

Labor Unions and Racial Wage Inequality

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Abstract

The stalling of convergence in racial wage inequality among men and the exacerbating Black-white wage gap among women coupled with higher de-unionization rates for Black people provokes an inquiry into the effects of labor unions on racial inequality. Exploiting variation from recent right-to-work legislation, I find evidence that laws that weaken unions increase Black-white wage inequality with the effects concentrated at the bottom of the wage distribution and among women. In addition to a baseline decrease in wages of over 3%, right-to-work laws lead to a disproportionate 3.88% decline in wages for Black women. These effects are largely driven by spillovers which are harshest around the twentieth centile of the wage distribution where the associated surplus union premium for Black women is the strongest.

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1 Introduction

Prima facie race-neutral changes to labor market institutions including increases to the minimum wage have disparate impacts on outcomes of underrepresented workers (see, for example, [Derenoncourt and Montialoux, 2020](#)). Strengthening labor market institutions - by, for example, encouraging and buttressing labor unions or increasing the minimum wage - could therefore reduce racial inequality. While targeted policies that seek to address these inequalities are indeed important, they are often limited in scope and susceptible to legal challenges. The broader question I seek to contribute towards answering is what is the effect of changes to labor market institutions on racial inequality.

Between 1983 and 2019, the Black-white wage gap at the median has stagnated for men and exacerbated for women (see [Figure 3](#)). Being a Black man consistently carries an associated Black wage penalty of over 15% relative to white men (see [Table 4](#)). For Black women, the associated wage penalty starts at 6.7% in the 1983-1988 period, increasing to around 10% in the 2000-2019 period. Black people are consistently over-represented in labor unions (see [Figure 1](#)). However, since 1983, unionization rates for Black people have declined at a much higher rate relative to white people, with the contrast more striking for Black women relative to white women.

With this paper, I seek to understand the effect of labor unions, particularly de-unionization, on Black-white wage inequality for men and women. I build on [Rosenfeld and Kleykamp \(2012\)](#) by extending the end period of the study from 2007 to 2019, adding back in the public sector to the analysis, as well as characterizing the association between labor unions and racial wage inequality along the wage distribution. A clear shortcoming of such exercises, however, is that I expect endogeneity to be inherently present in the model as unions may organize more among less-educated labor or Black people may select into unions in order to seek protection from compensation that is discriminatory. In order to address these concerns about potential endogeneity, I notice that through the period from 1989 to 2019, a series of state-level right-to-work laws that weaken labor unions have been enacted, thereby providing a mechanism by which one can price this declining unionization rate in terms of wages. A first-order question then is whether the Black-white wage gap has exacerbated as a consequence of these right-to-work laws, if so, by how much, and where in the wage distribution are these effects the strongest. I also ask how much of this effect is driven

by spillovers. Spillover effects capture the idea that in highly unionized environments, firms whose employees are not covered by a union may raise wages to minimize potential unionization threats. Laws that weaken unions make these potential threats less credible. In order to answer this series of questions, I revisit [Fortin et al. \(2018\)](#) who studied the secular effects of the laws for men and women and thus study the heterogeneous, particularly the disproportionate effects of the right to work laws on the wages of Black people relative to white people.

I find that these laws that weaken unions appear to increase Black-white wage inequality with the effects concentrated at the bottom of the wage distribution and among women. Specifically, I find that for women, in addition to a baseline decrease in wages of over 3%, right-to-work laws lead to a disproportionate 3.88% decline in wages for Black women. These disproportionate effects are largely driven by spillovers. In addition to the baseline 2.51% decline in wages for women not covered by a union, there is an additional 2.78% decline in wages for Black women not covered by a union. Further, these disproportionate spillover effects of the right-to-work legislation are harshest around the twentieth centile of the wage distribution, the same place where the associated surplus union premium for Black women is the strongest.

The remainder of the thesis is organized as follows. I thoroughly review the literature in [Section 2](#). I briefly describe the dataset, present summary statistics, and characterize associations between union coverage and racial inequality in [Section 3](#). [Section 4](#) presents the results of the estimation strategy characterizing the effect of right-to-work legislation on Black-white wage inequality at the mean and along the wage distribution. [Section 5](#) further discusses these results and goes over robustness checks and caveats. [Section 6](#) concludes.

2 Literature Review

2.1 Labor Unions and Racial Inequality Literature

The relationship between labor unions and racial inequality has long piqued cross-disciplinary interest across the social sciences, including sociology (see, for example, [Rosenfeld and Kleykamp, 2012; McCall, 2001](#)) and economics (see, for example, [Ashenfelter, 1972; Bound and Freeman, 1992](#)), and the racial aspect of unions and their members has been studied in the political sciences as well (see, for example, [Frymer and Grumbach, 2020](#)). It has been a while since this question has

been thoroughly explored in published economics research. Since the eighties, convergence in the median Black-white wage gap for men has stalled and for women has even widened. In this paper, I seek to revisit this relationship - I am interested in studying the effect of de-unionization on the Black-white wage gap along the wage distribution leverage policy variation from policies that weaken unions and taking into account their spillover effects.

[Donohue and Heckman \(1991\)](#) explain that "the story of Black economic progress is not one of uniform secular advance, but rather of episodic change". Several policies and institutions have been evaluated for their potency and the mechanisms by which they reduce racial inequality, including the minimum wage (see, for example, [Derenoncourt and Montialoux, 2020](#)), the Civil Rights Act of 1964 (see, for example, [Heckman and Payner, 1989; Freeman et al., 1973](#)), government transfer programs (see, for example, [Butler and Heckman, 1977](#)). Factors that have been implicated as determinants of the Black-white gap include a focus on characteristics prior to labor market entry such as a skills gap (see, for example, [Neal and Johnson, 1996](#)), parenting differences (see, for example, [Arcidiacono et al., 2015](#)), and a college education (see, for example, [Arcidiacono et al., 2010](#)). The literature also alludes to under-representations of Black-white earnings inequality due to the growing absence of Black men in the workforce, in particular, due to incarceration (see, for example [Bayer and Charles, 2018; Neal and Rick, 2014](#)). [Mueller-Smith \(2015\)](#) documents the persistent negative effects of incarceration on employment outcomes and on earnings. [Miller \(2017\)](#) show that federal affirmative action programs had a significant effect on the percent of Black employees which persisted after the program was rendered moot.

A growing literature has sought to evaluate changes to labor market institutions and their effect on wage inequality (see, for example, [Fortin et al., 2021; Farber et al., 2021; Card et al., 2020; Autor et al., 2016; Firpo et al., 2009; DiNardo et al., 1996](#)). [Card et al. \(2020\)](#) document the contrasting decline of unionization in the private sector and the rise of unionization in the public sector. They explain that it thus follows that public sector unions have a larger effect on wage inequality. Using Gallup polling data going as far back as 1936, [Farber et al. \(2021\)](#) show that unions indeed contributed to the fall in income inequality through the forties. Looking beyond class-based perspectives, studies have also sought to approach inequality through demographic-specific analyses (see, for example, [Biasi and Sarsons, 2020; Blau and Kahn, 1996](#)). [Biasi and Sarsons \(2020\)](#) look at the effect of flexible pay on the gender wage gap for teachers. In particular,

they leverage the Wisconsin Budget Repair Bill (Act 10) that, among other things, placed severe limitations on the power of collective bargaining in public sector unions. [Biasi and Sarsons \(2020\)](#) explain that as collective bargaining agreements expired, some school districts adopted a flexible pay model - including individual negotiations with teachers - which contributed to an increase in the gender wage gap. It is also possible that the presence of unions in the industry presents a barrier of hesitancy to firms with non-union workers seeking to adopt models such as flexible pay and performance pay.

[Ashenfelter \(1972\)](#) seeks to understand the effect of labor unions on racial discrimination in labor markets. He finds that on average racial discrimination is less prevalent in labor markets that are unionized but cautiously notes the presence of racial discrimination by labor unions themselves in the late sixties. This echoes the changing roles unions have played over time and the heterogeneity among unions in any given time as discussed by [Frymer \(2007\)](#) - from once being discriminatory and exclusionary, being indifferent about racial discrimination, to taking on leadership roles in the fight for civil rights. [Ashenfelter \(1972\)](#) also observes that the presence of labor unions had a positive effect on narrowing the Black-white wage gap over the same period. [Leigh \(1978\)](#) is fascinated by the finding in [Ashenfelter \(1972\)](#) that craft unions - in contrast with industrial unions - had exacerbated the Black-white wage gap for men. [Leigh \(1978\)](#) also confirms said findings in [Ashenfelter \(1972\)](#) by looking at "middle-aged men". [Bound and Freeman \(1992\)](#) explain that, among other factors, de-unionization contributed to an increase in the Black-white wage gap among young men in the eighties. My work is distinct from [Ashenfelter \(1972\)](#), [Leigh \(1978\)](#), [Bound and Freeman \(1992\)](#) as I will be employing econometric tools that were developed after these papers were published, in particular RIF-regression analysis which allows one to characterize the effect along the wage distribution, and will address concerns about endogeneity in baseline OLS specifications by leveraging policy variation from right-to-work laws to study the effect of de-unionization on racial inequality - policies that do not drive their analysis. Further, the focus of discourse in the economics profession has shifted away from studying the effect of labor unions on racial inequality for a while, and I hope to reignite said discourse again.

[Rosenfeld and Kleykamp \(2012\)](#) seek to understand the relationship between de-unionization in the private sector and racial wage inequality. Through counterfactual analysis, they find that de-unionization contributes to exacerbating the Black-white wage gap - with the effect being

stronger for women than for men. They explain that de-unionization disproportionately affects Black workers as they are more likely to be in unions than white workers. They argue that the demand for protection against discrimination drives the over-representation of Black workers in unions. Similarly, they present that the difference between unionization for Black women relative to white women is greater than the difference for Black men relative to white men which explains the difference in the strength of the effect. Notably however, [Rosenfeld and Kleykamp \(2012\)](#) neither seem to account for spillover effects nor do they look at the effect of unions on racial wage inequality along the wage distribution. They compare the effects for union members to workers not in unions who would be "otherwise similar". This may be an issue in the presence of spillover effects - in particular union threat effects as discussed by [Fortin et al. \(2021\)](#). As [Fortin et al. \(2021\)](#) explain, union threat effects capture the idea that firms may offer workers higher wages or higher benefits in order to placate interest in and threats to unionize. Looking at the overall distribution of racial inequality is indeed important as it allows one to understand how these labor market institutions change the shape of the wage distribution and where in the distribution the effects are strongest. [Heywood and Parent \(2012\)](#), for instance, notably find that the effect of performance pay through wage structure found in [Lemieux et al. \(2009\)](#) only applies to white people at the top end of the distribution.

Written effectively in response to [Autor et al. \(2016\)](#), [Fortin et al. \(2021\)](#) extend the analysis put forth in the seminal [DiNardo et al. \(1996\)](#) to include spillover effects when estimating the effect of the minimum wage on wage inequality and the effect of unions on the distribution of wages. [Firpo et al. \(2009\)](#) develop the recentered influence function regression analysis and apply the method to understand the effects of labor unions on the unconditional wage dispersion accounting for both "within-group (same covariates)" and "between-group (different covariates)" effects. My work is distinct from [Fortin et al. \(2021\)](#), [Autor et al. \(2016\)](#), [Firpo et al. \(2009\)](#), [DiNardo et al. \(1996\)](#) as I extend their analyses to explore heterogeneous effects by race.

[Bayer and Charles \(2018\)](#) document a decline in racial earnings inequality for men from 1940 to 1970 followed by an increase in racial earnings inequality at both the median and the 90th percentile when those with non-positive earnings are included in the analyses. They suggest, but do not test, that, among other factors, changing minimum wages and declining unionization contributes towards an effect on "distributional convergence" - "changes in the shape of the overall earnings

distribution that affect Black–white relative earnings because [Black men] and [white men] occupy different initial positions in that distribution".

2.2 Right-To-Work Laws and Associated Literature

The National Labor Relations Act of 1935 required that all people, including non-union members, who are covered by a collective bargaining agreement pay dues to unions. The Labor Management Relations Act of 1947 amended the National Labor Relations Act allowing states to pass legislation that does not permit unions to collect dues from people covered by a collective bargaining agreement or require that they join the union, "right-to-work" laws. Several states implemented right-to-work legislation through the mid-twentieth century, denoted "always right-to-work states" in [Figure 5](#). However, I am not able to exploit policy variation from these states as the NBER extracts of the CPS Merged Outgoing Rotation Groups do not have a variable identifying union coverage before 1983. I do not include policy variation from Idaho (1985) as I am concerned with the potential insufficiency of the pre-treatment period. Consistent with [Fortin et al. \(2018\)](#), I do not include policy variation from Texas (1993) as Texas has had some form of right-to-work legislation since the late forties. I do leverage policy variation from "new right-to-work states", that is states that have recently enacted right-to-work legislation. These states include Oklahoma (2001), Indiana (2012), Michigan (2013), Wisconsin (2015), Kentucky (2017). I do not include policy variation from West Virginia's 2016 right-to-work law due to concerns about long-run anticipatory effects that may bias the results, particularly an over-estimation of the baseline and disproportionate effects of the policy on wages.

[Lewis \(1963\)](#) identifies two mechanisms by which labor unions affect the wages of people not covered by a union: "spillover effects", by which, as [Farber \(2005\)](#) explains, "workers who are no longer employed in the union sector move to the nonunion sector, resulting in an increase in labor supply in that sector that reduces the equilibrium wage" and "threat effects" where firms whose employees are not covered by a union adopt wage structures similar to unionized workplaces, that is raise wages, in order to mitigate any potential threats of unionization. [Fortin et al. \(2021\)](#) provide an alternative characterization of spillover effects suggesting that in imperfectly competitive labor markets, non-union firms, in competition with union firms, may increase wages in order to attract workers. Through the paper, I use "spillover effects" and "threat effects" interchangeably in order

to allude to the threat effects identified by [Lewis \(1963\)](#) and spillovers as defined by [Fortin et al. \(2021\)](#). This is because I am not able to parse the mechanism by which the effects take place. A further breakdown of these two effects would warrant the need for matched employer-employee administrative data as discussed in [Fortin et al. \(2021\)](#).

Leveraging policy variation from Idaho (1985) and Oklahoma (2001), [Farber \(2005\)](#) studies the effect of right-to-work legislation on non-union wages and finds evidence that right-to-work laws are associated with a decrease in non-union wages. [VanHeuvelen \(2020\)](#) finds that right-to-work laws exacerbate economic inequality with heterogeneous effects across the wage distribution. [Firpo et al. \(2009\)](#) identify largest union wage premiums at and just below the median of the wage distribution. [Callaway and Collins \(2018\)](#) find that through the mid-twentieth century, the associated union wage premium was greater for Black people than for white people and was concentrated at the bottom end of the wage distribution. [Taschereau-Dumouchel \(2020\)](#) develops a theoretical foundation, illustrating the union threat effect where "to prevent their own unionization, nonunion firms distort their hiring decisions in a way that also compresses the range of wages", thereby providing a mechanism by which right-to-work laws weaken unions and impact non-union wages. Among other things, [Fortin et al. \(2018\)](#) revisit the right-to-work study by [Farber \(2005\)](#) leveraging later policy variation with a focus on legislation in the rust belt states, particularly, Indiana (2012), Michigan (2013), and Wisconsin (2015), and identify a negative effect on wages, driven partially by union threat effects. My work is indeed distinct from [Farber \(2005\)](#), [VanHeuvelen \(2020\)](#), [Taschereau-Dumouchel \(2020\)](#), and [Fortin et al. \(2018\)](#) as I am interested in the disproportionate effects of the legislation on the wages of Black people.

3 Data Description, Summary Statistics, and Descriptive Analysis

I use the National Bureau of Economic Research extracts of the Current Population Survey Merged Outgoing Rotation Groups for the years 1983 to 2019 ([Feenberg and Roth, 2021](#)). I closely follow the data cleaning process outlined in [Fortin et al. \(2021\)](#). The race variable used by [Fortin et al. \(2021\)](#) only identifies white people and non-white people. I have recovered a much richer race variable and can identify non-Hispanic white people, non-Hispanic Black people, and people of other races. I am also able to identify ethnicity and hispanicity. Through the paper, for the sake of

brevity, I define white people to be non-Hispanic white people, Black people to be non-Hispanic Black people, and Hispanic people to be Hispanic people of all races unless specified otherwise. Each observation in my dataset represents an individual.

Data on labor union membership and labor union coverage is available in the CPS MORG for the years following 1983. I define a binary variable for union coverage that takes a value of one for non self-employed people who are eligible for pay who are labor union members or covered by a union contract. I use the NBER's recommended hourly wage definition, that is, weekly earnings including overtime, tips, and commissions divided by usual hours per week - a notable divergence from [Fortin et al. \(2021\)](#) who do not include overtime, tips, and commissions. The weekly earnings variable also includes hourly workers which have been computed as earnings per hour multiplied by usual hours per week. I am unable to identify observations that work in multiple jobs.

I drop all observations with an allocated hourly wage, weekly earnings, or usual hours and am therefore forced to drop observations from 1994 and 1995 due to the absence of an allocation flag. I drop observations that are self-employed and those without pay. I follow [Fortin et al. \(2021\)](#) in imputing top-coded wages with a stochastic Pareto distribution. I use Pareto parameters for 2010 and 2011 for all years after 2010. I deflate wages to 1979 dollars using the consumer price index for all urban consumers provided by the Federal Reserve of St. Louis ([U.S. Bureau of Labor Statistics, 2021](#)). I drop all observations with real wages less than \$1 and greater than \$100. I drop observations with missing real log wages. I generate a uniform one-digit industry variable across all years in my sample following the definitions outlined in [Fortin et al. \(2021\)](#) which I adapt to create using NBER's two-digit industry variable. I use earnings weights through the analysis and use earnings weights weighed by usual hours per week for kernel density estimates. I keep observations of ages 16 to 64.

I present summary statistics for relevant variables in [Table 1](#). Experience is defined as age less education less six as is standard in the literature. Therefore after cleaning, I am left with over 1.5 million observations for white men and white women, over 150,000 observations for Black men, and around 195,000 observations for Black women. In addition, the sample includes around 210,000 observations for Hispanic men and around 165,000 observations for Hispanic women. By definition, union coverage is marginally higher than union membership - the difference of which is attributed to those covered by a union contract but not union members. As expected, mean real

log wages are indeed higher for white people than Black people and education levels are indeed higher for women than men of the respective races. Black women, white women, Black men, and Hispanic women are over-represented in public sector jobs relative to white men and Hispanic men.

In [Figure 1](#), I present union coverage time trends by hispanicity, race, and sex. In addition to the well-documented fact that unionization rates in the United States have fallen over time, there exists a consistent over-representation of Black people across time relative to their white counterparts. Unionization rates also appear to decline at a much higher rate for Black people relative to white people, with the contrast more striking for Black women relative to white women. Over 36.28% of Black men and 27.8% of Black women were covered by a union in 1983 compared to 27.39% of white men and 16.78% of white women at the same time. As of 2019, these union coverage rates have dropped to 13.28% for Black men and 11.67% for Black women which still remain greater than the percentage of white people that were unionized, specifically, 12.71% for white men and 11.4% for white women.

[Figure 2](#) shows union coverage time trends specific to the private and public sectors. Looking beyond the disparities in unionization rates between the private and public sectors, the aforementioned consistent over-representation of Black people covered by a union appears to be largely driven by the private sector relative to the public sector. For instance, in 1983, 32.1% of Black men in the private sector, 21.08% of Black women, 23.45% of white men, and 10.47% of white women were unionized - figures which drop to 9.74%, 7.31%, 8.8%, and, 5.05% respectively in 2019. For the public sector, on the other hand, in 1983, 50.94% of Black men, 44.73% of Black women, 48.77% of white men, and 44.35% of white women were unionized. Relative to the private sector, there is more intertemporal volatility in public sector unionization rates for Black people even though long-run public sector unionization rates hover around a relatively stable range. There is nevertheless, a sharper decline for Black people in the public sector relative to white people. The year 2019 saw public sector unionization rates at 31.99% and 29.58% for Black men and Black women, whereas unionization rates for white men and white women only dropped to 39.06% and 38.98%.

I further break down observations by industry in [Table 2](#) along with associated union coverage rates in [Table 3](#). Educational services are among the top three industries where white women and Black women are concentrated in with 15.45% of white women and 11.81% of Black women in

the industry. Notably, 42.9% of white women and 38.3% of Black women in educational services are unionized. Black women and white women are also over-represented in health and welfare services where 26.63% of Black women and 20.54% of white women are and 14.8% of Black women and 8.8% of white women are unionized. While 6.1% of Black women and 4.4% of white women are in transportation and utilities, 36.1% of Black women in the industry are unionized compared to 23.4% of white women. 13.54% of Black men and 9.89% of white men are in transportation and utilities where 36.6% of Black men and 32.2% of white men are unionized. Manufacturing sees 20.34% of white men and 18.04% of Black men in the industry with 18.4% of white men and 25.2% of Black men unionized. Around 6% of white men and Black men are in educational services where over 35% of white men and Black men are unionized.

Median real log wage time trends are plotted in [Figure 3](#). Throughout the period being studied, that is, 1983 to 2019, any convergence in the Black-white wage gap that took place in previous periods has stalled for men and is even diverging for women. In 1979 dollars, the difference between log wages for white men and Black men in 1983 was 0.336 and in 2019 remained 0.334. It is important to reiterate here, particularly within the context of the discussion in [Bayer and Charles \(2018\)](#), that my wage specification drops observations with zero earnings. For women, the Black-white wage gap at the median has, in fact, exacerbated over time. The difference between log wages for white women and Black women in 1983 was 0.118, increasing to 0.178 in 2019. I further render trends in the position of the median Black person and median white person on the wage distributions of white people in [Figure 4](#). Notice that between 1983 and 2019, the median Black man consistently hovered around the thirtieth percentile of the wage distribution of white men. While the median Black woman was above the fortieth centile of the wage distribution of white women through the eighties, their position on the white wage distribution appears to decline, falling to the thirty-seventh percentile in 2019.

3.1 OLS and RIF-OLS Regressions

Recall that I seek to understand the effect of labor unions, particularly de-unionization on racial wage inequality along the wage distribution. To that end, I first proceed with the following baseline OLS regression specification that characterizes the association between log wages and

union coverage for Black people and white people

$$\ln(w_{ijqst}) = \beta_0 + \beta_1 Black_i + \beta_2 covered_i + \beta_3 (covered_i \times Black_i) + X_i' \Gamma + \zeta_j + \psi_q + \lambda_s + \delta_t + \epsilon_{ijqst}, \quad (1)$$

where the dependent variable $\ln(w_{ijqst})$ represents the real log wages of worker i in industry j , quarter q , state s , and at time (year) t , $covered_i$ is a binary variable which represents being covered by a union, and $Black_i$ is a binary variable which takes a value of 1 for Black people and a value of 0 for white people. I include X_i in the regression specification which captures the following covariates: education, a quartic in experience, education-experience interaction, marital status, part-time status, public sector, CMSA, and 12 occupation categories, in addition to ζ_j , ψ_q , λ_s , and δ_t which are industry, quarter, state, and time fixed effects. I run this regression separately for men and women and separately for the periods 1983-1988, 1988-2000, and 2000-2019. Breaking down the period of study into three periods allows me to observe trends in the coefficients over time. The coefficient β_1 captures the Black wage penalty in the period relative to white people and β_2 captures the wage premium associated with union coverage for white people and Black people. The coefficient of interest, β_3 , captures the additional wage premium associated with union coverage for Black people.

I present the results of the OLS regression of real log wages on union coverage and race in [Table 4](#). After controlling for human capital controls, other covariates, and fixed effects, for Black men, there exists an associated wage penalty of around 15% relative to white men in all three periods. For men, the wage premium associated with being covered by a union appears to be weakening over time. For white men, being covered by a union is associated with a 20.92% increase in wages in the 1983-1988 period, a 19.6% increase in the 1988-2000 period, and an 18.65% increase in the 2000-2019 period. Some of this weakening can be attributed to the aforementioned downward trend in union coverage rates. In addition to the increase in wages associated with union coverage for white men, being a Black man covered by a union is associated with a surplus 4.51% increase in wages in the 1983-1988 period, a surplus 3.09% increase in the 1988-2000 period, and a surplus 2.65% increase in the 2000-2019 period.

