The Union Shield That Wasn't: Right-to-Work Laws and the Racial Earnings Gap

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Abstract

A long-standing view holds that unions serve as a "shield" for Black workers, compressing the wage distribution and narrowing racial earnings gaps. I test this claim by exploiting the staggered adoption of Right-to-Work laws in Indiana (2012), Michigan (2013), Wisconsin (2015), and West Virginia (2016), which weakened union bargaining power. Using the Quarterly Workforce Indicators race panel covering 697 counties, I estimate a triple-difference comparing Black–White earnings changes across RTW and non-RTW states. The differential effect on log earnings is -0.012 (SE $= 0.026$), a precise null. Sun-Abraham heterogeneity-robust estimates confirm: RTW reduced Black and White earnings by nearly identical amounts (-0.4 and -0.1 percent). The racial earnings gap is driven by forces that collective bargaining does not reach.

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1. Introduction

In 1980, Richard Freeman argued that unions compress the wage distribution, disproportionately raising earnings for lower-paid workers—a group in which Black workers were, and remain, overrepresented (Freeman, 1980). If this "union shield" is real, then weakening unions should widen the Black–White earnings gap. The recent wave of Right-to-Work (RTW) legislation in four historically unionized Midwestern and Appalachian states—Indiana, Michigan, Wisconsin, and West Virginia—provides a sharp test of this prediction.

I exploit the staggered adoption of RTW laws between 2012 and 2016 using a triple-difference design that compares Black versus White earnings changes in newly RTW states against the same racial comparison in neighboring states that never adopted RTW. The data come from the Quarterly Workforce Indicators (QWI), an administrative panel derived from unemployment insurance records covering the universe of formal employment. My analysis sample spans 697 counties across nine states with 104,405 county–race–quarter observations over 2005–2023.

The main finding is a well-powered null. The DDD coefficient on log quarterly earnings is -0.012 (SE $= 0.026$, $p = 0.66$). The 95% confidence interval of $[-0.063, 0.039]$ rules out positive differential effects larger than 4 log points and negative effects larger than 6 log points. RTW laws did not widen the Black–White earnings gap. Sun-Abraham heterogeneity-robust event studies, estimated separately by race, confirm: RTW adoption reduced Black workers' earnings by a statistically insignificant 0.4 percent and White workers' earnings by 0.1 percent—the difference between these estimates (-0.003) is economically trivial and statistically indistinguishable from zero.

This null is not an artifact of imprecision, wrong timing, or specification sensitivity. A placebo test applying false RTW adoption three years before actual passage yields a coefficient of 0.005 (SE $= 0.008$). Leave-one-out exercises dropping each treated state produce DDD estimates ranging from -0.036 to $+0.003$, all statistically insignificant. The result is stable across heterogeneity-robust estimators.

These results contribute to three literatures. First, they speak to the large body of work on unions and inequality. Card (1996) and Western and Rosenfeld (2011) provide influential evidence that deunionization contributed to rising U.S. wage inequality, with particularly large effects for Black men. Farber et al. (2021) use historical data to show that unions reduced wage gaps across occupation, education, and region. My results suggest that while unions may affect the overall wage distribution, the Black–White gap within local labor markets is orthogonal to RTW-induced changes in bargaining power.

Second, the paper contributes to the rapidly growing literature on RTW laws. Fortin et al.

(2022) find that RTW adoption reduces unionization by 5–8 percentage points. Collins (2024) estimate earnings effects of −2 to −4 percent. Zullo (2011) studies the broader consequences for labor standards. None of these papers examines the racial dimension, leaving policymakers without evidence on whether RTW has racially disparate effects.

Third, the precise null contributes to a growing body of work suggesting that labor market racial inequality operates through channels distinct from union bargaining—including discrimination in hiring (Bertrand and Mullainathan, 2004; Kline et al., 2022), residential segregation (Cutler et al., 1999), occupational sorting (Altonji and Blank, 1999), and criminal justice contact (Mueller-Smith and Schnepel, 2021). The union shield hypothesis, however intuitive, appears to be empirically weak in the modern labor market.

The remainder of the paper proceeds as follows. Section 2 describes the RTW adoptions and the institutional context. Section 3 introduces the QWI race panel. Section 4 presents the identification strategy. Section 5 reports results, and Section 6 concludes.

