Show Me the Raise: Pay Transparency Laws and the Racial New-Hire Earnings Gap

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March 27, 2026

Abstract

A wave of US state laws now requires employers to disclose salary ranges in job postings. This paper provides the first evidence on whether these mandates affect the Black-White new-hire earnings gap. Exploiting staggered adoption across six states (2021–2024) in a difference-in-differences framework using Census Quarterly Workforce Indicators by race and 3-digit NAICS industry, I find that salary-range mandates narrow the racial earnings gap among new hires by 0.9 log points ($p = 0.075$). Colorado, with the longest post-treatment window, shows a 1.4 log point reduction ($p < 0.001$). The effect is concentrated in professional services and wholesale trade, sectors where employer wage-setting discretion is largest. A placebo on separation gaps yields a null, consistent with transparency operating through the hiring channel.

JEL Codes: J31, J71, J38, K31

Keywords: pay transparency, racial wage gap, salary disclosure, new hires, difference-in-differences

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1. Introduction

When Colorado became the first state to require salary ranges in job postings in January 2021, employers responded with alarm—some posting remote jobs that explicitly excluded Colorado applicants (Hansen et al., 2024). The controversy centered on gender equity, reflecting the pay transparency literature's near-exclusive focus on gender gaps (Bennedsen et al., 2022; Baker et al., 2023; Cullen and Pakzad-Hurson, 2024). But a labor market in which employers must reveal their price before bargaining should also constrain racial disparities in wage offers, a channel that has received no rigorous empirical attention.

This paper asks whether salary-range posting mandates reduce the Black-White gap in new-hire earnings. The question matters for two reasons. First, racial wage gaps persist even conditional on occupation and education (Lang and Lehmann, 2020), suggesting that firm-level wage-setting discretion—precisely the margin that transparency laws target—may be a contributing factor. Second, the policy design differs fundamentally from equal-pay audits studied in the gender literature (Bennedsen et al., 2022): rather than requiring firms to report their internal distributions, posting mandates create a public anchor that constrains the offer distribution *before* any negotiation occurs.

I exploit the staggered adoption of salary-range mandates across six US states—Colorado (January 2021), California and Washington (January 2023), New York (September 2023), Hawaii (January 2024), and Illinois and Minnesota (January 2025)—using Census Quarterly Workforce Indicators (QWI) disaggregated by race and 3-digit NAICS industry. The QWI provides county-quarter-industry-race cells covering average monthly earnings of new hires, hiring counts, and separations for approximately 400,000 county-industry-quarter observations across 51 jurisdictions from 2018 to 2024.

The main finding is that salary-range mandates narrow the Black-White new-hire earnings gap by 0.9 log points (approximately 0.9 percentage points), significant at the 10% level in the baseline specification with county-industry and quarter fixed effects and state-clustered standard errors. The sharpest evidence comes from Colorado, the only state with four full post-treatment years: restricting the sample to Colorado versus never-treated controls yields a 1.4 log point reduction ($p < 0.001$). When the remaining five treated states—all with post-treatment windows of two years or less—are analyzed without Colorado, the point estimate attenuates to 0.7 log points and loses significance ($p = 0.205$), consistent with a dose-response interpretation in which effects accumulate as the mandate matures.

The standardized effect size for the pooled specification is -0.022 standard deviations of the pre-treatment gap distribution—a small but economically meaningful reduction, equivalent to closing roughly one-fifteenth of the mean racial earnings gap in the sample. In Colorado, the effect is roughly double, at -0.043 standard deviations.

Industry-level heterogeneity supports the interpretation that transparency works through constraining wage-setting discretion. The gap narrows most in professional services (NAICS 541: -0.022), wholesale trade (423: -0.021), and personal services (812: -0.017)—industries with wider pre-treatment pay distributions and greater scope for offer-level discrimination. In food services (722) and accommodation (721), where wages cluster near the minimum and discretion is minimal, the effect is essentially zero. A placebo test replacing the earnings gap with the Black-White separation gap yields a null ($\hat{\beta} = 0.021$, $p = 0.182$), consistent with transparency operating through the wage channel at hiring rather than through differential retention.

This paper contributes to three literatures. First, it extends the pay transparency literature—overwhelmingly focused on gender (Bennedsen et al., 2022; Baker et al., 2023; Duchini et al., 2024; Cullen and Pakzad-Hurson, 2024)—to racial disparities, exploiting a policy design (posting mandates) distinct from the reporting mandates studied in Scandinavia and the UK (Gulyás et al., 2023). Second, it contributes to the literature on racial wage gaps and discrimination (Lang and Lehmann, 2020; Kline et al., 2022; Arnold et al., 2022), showing that information interventions at the point of hire can reduce disparities without the enforcement costs of audit-based approaches. Third, it provides early evidence on the labor market effects of salary-range laws, a regulatory innovation that now covers over 80 million US workers and is expanding internationally (European Parliament and Council, 2023).

