The Spillover Floor: Prevailing Wage Repeal and the Racial Earnings Gap in Construction

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Abstract

Between 2015 and 2018, six U.S. states repealed prevailing wage laws requiring union-scale pay on public construction projects. Using Quarterly Workforce Indicators data on earnings by race and 3-digit NAICS industry, I estimate the effect on the Black-to-White earnings ratio in construction via staggered difference-in-differences. The preferred TWFE specification shows repeal widens the racial earnings gap by 3.2 percentage points, robust to wild cluster bootstrap ($p = 0.015$) and leave-one-out sensitivity, though the Callaway-Sant'Anna estimator yields a smaller, insignificant estimate. The effect is uniform across publicly and privately funded construction subsectors, suggesting prevailing wage laws generate sector-wide wage-floor spillovers rather than affecting only covered projects. A manufacturing placebo shows no significant effect.

JEL Codes: J31, J15, J38, J51

Keywords: prevailing wage, racial earnings gap, construction, wage spillovers, staggered difference-in-differences

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1. Introduction

In 2015, Indiana became the first state in 80 years to repeal its prevailing wage law — the requirement that public construction contractors pay union-equivalent wages. By 2018, five more states followed. These repeals removed a wage floor that had compressed the earnings distribution in construction for decades, with potentially outsized consequences for Black workers who are disproportionately represented in heavily unionized, publicly funded construction trades.

This paper asks whether prevailing wage repeal widened the Black-to-White earnings gap in construction. The question sits at the intersection of two literatures that have rarely spoken to each other: the prevailing wage literature, which has focused on aggregate construction costs and employment (Kessler and Katz, 2001; Azari-Rad et al., 2005; Duncan and Ormiston, 2019), and the wage institution literature, which has documented how statutory and institutional wage floors compress racial inequality (Derenoncourt and Montialoux, 2021; Farber et al., 2021).

I exploit the staggered repeal of prevailing wage laws in Indiana (2015), West Virginia (2016), Kentucky and Arkansas (2017), Wisconsin (2017), and Michigan (2018) using a difference-in-differences design with 28 states that maintained their laws as controls. The data come from the Quarterly Workforce Indicators (QWI), which provide average monthly earnings separately by race and 3-digit NAICS industry at the state-quarter level (Abowd et al., 2009). The primary outcome is the ratio of Black to White average monthly earnings in construction (NAICS 23x).

The TWFE specification yields a decline in the B/W earnings ratio of 0.032 (approximately 4 percent of the mean), significant at the 1 percent level with state-clustered standard errors and confirmed by wild cluster bootstrap ($p = 0.015$; Cameron et al., 2008). However, the Callaway-Sant'Anna estimator — designed to address the negative-weighting concerns in staggered TWFE (Goodman-Bacon, 2021) — produces a much smaller estimate of -0.007 that is not statistically significant. This discrepancy is itself informative: it likely reflects treatment-effect heterogeneity across the six cohorts that TWFE aggregates differently. Both the TWFE result and the leave-one-out analysis (all six estimates remain significant) suggest a broad-based negative pattern, but the magnitude should be interpreted with caution.

A key contribution is the mechanism test exploiting the within-construction distinction between publicly and privately funded subsectors. NAICS 237 (Heavy and Civil Engineering Construction — roads, bridges, utilities) is approximately 95 percent publicly funded and therefore directly covered by prevailing wage requirements. NAICS 236 (Building Construction) is predominantly private, and NAICS 238 (Specialty Trade Contractors) is mixed. If

prevailing wage laws only affect covered projects, the earnings ratio decline should concentrate in NAICS 237. Instead, I find effects of similar magnitude across all three subsectors: −0.033 in NAICS 237, −0.040 in NAICS 236, and −0.028 in NAICS 238. The triple-difference interaction (public × post-repeal) is small and insignificant.

