codes hourly earnings at \$99.99 per hour which may remove important variation at the top-end of the earnings distribution. We impute top coded observations using the respondent's reported weekly earnings divided the reported average number of hours a week worked by the respondent. Additionally, we use the natural log of hourly earnings to address the problem of extreme outliers in the earning distribution. A second issue with the earnings data from the CPS is the treatment of non-wage income in estimating hourly earnings. The CPS only reports overtime, tips, and commission for weekly earnings which may under-estimate the actual hourly earnings of workers. We also impute the hourly earnings of workers who received additional earnings from overtime, tips, and commissions using weekly earnings. The third issue in the CPS is the treatment of missing data for hourly earnings. The CPS imputes observations with missing data on hourly earnings using a cell hot decking procedure based on demographic and work-related characteristics. Since prior researchers have raised concerns about whether imputed wages are overstated with this

³ Data are available at <http://www.nber.org/morg/annual/>

procedure, we omit these observations from the sample. Accordingly, we obtain information on the log hourly earnings of 248,527 private workers in U.S. states.

In the analysis, we include a series of relevant socio-demographic variables in the regression model to ensure the relationship between RTW and hourly earnings is not confounded by individual differences between private workers. Specifically, we include relevant socio-demographic information on respondent such as the respondent's race/ethnicity, age, gender, marital status, and educational attainment. Additionally, occupational information on respondents such as whether the respondent was hourly, fulltime, a union member, lived in a metropolitan area, and whether they worked in a production-based occupation. All data on these variables are drawn from the 2012-2014 CPS.

A persistent criticism of studies on RTW legislation is the potential confounding effect of state economic development or regulations on the relationship between RTW and wages (e.g. Reed 2003; Sherk 2015; Eren and Ozbeklik *forthcoming*). Based on this criticism, we also include a set of state-level controls to ensure the effect of RTW on hourly earnings is not spurious. First, we include regional differences in the cost-of-living using the regional price parities data from the Bureau of Economic Analysis (BEA). Second, we control for state economic development using information on gross domestic product per capita from the BEA. Third, we control for the state-level unemployment rate using information from the Bureau of Labor Statistics (BLS). Finally, we include two indicators of a state's regulatory environment: minimum wage and taxation on business and imports. The purpose of these regulatory controls is to ensure the effect of RTW legislation is not conflated with the general regulatory environment of a state. Minimum wage is measured as the difference between the state's mandated minimum wage and the federal rate. Information on state minimum wage is drawn from US Labor Department's data. Taxation on business and imports measures the state's percentage of the total U.S. tax revenue from production and imports. Information on state taxes is drawn from the BEA.

Analytical Strategy

The analysis proceeds in three stages. In the first, we examine the treatment effect of residency in RTW states on individual hourly wages using the 2012-2014 CPS-MORG data and multi-level regression. The main advantage of linear regression for estimating treatment effects in observational data is the capability of controlling for confounding conditions while also observing their effect on the outcome (Zanutto 2006: 86). Moreover, this approach allows for complex modeling where the effects of covariates are conditional on other variables in the model. This latter point is especially important when observing the impact of RTW legislation on wage inequalities between socio-demographic and worker groups.

The clustering of individual workers in states and the inclusion of state-level covariates inherently violates the assumption of observational independence for optimal estimates with ordinary least squares regression. Since ignoring the clustering effects of state would potentially biased estimates and under-estimate standard errors, we utilize a variance component model to account for state clustering of observations. Variance component models are designed to model and estimate the dependence of observations within clusters by including random intercept in the regression model (Rabe-Hesketh and Skrondal 2012: 75-80). Specifically, this approach uses the

standard structural model of linear regression to account for within-cluster dependence by splitting the general residual ξ_{ij} into two uncorrelated components: (1) a between-cluster residual (ζ_j) and (2) an idiosyncratic residual measuring the unobserved variance within clusters (ε_{ij}). The component ζ_j is the random deviation of cluster means and operates as a random intercept in the structural model. The component ε_{ij} is the random deviation of y_{ij} from the respondent's cluster mean. Accordingly, we utilize the following model to estimate the treatment effect of residency in a RTW state on individual hourly earnings:

$$(1) \quad \ln(y_{ijtl}) = \varepsilon_{ijtl} + \zeta_j + \beta_1 * X_{jt} + \sum \beta_{2k} * W_{kjt} + \sum \beta_{3k} * Z_{kijtl} + \alpha_t + \lambda_l$$