The Black wage penalty appears to be exacerbating over time for women even after adding in covariates and fixed effects. Being a Black woman is associated with 6.7% lower wages than white

women in the 1983-1988 period, 8.72% lower wages in the 1989-2000 periods, and 9.79% lower wages in the 2000-2019 period. The baseline wage premium associated with being covered by a union appears to be weakening over time. Being covered by a union is associated with an 18.89% increase in wages in the 1983-1988 period, a 16.3% increase in the 1988-2000 period, and a 12.75% increase in the 2000-2019 period. Interestingly, the surplus association between union coverage and real log wages for Black women increases over the three periods despite the downward trend in union coverage rates. Being a Black woman covered by a union is associated with an additional 1.61% increase in wages in the 1983-1989 period, an additional 3.42% increase in the 1989-2000 period, and an additional 4.13% increase in the 2000-2019 period. Therefore, union coverage crucially appears to mitigate the racial wage penalty. However, this is indeed a presentation of correlation, not causation. I expect endogeneity to be inherently present in the model as unions may organize more among less-educated labor or Black people may select into unions in order to seek protection from compensation that is discriminatory.

Before I go on to address these concerns about endogeneity, I characterize the association between real log wages and union coverage along the wage distribution. Specifically, I seek to understand the association between union coverage and the unconditional quantiles of the wage distribution. Therefore, I use recentered influence functions, in particular, RIF-OLS analysis. Following [Firpo et al. \(2009\)](#), for quantile q_τ , I define RIF_i which is given by

$$RIF_i = q_\tau + \frac{\tau - \mathbb{1}\{w_i \leq q_\tau\}}{f(q_\tau)}, \quad (2)$$

where $\mathbb{1}\{w_i \leq q_\tau\}$ indicates if the observed wage is less than the quantile in question and $f(q_\tau)$ is the density of the marginal wage distribution. [Firpo et al. \(2009\)](#) show that this yields a consistent estimate of the association between the independent variable - in this case, union coverage - and the unconditional quantile τ of the wage distribution. I estimate this specification for all $\tau \in [10, 90] \subseteq \mathbb{10N}$. I refrain from testing the association at higher quantiles as they may be affected by the Pareto imputation procedure which does not take race into account.

The RIF-OLS results corresponding to the OLS regression estimates found in [Table 4](#) are presented in [Figure 6](#) for men and [Figure 7](#) for women. The associated Black wage penalty for men appears to peak at the twentieth percentile of the overall male wage distribution. I expect

that the reduction in the Black wage penalty for men at the tenth centile over time can largely be attributed to increases in the minimum wage. However, the stagnant associated Black wage penalty for men at the mean as discussed in [Table 4](#) and at the median as discussed in [Figure 3](#) obscures an apparent increasing Black wage penalty in the upper echelons of the overall wage distribution, thereby offsetting the reduction at the lower percentiles. [Fortin et al. \(2021\)](#) and [Firpo et al. \(2009\)](#) find that unions have their strongest impact at and just below the median. I find that the shape of the baseline association with union coverage for men largely follows the same pattern: that the association is largest at and just below the median. The peak baseline association for men appears to move towards the median over the three periods indicating an increase in wage inequality in non-union positions over time. Interestingly, I find that for Black men relative to white men, the surplus association of union coverage is largest towards the bottom end of the overall wage distribution. The surplus association for men appears to decline as one moves along the overall wage distribution and is even negative beyond the median of the overall wage distribution. A part of the story here indeed appears to be that the Black wage distribution is more heavily skewed to the right as shown in [Figure 20](#). Also, recall that the median Black man would be at the thirtieth percentile of the wage distribution of white men as shown in [Figure 4](#). The other, more fascinating part of the story appears to be that union coverage appears to have its largest surplus association for Black men in parts of the wage distribution where the associated Black wage penalty is the strongest. I expect that the decline in the surplus association for Black men over the three periods can be attributed to the sharper de-unionization rates for Black men relative to white men as presented in [Figure 1](#) and in part to increases in the minimum wage.

For women, as seen in [Figure 7](#), I find the associated Black wage penalty to peak at the twentieth centile of the overall female wage distribution in the 1983-1988 period. As the periods progress, in addition to the finding that the associated Black wage penalty at the mean increases for women, the peak Black wage penalty moves to the fortieth centile of the overall female wage distribution. The peak baseline association for women is at the seventieth percentile of the overall wage distribution and shifts to the median through the periods. The surplus association of union coverage for Black women relative to white women appears to mirror, and therefore offsets, the Black wage penalty for women. In the 1983-1988 period, when Black-white wage inequality among women is at its peak around the twentieth centile, the surplus associated benefit from union coverage for Black women

appears to be at the twentieth centile of the overall female wage distribution as well. Similarly, in the 2000-2019 period, it appears that the surplus association of wages and union coverage takes on a much smoother hump-like shape between the tenth and sixtieth quantiles, in the same period when the shape of the Black wage penalty over the distribution for women appears to be smoother.

I also run the same RIF-OLS regressions separately for each subgroup for the three periods, with results depicted in [Figure 8](#) for white men and Black men and [Figure 9](#) for white women and Black women. For white men, the peak association between real log wages and union coverage is indeed at and just below the median of the overall white male wage distribution, with the peak association moving closer to the median as one moves to more recent periods. For Black men, the peak association is at the seventieth quantile of the Black male wage distribution. For white women, the peak association, is at the seventieth percentile of the white female wage distribution in the 1983-1988 period, moving towards the median in later periods. For Black women, the peak association fluctuates between the sixtieth and seventieth centiles of the Black female wage distribution. This analysis hence branches to two conjectures. First, the surplus associations found in [Figure 6](#) and [Figure 7](#) is partly driven by the inherent differences in the wage distributions for Black people and white people as documented in [Figure 20](#) and [Figure 21](#) as well as the relative positions of the median Black man and Black woman on the wage distributions of white men and white women respectively as presented in [Figure 4](#). Second, and more interestingly, in analyzing Black people and white people separately, one crucially loses information about the Black-white wage gap, that is, the Black wage penalty relative to white people and the association between wages and union coverage that appears to offset the Black wage penalty as described earlier.

In light of the exposition by [Fortin et al. \(2021\)](#) on the importance of unionization spillover effects in getting a fuller understanding of the effect of unionization on wages - that non-union firms in state-industries with a high unionization rate may, for example, emulate the wage structures of union firms to mitigate potential unionization threats, I sought to understand the differential effects of unionization by race, capturing these spillovers. In order to do so, I follow [Fortin et al. \(2021\)](#) in using union coverage rate at a state-industry-year level as an instrument for the unionization rate. I use all 50 states, the 11 industry categories outlined in [Table 2](#), and the years 1983 to 2019 as part of my definition of unionization rate. Notice that no individual person here can significantly affect the unionization rate in a state-industry by merely selecting into or dropping out. This, therefore,

assuages some of my earlier endogeneity concerns. I first present results from an OLS regression specification of log wages on this unionization rate, running the model separately for white men, Black men, and Hispanic men of all races who are not covered by a union, with corresponding estimates for women who are not covered by a union. Thus, the regression I run is given by

$$\ln(w_{ijqst}) = \beta_0 + \beta_1 coveragerate_{jst} + X_i'\Gamma + \zeta_j + \psi_q + \lambda_s + \delta_t + \epsilon_{ijqst}, \quad (3)$$

where the dependent variable, the covariates, and the fixed effects remain as defined before. The coefficient of interest β_1 , in particular its transformed form $100(e^{\beta_1} - 1)$, captures the effect of a one percent increase in unionization rate in a state-industry-year on real log wages.

Table 5 shows the estimates of these union "threat" effects. The effect of the unionization rate on white men not covered by a union remains significant but trends downwards over time. A one percent increase in the unionization rate leads to a 13.77% increase in the wages of white men in the 1983-1988 period, an 11.4% increase in the 1988-2000 period, and a 7.06% increase in the 2000-2019 period. For Black men not covered by a union, the estimates are statistically imprecise, likely due to sampling variation. Alas, a one percent increase in the unionization rate leads to an increase in wages of up to 11.52% for Black men as seen in the 2000-2019 period. The estimates for white women not covered by a union and Black women not covered by a union appear to be closer to zero. Notably, union threat effects as measured by the spillover effects of unionization rate appear to be strongest for Hispanic people not covered by a union. For Hispanic men, a one percent increase in the unionization rate leads to an increase in log wages ranging from 29.05% in the 2000-2019 period to 37.03% in the 1988-2000 period, and for Hispanic women it leads to an increase ranging from 13.88% in the 1983-1988 period to 38.82% in the 1988-2000 period.

4 Difference in Differences and RIF-DID Analysis

In order to address aforementioned endogeneity concerns from the naive OLS specification characterizing the association between deunionization and racial wage inequality, I next leverage exogenous policy variation created by state-level right-to-work legislation that does not permit unions to collect dues from people covered by a collective bargaining agreement or require that

they join the union. This is expected to, and has been documented in the literature to, weaken labor unions (see, for example, [Taschereau-Dumouchel, 2020](#)), thereby providing for a quasi-natural experiment that allows one to price the disproportionate effects of de-unionization and to test spillover effects of such policies that weaken unions. I look at the effect of this right-to-work law treatment on real log wages. I run the following regression

$$\ln(w_{ijqst}) = \beta_0 + \beta_1 Black_i + \beta_2 RTW_{st} + \beta_3(RTW_{st} \times Black_i) + X_i' \Gamma + \zeta_j + \psi_q + \lambda_s + \delta_t + \epsilon_{ijqst}, \quad (4)$$

where $RTW_{st} = 1$ if state s has a right-to-work law in time t , $Black_i$ indicates whether the observation is a Black person or a white person, X_i includes the same set of covariates as above, and fixed effects are defined as before. I run this regression separately for men, women, people not covered by a union, and people covered a union. Consistent with [Taschereau-Dumouchel \(2020\)](#), I use a sample of years from 1989 to 2019. Policy variation comes from Oklahoma (2001), Indiana (2012), Michigan (2013), Wisconsin (2015), and Kentucky (2017). I do not include variation from Texas (1993) as Texas has had some form of a right-to-work law in place since 1947 and my sample has dropped 1994 and 1995 due to missing allocation flags. All states, except West Virginia, Montana, Maine, New Hampshire, and Vermont are in the sample. West Virginia and variation from its 2016 right-to-work legislation have been dropped from the analysis as it appears to be likely that the policy had been anticipated which could bias the results of the estimation. The latter states have been dropped for lack of common support. The coefficient β_1 captures the Black wage penalty, β_2 captures the baseline effect of the right-to-work laws, and β_3 , the coefficient of interest, captures the disproportionate effect of the right-to-work laws for Black people, that is, the effect of the legislation on the Black-white wage gap.

The difference in differences results of the effect of right-to-work laws on real log wages are summarized in [Table 6](#). Columns 1 and 4 show the estimates of the model as outlined in [Equation 4](#). The Black wage penalty is 13.41% for men and 6.41% for women relative to white men and white women respectively. Being in a right-to-work state after the legislation becomes effective leads to a baseline 3.7% decrease in wages for men and 3.13% decrease for women. Laws that weaken unions disproportionately affect Black people with the effects concentrated among women. Right-to-work laws appear to be associated with an additional 3.88% decline in wages for Black women and an

additional 2.17% decline in wages for Black men.

The Black wage penalty among people not covered by a union is 14.7% for men and 8.06% for women. Right-to-work laws indeed have significant spillover effects, leading to a baseline 2.63% decline in wages for men not covered by a union, and a baseline 2.51% decline in wages for women not covered by a union. There exist sizable disproportionate spillover effects of right-to-work legislation for Black women. In addition to the baseline 2.51% decline in wages for women not covered by a union, there is an additional 2.78% decline in wages for Black women not covered by a union. There do not appear to exist disproportionate spillover effects for Black men not covered by a union beyond the baseline 2.63% decline in wages. The significant disproportionate effects when all Black and white men are included in the sample therefore appear to be driven by people that have dropped out of unions as a consequence of the right-to-work laws. The direct effects of right-to-work laws on the wages of those covered by a union appear to be borne equally between unionized Black people and unionized white people, that is, there are no disproportionate effects for unionized Black people. Right-to-work laws appear to decrease the wages of unionized men by 5.56% and decrease the wages of unionized women by 5.89%.

One concern raised in analyses of recent right-to-work legislation is that the policies have generally been implemented in the aftermath of adverse shocks to the real business cycle, such as the Great Recession, and the ensuing recoveries. Therefore, Columns 2 and 5 of [Table 6](#) include state monthly unemployment rates, to which the results are robust. In Columns 3 and 6, the preferred specification, I replace the year fixed effects with state-specific linear time trends and keep state monthly unemployment rates in the model. Estimates of the disproportionate effect are robust to the inclusion of state linear time trends. The baseline effect of right-to-work laws for men remains significant, but the point estimate shrinks to 1.97%, a value shared by the baseline spillover effect of the laws for men not covered by a union. Interestingly, the replacement of year fixed effects with state linear time trends appears to exacerbate the baseline effect of the laws for women and women not covered by a union to a decline in wages that is as large as 5.56% and 5.73% respectively. For men and women covered by a union, the effect of the legislation, once state linear time trends are accounted for, is a 4.59% decline in wages with smaller standard errors for the former and a 5.25% decline in wages for the latter. I further estimate the specification with year fixed effects as well as with state-specific linear time trends and state monthly unemployment rates in the model which

are found in Columns 4 and 8 of [Table 6](#). With the inclusion of year fixed effects and state-specific linear time trends, notice that the baseline effect of the right-to-work laws on wages drops to zero for men and corresponds to a 1.97% decline for women. The disproportionate effects of the policies for Black men and Black women remain robust to this specification and are sustained at a 2.14% decline for Black men and a 3.8% decline for Black women. Under this adaptation of the model, the baseline and disproportionate effects of the legislation do not appear to be different from zero. For Black women, with the inclusion of year fixed effects and state-specific linear time trends, there appears to be a disproportionate 3.8% decline in wages in addition to a baseline 2.25% decline as a consequence of the right-to-work laws.

Following my above analysis of the association of de-unionization and real log wages using RIF-OLS, I adopt a similar approach to further flesh out the difference in difference analysis of the effect of de-unionization on racial wage inequality via the right-to-work laws. I run a similar series of RIF-DID, that is, recentered influence function difference in difference regressions where RIF_i remains defined as in [Equation 2](#), with controls and fixed effects as outlined in [Equation 4](#). I use the preferred specification which includes state-level unemployment rates and replaces year fixed effects with state-specific linear time trends.

[Figure 10](#) and [Figure 11](#) contain the results of this RIF-DID regression for all (Black and white) people and people not covered by a union, where again, the latter is intended to capture spillover effects of the legislation. This RIF-DID analysis indeed corresponds to the DID analysis at the mean presented in Panels A and B of [Table 6](#). For men and women, with the inclusion of state linear time trends, the baseline effect of right-to-work laws appears to be similar along the wage distribution as the RIF-DID plot is tightly wrapped around the DID estimate. Notably, the disproportionate effect of the right-to-work laws for both Black men and Black women relative to white men and white women respectively are consistent with the RIF-OLS finding in [Figure 6](#) and [Figure 7](#). Specifically, for quantiles of the overall female wage distribution below the seventieth centile, right-to-work laws appear to have exacerbated Black-white wage inequality for women. The laws have their sharpest disproportionate impacts for Black women at the twentieth centile of the overall female wage distribution, the same centile where the surplus association between real log wages and union coverage was the strongest. The shape of the disproportionate effect throughout the wage distribution appears to be a reflection of the shape found in the association.

Similarly, the legislation has its disproportionate impact for Black men around the bottom of the overall male wage distribution and has a positive impact, that is, reduces Black-white wage inequality in the upper echelons of the overall male wage distribution. This finding is consistent with the RIF-OLS regression in [Figure 6](#) where it was seen that the surplus association with union coverage at quantiles above the median was negative. Although, the negative effects at the bottom of the wage distribution appear to be almost twice in magnitude as the positive effects at the top of the wage distribution, thereby yielding a negative disproportionate effect of the laws for Black men at the mean.

In highly unionized environments, firms whose employees are not covered by a union may adopt wage structures similar to those of unionized workplaces, that is, raise wages to minimize potential unionization threats. These wage structures may even be less discriminatory. Laws that weaken unions, such as the right-to-work laws, make these threats less credible. These very spillover effects appear to largely drive the results seen along the overall wage distribution. [Figure 11](#) summarizes results of the spillover effects of the right-to-work laws, that is, the effect of the legislation on the wages of people not covered by a union. The baseline spillover effect of the laws for women also appears to be consistent with the finding in the RIF-OLS in [Figure 7](#) that the association is strongest at the median of the female wage distribution. The striking negative disproportionate spillover effects at the bottom of the overall wage distribution underscore the fact that looking solely at the first moment of the distribution may obscure the richer perspective that quantile analysis provides.

As was done for the RIF-OLS regressions in [Figure 8](#) for men and [Figure 9](#) for women, the RIF-DID analysis is broken down further with regressions run separately for each subgroup with [Figure 12](#) containing results for the effect along the overall wage distribution for all people and [Figure 13](#) containing results for the shape of the spillover effects along their respective wage distributions. The effects of the right-to-work legislation appear to be tightly wrapped around the mean of the wage distributions by race and sex. This suggests that the concentration of the surplus effect at the bottom of the wage distributions for Black people relative to white people as found in [Figure 10](#) and [Figure 11](#) appears to be largely driven by the over-representation of Black people at the bottom of the wage distribution as well as by the introduction of the interaction between the Black wage distributions and white wage distributions through the Black wage penalty. Recall

that median Black men would be consistently at the thirtieth percentile of the wage distribution of white men and that women earning wages at the median of the wage distribution of Black women would find that their wages correspond to the thirty-second to forty-third centiles of the wage distribution of white women as illustrated in [Figure 4](#).

5 Discussion

Motivated by the stagnating Black-white wage gap among men, the exacerbating racial gap among women, and the higher de-unionization rates for Black people, I sought to understand the effect of labor unions, particularly de-unionization on racial wage inequality. In addition to documenting associations between union coverage and real log wages by race, both at the mean and along the wage distribution, I leveraged policy variation created by state-level right-to-work laws in order to address endogeneity which would have otherwise precluded one to parse causality from the correlation. I find that union coverage appears to mitigate the racial wage penalty. The surplus association for union coverage for Black women relative to white women appears to mirror, and therefore offsets, the Black wage penalty for women. Right-to-work laws provided a mechanism by which one can price the disproportionately higher rates of de-unionization for Black people in terms of wages. I find that laws that weaken unions appear to increase Black-white wage inequality with the effects concentrated at the bottom of the wage distribution and among women. There also appear to exist sizable disproportionate spillover effects of the legislation for Black women relative to white women that drives this finding where firms whose employees are not covered by a union may raise wages to minimize potential unionization threats. I also find that these spillovers are harshest around the twentieth centile of the wage distribution, the same place where the associated surplus union premium for Black women is the strongest.

For robustness, I first run the difference in difference model without "always right-to-work" states, that is, without states that have had right-to-work laws before the sample starts. Results of the exercise have been relegated to the appendix and are presented in [Table 8](#). The baseline effect for men ranges from a decrease in wages by 2.13% with the inclusion state linear time trends to a decrease by 3.54% without, and for women ranging from a decrease in wages by 3.65% without state linear time trends to a decrease by 5.73% with. There remains a significant disproportionate effect

for Black women relative to white women, a little over two-fifths of the effect found when estimated with always right-to-work states included in the specification. The disproportionate effect for Black men relative to white men becomes hard to distinguish from zero. As before, a significant amount of the effect appears to be driven by spillover effects with the baseline spillover effects being as strong as they were under the previous specification in [Table 6](#) and the disproportionate effects being halved. I also present the corresponding RIF-DID regressions in [Figure 23](#) for all Black people and white people, [Figure 24](#) for spillover effects, and [Figure 25](#) for direct effects.

In order to test for parallel trends, I plot real log wage trends in each of the treatment states and the never right-to-work states. After dropping observations from Montana, Maine, New Hampshire, and Vermont for lack of common support, I regress real log wages on the covariates and industry, quarter, state, and year fixed effects separately for each subgroup. I then plot the resulting predicted real log wage trends for the never right-to-work states and the states that form the policy variation for the analysis including: Oklahoma (2001) in [Figure 14](#), Indiana (2012) in [Figure 15](#), Michigan (2013) in [Figure 16](#), Wisconsin (2015) in [Figure 17](#), and Kentucky (2017) in [Figure 18](#). This exercise shows that parallel trends largely appear to hold for women and white men. For Black men, sampling variation precludes us from coming to the same conclusion. Specifically, this may be a feature largely driven by the number of Black men observed also reveals that West Virginia and the policy variation that comes from their 2016 right-to-work legislation needed to be dropped from the analysis as the policy may have been anticipated for a significant number of years implying that its inclusion would have led to an over-estimation of the effect of de-unionization on racial wage inequality. Predicted real wage trends for West Virginia are rendered in [Figure 26](#) in the appendix showing early an early onset divergence in wages between West Virginia and the control states. In order to test for excludability, as discussed earlier, I have estimated specifications with the inclusion of state monthly unemployment rates and state linear time trends, particularly since recent right-to-work policies have been enacted in the aftermath of recessions.

I next follow a synthetic control approach which allows for a data-driven method to pick the control states from a pool of donor states. In the difference in difference model outlined before, each of the states that comprised the control group had been given the same weights. The synthetic control specification allows for one to construct a "synthetic" state by matching the treatment state with an optimally weighted average of the donor states in the period before the legislation has

passed, thereby allowing one to estimate the difference between the new right-to-work state and its synthetic counterpart to find the treatment effect. In order to run the estimation following the synthetic control method, I collapse the dataset by state and year to generate panel data. Since 1994 and 1995 have been dropped due to missing allocation flags, in order to use a panel without gaps I drop all observations before 1996. The inclusion of Montana, Maine, New Hampshire, and Vermont created an unbalanced panel for the analyses for Black people indicating an underlying lack of common support. I, therefore, have dropped these states from the analysis.

I estimate the specification separately for each subgroup. The predictor set includes the collapsed state-year proportions of covariates, including proportion in each industry, occupation, quarter, and education-experience cells, average education level, a quartic in experience, proportion in part-time positions, the public sector, married, in a CMSA, along with average real log wages from the entire pre-treatment period. I use the never right-to-work states as donor states. Results from policy variation generated by Michigan (2013) are presented in [Figure 19](#).¹ Associated weights assigned to donor states that comprise the synthetic states are presented in the appendix in [Table 9](#) for white men, [Table 10](#) for Black men, [Table 11](#) for white women, and [Table 12](#) for Black women. Reassuringly, for the vast majority of subgroups, Midwestern and rust-belt states feature in the synthetic states, including Ohio and at times Pennsylvania. Notice that I have treated the labor markets for white men, Black men, white women, and Black women as separate and have found synthetic control states for each. As a further robustness check, I also run a version of the synthetic control where donor states that have been dropped for Black people have also been dropped for white people, results of which are available upon request.² Unsurprisingly, I do not get as good a match for white people particularly because non-zero weights have been assigned to the states that have been dropped under certain permutations. Future research would benefit from implementing the synthetic control method on the Black-white interaction.

Synthetic control methods reveal possible underlying heterogeneity in the effect of right-to-work laws on real log wages across the states that generate policy variation. The effects range from a negative effect on real log wages to no effect. Splitting the samples by state, by race, and by sex indeed makes it hard to parse out a clear effect from the noise created due to sampling variation.

¹Results from policy variation generated by Oklahoma (2001) are presented in [Figure 27](#), Indiana (2012) in [Figure 28](#), Wisconsin (2015) in [Figure 29](#), and Kentucky (2017) in [Figure 30](#).

²Results can be found in the replication package.

This also in many cases makes it difficult to come up with good synthetic counterfactuals. Hence, the synthetic control method is not my preferred estimate. Clearly, as the figures show, the counterfactuals are relatively less noisy for white people in the pre-treatment period than they are for Black people. Thus, pooling the states together in a two-way fixed effects-esque staggered difference in difference specification is preferred in order to ensure that enough observations are in the requisite subgroups. Further, spillover effects may be sensitive to anticipation in the months leading up to the legislation given that they are grounded in the environment in which the policy is enacted. For example, if the political climate heavily leans towards an anti-union environment, then potential union threats could be seen as less credible in that environment even before the legislation has been enacted. Further, these laws are debated in view of the general public and the likelihood of the legislation passing is often discussed in the media in the weeks and months leading to the bill's passing and enactment. Therefore, employers may not feel much pressure to raise wages or adopt wage structures similar to unionized workplaces or less discriminatory wage structures. Further complications arise from the fact that most right-to-work laws come into force as existing collective bargaining agreements expire. For these reasons, an event study is not an appropriate econometric tool to analyze the effects of the right-to-work legislation, particularly the spillover effects of the policy.

With the RIF-regression analysis, I do not consider heaping of wages at \$10 and \$5 levels which has been documented to bias treatment effects, thereby causing measurement error (see, for example [Fortin et al., 2021](#); [Autor et al., 2016](#)). Another variable that has been omitted from the specification is state-level minimum wage increases which may be relevant to the analysis given that the disproportionate effects are concentrated at the bottom of the distribution. I am also clearly constrained by the number of Black observations in the CPS MORG for specific states in specific years.