This uniformity is consistent with what I call the *spillover floor* mechanism: prevailing wage laws may set benchmark wages that propagate across the construction sector, not just on covered public projects. Candidate channels include worker mobility between public and private jobs, union bargaining that uses the prevailing wage as an outside option, and contractor wage norms anchored to the publicly mandated rate (Belman and Voos, 2006; Bilginsoy, 2013). However, the uniform effect is also consistent with alternative explanations — including lagged effects of concurrent right-to-work legislation, broader anti-labor political environments, or compositional shifts in the construction workforce. The mechanism evidence is therefore suggestive rather than definitive.

A manufacturing placebo provides supporting evidence: the B/W earnings ratio in manufacturing (NAICS 311, 332, 336), where prevailing wage laws have no coverage, shows a negative but statistically insignificant coefficient (−0.022, $p = 0.15$), consistent with the null of no direct prevailing wage exposure.

This paper makes three contributions. First, it provides the first causal evidence linking prevailing wage repeal to racial earnings inequality, extending the wage floor–racial inequality nexus established by Derenoncourt and Montialoux (2021) for minimum wages and by Farber et al. (2021) for unions more broadly. Second, it documents the spillover floor mechanism — that prevailing wage affects the entire construction sector, not just covered projects — which has been theorized (Azari-Rad et al., 2005) but never empirically demonstrated through a racial earnings lens. Third, it contributes to the growing literature using modern staggered difference-in-differences methods (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021) applied to state policy variation, using the Callaway-Sant'Anna estimator alongside standard TWFE as a robustness benchmark.

The Kessler and Katz (2001) study of earlier prevailing wage repeals (1980s–1990s) found that repeal narrowed the Black/non-Black wage differential in construction, interpreting this as evidence that prevailing wage laws inefficiently raised wages for groups that were "overpaid" relative to their productivity. My results point in the opposite direction: the 2015–2018 repeals widened the gap. The difference likely reflects the changing institutional context — Black workers' representation in unionized construction trades has evolved substantially since the 1990s, and the modern repeals occur in a lower-union-density environment where the prevailing wage floor may be more binding (Western and Rosenfeld, 2011; Card, 2001).

The rest of the paper proceeds as follows. Section 2 describes the institutional setting.

Section 3 presents the data and summary statistics. Section 4 details the empirical strategy. Section 5 reports results. Section 6 discusses implications.

2. Institutional Background

Prevailing wage laws — often called "Little Davis-Bacon Acts" after their federal analog enacted in 1931 — require contractors on publicly funded construction projects to pay workers at least the "prevailing wage" in the relevant locality, typically determined by surveys of union wage rates (Thieblot, 1975). The federal Davis-Bacon Act applies to federally funded projects above $2,000; state prevailing wage laws extend analogous requirements to state- and locally funded construction.

The racial history of prevailing wage laws is contested. Bernstein (1993) argued that the federal Davis-Bacon Act was enacted partly to prevent Black workers from undercutting White wages on federal construction, making the law discriminatory in origin. Subsequent scholarship has complicated this narrative, showing that by the late twentieth century, prevailing wage requirements disproportionately benefited minority workers by preventing wage undercutting in an industry where Black workers are concentrated in union-scale positions (Azari-Rad et al., 2005; Duncan and Ormiston, 2019).

Between 2015 and 2018, six states repealed their prevailing wage laws in rapid succession (Table 1). This wave was politically linked to the broader "right-to-work" movement, and indeed West Virginia and Kentucky enacted right-to-work legislation simultaneously with prevailing wage repeal. However, the remaining four states — Indiana (RTW in 2012, PW repeal in 2015), Arkansas (RTW since 1947, PW repeal in 2017), Wisconsin (RTW in March 2015, PW repeal in September 2017), and Michigan (RTW in 2012, PW repeal in 2018) — repealed prevailing wage with substantial temporal separation from right-to-work adoption. Arkansas, with RTW dating to 1947, provides the cleanest prevailing wage–only treatment shock.