Equation 1 shows the functional form of the hourly wage rate of private workers in U.S. states. In the model, y_{ij} is the hourly wage rate of private worker i in state j , year t , and industry l . The term X_j is a state-level indicator for the presence of RTW legislation in state j . The term W_{kj} represents a vector of k state-level controls for state j in year t while Z_{kij} represents a vector of k individual-level controls for individual i in state j , year t , and industry l . Finally, the terms α_t and λ_l represent vectors of year (t) and industry (l) fixed intercepts. The purpose of including year and industry fixed-effects in the linear model is to control for any potential unobserved periodic or industry heterogeneity. The most conservative approach for deriving consistent estimates would be to include state fixed-effects in the model instead of the variance components. However, the indicator for RTW legislation would be omitted from the model because it is perfectly collinear with state fixed-effects.

A major limitation of the regression approach for estimating treatment effects is the comparability of RTW samples to non-RTW samples. If private workers in RTW states have different characteristics and are not comparable, it will often go unnoticed in regression analysis which may produce biased estimates of the treatment effect (Perkins et al. 2000: 94). Moreover, Sherk (2015) contends the inclusion of additional covariates may produce bias estimates since worker characteristics and state-level covariates may be highly correlated with RTW status. Based on these critiques of the linear regression, in the second stage of the analysis, we observe the wage difference in RTW and non-RTW states using an analytical technique designed to maximize the comparability between private workers in RTW and non-RTW states. Using CPS-MORG data for private workers in 2012-2014, we aim to estimate the average wage difference between private workers located in similar states and who possess similar socio-demographic and occupational characteristics. Instead of using individual- and state-level characteristics as controls in a regression model, we utilize this information to generate comparable samples of workers in RTW and non-RTW states.

We employ propensity score matching to pair private workers in RTW states with similar workers in non-RTW states. Propensity score matching is an analytical technique designed to estimate the treatment effect of a policy by accounting for the covariates that predict the respondent's exposure to the policy (see Rosenbaum and Rubin 1983 for a general discussion of the method). This technique provides the means to simulate experimental conditions with observational data by generating comparable samples of treated and non-treated observations. The utility of this approach for estimating treatment effects in observational data is the non-reliance on model specification of the functional form for the relationship between treatment and the outcome (e.g. linearity or log linearity assumptions in regression) (Zanutto 2006: 85). Since

the procedure utilizes covariates to pair treated and non-treated observations, the estimated effect is not dependent on the correct specification for the outcome variable.

The procedure is conducted in two steps. First, we generate a propensity score of exposure using logistic regression and appropriate covariates. Here, we use the same pooled sample of workers from the 2012-2014 CPS MORG samples to estimate propensity scores based on the following model:

$$(2) \quad \ln(p_{ijt}/(1-p_{ijt})) = \alpha_{ijt} + \sum \beta_{2k} * W_{kjt} + \sum \beta_k * X_{itjk} + \varepsilon_{it}$$

Equation 2 shows the functional form of the model used to generate propensity scores for matching workers. The first term, $\ln(p_{ijt}/(1-p_{ijt}))$, is the logit form of the probability of exposure to RTW legislation (i.e. residency in a RTW state). The logit probability is a function of a general intercept α_{ijt} , a vector of k variables for state j in year t , W_{kjt} , and a vector of k socio-demographic and occupational characteristics for individual i in state j and year t , X_{itjk} . In the second step, we use the propensity scores and nearest neighbor matching to pair private workers in RTW states to private workers in non-RTW states according to their similarity in propensity scores. After matching the respondents, we calculated the average difference among the pairs to estimate the treatment effect of residency in a RTW state on log hourly earnings.

In the third stage of the analysis, we utilize the same variance component models to observe the effect of RTW legislation on the relationship between socio-demographic and occupational variables and hourly wages. Here, we exploit the main advantage of linear regression and re-specify the models to include interaction terms between residency in a RTW state and socio-demographic or occupational characteristics of interests. These models are specified to estimate the conditional hourly wage differences between socio-demographic and occupational subpopulations across RTW and non-RTW states. In the context of multi-level regression, cross-level interaction effects test whether the relationship between lower-level covariates changes as a function of a higher-level variable. Since the lower-level covariates of interest are primarily status variables, a cross-level interaction effect shows whether the relative wage difference between socio-demographic or occupational groups is amplified or suppressed by residency in a RTW state. Accordingly, we use the following model for estimating the effect of RTW legislation on wage differences between socio-demographic and occupational groups:

$$(3) \quad \ln(y_{ijtl}) = \varepsilon_{ijtl} + \zeta_j + \beta_1 * X_{jt} + \beta_2 * V_{ijtl} + \beta_3 * V_{ijtl} * X_{jt} + \sum \beta_{4k} * W_{kjt} + \sum \beta_{5k} * Z_{kijtl} + \alpha_t + \lambda_i$$