6 Conclusion

I sought to understand the effect of labor unions, particularly the effect of de-unionization on racial wage inequality. In addition to outlining an OLS specification that characterizes the association between the two both at the mean as well as the throughout the wage distribution, I address

concerns about endogeneity by leveraging policy variation from recent right-to-work legislation in asking whether the Black-white wage gap has exacerbated as a consequence of right-to-work laws, where in the wage distribution are the effects are strongest and how much of the effect is driven by spillovers. I find that *ceteris paribus* the laws indeed appear to increase racial wage inequality with the effects concentrated at the bottom of the wage distribution and among women. These disproportionate effects are largely driven by spillovers. Interestingly, the legislation does not disproportionately affect Black people in labor unions as the effects are borne equally by Black people and white people who are covered by unions. I, therefore, provide evidence that *prima facie* race-neutral changes to labor market institutions including right-to-work laws that weaken labor unions have disparate impacts on outcomes of people of color.

Indeed, future work certainly needs to build on this study analyzing the relationship between labor unions and racial inequality and more generally the heterogeneous effects of changes to labor market institutions. As a start, decomposing the difference in difference specification of the effect of right-to-work laws on racial inequality following an extended version of the decomposition method outlined by [Goodman-Bacon \(2018\)](#) or an exploration of the surplus effects of the legislation using the synthetic control method with data that better represents the Black population would be fruitful avenues for further research. Future research will find it useful to replicate this analysis using data from the American Community Survey which may not include a question about union membership but would potentially provide a richer set of Black observations in order to test for the disproportionate spillover effects of de-unionization. As recommended by [Fortin et al. \(2021\)](#), future studies would also indeed benefit from the use of matched employer-employee datasets to better understand underlying mechanisms. Future work may also explore a diverse set of labor market institutions including the disparate impacts of shared corporate governance relationships building on work by [Jäger et al. \(2020\)](#).

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Figures

Figure 1: Union Coverage Time Trends

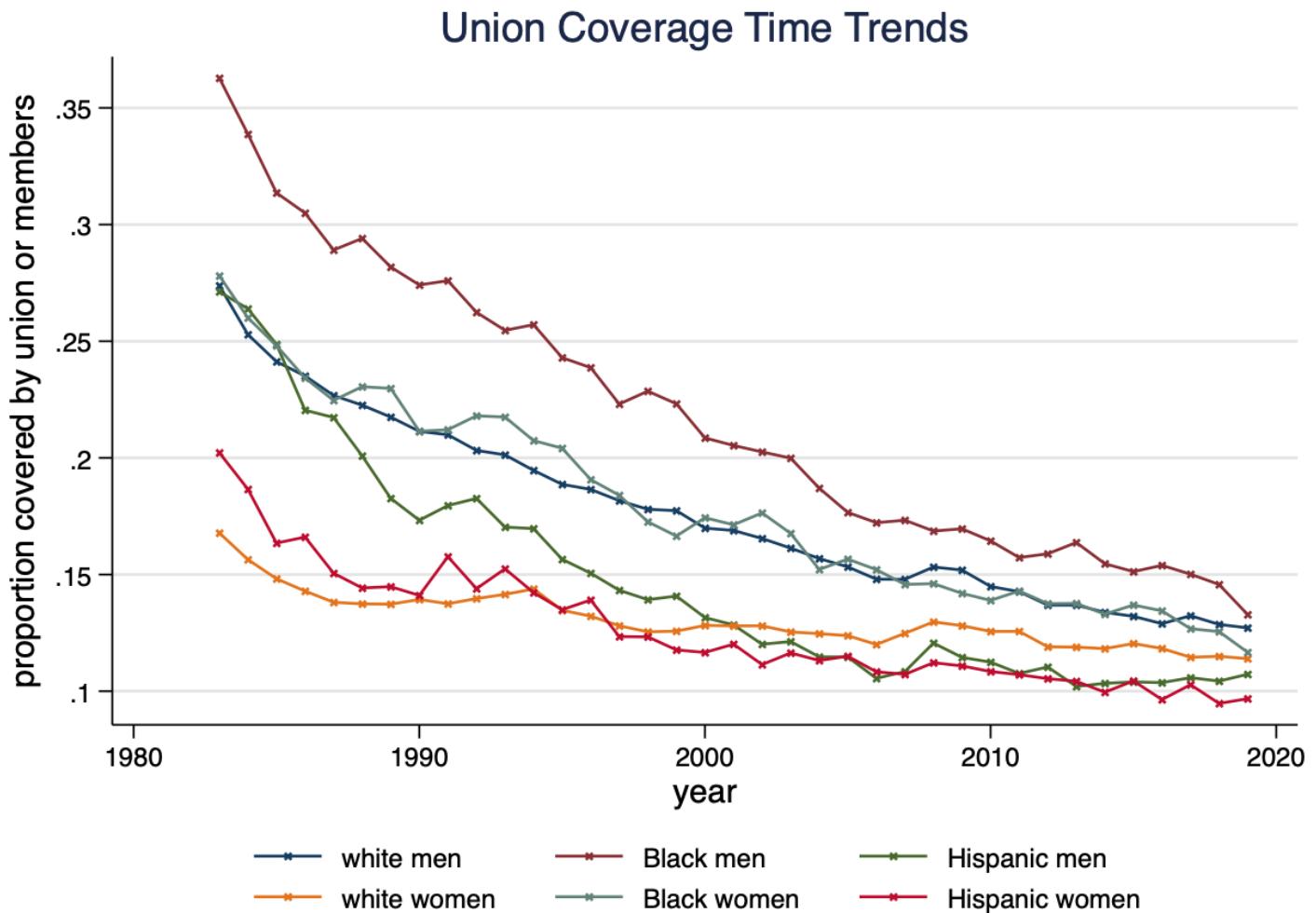


Figure 2: Private and Public Sector Union Coverage Time Trends

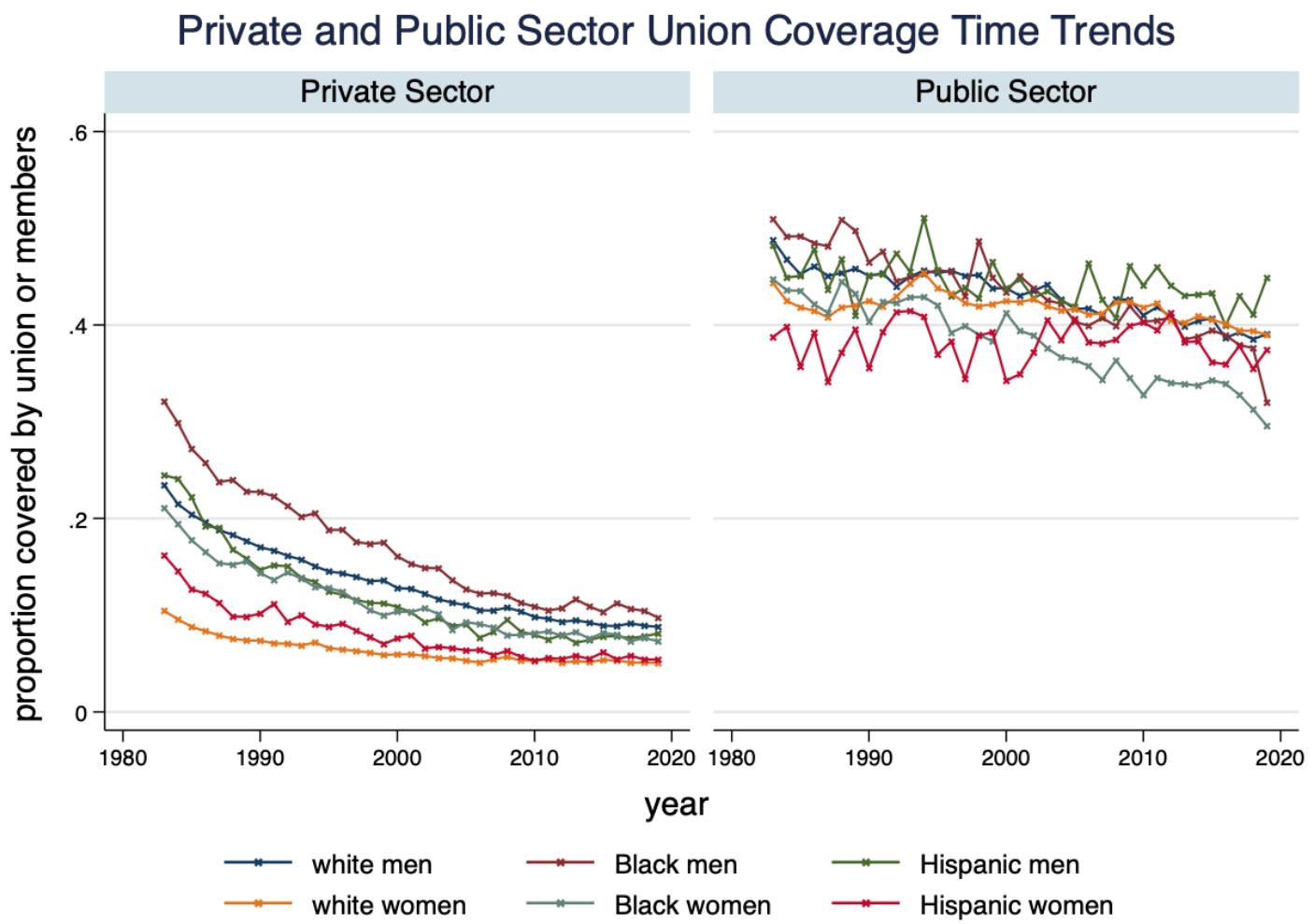


Figure 3: Median Real Log Wages Time Trends

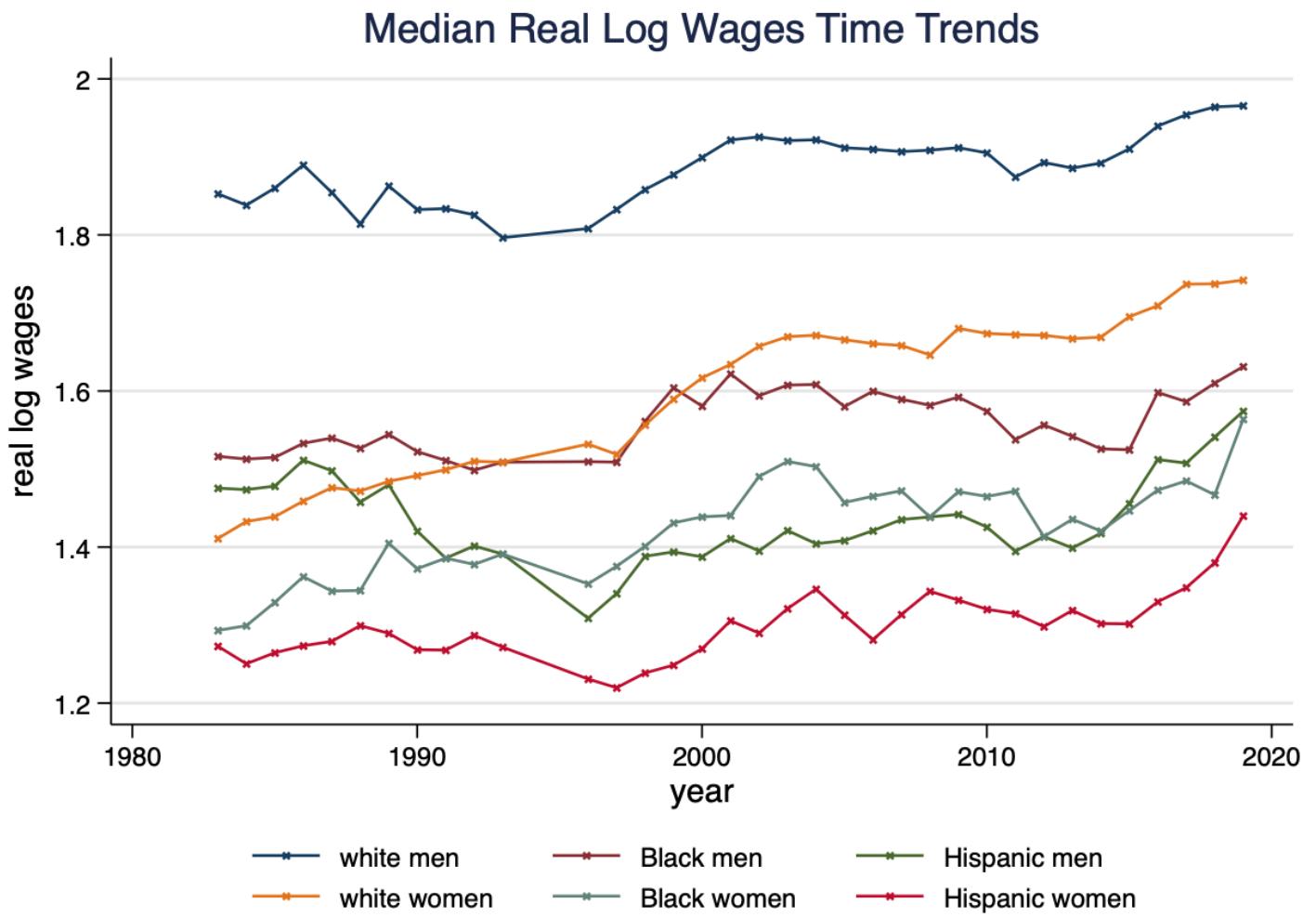
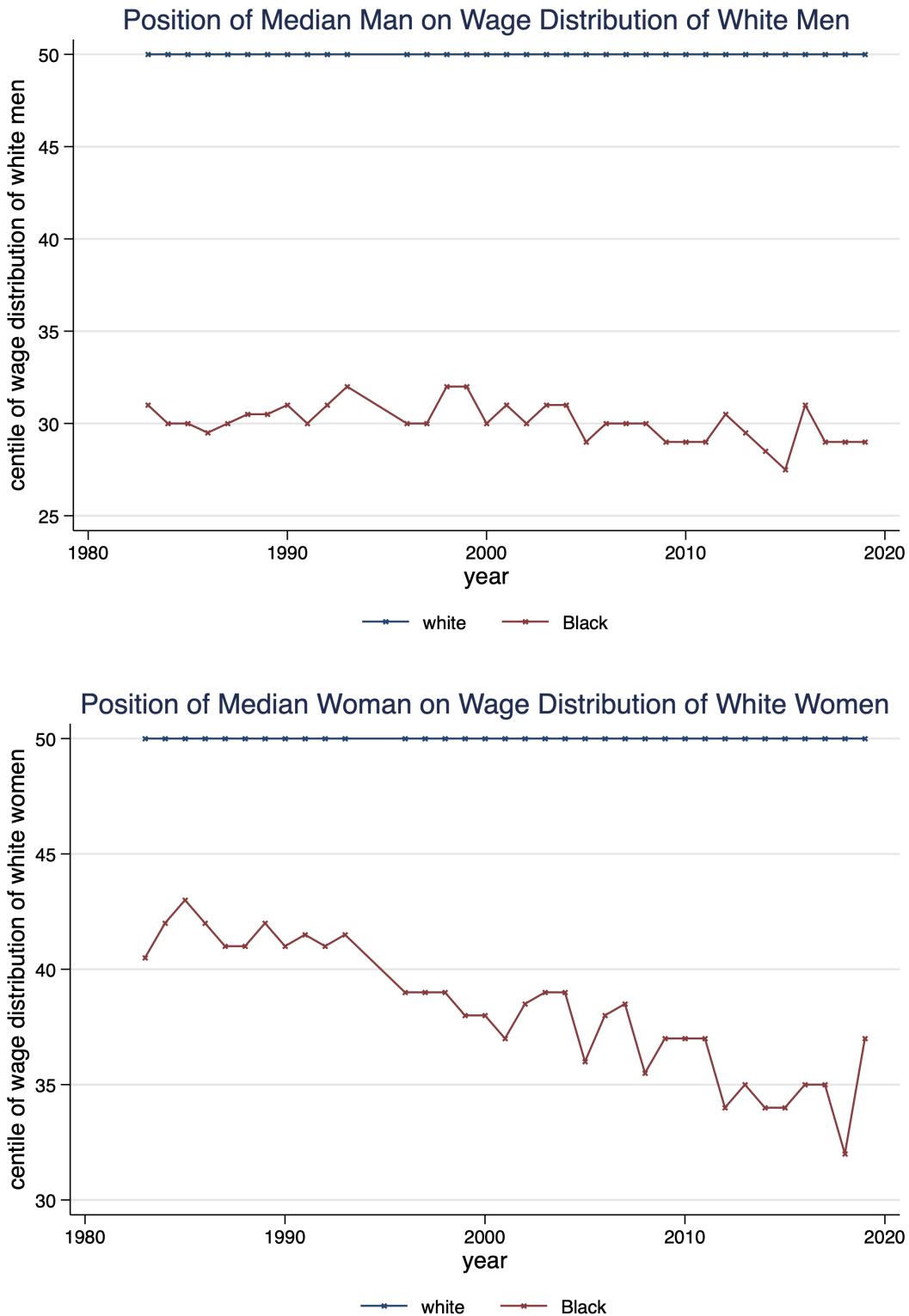
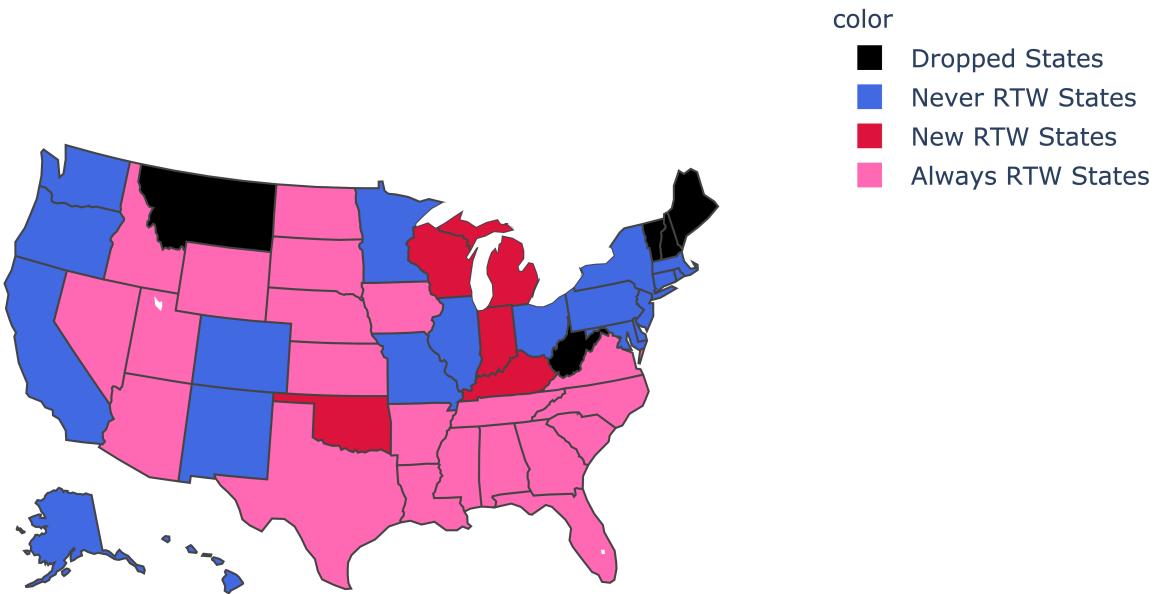


Figure 4: Position of Median Person on Wage Distribution of White Person



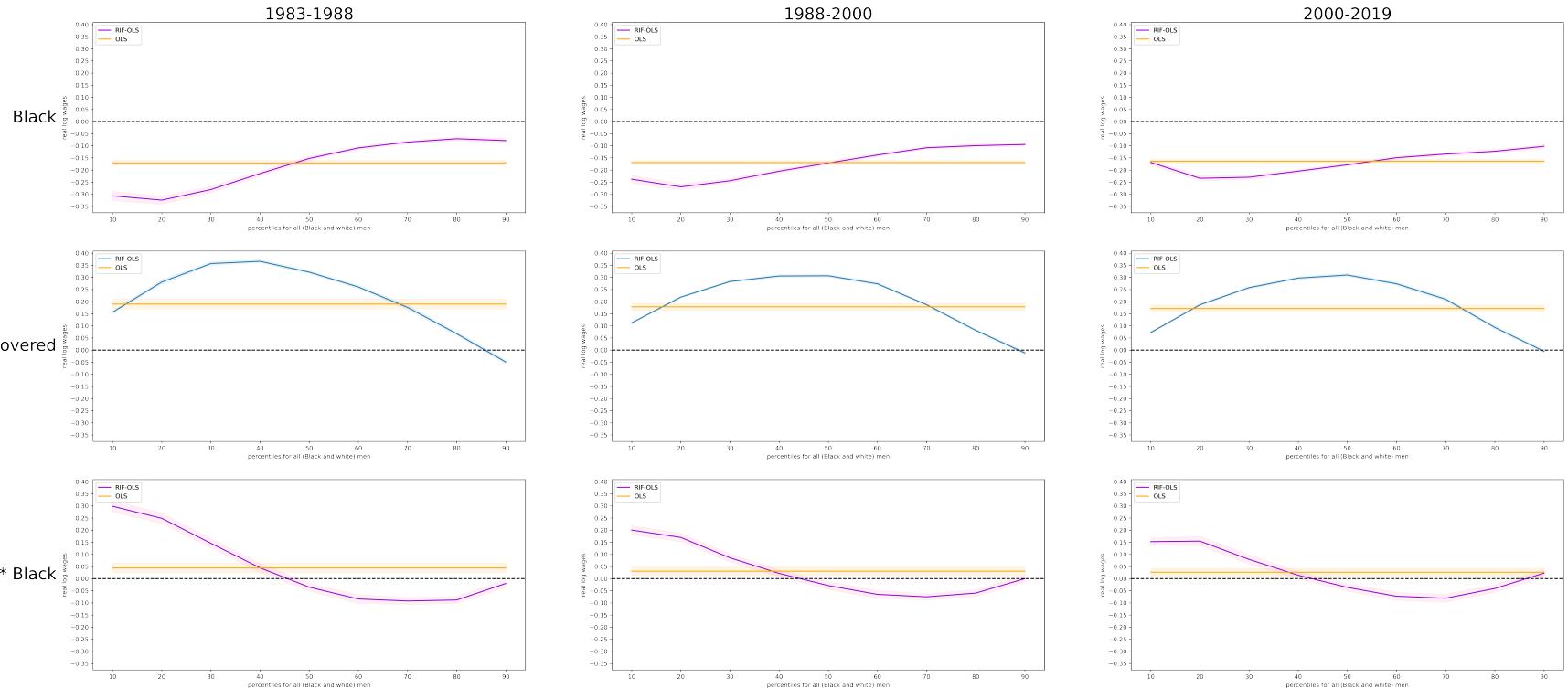
Sample include years 1983-2019 excluding 1994 and 1995.

Figure 5: Right-to-Work States



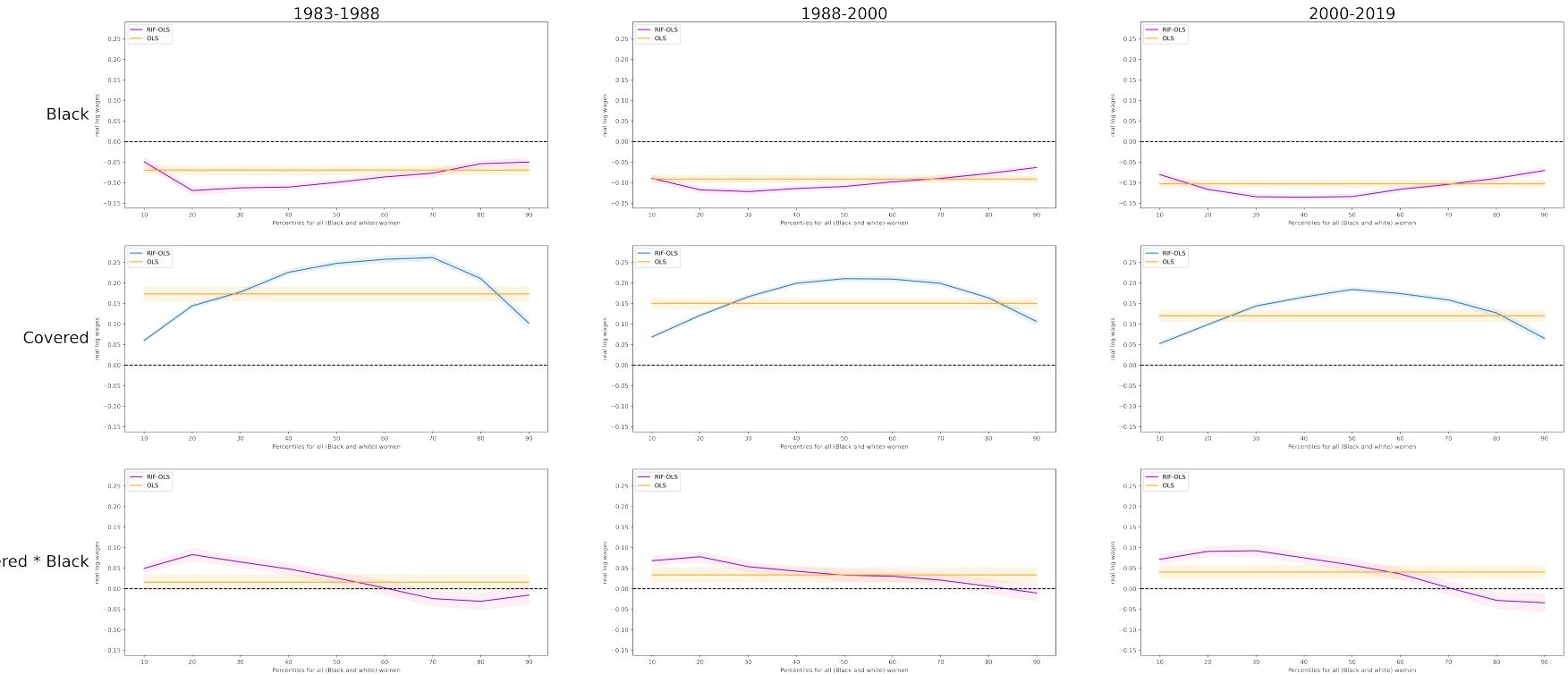
Always RTW states defined as states that have had RTW laws before 1989.