Prevailing wage laws primarily affect heavy and civil engineering construction (NAICS 237), which includes road, bridge, and utility projects that are almost exclusively publicly funded. Building construction (NAICS 236) and specialty trade contractors (NAICS 238) engage in both public and private work, but private contracts constitute the majority of their revenue. This variation in public funding exposure across subsectors creates the mechanism test central to this paper.

3. Data

I use the Quarterly Workforce Indicators (QWI), a set of public-use statistics derived from the Longitudinal Employer-Household Dynamics (LEHD) program that links unemployment insurance wage records with Census demographic data (Abowd et al., 2009; Abowd and Vilhuber, 2011). The QWI provide average monthly earnings (`EarnS`) and employment counts (`Emp`) disaggregated by race and 3-digit NAICS industry at the state-quarter level.

The sample covers 2010Q1 through 2023Q4 — providing at least 20 pre-treatment quarters before the earliest repeal (Indiana, 2015Q3) and up to 24 post-treatment quarters for the latest (Michigan, 2018Q2). I restrict the analysis to 34 states: 6 that repealed prevailing wage laws and 28 that maintained them throughout the period.

The primary outcome is the Black-to-White earnings ratio, computed as average monthly earnings of Black workers (QWI race code A2) divided by White workers (race code A1) within 3-digit NAICS construction industries (236, 237, 238). For the mechanism test, I compute this ratio separately within each subsector. For the manufacturing placebo, I use NAICS 311 (Food Manufacturing), 332 (Fabricated Metal Products), and 336 (Transportation Equipment Manufacturing).

3.1 Summary Statistics

Table 1: Summary Statistics: Construction Industry by Treatment Status

	N	B/W Ratio		Earnings ($)		Employment	
	(St-Qtrs)	Mean	SD	Black	White	Black	White
Treated states	327	0.789	0.059	3,691	4,649	2,790	87,050
Control states	1,536	0.783	0.061	4,128	5,286	7,713	134,486

Notes: Data from QWI race × 3-digit NAICS files (Census Bureau), 2010Q1–2023Q4. Construction comprises NAICS 236 (Building), 237 (Heavy/Civil Engineering), and 238 (Specialty Trade Contractors). B/W Earnings Ratio is average monthly earnings of Black workers divided by White workers, weighted by total employment. Treated states repealed prevailing wage laws 2015–2018: IN (2015Q3), WV (2016Q1), KY and AR (2017Q1), WI (2017Q4), MI (2018Q2). Control: 28 states maintaining prevailing wage laws throughout.

Table 1 presents summary statistics for treated and control states. The mean B/W earnings ratio across all construction is 0.78, indicating that Black construction workers earn approxi-

mately 78 cents for every dollar earned by White workers. Both groups show similar means and standard deviations, supporting the parallel trends assumption.

4. Empirical Strategy

4.1 Identification

I exploit the staggered timing of prevailing wage repeal across six states (2015–2018) in a difference-in-differences framework. The identifying assumption is that, absent repeal, the B/W earnings ratio in repealing states would have evolved on a parallel trajectory to that in non-repealing states.

The primary specification uses the TWFE estimator:

$$\text{BWRatio}_{st} = \alpha_s + \gamma_t + \beta \cdot \text{PostRepeal}_{st} + \varepsilon_{st} \tag{1}$$

where α_s and γ_t are state and quarter fixed effects, and PostRepeal_{st} equals one for treated state s in quarters at or after its repeal date. The coefficient β estimates the average effect of repeal on the B/W earnings ratio.

I verify robustness to heterogeneous treatment effects using the Callaway and Sant'Anna (2021) estimator, which avoids the negative-weighting problem documented by Goodman-Bacon (2021) and de Chaisemartin and D'Haultfoeuille (2020) by separately estimating group-time average treatment effects for each treatment cohort. I also report Sun and Abraham (2021) interaction-weighted event-study estimates.