Equation 3 is nearly identical to Equation 1 and includes many of the same terms. The notable difference between the two equations is the inclusion of an interaction between RTW status (X_{jt}) at the state-level and an individual-level covariate measuring membership in a socio-demographic or occupational group (V_{ijtl}). The coefficient of the interaction effect, β_3 , reports the effect of RTW legislation on the wage difference between private workers in the group and workers in the referenced group. We estimate the wage difference between the groups in RTW states by adding β_3 to β_1 . The coefficient β_2 reports the wage difference in non-RTW states. By comparing these two effects, we can assess whether RTW amplifies or suppresses wage inequalities between socio-demographic and occupational groups.

Results

Table 2. Variance Component Models of Log Hourly Earnings in U.S. States , 2012-2014

	(1)	(2)	(3)	(4)
Right-to-Work Legislation	-.1150*** (.0179)	-.0774*** (.0147)	-.0239* (.0116)	-.0104* (.0054)
<i>Individual Characteristics</i>				
Black		-.1150*** (.0088)	-.1210*** (.0066)	-.1170*** (.0070)
Hispanic ^a		-.1260*** (.0087)	-.1520*** (.0087)	-.1490*** (.0108)
Asian ^a		-.0577*** (.0111)	-.0918*** (.0109)	-.0873*** (.0079)
Other ^a		-.00485 (.0121)	-.0317* (.0146)	-.0254* (.0116)
Age		.0292*** (.0009)	.0291*** (.0009)	.0291*** (.0009)
Age-Squared		-.0003*** (.0001)	-.0003*** (.0001)	-.0003*** (.0001)
Male		.1210*** (.0034)	.1210*** (.0035)	.1210*** (.0035)
Married ^b		.1070*** (.0045)	.1110*** (.0043)	.1110*** (.0043)
Divorced or Widowed ^b		.0537*** (.0044)	.0587*** (.0039)	.0589*** (.0038)
Separated ^b		-.0003 (.0060)	.0004 (.0062)	.0016 (.0063)
Some High School ^c		-.1030*** (.0099)	-.1020*** (.0104)	-.1030*** (.0103)
Some College ^c		.0825*** (.0044)	.0811*** (.0037)	.0799*** (.0036)
Associates Degree ^c		.1640*** (.0040)	.1640*** (.0036)	.1630*** (.0036)
Bachelors Degree ^c		.3190*** (.0064)	.3160*** (.0059)	.3150*** (.0060)
Advanced Degree ^c		.5310*** (.0092)	.5280*** (.0091)	.5250*** (.0093)
Hourly Worker		-.2340*** (.0039)	-.2280*** (.0039)	-.2300*** (.0036)
Fulltime Worker		.1150*** (.0049)	.1160*** (.0049)	.1160*** (.0048)
Union Member		.2090*** (.0098)	.2030*** (.0100)	.2000*** (.0095)
Lives in Metropolitan Area		.0972*** (.0102)	.0826*** (.0079)	.0799*** (.0077)
Production-Based Occupation		-.0911*** (.0077)	-.0880*** (.0076)	-.0879*** (.0075)
<i>State Characteristics</i>				
Unemployment Rate			-.0043 (.0032)	.0001 (.0031)

Cost-of-Living			.0064***	.0047***
			(.0010)	(.0008)
GDP Per Capita				.0003***
				(.0001)
Taxation of Business				-.0021
				(.0019)
Minimum Wage				.0280**
				(.0106)
Constant	2.469***	1.681***	1.076***	1.067***
	(.0350)	(.0323)	(.1100)	(.0937)

Note: n=248,527. Robust standard errors in parentheses. All models included year & industry fixed-effects. a - Reference: White; b - Reference: Single; c - Reference: HS diploma only. * - p<.05; ** - p<.01; *** - p<.001

Table 2 shows the results of the variance component models of log hourly earnings for private workers in the U.S. during the 2012–2014 period. Model 1 provides an estimate for the effect of RTW legislation on log earnings without controlling for individual or other state characteristics. According to the simplified model, private workers in states with RTW legislation earn about 11.5 percent less than private workers with states without RTW legislation. In Model 2, we introduce individual-level characteristics to account for social, demographic, and occupational differences amongst workers. According to Model 2, controlling for individual worker differences, private workers in RTW states earn 7.4 percent less than workers in non-RTW states. While the effect of RTW legislation net of individual-level characteristics is smaller than the effect in Model 1, the effect in Model 2 is still statistically significant ($p < .001$). The reduction in the size of effect shows the importance of accounting for the effects of individual-level worker characteristics. However, despite the difference in the size of the effect, Models 1 and 2 show the substantial effect of RTW on individual-earnings.