Figure 6: RIF-OLS Regression of Real Log Wages on Union Coverage and Race - Men



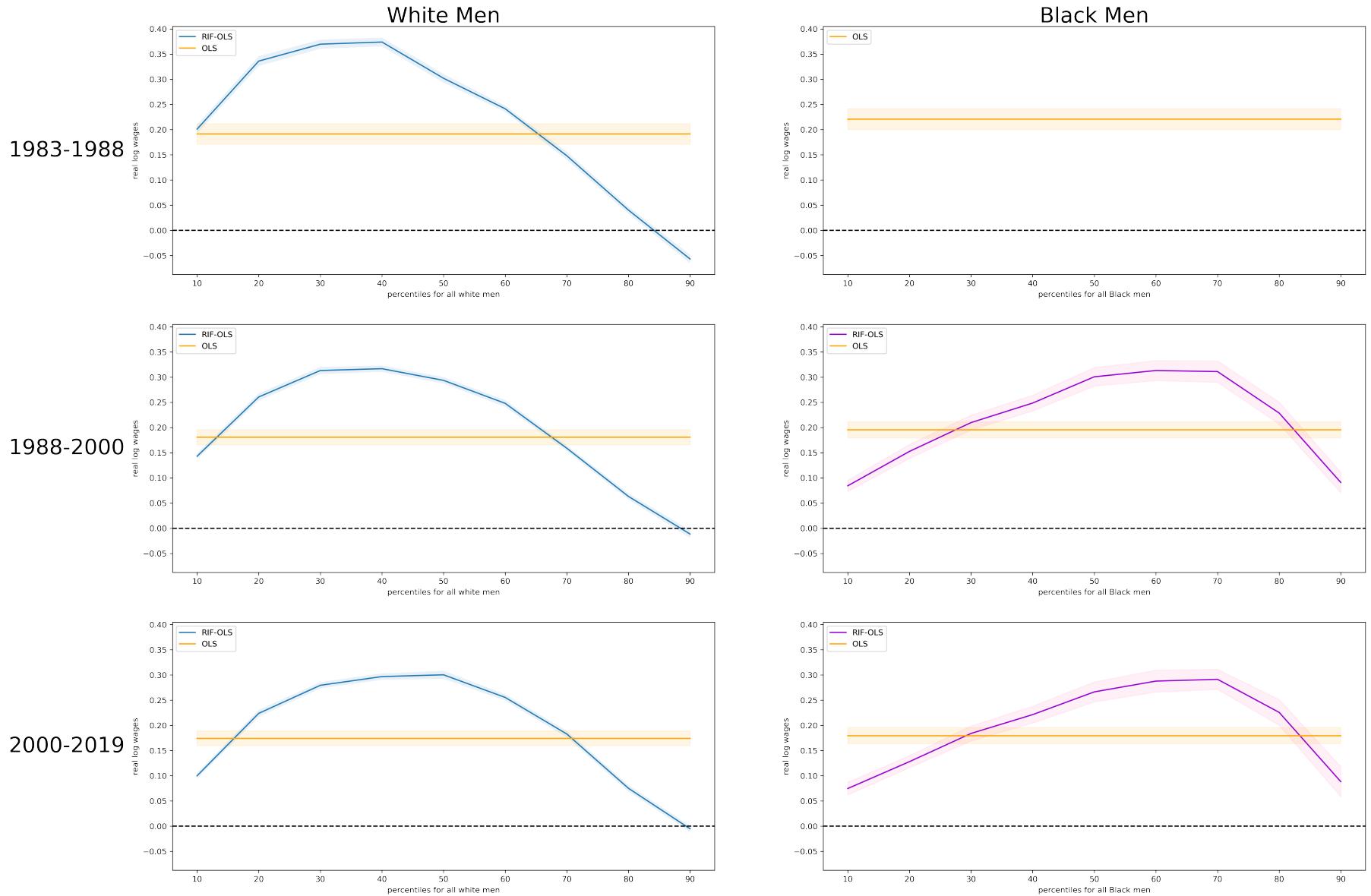
Standard errors for the OLS regression are clustered at the state-industry level. Standard errors for the RIF-OLS regression are bootstrapped with 100 replicates. Real log wages is the dependent variable throughout. Each column corresponds to a regression and associated set of RIF-OLS regressions. 95% confidence intervals presented. All regressions include covariates and fixed effects.

Figure 7: RIF-OLS Regression of Real Log Wages on Union Coverage and Race - Women



Standard errors for the OLS regression are clustered at the state-industry level. Standard errors for the RIF-OLS regression are bootstrapped with 100 replicates. Real log wages is the dependent variable throughout. Each column corresponds to a regression and associated set of RIF-OLS regressions. 95% confidence intervals presented. All regressions include covariates and fixed effects.

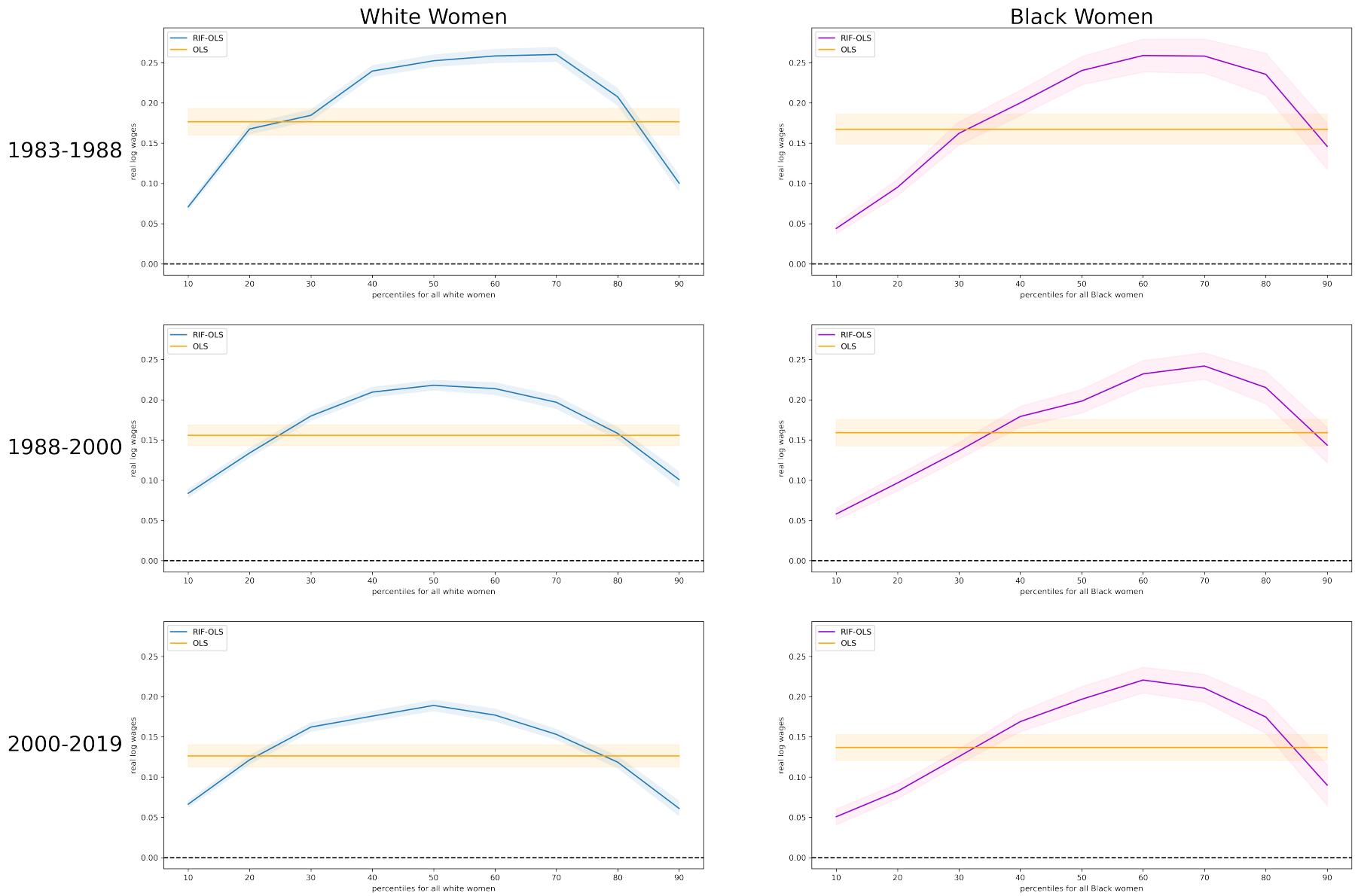
Figure 8: RIF-OLS Regression of Real Log Wages on Union Coverage by Race - Men



Standard errors for the OLS regression are clustered at the state-industry level. Standard errors for the RIF-OLS regression are bootstrapped with 100 replicates. Real log wages is the dependent variable throughout. Each sub-figure corresponds to a regression and associated set of RIF-OLS regressions. 95% confidence intervals presented. All regressions include covariates and fixed effects. RIF-OLS for Black men in 1983-1988 period not shown due to collinearity issues.

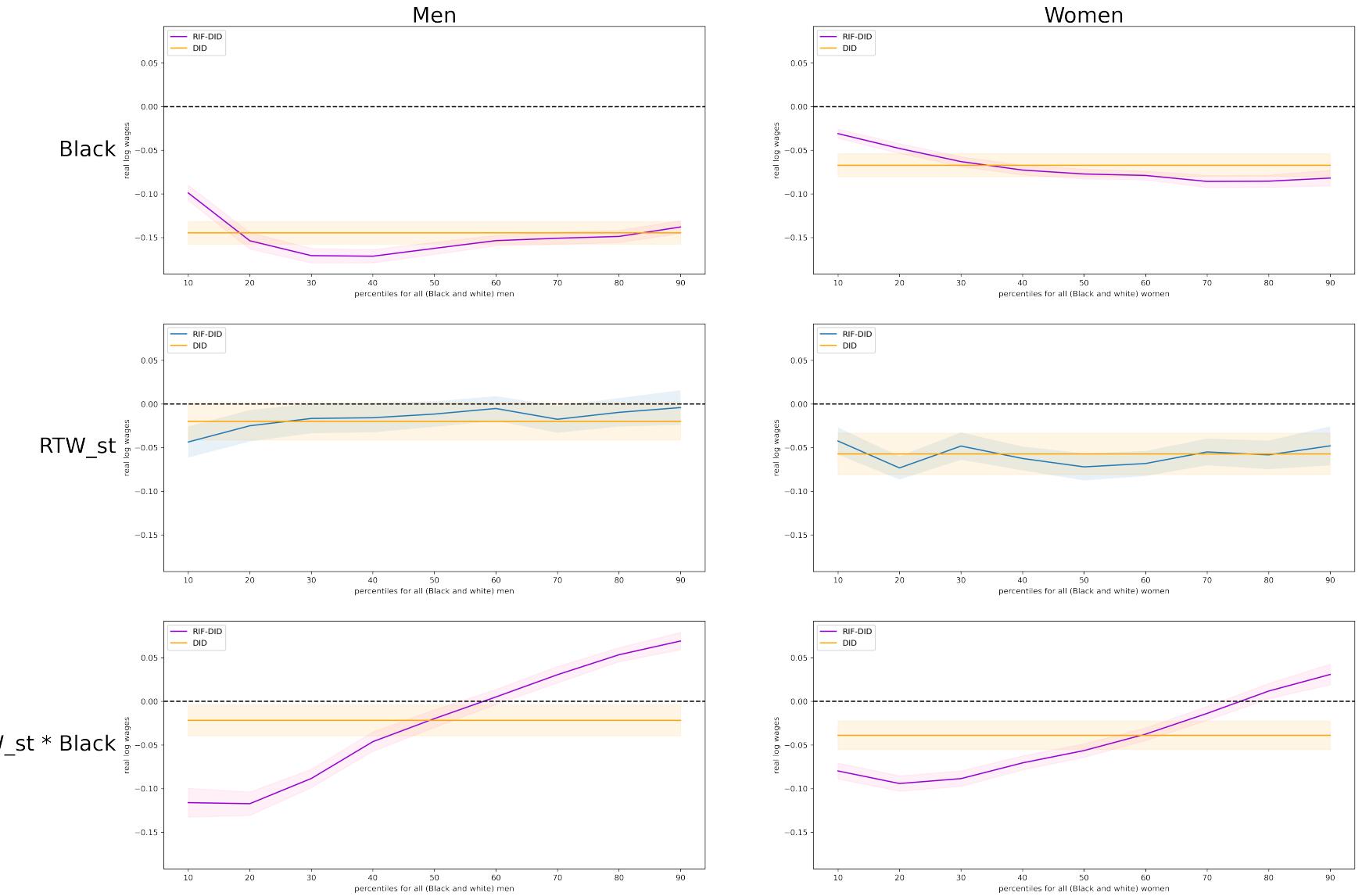
Figure 9: RIF-OLS Regression of Real Log Wages on Union Coverage by Race - Women

35



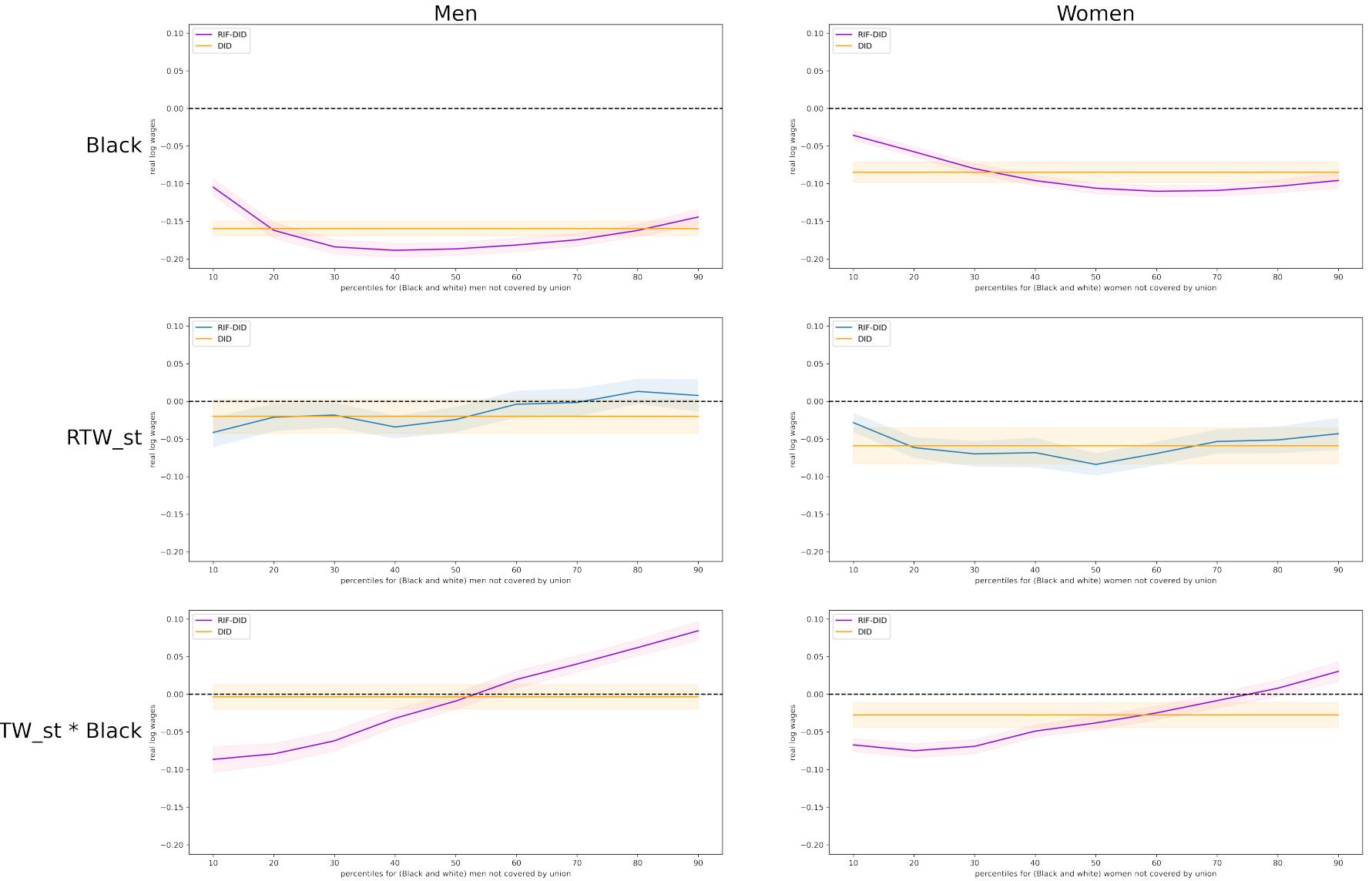
Standard errors for the OLS regression are clustered at the state-industry level. Standard errors for the RIF-OLS regression are bootstrapped with 100 replicates. Real log wages is the dependent variable throughout. Each sub-figure corresponds to a regression and associated set of RIF-OLS regressions. 95% confidence intervals presented. All regressions include covariates and fixed effects.

Figure 10: RIF-DID Regression of Real Log Wages on right-to-work Laws Treatment in State and Time - All (Black and White) People



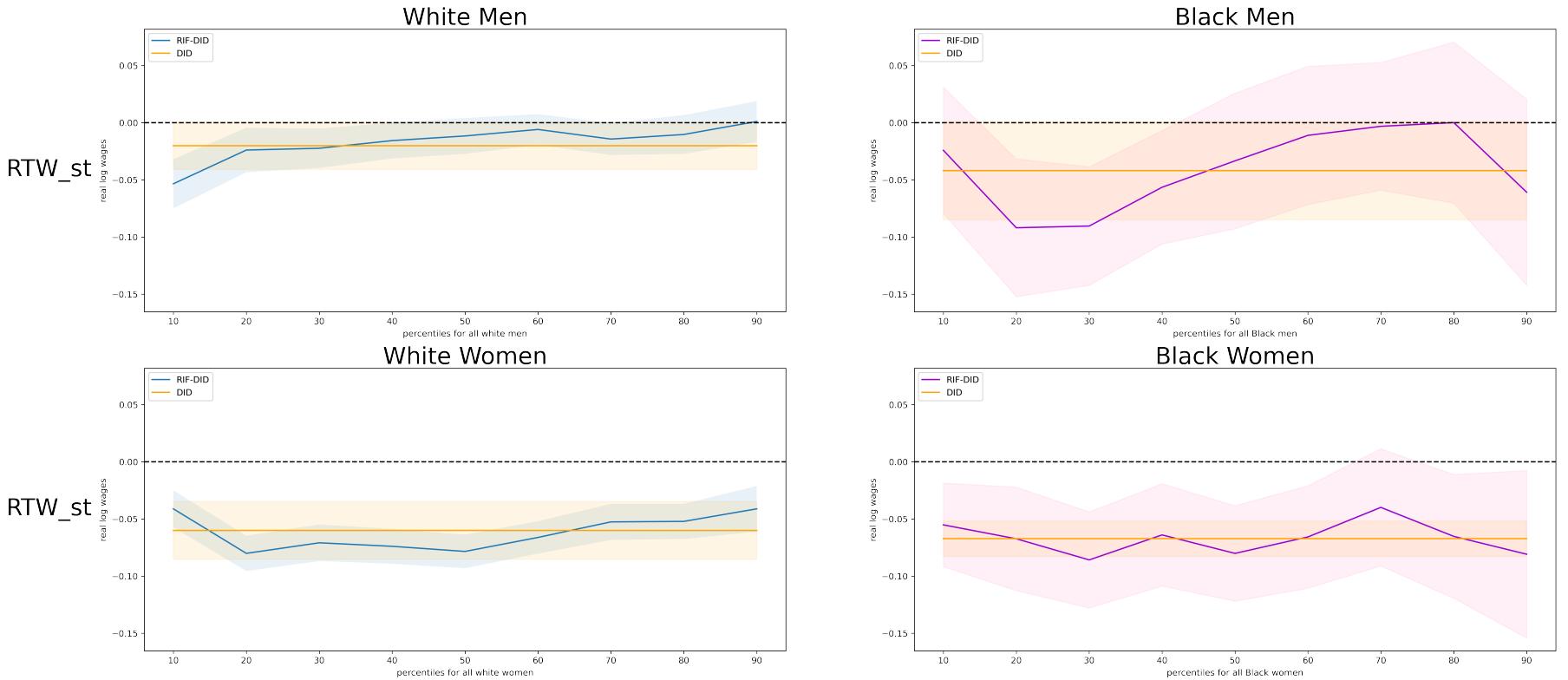
I define $RTW_{st} = 1$ if state s has a RTW law at time t . Sample includes years 1983-2019. Policy variation: Oklahoma (2001), Indiana (2012), Michigan (2013), Wisconsin (2015), and Kentucky (2017). All states, except West Virginia, Montana, Maine, New Hampshire, and Vermont are in the sample. Standard errors for the DID regression are clustered at the state-level. Standard errors for the RIF-DID regression are bootstrapped with 100 replicates. Real log wages is the dependent variable throughout. Each column corresponds to a regression and associated set of RIF-DID regressions. 95% confidence intervals presented. All regressions include covariates and fixed effects as outlined. State monthly unemployment rate and state linear time trends included. Year fixed effects not included.

Figure 11: RIF-DID Regression of Real Log Wages on Right-To-Work Laws Treatment in State and Time - People Not Covered by Union - Spillover Effect



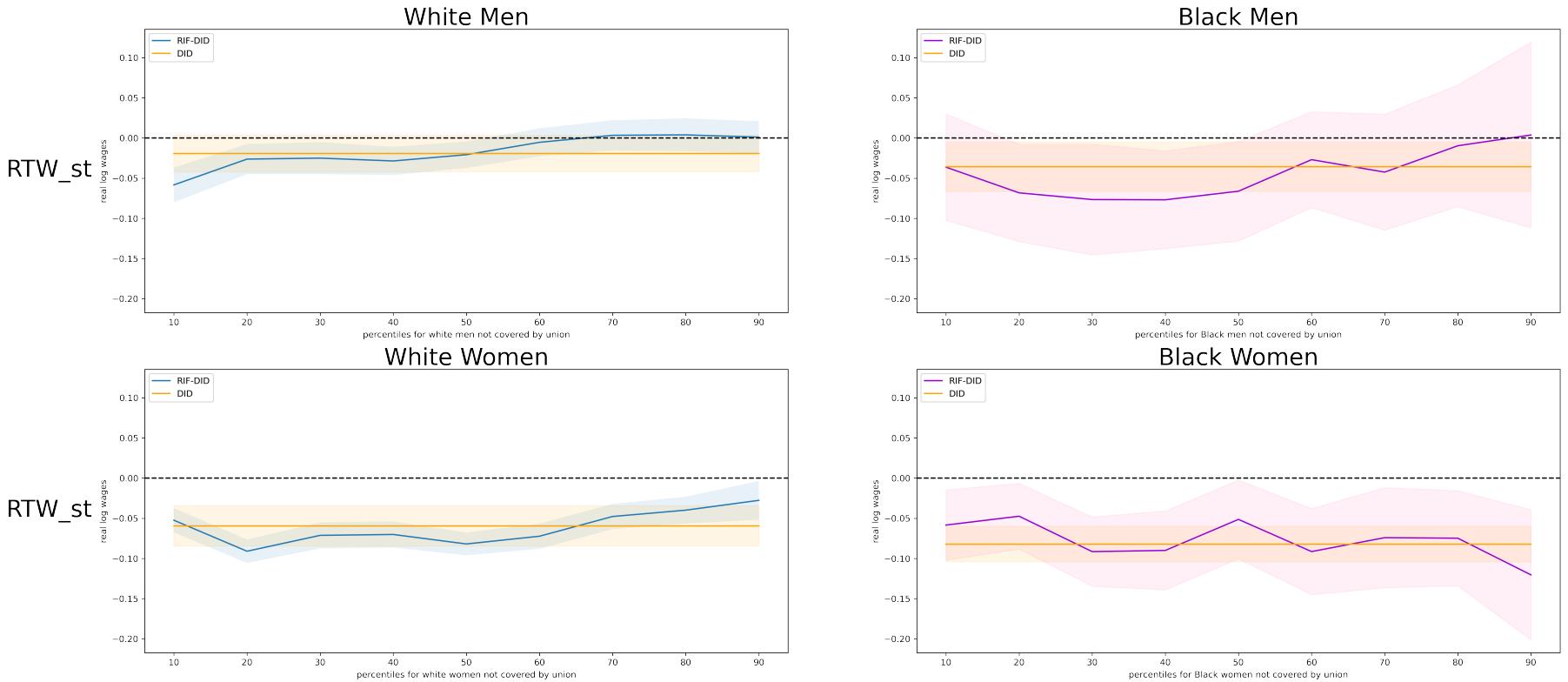
I define $RTW_{st} = 1$ if state s has a RTW law at time t . Sample includes years 1983-2019. Policy variation: Oklahoma (2001), Indiana (2012), Michigan (2013), Wisconsin (2015), and Kentucky (2017). All states, except West Virginia, Montana, Maine, New Hampshire, and Vermont are in the sample. Standard errors for the DID regression are clustered at the state-level. Standard errors for the RIF-DID regression are bootstrapped with 100 replicates. Real log wages is the dependent variable throughout. Each column corresponds to a regression and associated set of RIF-DID regressions. 95% confidence intervals presented. All regressions include covariates and fixed effects as outlined. State monthly unemployment rate and state linear time trends included. Year fixed effects not included.