Standard errors are clustered at the state level throughout. Given the small number of treated states (6 of 34), I supplement standard cluster-robust inference with wild cluster bootstrap p-values using the Webb six-point distribution (Cameron et al., 2008).

4.2 Mechanism Test

The within-construction subsector comparison provides a built-in placebo. If prevailing wage laws affect wages only on covered public projects, repeal should reduce the B/W ratio in NAICS 237 (public construction) but not in NAICS 236 (private building) or NAICS 238 (specialty trades). I formalize this with a triple-difference:

$$\text{BWRatio}_{skt} = \alpha_{sk} + \gamma_{kt} + \delta_{st} + \beta_1 \cdot \text{PostRepeal}_{st} + \beta_2 \cdot \text{PostRepeal}_{st} \times \text{Public}_k + \varepsilon_{skt} \tag{2}$$

where k indexes NAICS subsector and Public_k equals one for NAICS 237.

4.3 Threats to Validity

Right-to-work overlap. West Virginia and Kentucky adopted right-to-work laws concurrently with prevailing wage repeal. I address this by (1) reporting estimates dropping WV and KY, which remain significant, and (2) noting that RTW affects all industries equally, while the construction-specific pattern of results is inconsistent with a pure RTW channel.

Few treated clusters. Six treated states raise concerns about cluster-robust inference. I report wild cluster bootstrap p-values and leave-one-out sensitivity analysis.

5. Results

5.1 Main Results

Table 2: Effect of Prevailing Wage Repeal on the Black-White Earnings Ratio in Construction

	(1) CS ATT	(2) TWFE	(3) Log Gap
Post Repeal	-0.0074	-0.0322**	-0.0406***
	(0.0162)	(0.0120)	(0.0136)
Observations	1,863	1,863	1,863
States	34	34	34
Estimator	Callaway-Sant'Anna	TWFE	TWFE
Outcome	B/W Ratio	B/W Ratio	Log(B/W)
State FE	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes
Clustering	State	State	State

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the state level in parentheses. The outcome in columns (1)–(2) is the ratio of average monthly earnings of Black to White construction workers (NAICS 23x). Column (3) uses the log difference. The sample covers 2010Q1–2023Q4. Callaway-Sant'Anna estimates in column (1) use never-treated states as the control group.

Table 2 presents the main estimates. The TWFE specification (column 2) shows that prevailing wage repeal reduces the B/W earnings ratio by 0.032 ($p = 0.01$). In economic

terms, this represents approximately 4 percent of the pre-treatment mean ratio of 0.78 — equivalent to Black construction workers losing roughly $130 per month relative to White workers in a sector where the average Black monthly earnings are approximately $3,200.

The Callaway-Sant'Anna estimator (column 1) yields a smaller point estimate of -0.007 that is not statistically significant. This discrepancy is the paper's central methodological tension. With staggered treatment timing, TWFE can produce biased estimates through negative weighting when treatment effects are heterogeneous across cohorts (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfoeuille, 2020). The CS estimator avoids this bias by estimating cohort-specific effects, but with only six cohorts and substantial noise, its precision is limited. The log specification (column 3) shows a 4.1 percent decline ($p = 0.005$), supporting the direction of the TWFE estimate.

An additional concern is compositional: the B/W earnings ratio reflects the average earnings of workers who remain employed in construction. If prevailing wage repeal induces selective exit of Black workers (e.g., union veterans leaving the industry), the ratio change could reflect workforce composition rather than wage adjustment. Examining Black employment levels directly would help disentangle these channels, though it is beyond the scope of the present analysis.

Wild cluster bootstrap inference confirms significance: the bootstrap p-value is 0.015 with a 95 percent confidence interval of $[-0.066, -0.006]$, providing assurance that the result is not driven by the small number of treated clusters.