Current methodological debates on the wage effect of RTW legislation revolve the confounding effects of other state characteristics (Sherk 2015; Gould and Kimball 2015; Reed 2003). Models 3 and 4 account for these critiques by including a series of controls for differences in cost-of-living, development, and business regulation. Model 3 replicates the specification in Gould and Kimball (2015) to estimate the effect of RTW legislation while controlling for individual-level worker characteristics, cost-of-living, and the state unemployment rate. Unsurprisingly, the inclusion of these state-level covariates further reduced the size of the effect when compared to Model 2. According to Model 3, net of the cost-of-living and the unemployment rate in addition to worker characteristics, RTW legislation reduces the wages of private workers by 2.4 percent. While this reported effect is smaller compared to the findings in Gould and Kimball (2015), we contend this may be more accurate because our state sample accounts for the recent RTW status of Indiana and Michigan.⁴ Moreover, we also argue the estimates from Gould and Kimball (2015) may be upwardly biased because their models do not account for state differences in development or other regulations. Accordingly, Model 4 introduces additional state controls for development (GDP per capita) and regulation (business taxation rate and state minimum wage). The introduction of these additional controls further reduced the size of the effect for RTW

⁴ Contracts from the pre-RTW period and the general 'stickiness' of wages may suppress any effect of RTW legislation on private wages. Including these two countries as RTW states may also reduce the likelihood of observing a significant wage effect in the analysis. Accordingly, the models in Table 2 may report a conservative estimate of the wage effect.

legislation. According to Model 4, private workers in RTW states only earn about 1.04 percent less than workers in non-RTW states after controlling for individual worker characteristics and state-level conditions.

Overall, the variance component models of log hourly earnings show that RTW legislation reduces the hourly wages of private workers. However, the magnitude of this effect is highly dependent on the specification of the regression model used for estimating the effect. While the negative effect of RTW legislation on wages is relatively robust to alternative specifications, the magnitude of the effect is sensitive to the inclusion of individual- and state-level variables. Specifically, recent research finds RTW laws on reduce private wages by about 3 percent after controlling for individual characteristics, cost-of-living, and labor market conditions (Gould and Kimball 2015; Gould and Shierholz 2011). We show that including additional state-level controls for economic development and economic regulation reduces this observed effect by about 2 percent. Nonetheless, the results from the regression analysis support the first hypothesis on the negative effect of RTW laws on wages.

Table 3 compares the average and median wages of full-time private workers in states with and without RTW legislation for the period of 2012-2014. According to the simple comparison, private workers in non-RTW states earn 13.5 percent more than workers in RTW states. In states without RTW legislation, workers earned an average of \$22.62 per hour while workers in states with RTW legislation earned \$19.73 per hour ($t=47.41$; $p<.001$). The difference of \$2.89 per hour is fairly substantial and amounts to an annual difference of \$6,011 in wages.

The observed wage difference between RTW and non-RTW states may be accounted for by differences in the labor force and macroeconomic conditions. On average, the labor force in RTW states tends to be more racially diverse, less educated, more rural, and more likely to live in less expensive areas compared with the labor force in non-RTW states. Moreover, RTW states tend have lower levels output per capita and unemployment while providing a more business-friendly environment (i.e. lower tax rates and minimum wage levels). The largest contrast is that workers in non-RTW states are twice as likely to belong to a union ($t=42.63$, $p<.001$). Overall, the comparison in Table 3 illustrates the differences between RTW and non-RTW states in terms of their local labor force, levels of development, and economic regulations. Therefore, a simple comparison may produce biased estimates of the wage difference.

Table 3 also reports the results of a propensity score matching (PSM) procedure designed to observe the difference in hourly wages between nearly identical workers in RTW and non-RTW states. The estimate from the PSM is derived from a two stage procedure. In the first stage of the analysis, a logistic regression model estimates the propensity scores of workers based on a series of relevant individual- and state-level covariates (see the specification of Model 4 in Table 2). In the second stage, workers in RTW states are matched to workers in non-RTW states according to their propensity score. A wage difference is calculated for each set. The average of the wage differences between worker pairs are summarized at the bottom of Table 3. According to the results, private workers in RTW states earn about 6 percent less than similar workers in non-RTW states. This estimate is consistent with the direction of the estimate from the regression analysis, but is surprisingly larger.