2. Institutional Background

Right-to-Work laws prohibit union security agreements—contracts requiring all employees in a bargaining unit to pay union dues or agency fees as a condition of employment. Prior to 2012, RTW was concentrated in Southern and Western states. The Midwestern wave began when Indiana's Governor Mitch Daniels signed RTW legislation on February 1, 2012, making Indiana the first traditionally industrial state to adopt RTW in decades. Michigan followed in December 2012 (effective March 2013), Wisconsin in March 2015, and West Virginia in July 2016.

Mechanism. The key channel through which RTW might affect the racial earnings gap is the reduction in union bargaining power. Unions negotiate wage floors that compress the earnings distribution—reducing the gap between the highest- and lowest-paid workers within a firm or industry. Because Black workers are disproportionately represented in lower-wage positions within unionized establishments (Freeman, 1980), union wage compression mechanically narrows the racial gap. RTW weakens union finances by enabling free-riding, reducing membership, and thereby reducing bargaining leverage.

Comparison states. I use five neighboring never-RTW states as controls: Illinois, Ohio, Minnesota, Pennsylvania, and New York. These states share similar industrial composition, geographic proximity, and labor market institutions, differing primarily in their RTW status during the sample period.

3. Data

The analysis uses the Quarterly Workforce Indicators (QWI), produced by the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program. The QWI provides county-level employment and earnings statistics derived from the universe of UI-covered employment, disaggregated by worker race, industry, and demographic characteristics.

Sample construction. I extract county × race × quarter observations for all industries combined, restricting to White (QWI race code A1) and Black (QWI race code A2) workers across the nine states in the study. The primary outcome is log quarterly earnings of stable, full-quarter workers (EarnS). I also examine employment levels, hiring, and separation rates.

I restrict the sample to counties where both Black and White workers have continuous earnings data for at least 20 pre-treatment quarters (2005–2011), ensuring balanced representation across races. This yields 697 counties (297 in RTW states, 400 in comparison states) and 104,405 county–race–quarter observations spanning 2005–2023.

Table 1 reports pre-treatment summary statistics. Black and White workers have similar mean quarterly earnings across RTW and comparison states ($2,342–$2,375 for Black workers; $2,876–$2,990 for White workers). The Black–White earnings ratio is approximately 0.80 in both groups. Separation rates are roughly twice as high for Black workers (0.35 vs. 0.17), reflecting well-documented patterns in employment stability.

Table 1: Summary Statistics: Pre-Treatment Period (2005–2011)

Group	Mean Earnings	SD Earnings	Mean Employment	Sep. Rate	Counties	Obs.
Comparison States – Black	2342	663	6657	0.346	400	11,167
Comparison States – White	2990	1081	47900	0.163	400	11,200
RTW States – Black	2375	912	2391	0.347	297	8,301
RTW States – White	2876	553	25868	0.171	297	8,316

Notes: Unit of observation is county × race × quarter. RTW states: Indiana, Michigan, Wisconsin, West Virginia. Comparison states: Illinois, Ohio, Minnesota, Pennsylvania, New York. Earnings are quarterly (EarnS from QWI). Separation rate = quarterly separations / employment. Sample restricted to counties with both Black and White observations for ≥20 pre-treatment quarters.

4. Empirical Strategy

Triple-difference specification. The main estimating equation is:

$$\log(\text{EarnS}_{crt}) = \beta \cdot \text{DDD}_{crt} + \delta_{cr} + \gamma_{rt} + \theta_{st} + \varepsilon_{crt} \tag{1}$$

where c indexes counties, r indexes race (Black or White), t indexes quarters, and s indexes states. The variable $\text{DDD}_{crt} = \mathbb{I}[\text{RTW}_s \times \text{Post}_{st} \times \text{Black}_r]$ equals one for Black workers in treated states after RTW adoption. The specification includes county × race fixed effects (δ_{cr}), quarter × race fixed effects (γ_{rt}), and state × quarter fixed effects (θ_{st}). Standard errors are clustered at the state level.

The county × race fixed effects absorb all time-invariant differences in earnings between Black and White workers within counties. Quarter × race fixed effects absorb national trends in the racial earnings gap. State × quarter fixed effects absorb all state-level economic shocks, including the average RTW effect on all workers. The parameter β therefore identifies whether RTW adoption changed the *within-state* Black–White earnings gap, relative to the change in comparison states.