Figure 12: RIF-DID Regression of Real Log Wages on Right-To-Work Laws Treatment in State and Time - All (Black and White) People Separately by Race and Sex



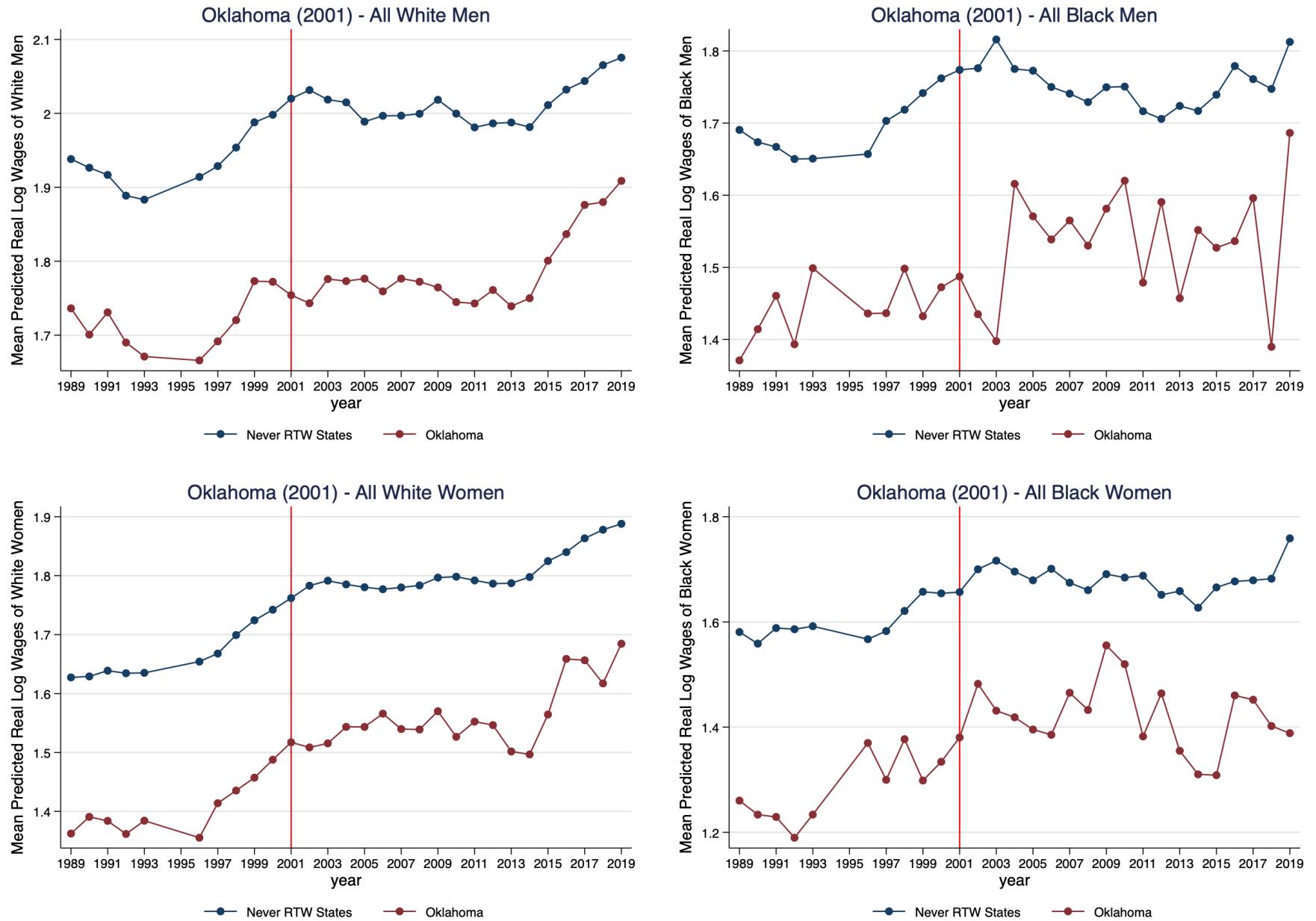
I define $RTW_{st} = 1$ if state s has a RTW law at time t . Sample includes years 1983-2019. Policy variation: Oklahoma (2001), Indiana (2012), Michigan (2013), Wisconsin (2015), and Kentucky (2017). All states, except West Virginia, Montana, Maine, New Hampshire, and Vermont are in the sample. Standard errors for the DID regression are clustered at the state-level. Standard errors for the RIF-DID regression are bootstrapped with 100 replicates. Real log wages is the dependent variable throughout. Each sub-figure corresponds to a regression and associated set of RIF-DID regressions. 95% confidence intervals presented. All regressions include covariates and fixed effects as outlined. State monthly unemployment rate and state linear time trends included. Year fixed effects not included.

Figure 13: RIF-DID Regression of Real Log Wages on Right-To-Work Laws Treatment in State and Time - People Not Covered by Union Separately by Race and Sex - Spillover Effect



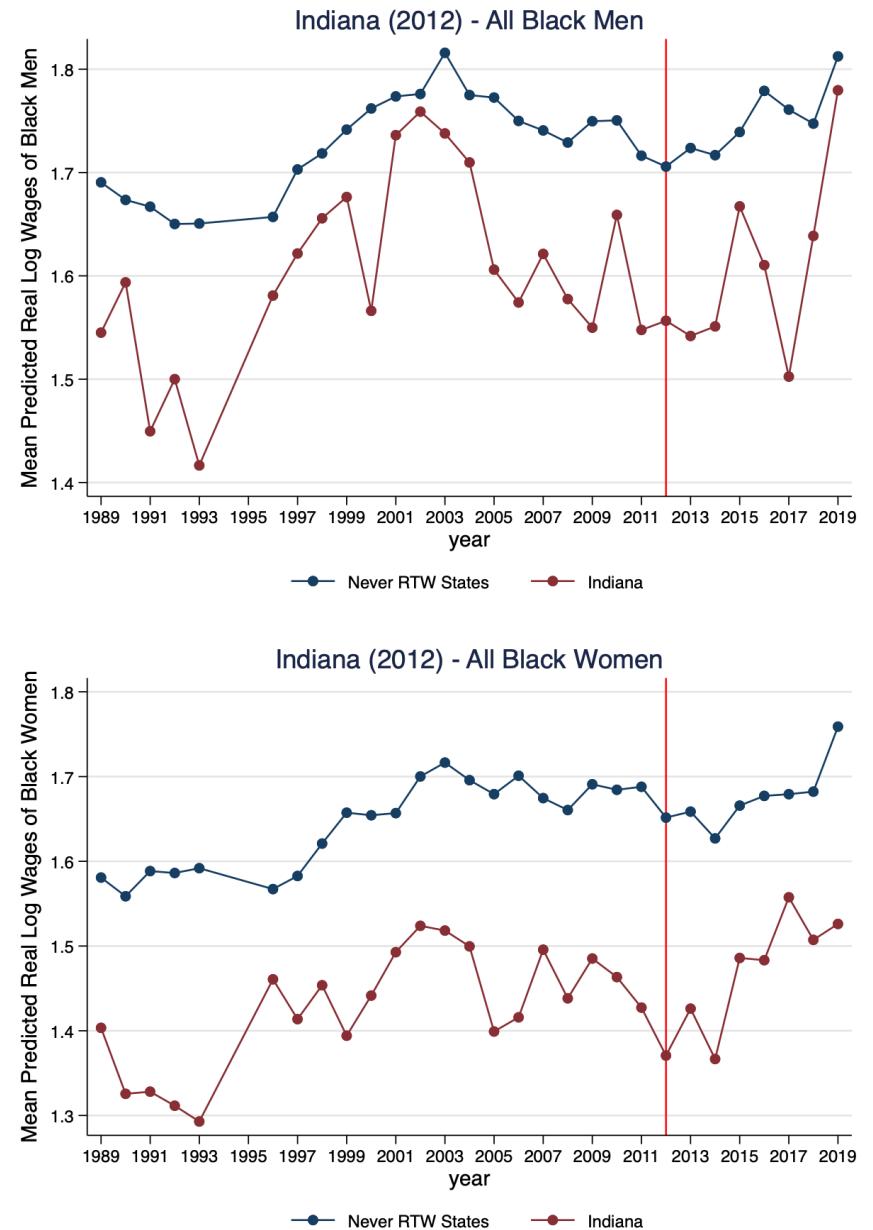
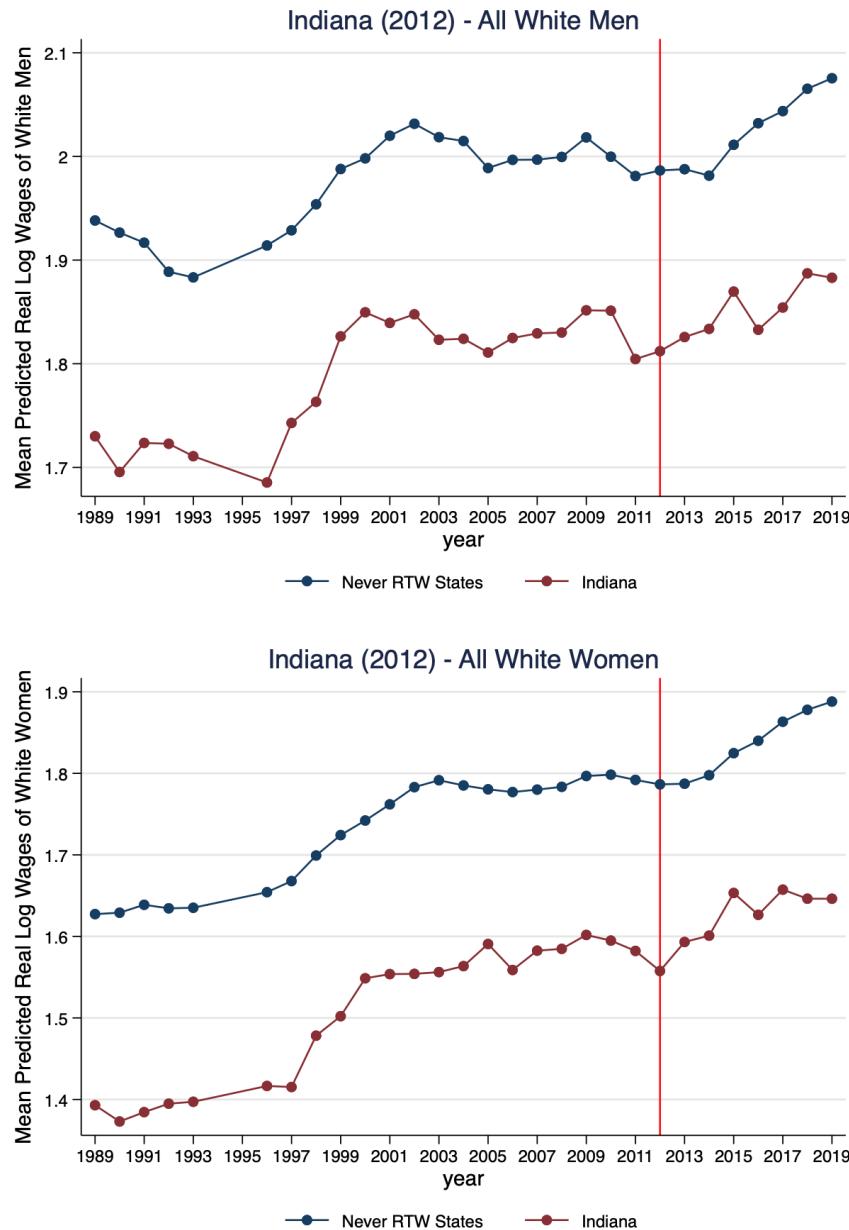
I define $RTW_{st} = 1$ if state s has a RTW law at time t . Sample includes years 1983-2019. Policy variation: Oklahoma (2001), Indiana (2012), Michigan (2013), Wisconsin (2015), and Kentucky (2017). All states, except West Virginia, Montana, Maine, New Hampshire, and Vermont are in the sample. Standard errors for the DID regression are clustered at the state-level. Standard errors for the RIF-DID regression are bootstrapped with 100 replicates. Real log wages is the dependent variable throughout. Each sub-figure corresponds to a regression and associated set of RIF-DID regressions. 95% confidence intervals presented. All regressions include covariates and fixed effects as outlined. State monthly unemployment rate and state linear time trends included. Year fixed effects not included.

Figure 14: Predicted Real Log Wage Trends - Oklahoma (2001)



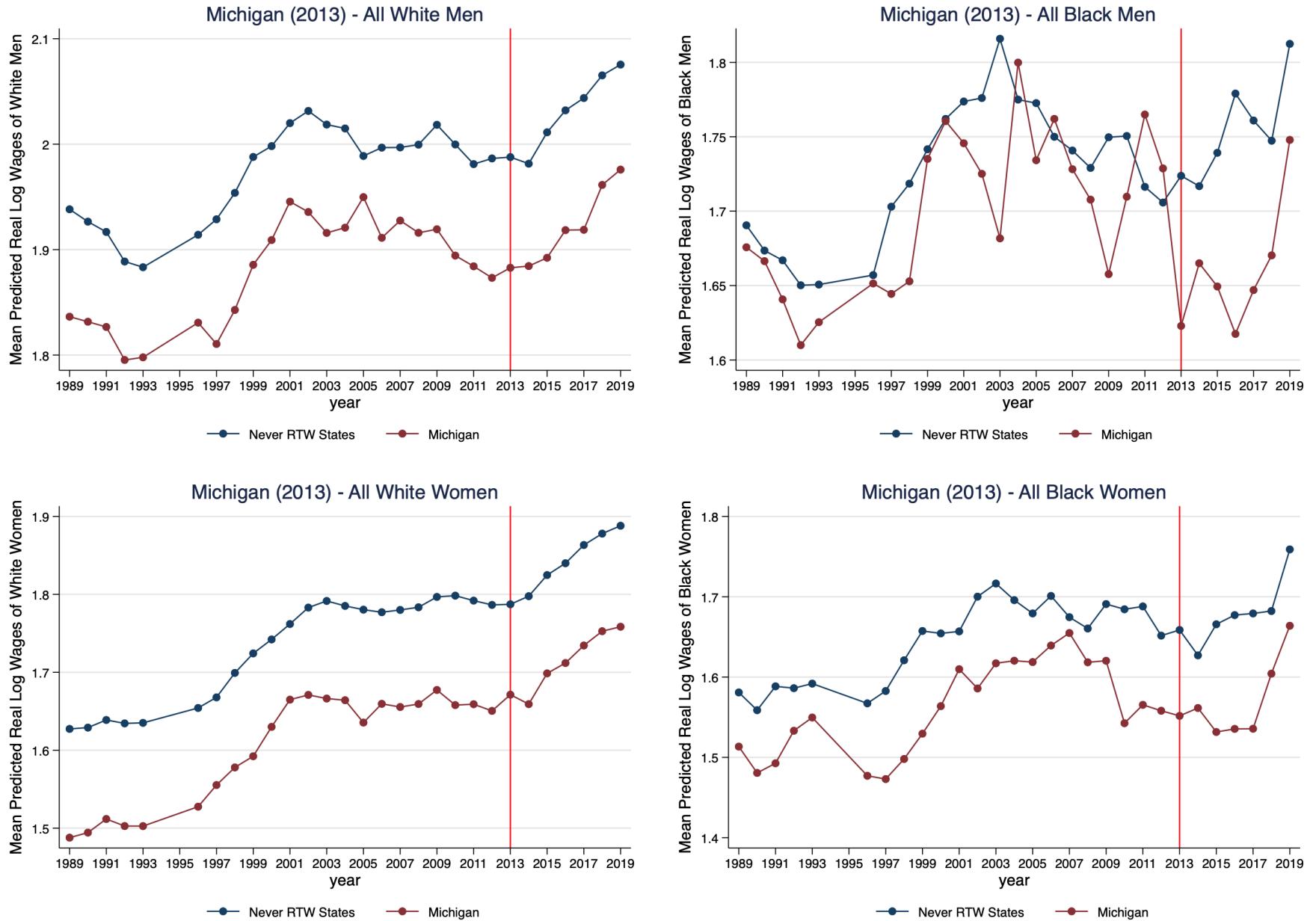
Predicted real log wages obtained from regression of real log wages on covariates and fixed effects separately for each subgroup.

Figure 15: Predicted Real Log Wage Trends - Indiana (2012)



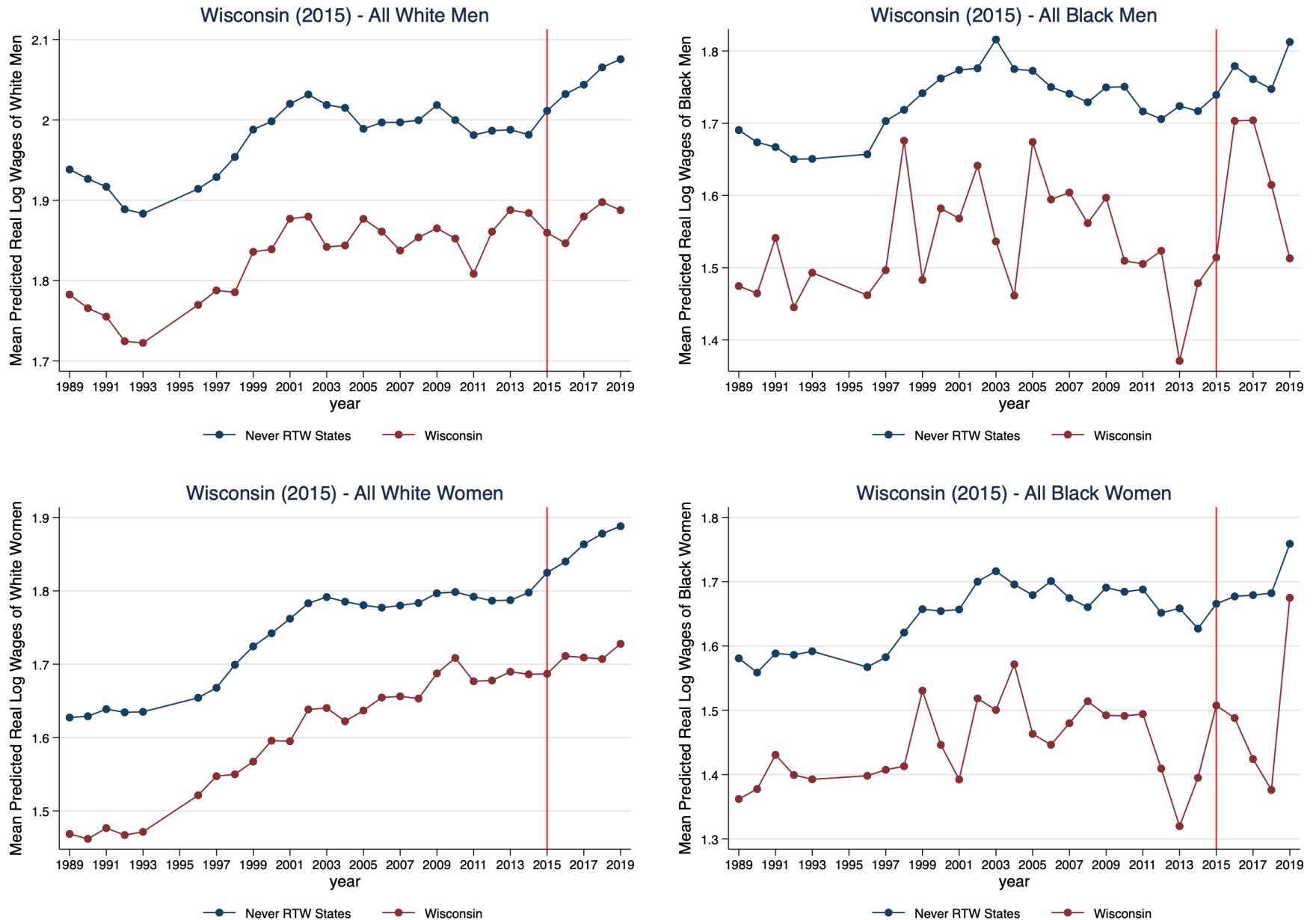
Predicted real log wages obtained from regression of real log wages on covariates and fixed effects separately for each subgroup.

Figure 16: Predicted Real Log Wage Trends - Michigan (2013)



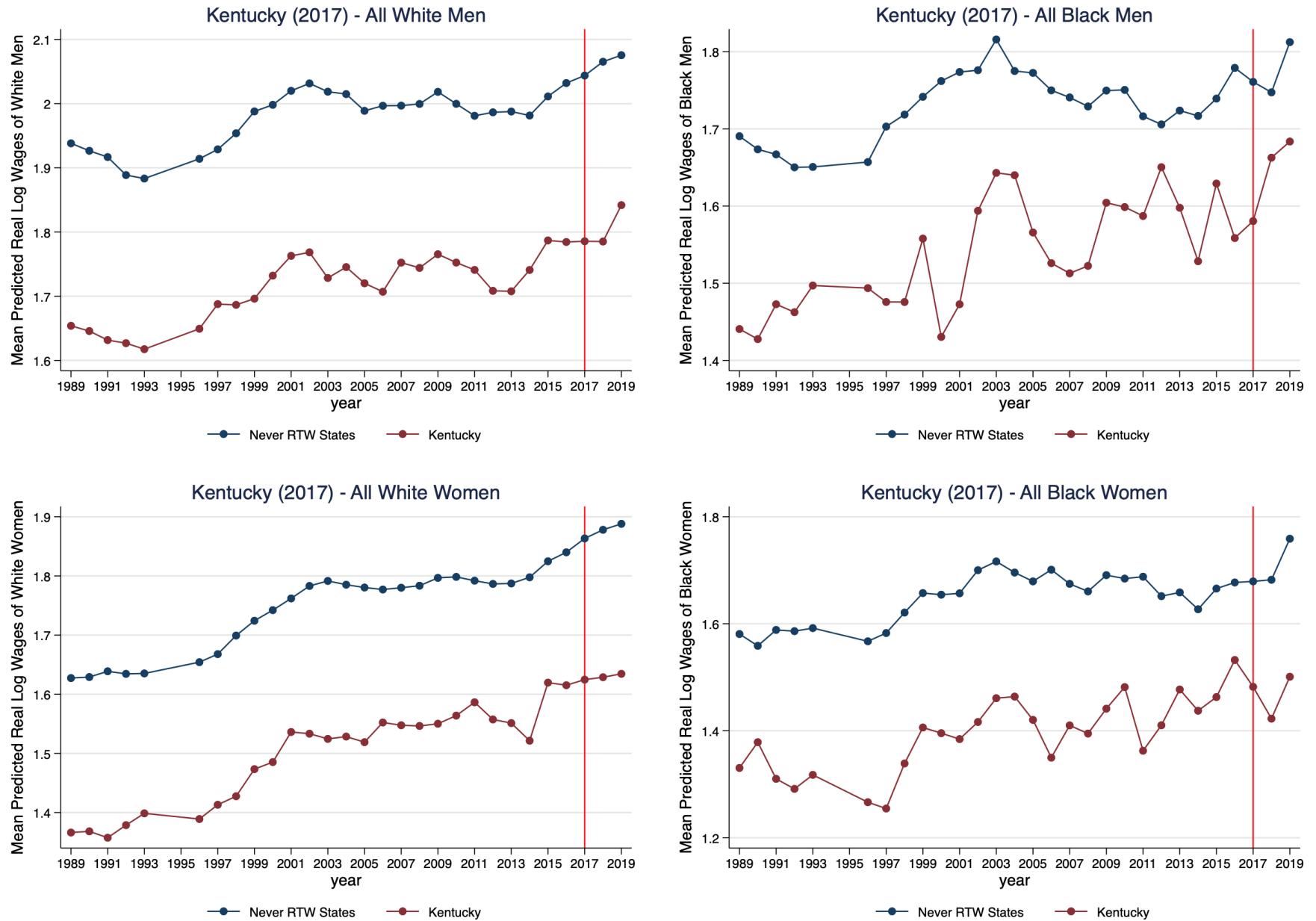
Predicted real log wages obtained from regression of real log wages on covariates and fixed effects separately for each subgroup.