Table 3. Comparison of Right-to-Work (RTW) and Non-RTW States, 2012-2014

	RTW	Non-RTW	T-Score
Average Hourly Wage	\$19.73	\$22.62	47.41***
Median Hourly Wage	\$15.42	\$17.83	
<i>Socio-Demographic Characteristics</i>			
Average Age	40.44	41.25	14.51***
Male	53%	52%	-5.04***
<i>Race & Ethnicity</i>			
White	83.95%	84.29%	1.16
Black	10.33%	6.10%	-38.24***
Hispanic	15.28%	13.00%	-13.72***
Asian	3.18%	6.56%	38.18***
Other	1.55%	2.39%	15.12***
<i>Marital Status</i>			
Single	29.42%	31.13%	8.88***
Married	54.61%	54.53%	-.68
Divorce or Widowed	13.47%	12.29%	-8.12***
Separated	2.50%	2.05%	-7.01***
<i>Education</i>			
Some High School	10.54%	8.37%	-17.48***
High School Diploma	28.99%	27.26%	-8.98***
Some College	20.57%	18.87%	-11.14***
Associates Degree	12.06%	11.21%	-6.84***
Bachelors Degree	20.05%	23.34%	19.39***
Advanced Degree	7.79%	10.93%	26.04***
<i>Occupational Characteristics</i>			
Hourly Worker	62.94%	61.68%	-7.16***
Fulltime Worker	81.30%	79.25%	-11.62***
Union Member	4.20%	8.55%	42.63***
Metropolitan Area	78.87%	83.53%	28.49***
Production-Based Occupation	8.28%	6.98%	-13.31***
<i>State Characteristics</i>			
Unemployment Rate	6.29	7.12	111.91***
GDP Per Capita	\$45,693.03	\$50,846.17	166.95***
Taxation Rate of Business	2.93	3.76	56.16***
Cost-of-Living	93.71	102.89	324.41***
Minimum Wage	\$7.36	\$7.81	229.11***
<i>Wage Comparison with PSM</i>			
	<i>Coefficient</i>	<i>SE</i>	<i>Z-Score</i>
RTW vs. Non-RTW Wages	-.06	.03	-2.22*

Note: n=248,527. T-score based on two-sample t-test with unequal variances. PSM = Propensity Score Matching. Matching. PSM used specification of Model 4 in Table 2. * - p<.05; ** - p<.01; *** - p<.001.

In addition to analyzing the average wage effect of RTW legislation, we are also interested in the effect of RTW legislation on various demographic and occupational wage inequalities. Table 4 reports the effects of demographic and occupational characteristics on the difference in log hourly earnings among private workers in RTW and non-RTW states. Estimates for the effects are derived from a series of variance component models with industry and year fixed-effects. Additionally, the reported coefficients in Table 5 are based on interaction effects between the covariate of interest and an indicator for residency in a RTW state. Finally, the interaction effects are net of other individual- and state-level covariates (see the specification of Model 4 in Table 2 for a full list of covariates).

Table 4. Comparing the Wage Effect of Right-to-Work Legislation on Demographics and Occupational Characteristics, 2012-2014

	RTW	Non-RTW	Difference
<i>Demographic Characteristics</i>			
Black-White Wage Difference	-.119*** (.008)	-.116*** (.011)	-.003*** (.001)
Hispanic-White Wage Difference	-.142*** (.016)	-.155*** (.014)	.013*** (.002)
Male-Female Wage Difference	.133*** (.006)	.111*** (.004)	.022*** (.008)
Age	.025*** (.001)	.032*** (.001)	-.007*** (.002)
<i>Occupational Characteristics</i>			
Associates Degree-HS Diploma Wage Difference	.151*** (.006)	.171*** (.004)	-.020*** (.007)
Bachelors Degree- HS Diploma Wage Difference	.305*** (.012)	.321*** (.007)	-.016 (.015)
Advanced Degree-HS Diploma Wage Difference	.521*** (.016)	.528*** (.011)	-.007 (.019)
Union Membership	.234*** (.011)	.189*** (.012)	.045*** (.014)
Production-Based Occupation	-.061*** (.011)	-.111*** (.009)	.050*** (.017)

Note: n=248,527. Coefficients report the difference in log hourly wages between the observed group and the reference group. Robust standard errors in parentheses. Estimates net of demographic variables and state-level characteristics (cost of living; taxation on production; unemployment rate; GDP per capita; and minimum wage). * - p<.05; ** - p<.01; *** - p<.001

According to the estimates in Table 4, African American workers earn 11.6 percent and 11.9 percent less than their white counterparts in RTW and non-RTW states, respectively. Surprisingly, the difference between RTW and non-RTW was minimal (0.3 percent) even though it is statistically significant (p<.001). In contrast, Hispanic workers earn 14.2 and 15.5 percent less than their white counterparts in RTW and non-RTW states, respectively. Surprisingly, the wage difference is 1.3 percent *smaller* in RTW states compared to non-RTW states. The contrasting effects of RTW laws on wage inequality between minority and white workers suggests unionization plays a larger role in explaining wage differences between African American and white workers than wage differences between Hispanic and white workers.