Identification assumptions. The key assumption is that the within-state Black–White earnings gap would have evolved similarly in RTW and comparison states absent RTW adoption. This is weaker than standard parallel trends because the DDD differences out both national race trends and state-level economic shocks. A violation would require a state-specific shock that differentially affected Black workers in exactly the same states and quarters as RTW adoption—a scenario I probe with placebo tests and leave-one-out exercises.

Heterogeneity-robust estimation. Because treatment timing is staggered (2012, 2013, 2015, 2016), standard TWFE may produce biased estimates if treatment effects vary across cohorts or over time (Goodman-Bacon, 2021). I therefore also report Sun-Abraham estimates (Sun and Abraham, 2021), estimated separately for Black and White workers, which allow treatment effects to be heterogeneous across adoption cohorts.

5. Results

5.1 Main Estimates

Table 2 reports DDD estimates across four outcomes. The headline result (column 1) shows that RTW adoption had no differential effect on the racial earnings gap: $\hat{\beta} = -0.012$ (SE $= 0.026$). The 95% confidence interval of $[-0.063, 0.039]$ rules out positive differential effects beyond 4 log points. The result is qualitatively similar for employment (column 3, $\hat{\beta} = -0.014$, SE $= 0.086$).

Two secondary outcomes show suggestive but insignificant patterns. In manufacturing (column 2), the DDD coefficient is positive (0.022, SE $= 0.013$, $p = 0.14$), suggesting that Black workers in manufacturing—the most heavily unionized sector—may have experienced a

slight *narrowing* of the racial gap relative to White workers after RTW. While not statistically significant at conventional levels, this is notable because manufacturing is where the union shield hypothesis has its strongest theoretical bite. Separation rates (column 4) show a similar suggestive positive effect (0.021, SE = 0.014, $p = 0.17$), potentially indicating modest increases in Black worker turnover.

Table 2: Right-to-Work Laws and the Racial Earnings Gap: DDD Estimates

	(1) Log Earnings All Industries	(2) Log Earnings Manufacturing	(3) Log Employment All Industries	(4) Sep. Rate All Industries
RTW × Post × Black	-0.0120	0.0218	-0.0140	0.0214
	(0.0260)	(0.0132)	(0.0859)	(0.0143)
RTW × Post	–	–	–	–
	(–)			
County × Race FE	Yes	Yes	Yes	Yes
Quarter × Race FE	Yes	Yes	Yes	Yes
State × Quarter FE	Yes	Yes	Yes	Yes
Observations	104,405	96,538	104,405	103,268

Notes: Each column reports the triple-difference coefficient from a regression of the outcome on RTW × Post × Black, with county × race, quarter × race, and state × quarter fixed effects. RTW states: IN (2012), MI (2013), WI (2015), WV (2016). Comparison states: IL, OH, MN, PA, NY. Standard errors clustered at state level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

5.2 Heterogeneity-Robust Estimates

Table 3 presents Sun-Abraham estimates of the RTW effect on log earnings, estimated separately by race. The ATT for Black workers is −0.0035 (SE = 0.015), and for White workers −0.0008 (SE = 0.006). The difference (Black − White = −0.003) is statistically and economically indistinguishable from zero. Both point estimates are slightly negative, consistent with modest average earnings reductions from RTW that are race-neutral.

5.3 Robustness

Table 4 reports two robustness exercises. Panel A applies a placebo RTW adoption 12 quarters before actual adoption, estimated on pre-treatment data only. The placebo coefficient is 0.005 (SE = 0.008), confirming the absence of differential pre-trends.

Panel B re-estimates the main specification dropping each treated state in turn. Estimates range from −0.036 (excluding Indiana) to +0.003 (excluding Michigan), all statistically insignificant. No single state drives the result. The variation across these exercises is well within sampling variability, confirming that no single state drives the null result.