Figure 17: Predicted Real Log Wage Trends - Wisconsin (2015)



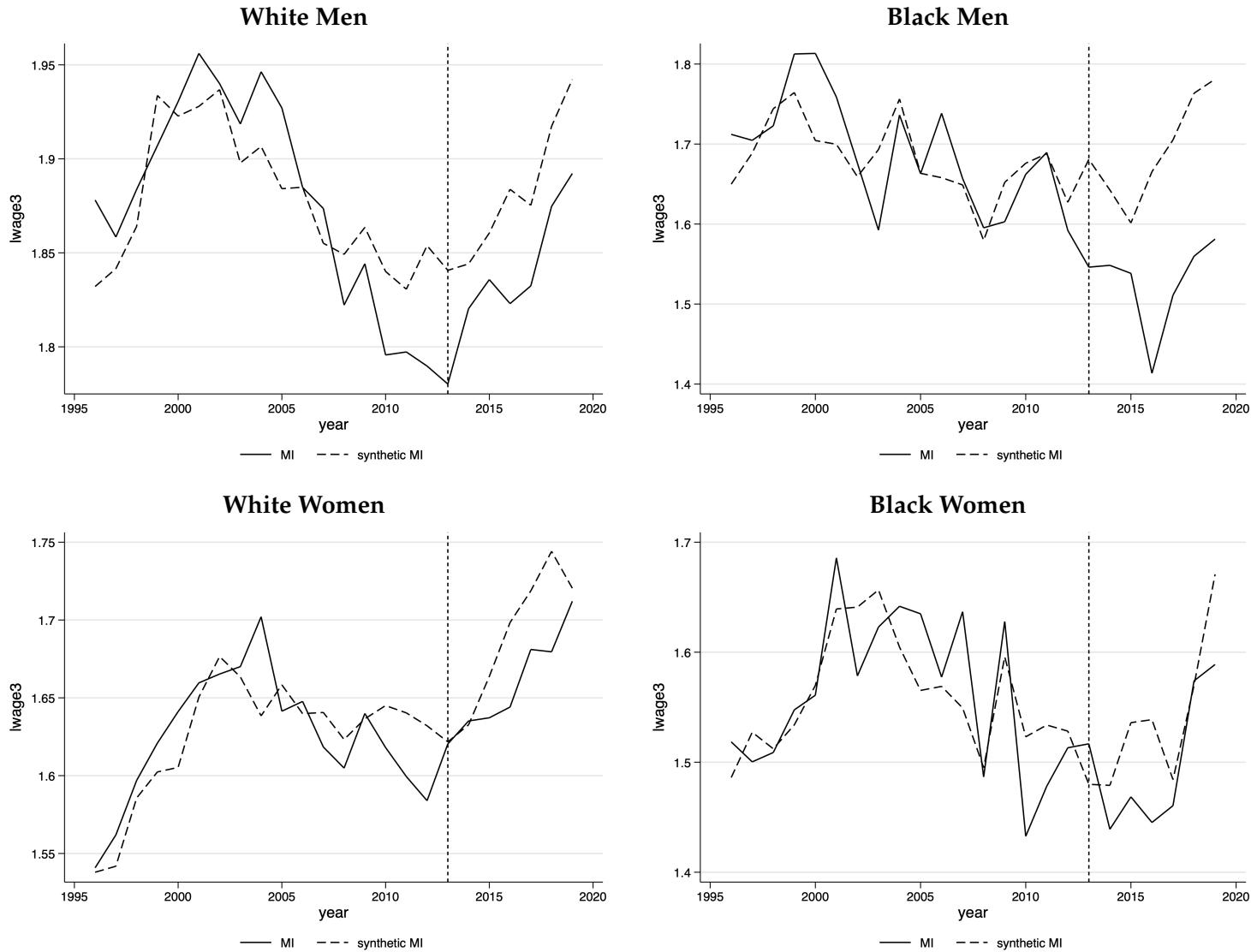
Predicted real log wages obtained from regression of real log wages on covariates and fixed effects separately for each subgroup.

Figure 18: Predicted Real Log Wage Trends - Kentucky (2017)



Predicted real log wages obtained from regression of real log wages on covariates and fixed effects separately for each subgroup.

Figure 19: Synthetic Control Method - Michigan (2013)



Real log wages is the dependent variable throughout. Synthetic control implemented using synth package with nested optimization and allopt. Each sub-figure corresponds to a regression.

Tables

Table 1: Summary Statistics by Hispanicity, Race, and Sex

	White Men	Black Men	Hispanic Men	White Women	Black Women	Hispanic Women
Union Coverage	0.183 (0.387)	0.225 (0.418)	0.136 (0.343)	0.137 (0.344)	0.183 (0.387)	0.125 (0.331)
Union Membership	0.168 (0.374)	0.203 (0.402)	0.124 (0.329)	0.119 (0.324)	0.159 (0.366)	0.109 (0.312)
Real Log Wage (1979\$)	1.874 (0.595)	1.594 (0.537)	1.507 (0.515)	1.617 (0.552)	1.480 (0.519)	1.385 (0.492)
Education	13.708 (2.409)	12.933 (2.331)	11.180 (3.425)	13.813 (2.282)	13.244 (2.244)	12.050 (3.111)
Experience	18.403 (12.300)	18.118 (12.039)	18.003 (11.976)	18.442 (12.638)	18.152 (11.962)	17.683 (12.345)
Public Sector	0.146 (0.353)	0.187 (0.390)	0.084 (0.278)	0.195 (0.397)	0.243 (0.429)	0.153 (0.360)
Observations	1609600	150237	209055	1550943	194743	163809

Sample includes years 1983-2019 excluding 1994 and 1995.

Table 2: Percentage in each Industry by Hispanicity, Race, and Sex.

Industry	White Men	Black Men	Hispanic Men	White Women	Black Women	Hispanic Women	Total
Primary Sector	2.311	1.383	5.899	0.713	0.237	1.584	1.802
Construction	9.360	5.809	15.908	1.283	0.469	0.988	5.735
Manufacturing	20.337	18.044	16.926	8.978	9.120	12.031	14.631
Wholesale and Retail Trade	20.625	21.416	24.557	20.878	18.279	26.258	21.261
Transportation and Utilities	9.893	13.540	7.975	4.449	6.149	4.075	7.358
Financial Services	5.285	4.202	3.351	8.473	7.041	6.507	6.423
Business and Professional Services	12.608	11.608	12.177	10.457	8.521	9.894	11.335
Health and Welfare Services	4.435	7.673	3.382	20.542	26.634	18.012	12.526
Educational Services	6.488	6.087	3.357	15.446	11.807	10.305	10.045
Personal Services	2.742	3.317	3.331	4.177	4.277	6.569	3.657
Public Administration	5.917	6.922	3.137	4.606	7.467	3.776	5.228
Total	100.000	100.000	100.000	100.000	100.000	100.000	100.000

Sample includes years 1983-2019 excluding 1994 and 1995.

Table 3: Union Coverage Proportion in each Industry by Hispanicity, Race, and Sex.

Industry	White Men	Black Men	Hispanic Men	White Women	Black Women	Hispanic Women	Total
Primary Sector	0.076	0.073	0.035	0.029	0.044	0.037	0.056
Construction	0.234	0.208	0.107	0.060	0.157	0.053	0.188
Manufacturing	0.184	0.252	0.145	0.101	0.183	0.105	0.162
Wholesale and Retail Trade	0.060	0.077	0.066	0.046	0.057	0.053	0.056
Transportation and Utilities	0.322	0.366	0.257	0.234	0.361	0.214	0.299
Financial Services	0.030	0.091	0.081	0.021	0.055	0.035	0.032
Business and Professional Services	0.042	0.081	0.059	0.022	0.054	0.051	0.039
Health and Welfare Services	0.109	0.186	0.144	0.088	0.148	0.118	0.105
Educational Services	0.379	0.356	0.348	0.429	0.383	0.367	0.405
Personal Services	0.093	0.140	0.132	0.038	0.082	0.069	0.072
Public Administration	0.412	0.396	0.454	0.305	0.329	0.350	0.369
Total	0.169	0.206	0.125	0.134	0.171	0.121	0.152

Sample includes years 1983-2019 excluding 1994 and 1995.

Table 4: OLS Regression of Real Log Wages on Union Coverage and Race

	Men	Women
Panel A: 1983-1988		
<i>Black</i>	-0.170*** (0.00549)	-0.0693*** (0.00576)
<i>covered</i>	0.190*** (0.0103)	0.173*** (0.00845)
<i>covered</i> × <i>Black</i>	0.0441*** (0.00979)	0.0160* (0.00921)
Observations	312495	286909
Panel B: 1988-2000		
<i>Black</i>	-0.169*** (0.00418)	-0.0912*** (0.00415)
<i>covered</i>	0.179*** (0.00756)	0.151*** (0.00640)
<i>covered</i> × <i>Black</i>	0.0304*** (0.00900)	0.0336*** (0.00905)
Observations	524915	513808
Panel C: 2000-2019		
<i>Black</i>	-0.163*** (0.00350)	-0.103*** (0.00426)
<i>covered</i>	0.171*** (0.00755)	0.120*** (0.00700)
<i>covered</i> × <i>Black</i>	0.0262*** (0.00770)	0.0405*** (0.00806)
Observations	628445	648645

Standard errors in parentheses

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

Standard errors are clustered at the state-industry level. Real log wages is the dependent variable throughout. Having the property of the coefficient in each panel is associated with a $100(e^\beta - 1)$ percent change in wages. Each column and panel corresponds to a regression. All regressions include covariates and fixed effects.

Table 5: OLS of Real Log Wages on Unionization Rate for People Not Covered by Union

	Men			Women		
	white	Black	Hispanic	White	Black	Hispanic
<i>Panel A: 1983-1988</i>						
coveragerate	0.129*** (0.0408)	0.0713 (0.0689)	0.263*** (0.0659)	0.0193 (0.0322)	0.0460 (0.0608)	0.130* (0.0674)
Observations	210643	20061	17369	211605	26249	13499
<i>Panel B: 1988-2000</i>						
coveragerate	0.108*** (0.0280)	0.0893* (0.0479)	0.315*** (0.0666)	0.0227 (0.0281)	0.0142 (0.0465)	0.328*** (0.0618)
Observations	378776	35044	44952	386721	48587	34217
<i>Panel C: 2000-2019</i>						
coveragerate	0.0682** (0.0300)	0.109** (0.0528)	0.255*** (0.0554)	-0.00995 (0.0308)	0.0283 (0.0429)	0.235*** (0.0449)
Observations	484955	47017	96726	493662	66038	77909

Standard errors in parentheses

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

Unionization rate is computed at the state-industry-year level. Standard errors are clustered at the state-industry level. Real log wages is the dependent variable throughout. A one percent increase in the unionization rate leads to a $100(e^\beta - 1)$ change in wages. Each column and panel corresponds to a regression. All regressions include covariates and fixed effects.

Table 6: Difference in Differences of Real Log Wages on Right to Work Laws Treatment in State and Time

	Men				Women			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: All (Black and White) People								
Black	-0.144*** (0.00647)	-0.144*** (0.00648)	-0.145*** (0.00638)	-0.144*** (0.00632)	-0.0663*** (0.00658)	-0.0663*** (0.00658)	-0.0672*** (0.00652)	-0.0668*** (0.00656)
RTW_{st}	-0.0377** (0.0182)	-0.0395** (0.0190)	-0.0199* (0.0106)	0.00242 (0.00290)	-0.0318*** (0.00834)	-0.0325*** (0.00872)	-0.0572*** (0.0117)	-0.0199*** (0.00328)
$RTW_{st} \times Black$	-0.0219** (0.00884)	-0.0219** (0.00887)	-0.0218** (0.00876)	-0.0216** (0.00873)	-0.0396*** (0.00801)	-0.0396*** (0.00801)	-0.0389*** (0.00807)	-0.0387*** (0.00813)
Observations	1005865	1005865	1005865	1005865	1014977	1014977	1014977	1014977
Panel B: People Not Covered by Union - Spillover Effect								
Black	-0.159*** (0.00503)	-0.159*** (0.00503)	-0.160*** (0.00498)	-0.159*** (0.00491)	-0.0840*** (0.00701)	-0.0840*** (0.00701)	-0.0848*** (0.00682)	-0.0845*** (0.00694)
RTW_{st}	-0.0266* (0.0136)	-0.0286* (0.0144)	-0.0199* (0.0114)	0.00426 (0.00371)	-0.0254*** (0.00706)	-0.0262*** (0.00728)	-0.0590*** (0.0119)	-0.0228*** (0.00471)
$RTW_{st} \times Black$	-0.00302 (0.00811)	-0.00301 (0.00814)	-0.00337 (0.00820)	-0.00306 (0.00816)	-0.0282*** (0.00827)	-0.0282*** (0.00828)	-0.0274*** (0.00832)	-0.0273*** (0.00843)
Observations	825912	825912	825912	825912	868290	868290	868290	868290
Panel C: People Covered by Union - Direct Effect								
Black	-0.140*** (0.0123)	-0.140*** (0.0123)	-0.141*** (0.0122)	-0.141*** (0.0122)	-0.0555*** (0.0100)	-0.0555*** (0.01000)	-0.0565*** (0.00969)	-0.0563*** (0.00973)
RTW_{st}	-0.0572* (0.0314)	-0.0581* (0.0322)	-0.0470** (0.0190)	-0.0210 (0.0145)	-0.0607*** (0.0184)	-0.0614*** (0.0190)	-0.0539** (0.0213)	-0.0109 (0.0160)
$RTW_{st} \times Black$	-0.0145 (0.0147)	-0.0145 (0.0147)	-0.0134 (0.0145)	-0.0132 (0.0144)	0.000499 (0.0156)	0.000546 (0.0156)	0.000630 (0.0150)	0.00140 (0.0150)
Observations	178512	178512	178512	178512	145102	145102	145102	145102
State Monthly Unemployment Rate	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	No	Yes	Yes	Yes	No	Yes
State Linear Time Trends	No	No	Yes	Yes	No	No	Yes	Yes

Standard errors in parentheses

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

I define $RTW_{st} = 1$ if state s has a RTW law at time t . Sample includes years 1983-2019. Policy variation: Oklahoma (2001), Indiana (2012), Michigan (2013), Wisconsin (2015), and Kentucky (2017). All states, except West Virginia, Montana, Maine, New Hampshire, and Vermont are in the sample. Standard errors are clustered at the state-level. Real log wages is the dependent variable throughout. All regressions include covariates and fixed effects as outlined.

A Appendix

A.1 Appendix - Wage Distributions and Counterfactual

In addition to presenting the wage distributions for white people and Black people, I ask what the wage distribution for white people who are covered by a union would look like if they had the same covariates as Black people who are covered by a union - with a corresponding question for those not covered by a union. Covariates include education, a quartic in experience, education-experience interaction, marital status, part-time status, public sector, CMSA, 11 industry categories, 12 occupation categories, and state, quarter, and year fixed effects. I follow the [Heywood and Parent \(2012\)](#) implementation of the DFL decomposition for its added dimension allowing for selection into unions. For the wage distributions for Black people $B = 1$ and white people $B = 0$ which are given as

$$g(w | B = 1) = \int f(w | x, B = 1)h(x | B = 1)dx \quad (5)$$

$$g(w | B = 0) = \int f(w | x, B = 0)h(x | B = 0)dx, \quad (6)$$

[Heywood and Parent \(2012\)](#) show that the counterfactual wage distribution for white people covered by a union with the same distribution of covariates x as Black people covered by a union $u = 1$ is given by

$$g_{B=1,u=1}^c(w | B = 0) = \int \Theta_2 f(w | x, B = 0, u = 1)h(x | B = 0, u = 1)dx, \quad (7)$$

where $\Theta_2 = \frac{Prob(B=1|x)Prob(u=1|x,B=1)}{Prob(B=0|x)Prob(u=1|x,B=0)}$. Similarly, the counterfactual wage distribution for white people not covered by a union with the same distribution of covariates as Black people who are not covered by a union is given by

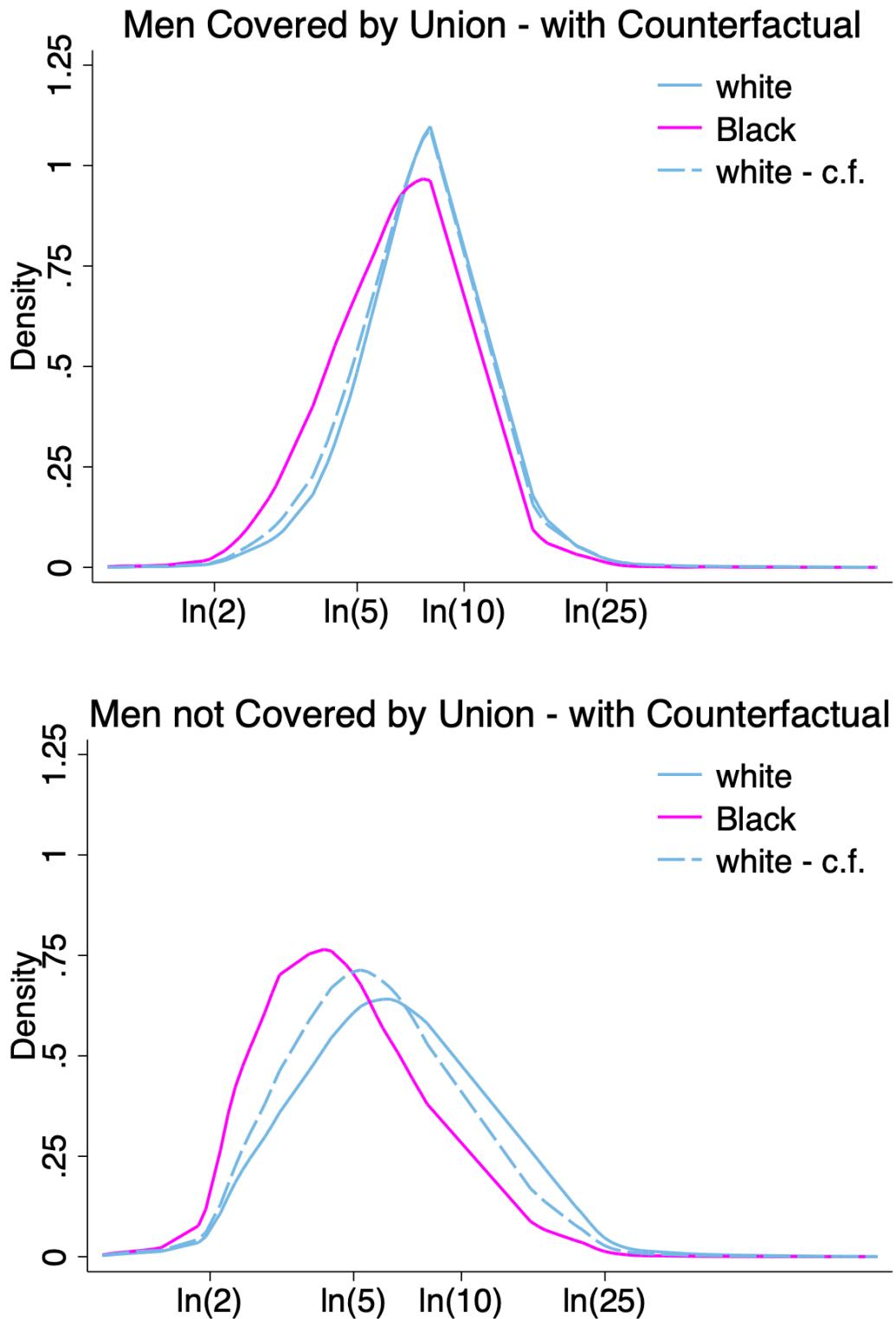
$$g_{B=1,u=0}^c(w | B = 0) = \int \Theta_3 f(w | x, B = 0, u = 0)h(x | B = 0, u = 0)dx, \quad (8)$$

where $\Theta_3 = \frac{Prob(B=1|x)Prob(u=0|x,B=1)}{Prob(B=0|x)Prob(u=0|x,B=0)}$. In order to estimate this reweighting parameter, I run probit regressions of race and union coverage on the covariates as defined. I show these wage distributions for Black people, white people, and the counterfactual white wage distribution in

[Figure 20](#) and [Figure 21](#) for men and women respectively. Indeed the wage distributions for Black people are more heavily right-skewed. Note that the wage distributions at the top end will be affected by the Pareto imputation procedure which does not take race into account. The difference in the skews of the distribution is much more striking for people not covered by a union. As well, the counterfactual wage distribution for white people covered by a union closely follows the actual wage distribution for white people covered by a union. For people not covered by a union, there persists a significant difference in the wage distributions when comparing the distributions for Black people to the counterfactual white distributions.