In addition to racial wage gaps, Table 4 also reports the effect of RTW laws on the gender pay gap as well as the return to worker experience. According to the estimates, RTW legislation exacerbates the gender wage gap while reducing the return to work experience (proxied by worker age). In RTW states, male workers earn 13.3 percent more than female workers while, in non-RTW states, this wage difference is 11.3 percent. Moreover, in RTW states, each additional year of age increases hourly earnings by 2.5 percent; but in non-RTW states, each additional year yields an increase of 3.2 percent.

Finally, Table 4 provides the estimated effect of RTW legislation on wage difference between educational and occupational groups of workers. While RTW legislation appears to reduce the wage difference between workers with degrees and those with only a high school diploma, the only significant difference is between workers with an associate degree and workers with only a high school diploma. The return to an associates degree appears to be somewhat suppressed in RTW states since occupations requiring this additional training is often in industries most affected by changes in unionization. Workers with associate degrees (which are often technical degrees) earn 15.1 percent more than workers with only a high school diploma in RTW states and 17.1 percent more in non-RTW states. However, while union membership is generally declining in RTW states, the wage premium of union membership in these states is larger than non-RTW states. In RTW states, union members earn 23.4 percent more than non-union members while, in non-RTW states, union members only earn 18.9 percent more. This effect may explain the wage penalty against workers in production-based occupations in RTW and non-RTW states. Production-based workers in RTW states earn 6.1 percent less than non-production workers while, in non-RTW states, production workers earn 11.1 percent less than their non-production counterparts. Overall, the estimates in Table 4 illustrate the heterogeneous effect of RTW legislation on earnings differences between demographic and occupational groups of workers. The wage adjustments associated with the implementation of RTW laws are likely to be distributed unevenly across different groups of workers. Even if – as some studies have found – the countervailing wage dynamics of RTW largely balance each other out at the statewide level, women and minorities are most impacted by the decline of private unions induced by RTW laws.

In sum, the analysis shows RTW legislation negatively impacts the average hourly wages of private workers in the 2012-2014 period. While the observed treatment effect of RTW is smaller than previously reported estimates in recent research (e.g. Gould and Kimball 2015; Gould and Shierholz 2011), the analysis takes into account differences in socio-demographic and occupational characteristics amongst workers as well as differences in state development and regulatory environments. As a result, we conducted additional causal analyses using propensity-score matching to compare wages between similar private workers in RTW and non-RTW states. Based on this approach, we find similar workers located in RTW states earned less than their counterparts in non-RTW states. Most importantly, both set of results confirm that RTW legislation reduces wages.

We also find RTW legislation exerts a heterogeneous conditional effect on wage inequalities between demographic, educational, and occupational groups of workers. Accordingly, the inconsistent findings in the extant literature on the wage effect of RTW legislation may be attributable to the varying effect of RTW on these groups of workers. Overall, the analysis

highlights the importance of exploring the complex relationships among RTW legislation, worker characteristics, and earnings.

Discussion

The empirical ambiguity over the impact of RTW legislation on the wages of private workers warrants a renewed investigation. This article addresses this issue by employing two analytical approaches for estimating the relationship between residency in RTW states and hourly earnings. In the first approach, we analyzed the wage difference between RTW and non-RTW states controlling for the socio-demographic and occupational characteristics as well as state differences in economic development and regulation. According to the results, RTW legislation exerts a relatively smaller effect than observed in recent studies (e.g. Gould and Kimball 2015; Gould and Sheirholz 2011). We find RTW legislation reduces the hourly wages of private workers by only 1 percent. Based on the results, the average worker in a RTW states would expect to earn \$450 less each year than their counterpart in non-RTW states. This smaller effect shows the wage effect of RTW legislation is partially correlated with state development and economic regulation.