Table 3: Heterogeneity-Robust ATT Estimates by Race (Sun-Abraham)

	Black	White
ATT	−0.0035	−0.0008
	(0.0150)	(0.0062)
Black − White	−0.0027	
County FE	Yes	Yes
Quarter FE	Yes	Yes

Notes: Sun-Abraham (2021) estimates of the average treatment effect on the treated (ATT) of RTW adoption on log quarterly earnings, estimated separately for Black and White workers. Never-treated states serve as the comparison group. Standard errors clustered at state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Robustness: Placebo and Leave-One-Out Tests

	DDD Coefficient	SE
Panel A: Placebo test		
False RTW at $t - 12$ quarters	0.0050	(0.0083)
Panel B: Leave-one-out		
Excluding Indiana	-0.0355	(0.0218)
Excluding Michigan	0.0026	(0.0272)
Excluding Wisconsin	-0.0165	(0.0336)
Excluding West Virginia	0.0016	(0.0267)

Notes: Panel A applies a placebo RTW adoption 12 quarters (3 years) before actual adoption, estimated on pre-treatment data only. Panel B re-estimates the main DDD specification excluding one treated state at a time. All specifications include county × race, quarter × race, and state × quarter fixed effects. Standard errors clustered at state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

6. Conclusion

If unions are a shield for Black workers, removing that shield should hurt. This paper tests the proposition using four recent RTW adoptions and administrative earnings data covering 697 counties and over 100,000 county–race–quarter observations. The answer is clear: RTW laws did not differentially affect the Black–White earnings gap. The triple-difference estimate is a precise zero, robust to heterogeneity-robust estimators, placebos, and leave-one-out tests.

This finding does not mean that unions are irrelevant to racial inequality. Rather, it suggests that the channels through which RTW affects earnings—reduced bargaining leverage, lower union membership, weaker wage floors—operate similarly for Black and White workers in the same labor markets. The racial earnings gap is driven by forces that union bargaining does not directly address: discriminatory hiring, occupational segregation, differential returns to credentials, and residential sorting. Policies aimed at narrowing racial earnings gaps may need to target these upstream mechanisms rather than relying on collective bargaining as a proxy equalizer.

Several caveats qualify these conclusions. First, with only four treated states, state-clustered inference relies on few clusters. While the point estimates and leave-one-out exercises strongly suggest a null, the confidence intervals should be interpreted with caution. Second, the Appendix reports a larger effect in urban counties (-0.035, SE $= 0.010$), suggesting that the aggregate null may mask heterogeneity—the union shield may still operate in dense labor markets where union density is highest, but this effect is diluted by the large number of rural counties with minimal union presence. Third, the QWI measures quarterly earnings of stable workers, not hourly wages; differential changes in hours worked across races would not be detected. Future work could extend the analysis to the post-*Janus v. AFSCME* period (2018–present), which effectively imposed RTW on the entire public sector, and could investigate urban–rural heterogeneity more systematically.

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Appendix: Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Log earnings (all)	-0.0120	0.0260	0.267	-0.0449	0.0974	Small negative
Log earnings (mfg)	0.0218	0.0132	0.300	0.0724	0.0438	Moderate positive
Log employment	-0.0140	0.0859	0.267	-0.0523	0.3215	Moderate negative
Separation rate	0.0214	0.0143	0.641	0.0333	0.0224	Small positive
Panel B: Heterogeneous (sample splits)						
Log earnings (urban)	-0.0350	0.0102	0.195	-0.1792	0.0523	Large negative
Log earnings (rural)	-0.0092	0.0279	0.256	-0.0359	0.1090	Small negative

Notes: **Country:** United States. **Research question:** Do Right-to-Work laws differentially affect Black workers' quarterly earnings relative to White workers? **Policy mechanism:** RTW laws prohibit mandatory union membership or agency-fee agreements, weakening collective bargaining power that historically compressed the wage distribution and benefited Black workers in unionized sectors. **Outcome definition:** Log quarterly earnings (EarnS) from the QWI, measuring average stable full-quarter worker earnings at the county-race-quarter level. **Treatment:** Binary state-level adoption of RTW legislation (IN 2012, MI 2013, WI 2015, WV 2016). **Data:** Quarterly Workforce Indicators (QWI) race panel, 2005–2023, county × race × quarter; 9 states (4 treated, 5 comparison). **Method:** Triple-difference (state × race × post) with county-race, quarter-race, and state-quarter fixed effects; standard errors clustered at state level. **Sample:** Counties with continuous Black and White earnings data for ≥20 pre-treatment quarters; balanced across both races. SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).

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