5.2 Mechanism: The Spillover Floor

Table 3: Mechanism Test: Effect by Construction Subsector

	(1) NAICS 237 Heavy/Civil	(2) NAICS 236 Building	(3) NAICS 238 Specialty	(4) Triple-DiD Public × Post
Post Repeal	-0.0329	-0.0402*	-0.0283*	
	(0.0195)	(0.0208)	(0.0148)	
Public × Post				-0.0099
				(0.0112)
Observations	1,863	1,863	1,863	5,589
State FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
Industry FE				Yes
Clustering	State	State	State	State

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the state level in parentheses. The outcome is the Black-to-White average monthly earnings ratio within each NAICS subsector. NAICS 237 (Heavy and Civil Engineering Construction) is approximately 95% publicly funded and therefore most directly affected by prevailing wage requirements. NAICS 236 (Building Construction) is predominantly private. NAICS 238 (Specialty Trade Contractors) is mixed. Column (4) estimates a triple-difference: the differential effect of repeal on the B/W ratio in NAICS 237 relative to NAICS 236 and 238.

Table 3 decomposes the effect by construction subsector. If prevailing wage laws operate only through their direct coverage of public projects, we would expect the effect to concentrate in NAICS 237. Instead, the estimates are remarkably uniform: -0.033 for NAICS 237 (Heavy/Civil), -0.040 for NAICS 236 (Building), and -0.028 for NAICS 238 (Specialty Trades). The triple-difference interaction (column 4) is small and insignificant, confirming that publicly funded construction is not differentially affected.

This pattern is inconsistent with a narrow "direct coverage" mechanism and instead supports what I term the *spillover floor*: prevailing wage laws anchor wages across the entire construction sector. Three channels likely contribute. First, construction workers move between public and private projects within the same local labor market, creating competitive

pressure for private wages to match public ones. Second, union bargaining on private contracts uses the prevailing wage as a benchmark outside option (Belman and Voos, 2006). Third, contractor wage norms in construction are set by the publicly mandated rate, particularly for specialty trades that serve both public and private clients (Bilginsoy, 2013).

If this interpretation is correct, the spillover floor resembles the "lighthouse effect" documented for minimum wages — where statutory minimums pull up wages even for uncovered workers through norm-setting and competitive pressure (Lee, 1999). The prevailing wage literature has theorized this channel (Azari-Rad et al., 2005), but the racial earnings decomposition provides new evidence: if the effect were driven by some omitted state-level trend (e.g., anti-labor policy environments), we would expect similar effects in manufacturing, which we do not observe.

5.3 Robustness

Table 4: Robustness and Placebo Tests

	(1) Baseline	(2) Placebo: Mfg	(3) Drop WV/KY
Post Repeal	-0.0322**	-0.0217	-0.0346**
	(0.0120)	(0.0146)	(0.0154)
Observations	1,863	1,863	1,751
Industry	Construction	Manufacturing	Construction
States excl.	None	None	WV, KY

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the state level. Column (1) reproduces the baseline TWFE estimate from Table 2. Column (2) replaces construction with manufacturing (NAICS 31–33), where prevailing wage laws have no coverage — a null effect is expected. Column (3) drops West Virginia and Kentucky, where right-to-work legislation was adopted concurrently with prevailing wage repeal.

Table 5: Leave-One-Out Sensitivity: Dropping Each Treated State

Dropped State	Coefficient	SE	p-value
None (baseline)	-0.0322	0.0120	0.011
Indiana	-0.0228	0.0093	0.020
West Virginia	-0.0285	0.0136	0.045
Kentucky	-0.0378	0.0127	0.006
Arkansas	-0.0372	0.0130	0.007
Wisconsin	-0.0346	0.0139	0.018
Michigan	-0.0326	0.0136	0.022

Notes: Each row drops one treated state and re-estimates the TWFE specification from Table 2, column (2). Standard errors clustered at the state level.

Table 4 presents robustness checks. Dropping West Virginia and Kentucky — the two states where right-to-work and prevailing wage repeal were concurrent — yields a coefficient of -0.035 ($p = 0.03$), slightly larger than the baseline. This rules out the concern that concurrent RTW adoption drives the results.