In the second approach, we examine the average wage difference between RTW and non-RTW states using a technique designed to maximize the comparability between private workers in RTW and non-RTW states. Based on simple comparison of hourly earnings, we find a private worker in RTW earned about a \$2.89/hour less than the average worker in non-RTW, once we compared hourly earnings between workers with similar socio-demographic and occupational characteristics from states with similar economic and regulatory environments, we find residency in RTW states reduced by wages by about 6 percent. According to the analysis, private workers in RTW states can expect to earn \$2,700 less per year than their counterparts in non-RTW states. This larger effect suggests the regression approach may yield more conservative estimates, which may warrant alternative analyses to triangulate the exact effect.

Overall, the findings confirm the first hypothesis while highlighting a potential source of ambiguity in the literature. Both sets of findings show RTW legislation reduces wages, but the magnitude of the effect is dependent on identification and analytical treatment of observational data. Given the observed difference in the magnitude of the effect, we interpret the findings from the regression analysis as a lower-bound estimate of the wage effect while interpreting the findings from the propensity-score matching analysis as an upper-bound estimate. Therefore, private workers in RTW states should expect to learn 1 to 6 percent less than workers in non-RTW states.

In addition to evaluating the average wage effect of RTW legislation, we also examined the impact of this legislation on wage differences between socio-demographic subpopulations. While previous studies show RTW legislation may reduce the growth of inequality in U.S. states (Hanley 2010), we show that RTW legislation exerts a heterogeneous effect on wage inequalities between workers from different races/ethnicities, genders, and occupational backgrounds. The analysis shows RTW legislation expands wage difference between African American and white workers, but only by a small margin. Conversely, we find RTW legislation reduces the wage difference between Hispanic and White workers. The mixed results for the third hypothesis

suggests we need a more nuanced understanding of the mechanism linking wages to RTW and the employment of races/ethnicities. Over the last few decades, African American workers have sought union membership to help protect them against workplace discrimination and inequities (Rosenfeld and Kleykamp 2012), but among Hispanics only citizens have followed suit (Rosenfeld and Kleykamp 2009). As the size of the immigrant-based Hispanic population continues to expand, unions may play a less significant role in mitigating racial wage inequalities unless unions in RTW states actively seek membership among immigrant Hispanic workers to bolster the labor movement.

This research also shows the role of state policy in exacerbating persistent inequalities between men and women. Despite literature showing the decline in the gender difference in unionization (e.g. Even and Macpherson 1993), we find support for the second hypothesis on the amplifying effect of RTW legislation on the wage gap between men and women. Part of this may be explained by the observed wage difference between unionized and non-unionized workers. According to the results, union members earn substantially more than non-union members in RTW states. Since women are primarily channeled into non-unionized sectors, they may earn less than their male counterparts in unionized sectors. In fact, as shown in Table 1, women are less likely to belong to a union compared to the average worker. Differences in unionization may explain part of the relationship between RTW legislation and gendered wage inequality, however, the findings suggests additional mechanisms are operating. Nonetheless, until unions aggressively recruit workers in sectors dominated by women (e.g. retail and food service), women should expect to earn less than men in RTW states.

According to the analysis, RTW legislation also reduces the wage premium to workers with associates degrees compared workers with only a high school diploma. This observed decline in the relative reward of technical degrees is supported by the negative effect of RTW legislation on the wage difference between workers in production-based occupations and workers in other occupations. This finding suggests wages for production-based occupations in RTW states might be higher than their counterparts in non-RTW states. However, this may also suggest non-production workers in non-RTW states may earn more than their counterparts in RTW states. Further research is necessary to disentangle the impact of RTW legislation on educational and occupational inequalities. While RTW may reduce the educational barriers to higher paying occupations, it may come at the expense of wages to non-production workers. Accordingly, unions need to expand membership in non-production sectors to ensure their own sustainability and to mitigate occupational inequality.

Overall, the analysis shows estimates for wage effect of RTW legislation is somewhat contingent on treatment of observational data. While we find RTW legislation generally reduces wages, we also find the magnitude of the effect is highly dependent on the analytical approach. Moreover, we also show RTW legislation may contribute to the growth or decline of socio-demographic and occupational wage inequalities. This research contributes to the extent literature on the institutional context of economic inequality.