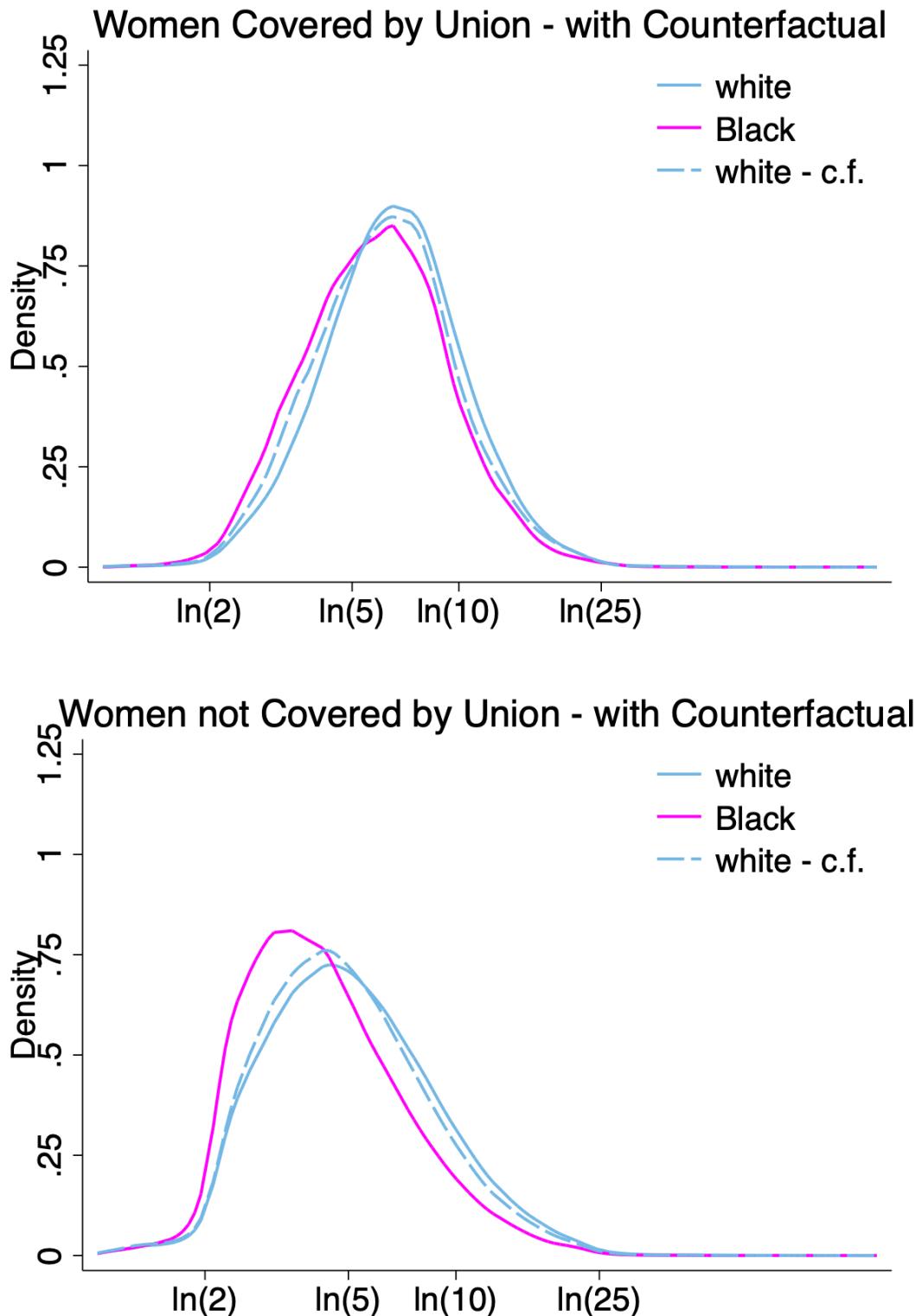
A.2 Appendix - Figures and Tables

Figure 20: Wage Distributions - Men



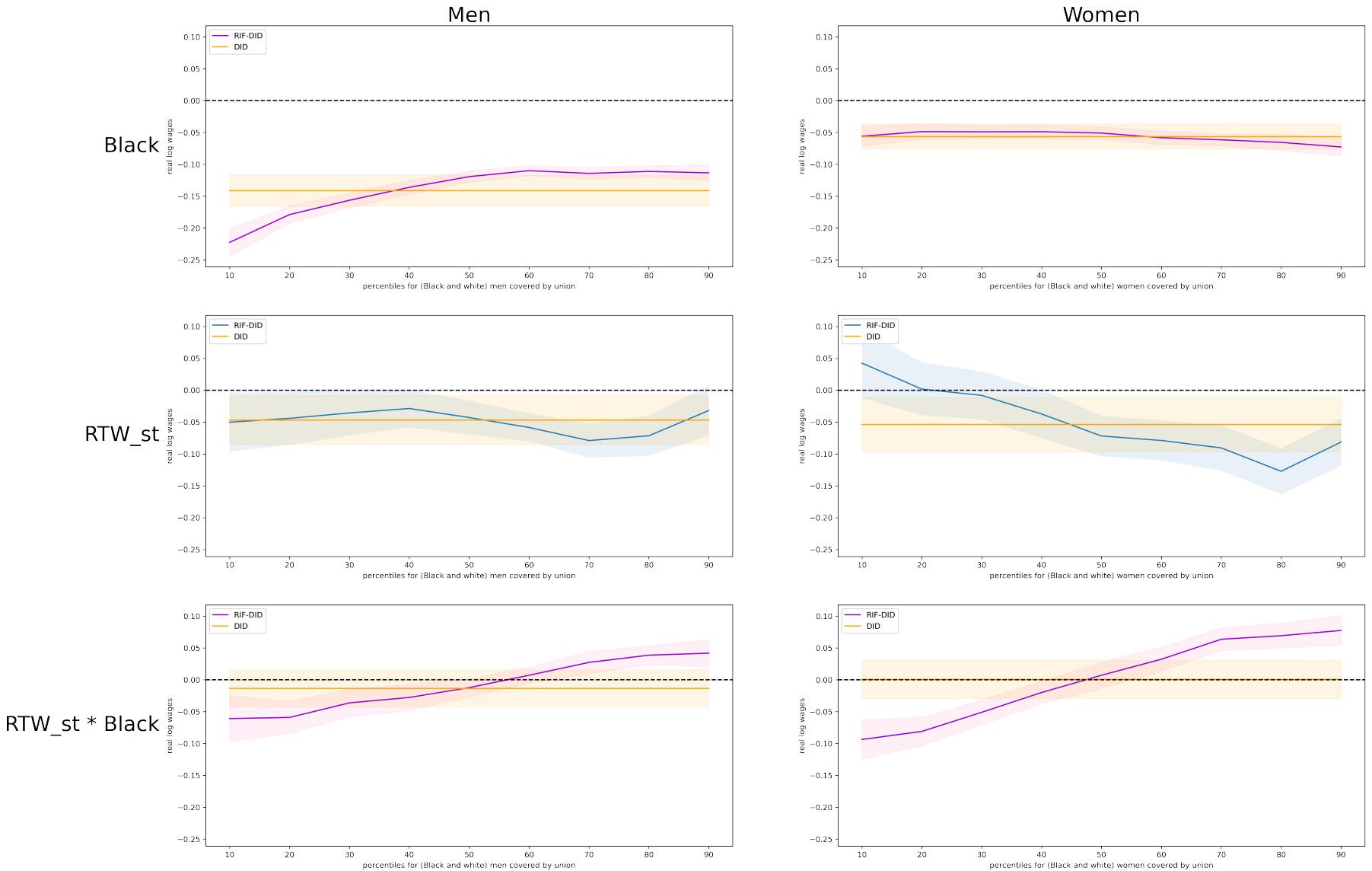
Sample include years 1983-2019 excluding 1994 and 1995.

Figure 21: Wage Distributions - Women



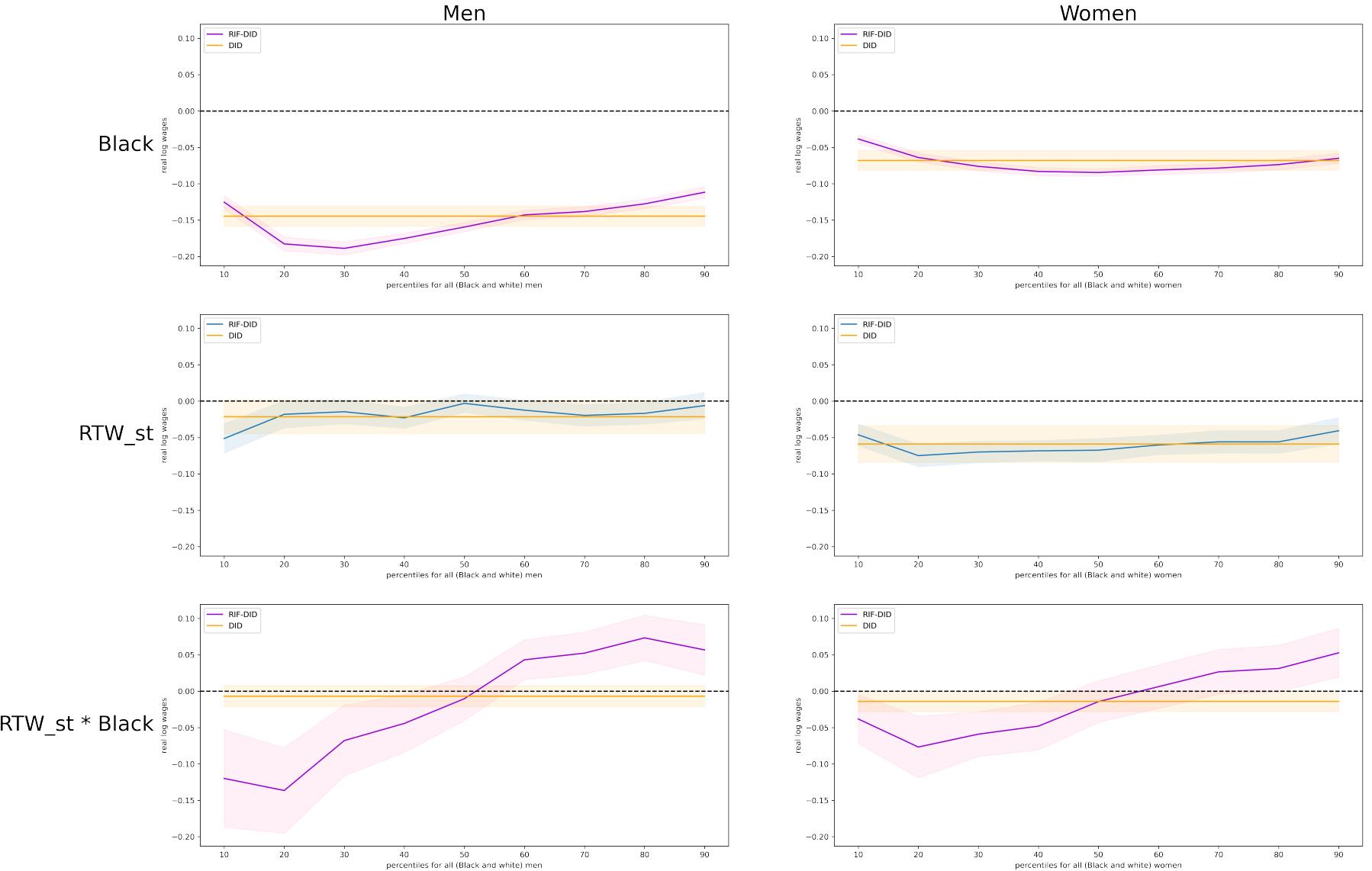
Sample include years 1983-2019 excluding 1994 and 1995.

Figure 22: RIF-DID Regression of Real Log Wages on Right-To-Work Laws Treatment in State and Time - People Covered by Union - Direct Effect



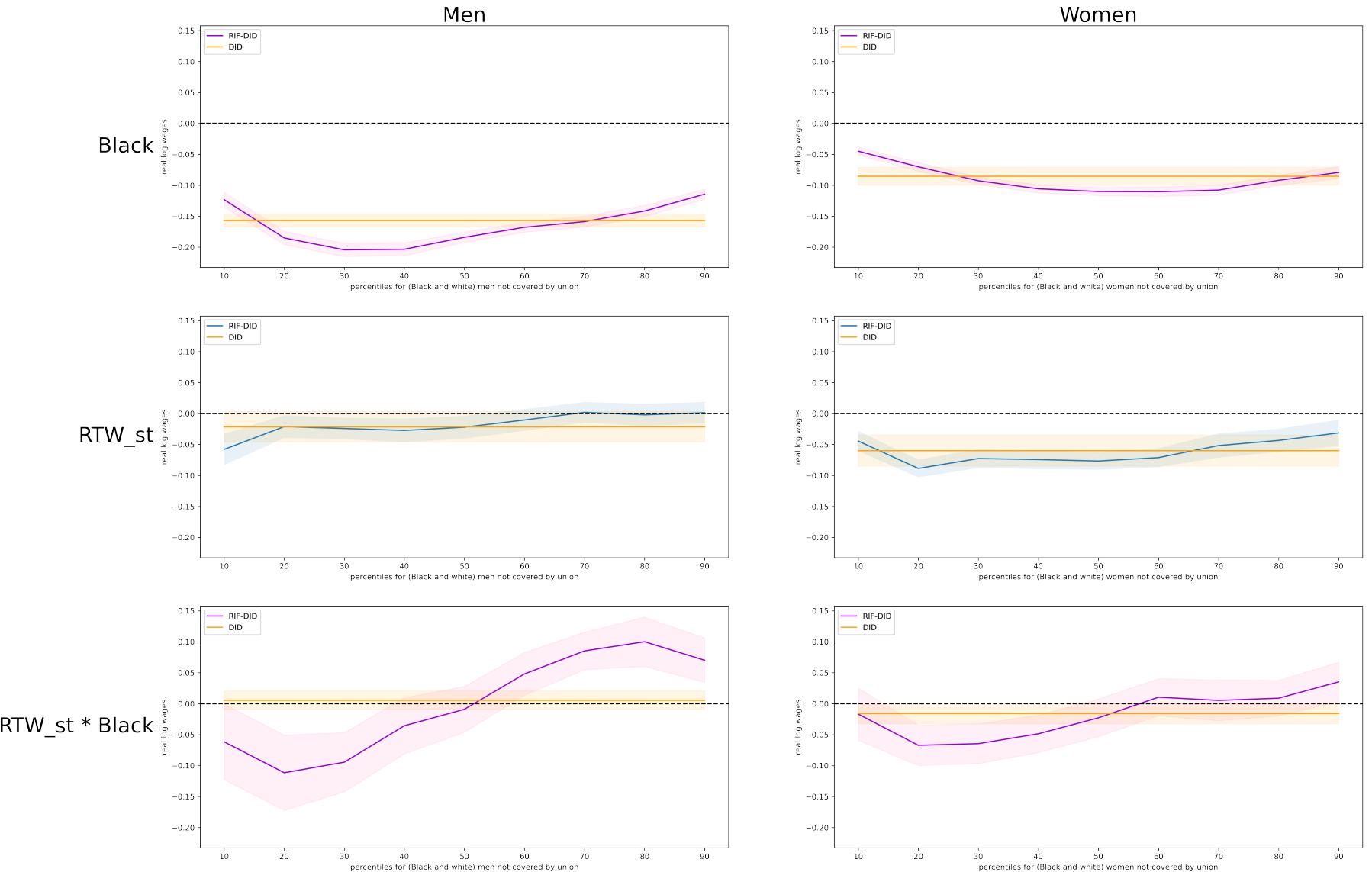
I define $RTW_{st} = 1$ if state s has a RTW law at time t . Sample includes years 1983-2019. Policy variation: Oklahoma (2001), Indiana (2012), Michigan (2013), Wisconsin (2015), and Kentucky (2017). All states, except West Virginia, Montana, Maine, New Hampshire, and Vermont are in the sample. Standard errors for the DID regression are clustered at the state-level. Standard errors for the RIF-DID regression are bootstrapped with 100 replicates. Real log wages is the dependent variable throughout. Each column corresponds to a regression and associated set of RIF-DID regressions. 95% confidence intervals presented. All regressions include covariates and fixed effects as outlined. State monthly unemployment rate and state linear time trends included. Year fixed effects not included.

Figure 23: RIF-DID Regression of Real Log Wages on Right-To-Work Laws Treatment in State and Time - All (Black and White) People - W/O Always RTW States



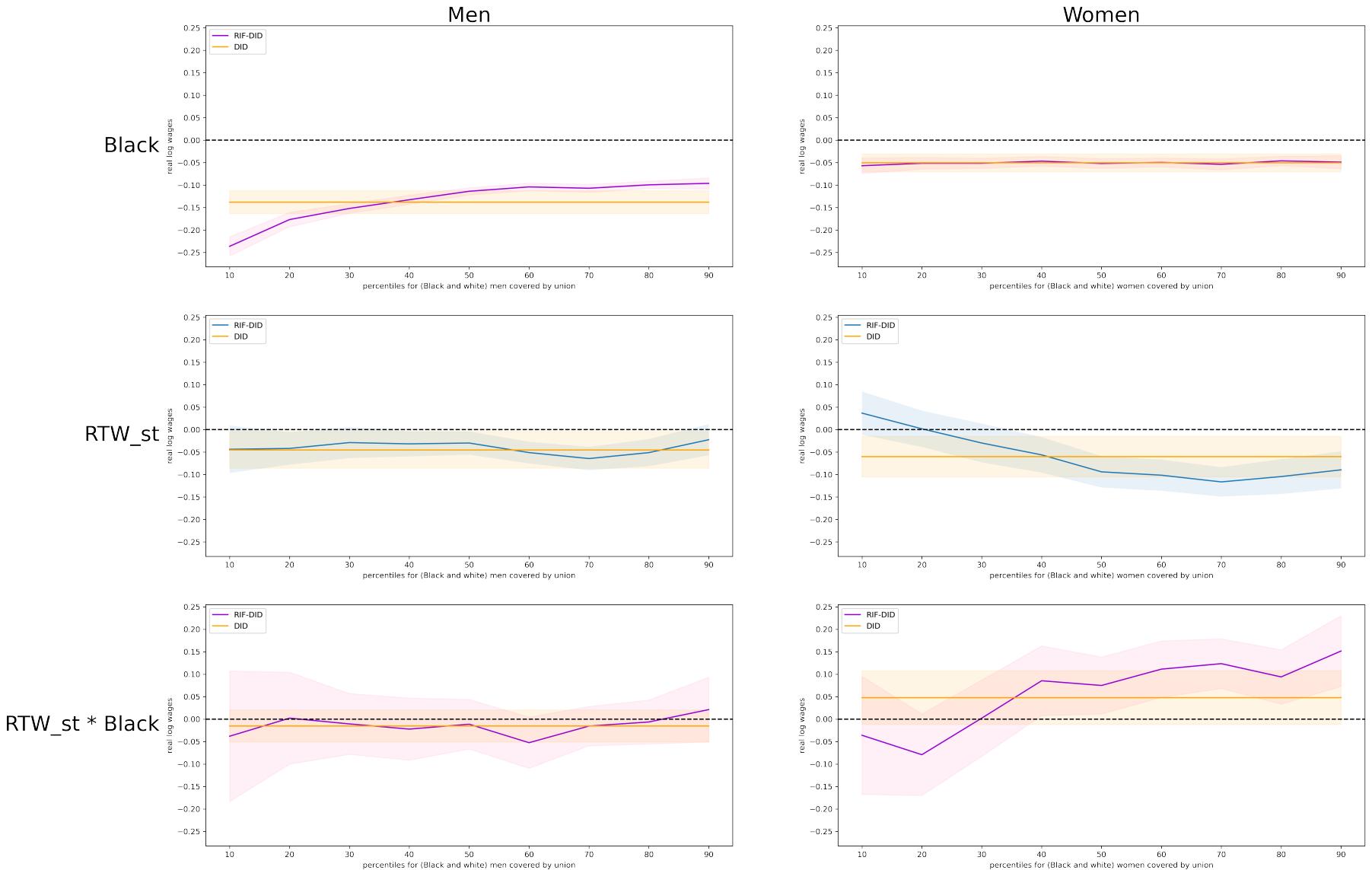
I define $RTW_{st} = 1$ if state s has a RTW law at time t . Sample includes years 1983-2019. Policy variation: Oklahoma (2001), Indiana (2012), Michigan (2013), Wisconsin (2015), and Kentucky (2017). All states, except West Virginia, Montana, Maine, New Hampshire, Vermont, and always right-to-work states are in the sample. Standard errors for the DID regression are clustered at the state-level. Standard errors for the RIF-DID regression are bootstrapped with 100 replicates. Real log wages is the dependent variable throughout. Each column corresponds to a regression and associated set of RIF-DID regressions. 95% confidence intervals presented. All regressions include covariates and fixed effects as outlined. State monthly unemployment rate and state linear time trends included. Year fixed effects not included.

Figure 24: RIF-DID Regression of Real Log Wages on Right-To-Work Laws Treatment in State and Time - People Not Covered by Union - Spillover Effect - W/O Always RTW States



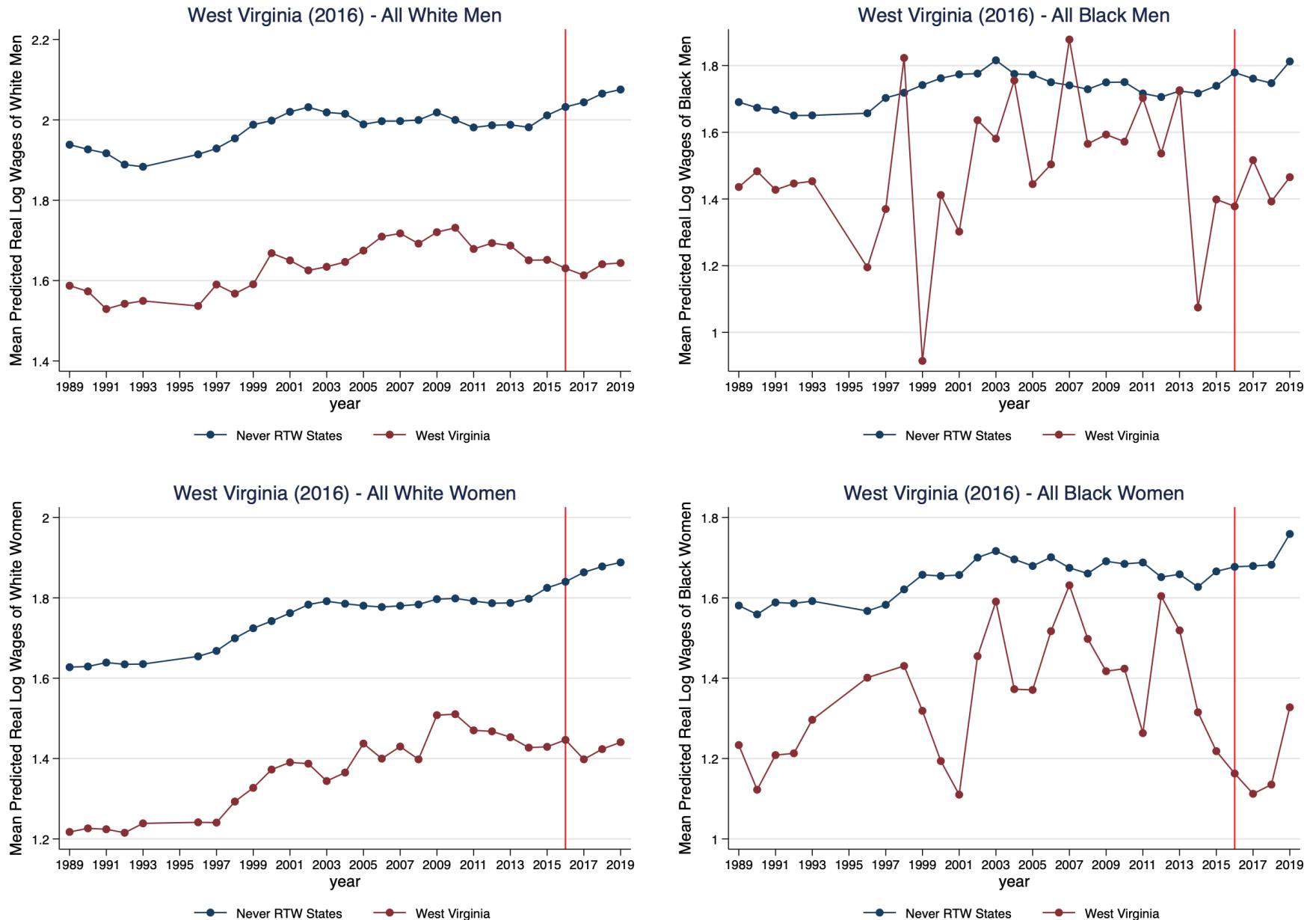
I define $RTW_{st} = 1$ if state s has a RTW law at time t . Sample includes years 1983-2019. Policy variation: Oklahoma (2001), Indiana (2012), Michigan (2013), Wisconsin (2015), and Kentucky (2017). All states, except West Virginia, Montana, Maine, New Hampshire, and Vermont, and always right-to-work states are in the sample. Standard errors for the DID regression are clustered at the state-level. Standard errors for the RIF-DID regression are bootstrapped with 100 replicates. Real log wages is the dependent variable throughout. Each column corresponds to a regression and associated set of RIF-DID regressions. 95% confidence intervals presented. All regressions include covariates and fixed effects as outlined. State monthly unemployment rate and state linear time trends included. Year fixed effects not included.