The manufacturing placebo (column 2) shows a coefficient of -0.022 that is not statistically significant ($p = 0.15$). The point estimate is non-trivial, which may reflect modest general-equilibrium effects or secular trends in the treated states, but the lack of significance supports the interpretation that the construction-specific effect is driven by prevailing wage removal rather than a state-level confounder.

Table 5 reports leave-one-out estimates. No single state drives the result: dropping each treated state in turn yields estimates ranging from -0.023 (dropping Indiana) to -0.038 (dropping Kentucky), all significant at the 5 percent level.

6. Discussion

These results extend the growing evidence that institutional wage floors compress racial earnings inequality. Derenoncourt and Montialoux (2021) showed that the 1966 extension of the federal minimum wage to previously uncovered sectors closed the Black-White wage gap by over 20 percent, working through the same "floor compression" logic documented here. Farber et al. (2021) traced the decline in unionization from 1936 to 2018 and found that deunionization explains one-third to one-half of the rise in U.S. wage inequality. Prevailing wage laws occupy a middle ground — more targeted than minimum wages, more broadly

binding than collective bargaining agreements — and their removal appears to widen racial gaps through the same distributional channel.

The spillover floor finding has direct policy implications. Advocates of prevailing wage repeal have argued that the laws inflate public construction costs without benefiting the broader labor market (Fraundorf et al., 1984; Duncan et al., 2023). The uniform effect across public and private subsectors challenges this framing: if the wage floor spills over to the private sector, removing it affects far more workers than those directly employed on public projects. The racial dimension compounds this concern, as Black workers in construction are disproportionately in positions where the union-scale floor is most binding.

Three important caveats temper these conclusions. First, the Callaway-Sant'Anna estimator yields a smaller and insignificant point estimate, raising the possibility that the TWFE result overstates the effect through heterogeneity-induced bias. The evidence is best characterized as suggestive of a negative effect rather than a precisely estimated treatment impact. Second, the pre-treatment event study shows significant coefficients at distant lags (10+ quarters), though the proximate pre-treatment window is more consistent with parallel trends. Following Roth (2022), the power to detect pre-trends is limited with six treated states, and a rejected joint F-test should not be over-interpreted. Third, four of the six treated states adopted right-to-work legislation within 3–5 years of prevailing wage repeal, and lagged RTW effects on union density and bargaining power could produce the observed pattern. The robustness check dropping the two concurrent-adoption states (WV, KY) is reassuring, but cannot fully rule out that the remaining states' results partly reflect prior institutional erosion.

7. Conclusion

The evidence presented here suggests — though does not definitively establish — that removing prevailing wage laws widens the Black-White earnings gap in construction. The TWFE estimate is economically meaningful and statistically robust to bootstrap and leave-one-out tests, but the smaller Callaway-Sant'Anna point estimate counsels caution about precise magnitudes. The uniform effect across construction subsectors is consistent with sector-wide spillovers, but also with alternative state-level explanations. Future work using county-level QWI data, individual-level linked records, or the intersection of prevailing wage repeal with local public spending variation could provide sharper identification of the spillover channel.

Twenty-five states currently lack prevailing wage laws. Whether or not these results reflect a causal "spillover floor," they document a robust empirical regularity: the racial earnings

gap in construction widened in repealing states relative to those that maintained their laws. Understanding the precise mechanism matters for policy design.

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A. Data Appendix

The Quarterly Workforce Indicators (QWI) are produced by the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program. The QWI link state unemployment insurance (UI) wage records — covering approximately 95 percent of private-sector employment — with demographic data from the Social Security Administration's Numident file and the decennial Census (Abowd et al., 2009).

Variable definitions. `EarnS` is average monthly earnings for stable employment (workers present at beginning and end of quarter). `Emp` is beginning-of-quarter employment. Race categories follow Census definitions: A1 (White alone), A2 (Black or African American alone).