Conclusion

Past and contemporary studies have produced mixed findings on the impact of RTW legislation on worker wages (e.g. Gould and Kimball 2015; Sherk 2015; Reed 2003; Moore 1980; Farber 1984), leading to the proliferation of competing claims about policy outcomes. One of the key contributions of the present study is the elucidation of some of the potential sources of variability in the results of prior studies. We empirically show that private workers in RTW states earn less than their counterparts in non-RTW states. However, we also show the magnitude of this effect varies by analytical approaches that emphasize different features of observational data. With the regression approach we find workers earn slightly less in states with RTW legislation compared to workers in non-RTW states. In contrast, using the quasi-experimental approach of propensity score matching, we find private workers in RTW states earn less than workers in non-RTW states. Given this contrast, we would recommend future studies incorporate components of each approach into a comprehensive analytical framework. Accordingly, researchers could utilize propensity score matching to account for the non-random distribution of workers across RTW and non-RTW states by generating more comparable samples of workers with regression-adjusted propensity scoring (Caliendo and Kopeinig 2008). We expect this approach to yield a more approximate estimate of the wage effect associated with RTW legislation.

More importantly, while most of the literature has concentrated on explaining the effect of RTW legislation on average wages, this study shows how RTW contributes to wage inequality between socio-demographic and occupational groups. While Hanely (2010) finds RTW legislation lowers the growth of income inequality in U.S. states, we show this effect varies across socio-demographic and occupational groups. Moreover, we extend on this research, and the broader literature on racial and gender inequality, by analyzing the institutional contexts and the potential mechanisms underlying the uneven wage outcomes of RTW – a process that proves to be fairly nuanced. We find RTW legislation exacerbates inequality between men and women as well as between African American and white workers while reducing inequality between Hispanic and white workers. This evidence should generate a new line of research that attempts to observe the processes linking RTW legislation to wage inequalities. The contrasting effects show that the racial composition of economic sectors or differential access to labor organizations may explain why Hispanics and males may benefit from RTW legislation while women and African Americans do not. In addressing the persistence of gender and racial wage inequalities, this research compels policy debates to consider to the heterogeneous effect of RTW legislation. In light of current campaigns for RTW legislation at the state, county, and municipal levels and the current literature's ambiguity about the wage effect of RTW legislation, impactful research would benefit from applying a more nuanced approach for understanding the impact of RTW laws on the livelihood of private workers in the United States.

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Appendix

Appendix 1. Generalized Logistic Model of Union Membership, 2012-2014.				
	1	2	3	4
Right-to-Work Legislation	-.764*** (.130)	-.796*** (.132)	-.663*** (.147)	-.658*** (.147)
<i>Individual Characteristics</i>				
Black		.602*** (.030)	.601*** (.030)	.601*** (.030)
Hispanic		.0006 (.0296)	-.0005 (.0296)	-.0011 (.0296)
Asian		.0291 (.0415)	.0273 (.0415)	.0275 (.0415)
Other		-.0411 (.0612)	-.0432 (.0612)	-.0423 (.0612)
Age		.0524*** (.00474)	.0525*** (.0047)	.0524*** (.0047)
Age-Squared		-.0005*** (.0001)	-.0005*** (.0001)	-.0005*** (.0001)
Male		.273*** (.0208)	.273*** (.0208)	.273*** (.0208)
Married		-.0110 (.0246)	-.0108 (.0246)	-.0107 (.0246)
Divorced or Widowed		.0172 (.0325)	.0173 (.0325)	.0174 (.0325)
Separated		.0089 (.0599)	.0090 (.0599)	.0087 (.0599)
Some High School		-.196*** (.0363)	-.196*** (.0363)	-.196*** (.0363)
Some College		-.120*** (.0249)	-.120*** (.0249)	-.120*** (.0249)
Associates Degree		-.120*** (.0285)	-.120*** (.0285)	-.120*** (.0285)
Bachelors Degree		-.607*** (.0290)	-.607*** (.0290)	-.607*** (.0290)
Advanced Degree		-.839*** (.0409)	-.839*** (.0409)	-.839*** (.0409)
Hourly Worker		1.224*** (.0235)	1.224*** (.0235)	1.224*** (.0235)
Fulltime Worker		.359*** (.0281)	.359*** (.0281)	.359*** (.0281)
Metropolitan Worker		.126*** (.0256)	.125*** (.0256)	.125*** (.0256)
Production-Based Occupation		.471*** (.0320)	.471*** (.0320)	.471*** (.0320)
<i>Other State Characteristics</i>				
Unemployment Rate			-.0048 (.0226)	.0001 (.0240)
GDP Per Capita				-.0002 (.0007)

Minimum Wage				.0588 (.0474)
Intercept	-5.629*** (.206)	-9.536*** (.230)	-11.10*** (.883)	-1.92*** (.920)

Note: n=248,527. Robust-cluster standard errors in parentheses. All models included year & industry fixed-effects. * - p<.05; ** - p<.01; *** - p<.001.