Figure 25: RIF-DID Regression of Real Log Wages on Right-To-Work Laws Treatment in State and Time - People Covered by Union - Direct Effect - W/O Always RTW States



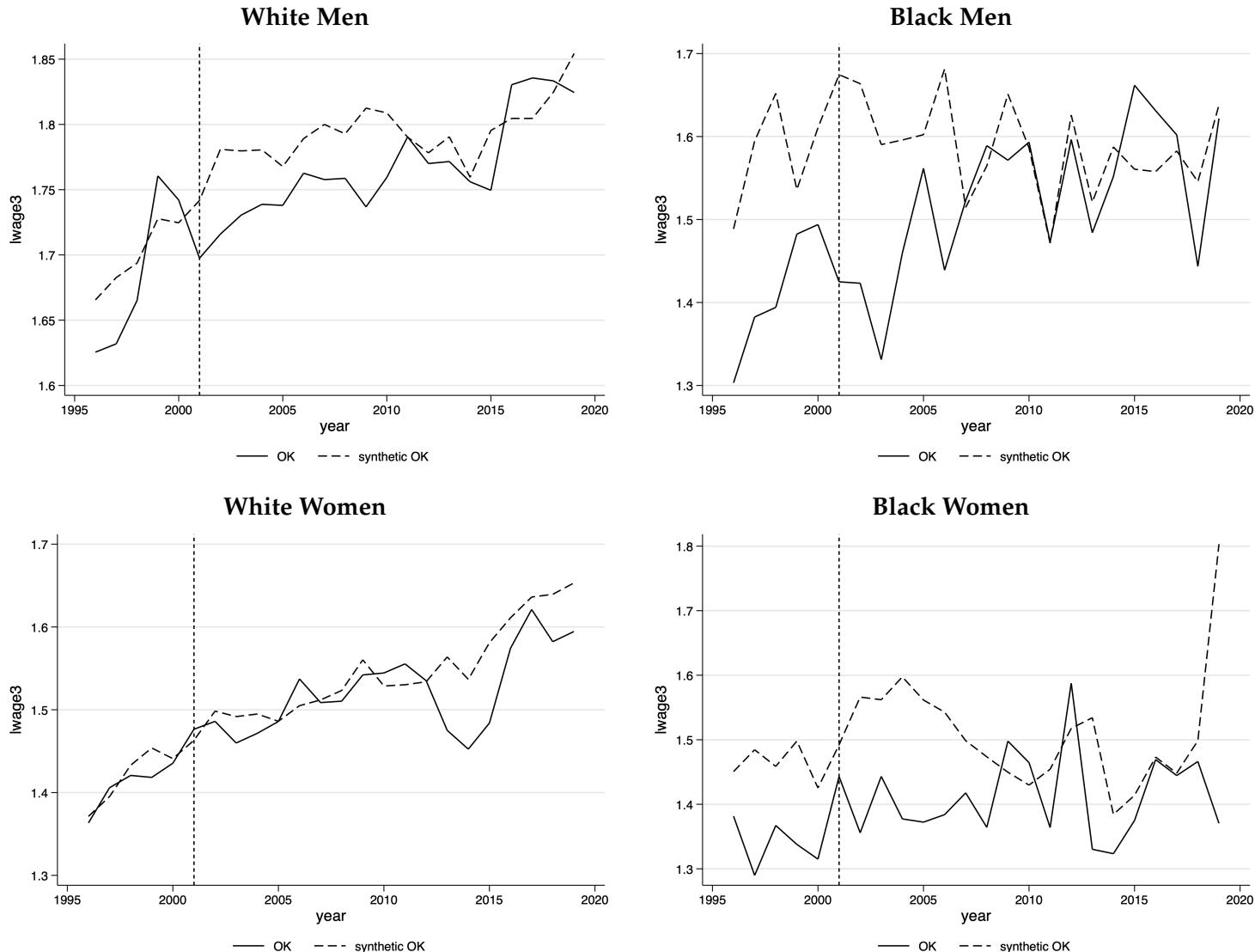
I define $RTW_{st} = 1$ if state s has a RTW law at time t . Sample includes years 1983-2019. Policy variation: Oklahoma (2001), Indiana (2012), Michigan (2013), Wisconsin (2015), and Kentucky (2017). All states, except West Virginia, Montana, Maine, New Hampshire, Vermont, and always right-to-work states are in the sample. Standard errors for the DID regression are clustered at the state-level. Standard errors for the RIF-DID regression are bootstrapped with 100 replicates. Real log wages is the dependent variable throughout. Each column corresponds to a regression and associated set of RIF-DID regressions. 95% confidence intervals presented. All regressions include covariates and fixed effects as outlined. State monthly unemployment rate and state linear time trends included. Year fixed effects not included.

Figure 26: Predicted Real Log Wage Trends - West Virginia (2016)



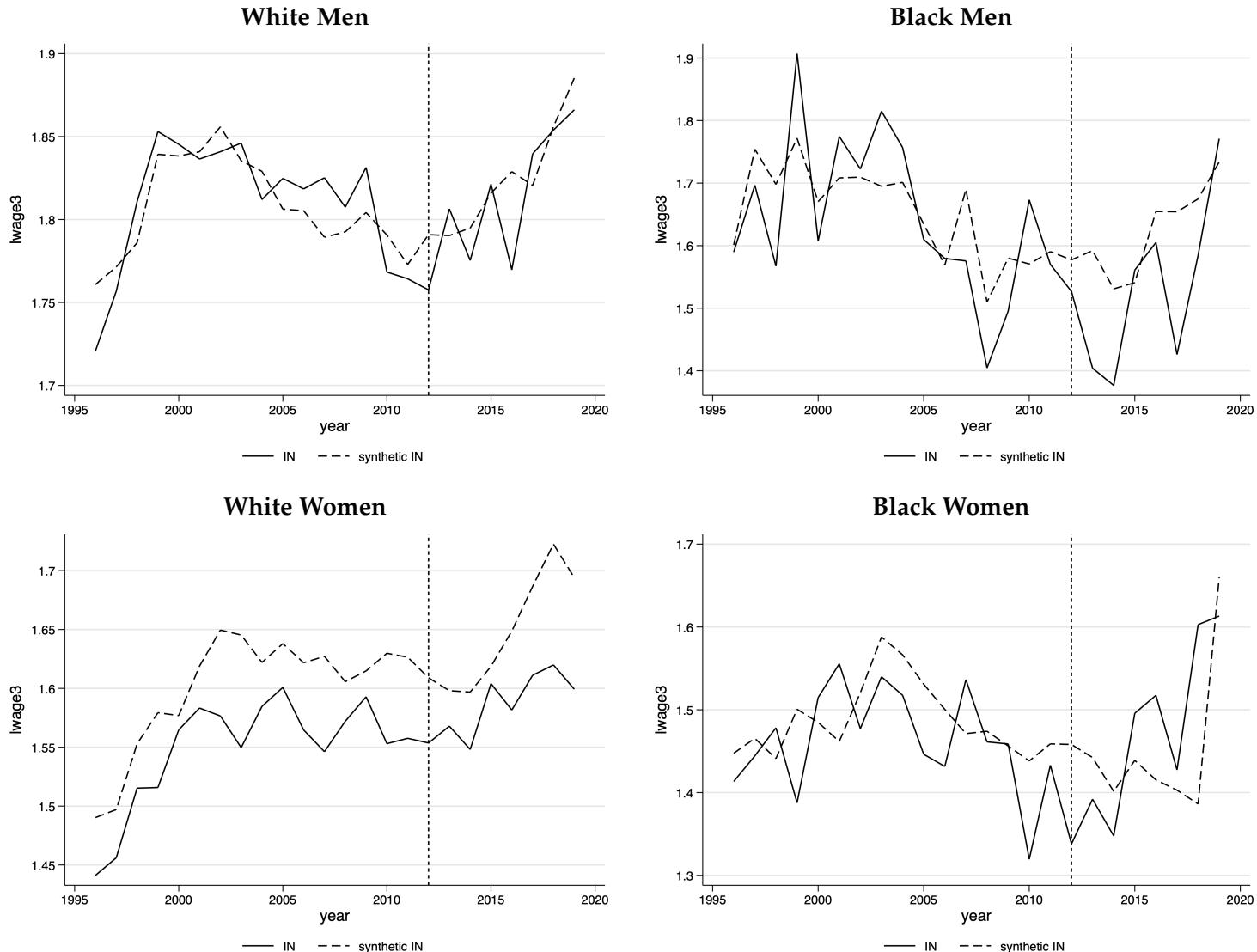
Predicted real log wages obtained from regression of real log wage on covariates and fixed effects separately for each subgroup.

Figure 27: Synthetic Control Method - Oklahoma (2001)



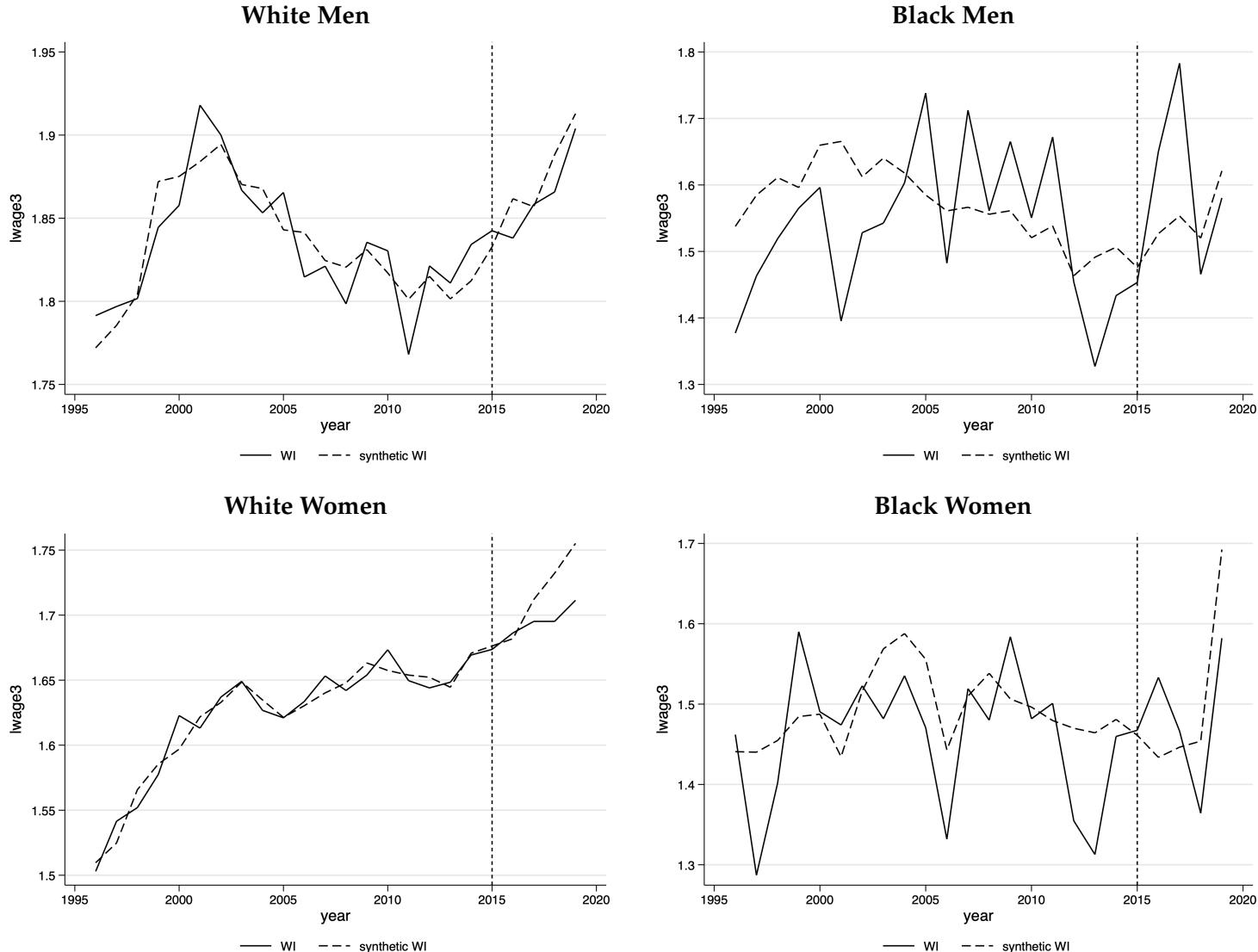
Real log wages is the dependent variable throughout. Synthetic control implemented using synth package with nested optimization and allopt. Each sub-figure corresponds to a regression.

Figure 28: Synthetic Control Method - Indiana (2012)



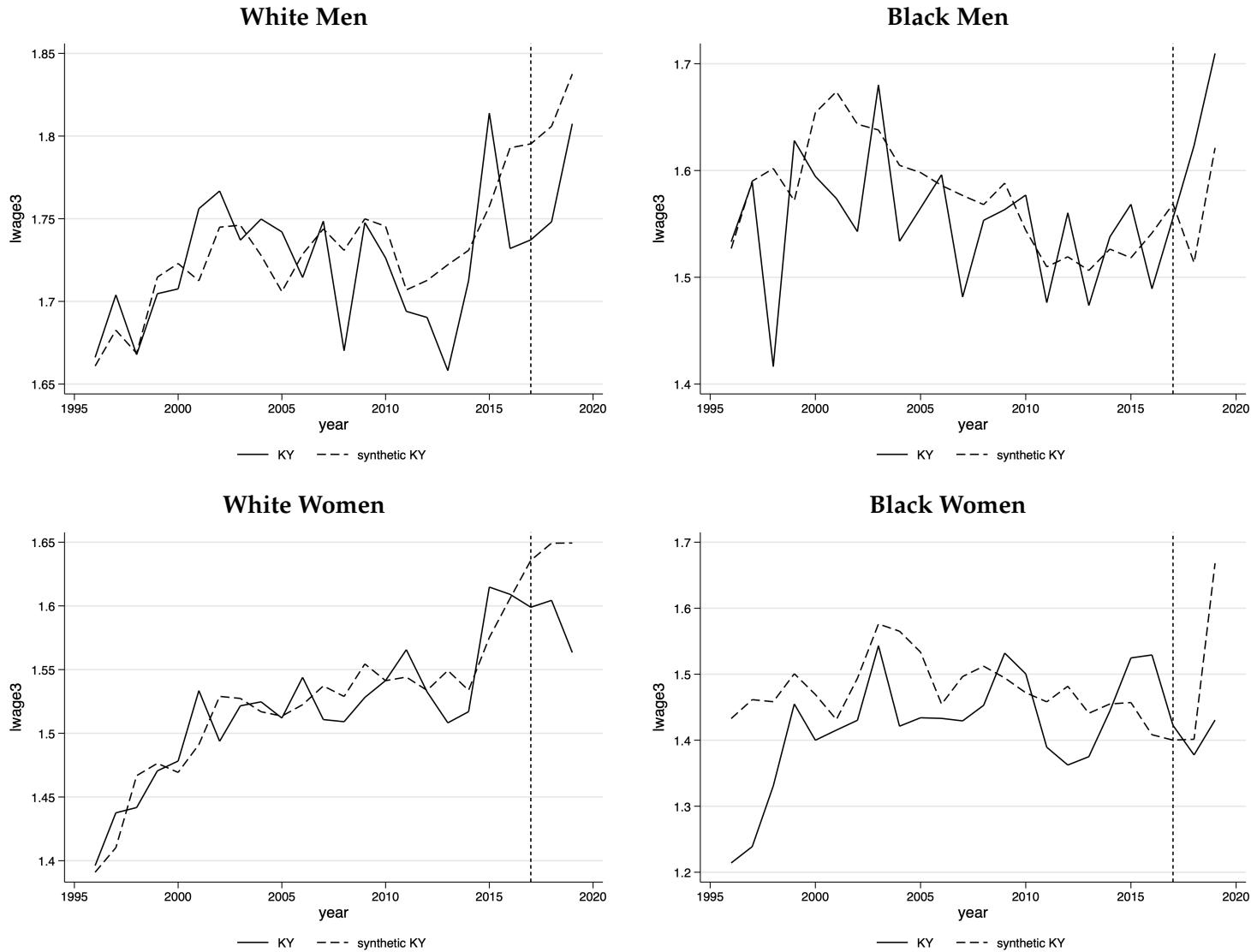
Real log wages is the dependent variable throughout. Synthetic control implemented using synth package with nested optimization and allopt. Each sub-figure corresponds to a regression. Nested optimization not used for white women due to unstable or asymmetric Hessian.

Figure 29: Synthetic Control Method - Wisconsin (2015)



Real log wages is the dependent variable throughout. Synthetic control implemented using synth package with nested optimization and allopt. Each sub-figure corresponds to a regression.

Figure 30: Synthetic Control Method - Kentucky (2017)



Real log wages is the dependent variable throughout. Synthetic control implemented using synth package with nested optimization and allopt. Each sub-figure corresponds to a regression.

Table 7: OLS of Real Log Wages on Unionization Rate for People Covered by Union

	Men			Women		
	white	Black	Hispanic	White	Black	Hispanic
Panel A: 1983-1988						
coveragerate	0.319*** (0.0403)	0.413*** (0.0829)	0.586*** (0.136)	0.343*** (0.0588)	0.420*** (0.0919)	0.418*** (0.160)
Observations	72125	9666	5600	39825	9230	2950
Panel B: 1988-2000						
coveragerate	0.286*** (0.0396)	0.335*** (0.0495)	0.566*** (0.0893)	0.224*** (0.0563)	0.358*** (0.0626)	0.515*** (0.152)
Observations	97722	13373	9306	64372	14128	5992
Panel C: 2000-2019						
coveragerate	0.374*** (0.0419)	0.343*** (0.0646)	0.397*** (0.0949)	0.280*** (0.0542)	0.218*** (0.0668)	0.321*** (0.121)
Observations	85640	10833	12779	76061	12884	10446

Standard errors in parentheses

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

Unionization rate is computed at the state-industry-year level. Standard errors are clustered at the state-industry level. Real log wages is the dependent variable throughout. A one percent increase in the unionization rate leads to a $100(e^\beta - 1)$ change in wages. Each column and panel corresponds to a regression. All regressions include covariates and fixed effects.

Table 8: Difference in Differences of Real Log Wages on Right to Work Laws Treatment in State and Time - W/O Always RTW States

	Men				Women			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: All (Black and white) People								
Black	-0.144*** (0.00647)	-0.144*** (0.00648)	-0.145*** (0.00638)	-0.144*** (0.00632)	-0.0663*** (0.00658)	-0.0663*** (0.00658)	-0.0672*** (0.00652)	-0.0668*** (0.00656)
RTW_{st}	-0.0377** (0.0182)	-0.0395** (0.0190)	-0.0199* (0.0106)	0.00242 (0.00290)	-0.0318*** (0.00834)	-0.0325*** (0.00872)	-0.0572*** (0.0117)	-0.0199*** (0.00328)
$RTW_{st} \times Black$	-0.0219** (0.00884)	-0.0219** (0.00887)	-0.0218** (0.00876)	-0.0216** (0.00873)	-0.0396*** (0.00801)	-0.0396*** (0.00801)	-0.0389*** (0.00807)	-0.0387*** (0.00813)
Observations	1005865	1005865	1005865	1005865	1014977	1014977	1014977	1014977
Panel B: People Not Covered by Union - Spillover Effect								
Black	-0.159*** (0.00503)	-0.159*** (0.00503)	-0.160*** (0.00498)	-0.159*** (0.00491)	-0.0840*** (0.00701)	-0.0840*** (0.00701)	-0.0848*** (0.00682)	-0.0845*** (0.00694)
RTW_{st}	-0.0266* (0.0136)	-0.0286* (0.0144)	-0.0199* (0.0114)	0.00426 (0.00371)	-0.0254*** (0.00706)	-0.0262*** (0.00728)	-0.0590*** (0.0119)	-0.0228*** (0.00471)
$RTW_{st} \times Black$	-0.00302 (0.00811)	-0.00301 (0.00814)	-0.00337 (0.00820)	-0.00306 (0.00816)	-0.0282*** (0.00827)	-0.0282*** (0.00828)	-0.0274*** (0.00832)	-0.0273*** (0.00843)
Observations	825912	825912	825912	825912	868290	868290	868290	868290
Panel C: People Covered by Union - Direct Effect								
Black	-0.140*** (0.0123)	-0.140*** (0.0123)	-0.141*** (0.0122)	-0.141*** (0.0122)	-0.0555*** (0.0100)	-0.0555*** (0.01000)	-0.0565*** (0.00969)	-0.0563*** (0.00973)
RTW_{st}	-0.0572* (0.0314)	-0.0581* (0.0322)	-0.0470** (0.0190)	-0.0210 (0.0145)	-0.0607*** (0.0184)	-0.0614*** (0.0190)	-0.0539** (0.0213)	-0.0109 (0.0160)
$RTW_{st} \times Black$	-0.0145 (0.0147)	-0.0145 (0.0147)	-0.0134 (0.0145)	-0.0132 (0.0144)	0.000499 (0.0156)	0.000546 (0.0156)	0.000630 (0.0150)	0.00140 (0.0150)
Observations	178512	178512	178512	178512	145102	145102	145102	145102
State Monthly Unemployment Rate	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	No	Yes	Yes	Yes	No	Yes
State Linear Time Trends	No	No	Yes	Yes	No	No	Yes	Yes

Standard errors in parentheses

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

I define $RTW_{st} = 1$ if state s has a RTW law at time t . Sample includes years 1983-2019. Policy variation: Oklahoma (2001), Indiana (2012), Michigan (2013), Wisconsin (2015), and Kentucky (2017). All states, except West Virginia, Montana, Maine, New Hampshire, and Vermont are in the sample. Standard errors are clustered at the state-level. Real log wages is the dependent variable throughout. All regressions include covariates and fixed effects as outlined.

Table 9: Synthetic Control Method - Weights Assigned to Donor States - White Men

Donor State	Oklahoma (2001) Unit Weight	Indiana (2012) Unit Weight	Michigan (2013) Unit Weight	Wisconsin (2015) Unit Weight	Kentucky (2017) Unit Weight
ME	0.3	0	0	0.179	0
NH	0	0	0	0	0
VT	0.262	0	0	0	0
MA	0	0	0	0	0
RI	0	0	0	0	0
CT	0	0	0	0	0
NY	0	0	0	0	0
NJ	0	0	0	0	0
PA	0	0.24	0	0	0
OH	0	0.559	0.887	0.6	0.24
IL	0	0	0	0	0
MN	0	0	0	0.197	0
MO	0.117	0.107	0	0	0.223
DE	0	0	0	0	0
MD	0	0	0	0	0
DC	0	0	0.113	0.024	0
MT	0.14	0.093	0	0	0.536
CO	0	0	0	0	0
NM	0.181	0	0	0	0
WA	0	0	0	0	0
OR	0	0	0	0	0
CA	0	0	0	0	0
AK	0	0	0	0	0
HI	0	0	0	0	0

Real log wages is the dependent variable throughout. Synthetic control implemented using synth package with nested optimization and allopt. Each sub-figure corresponds to a regression.

Table 10: Synthetic Control Method - Weights Assigned to Donor States - Black Men

Donor State	Oklahoma (2001) Unit Weight	Indiana (2012) Unit Weight	Michigan (2013) Unit Weight	Wisconsin (2015) Unit Weight	Kentucky (2017) Unit Weight
ME	-	-	-	-	-
NH	-	-	-	-	-
VT	-	-	-	-	-
MA	0	0.043	0.034	0	0
RI	0	0.109	0.354	0.167	0
CT	0	0	0	0	0
NY	0	0	0	0	0
NJ	0	0	0.04	0	0
PA	0	0	0	0.065	0.151
OH	0.709	0.43	0.002	0.768	0.774
IL	0	0	0	0	0
MN	0	0	0	0	0
MO	0	0	0.147	0	0
DE	0	0	0	0	0
MD	0	0	0	0	0
DC	0	0	0.15	0	0
MT	-	-	-	-	-
CO	0	0.224	0.082	0	0
NM	0.291	0	0	0	0.075
WA	0	0	0	0	0
OR	0	0.194	0.126	0	0
CA	0	0	0.064	0	0
AK	0	0	0	0	0
HI	0	0	0	0	0

Real log wages is the dependent variable throughout. Synthetic control implemented using synth package with nested optimization and allopt. Each sub-figure corresponds to a regression.

Table 11: Synthetic Control Method - Weights Assigned to Donor States - White Women

Donor State	Oklahoma (2001) Unit Weight	Indiana (2012) Unit Weight	Michigan (2013) Unit Weight	Wisconsin (2015) Unit Weight	Kentucky (2017) Unit Weight
ME	0.244	0	0	0	0
NH	0	0.177	0	0	0
VT	0	0	0	0	0
MA	0	0	0	0	0
RI	0	0	0	0	0
CT	0	0	0	0	0
NY	0	0	0	0	0
NJ	0	0	0	0	0
PA	0.069	0	0	0.565	0
OH	0	0.823	0.811	0.231	0.453
IL	0	0	0	0	0
MN	0	0	0	0	0
MO	0.06	0	0	0	0.012
DE	0	0	0	0	0
MD	0	0	0	0	0
DC	0	0	0	0	0
MT	0.627	0	0	0	0.528
CO	0	0	0	0	0
NM	0	0	0	0	0
WA	0	0	0	0	0
OR	0	0	0	0.204	0
CA	0	0	0	0	0
AK	0	0	0.189	0	0.007
HI	0	0	0	0	0

Real log wages is the dependent variable throughout. Synthetic control implemented using `synth` package with nested optimization and allopt. Each sub-figure corresponds to a regression. Nested optimization not used for Indiana (2012) due to unstable or asymmetric Hessian.

Table 12: Synthetic Control Method - Weights Assigned to Donor States - Black Women

Donor State	Oklahoma (2001) Unit Weight	Indiana (2012) Unit Weight	Michigan (2013) Unit Weight	Wisconsin (2015) Unit Weight	Kentucky (2017) Unit Weight
ME	-	-	-	-	-
NH	-	-	-	-	-
VT	-	-	-	-	-
MA	0	0	0	0	0
RI	0	0	0	0.348	0.103
CT	0	0	0	0	0
NY	0	0	0	0	0
NJ	0	0	0.198	0	0
PA	0	0	0	0	0
OH	0.87	0.906	0.276	0.51	0.711
IL	0	0	0	0	0
MN	0	0	0.402	0	0
MO	0	0	0	0	0
DE	0	0	0	0	0
MD	0	0	0	0	0
DC	0	0	0	0	0
MT	-	-	-	-	-
CO	0	0	0	0	0
NM	0	0.094	0.091	0.143	0.185
WA	0	0	0.033	0	0
OR	0	0	0	0	0
CA	0	0	0	0	0
AK	0	0	0	0	0
HI	0.13	0	0	0	0

Real log wages is the dependent variable throughout. Synthetic control implemented using synth package with nested optimization and allopt. Each sub-figure corresponds to a regression.