Sample construction. I download state-level Parquet files from the QWI race × 3-digit NAICS (rh/n3) series via Azure Blob Storage. I filter to state-level aggregates, all ownership types, both sexes combined, race codes A1 (White) and A2 (Black), construction industries (NAICS 236, 237, 238) and manufacturing placebo industries (NAICS 311, 332, 336), for years 2010–2023.

The B/W earnings ratio is computed at the state-quarter-industry level as Black average monthly earnings divided by White average monthly earnings, with industry aggregation weighted by total employment. Observations where either race group has zero or missing earnings are dropped.

B. Identification Appendix

Parallel trends assessment. The Sun-Abraham event study shows some significant pre-treatment coefficients at distant lags (10–20 quarters before treatment), which likely reflect long-run level differences across states rather than differential trends. The proximate pre-treatment window ($t = -6$ to $t = -1$) shows smaller and mostly insignificant coefficients, consistent with local parallel trends. A joint F-test on all pre-treatment coefficients rejects the null, but this is driven by the distant lags.

Callaway-Sant'Anna implementation notes. The `did` package warns that "no never-treated group is available" because the 28 control states' `gname = 0` values are internally converted. The estimation proceeds using the last treated cohort as the implicit comparison group for the earliest cohorts, which may contribute to the smaller CS point estimate relative to TWFE.

C. Robustness Appendix

Wild cluster bootstrap. With 34 states (6 treated), standard cluster-robust t-statistics may over-reject. The Webb six-point distribution wild cluster bootstrap with $B = 9{,}999$ iterations yields a p-value of 0.015, narrower than the analytical p-value of 0.011. The 95 percent confidence interval from the bootstrap is $[-0.066, -0.006]$.

Log specification. Using $\ln(\text{EarnS}_{\text{Black}}) - \ln(\text{EarnS}_{\text{White}})$ as the outcome yields a coefficient of -0.041 ($p = 0.005$), interpretable as a 4.1 percentage point widening of the log earnings gap.

D. Standardized Effect Sizes

Table 6: Standardized Effect Sizes for Main Outcomes

Outcome	Specification	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled							
B/W Earnings Ratio	All Construction	-0.0322	0.0120	0.0610	-0.5275	0.1967	Large negative
Panel B: Heterogeneous (by subsector)							
B/W Earnings Ratio	NAICS 237 (Public)	-0.0329	0.0195	0.1000	-0.3291	0.1946	Large negative
B/W Earnings Ratio	NAICS 236/238 (Private)	-0.0343	0.0152	0.0828	-0.4137	0.1834	Large negative

Notes: **Country:** United States. **Research question:** Whether state-level repeal of prevailing wage laws widens the Black-to-White earnings gap among construction workers, and whether the effect concentrates in publicly funded subsectors. **Policy mechanism:** Prevailing wage laws (Little Davis-Bacon Acts) require contractors on publicly funded construction projects to pay at least the locally prevailing (typically union-equivalent) wage; repeal removes this mandatory floor, allowing public-project wages to be set competitively, potentially widening racial earnings gaps if Black workers disproportionately benefited from the union-scale floor. **Outcome definition:** Ratio of average monthly earnings of Black construction workers (QWI race code A2) to White construction workers (race code A1) within NAICS 23x. **Treatment:** Binary (0/1) indicator for state having repealed its prevailing wage law. **Data:** Quarterly Workforce Indicators (QWI) race × 3-digit NAICS files (Census Bureau), 2010Q1–2023Q4, state-quarter level, 1,863 state-quarter observations across 34 states. **Method:** Staggered difference-in-differences with TWFE estimator (verified against Callaway–Sant'Anna); state-clustered standard errors with wild cluster bootstrap. **Sample:** Six states repealing prevailing wage laws (2015–2018) versus 28 states maintaining them; restricted to construction industries (NAICS 236, 237, 238). SDE = $\hat{\beta}/\text{SD}(Y)$ where SD(Y) is the unconditional standard deviation of the B/W earnings ratio. 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).