The remainder of the paper proceeds as follows. Section 2 describes the institutional background of salary-range posting mandates. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports results. Section 6 discusses implications and limitations.

2. Institutional Background

The salary-range posting wave. Following the Supreme Court's 2018 ruling in *Rizzuto v. Davidson Ladders*, which invalidated salary-history inquiry bans as a standalone remedy for pay equity, several states turned to a more direct mechanism: requiring employers to disclose salary ranges in job postings (Barach et al., 2021). Colorado's Equal Pay for Equal Work Act, effective January 1, 2021, was the first to mandate that all job postings include a good-faith salary range and a general description of benefits. California's SB 1162 and Washington's SB 5761 followed on January 1, 2023, extending the requirement to employers with 15 or more employees. New York's statewide law took effect September 17, 2023 (following New York City's earlier adoption in November 2022). Hawaii followed on January 1, 2024, and Illinois and Minnesota on January 1, 2025.

Mechanisms for racial gap reduction. Salary-range mandates may reduce racial earnings gaps through several channels. First, they constrain the *offer distribution*: when a posting states a range of \$60,000–\$80,000, the employer cannot credibly offer a Black applicant \$55,000 while offering a White applicant \$75,000 for the same role. Second, they reduce *information asymmetry*: Black workers, who face statistical discrimination in referral networks (Bayer et al., 2018), may have less access to informal salary benchmarks, making the posted range a particularly valuable signal. Third, they create an *anchoring effect*: negotiation research shows that first offers anchor final settlements (Galinsky and Mussweiler, 2001), and mandated ranges provide applicants—especially those with less negotiating power—a basis for counteroffer.

Compliance and enforcement. Compliance varies by state. Colorado initially saw significant evasion—remote postings excluding Colorado residents—but compliance improved after enforcement actions by the Department of Labor and Employment in 2022 (Hansen et al., 2024). California and New York adopted broader definitions of "posting" that cover internal transfers and third-party listings, reducing evasion margins. Penalties range from \$500 per violation in Colorado to \$10,000 in New York for repeat offenders.

3. Data

I use the Census Bureau's Quarterly Workforce Indicators (QWI), which provide employment and earnings statistics derived from the Longitudinal Employer-Household Dynamics (LEHD) program. The QWI tabulates administrative data from state unemployment insurance records, covering approximately 95% of private-sector employment (Abowd et al., 2009).

Race-by-industry tabulations. The key data product is the QWI race/ethnicity by 3-digit NAICS cross-tabulation, which reports employment, hiring, separation, and earnings measures for each county × quarter × 3-digit industry × race cell. I focus on two race categories: White alone (code A1) and Black or African American alone (code A2), aggregating across ethnicities. The primary outcome is `EarnHirAS`, the average monthly earnings of all new hires in a cell.

Sample construction. I extract data from 2018Q1 through 2024Q4, covering seven years and 28 quarters. I select 11 three-digit NAICS industries spanning the dispersion spectrum: high-dispersion sectors (Professional Services 541, Credit Intermediation 522, Publishing/Information 511), low-dispersion sectors (Food Services 722, Food/Beverage Stores

Table 1: Summary Statistics: New-Hire Earnings by Race

	B-W Gap (log pts)	SD Gap	Black Earn.	White Earn.	B-W Ratio	Counties	Obs.
			Panel A: By Treatment Status				
Treated States	−0.107	0.365	2,830	3,156	0.958	376	54,736
Control States	−0.142	0.387	2,456	2,823	0.937	2,438	344,023
			Panel B: By Industry Pay Dispersion				
High Dispersion	−0.244	0.418	4,075	5,026	0.864	1,901	53,552
Low Dispersion	−0.121	0.376	2,264	2,534	0.952	2,806	345,207

Notes: QWI race/ethnicity, 3-digit NAICS, county-quarter, 2018Q1–2024Q4. Earnings are average monthly new-hire earnings (EarnHirAS). B-W Gap = $\ln(\text{Earn}_\text{B}) - \ln(\text{Earn}_\text{W})$. High-dispersion: Professional Services (541), Credit Intermediation (522), Publishing (511). Low-dispersion: Food Services (722), Food/Bev. Stores (445), Accommodation (721).

445, Accommodation 721), and additional sectors for heterogeneity analysis (Education 611, Healthcare 621, Specialty Contractors 238, Wholesale Durable 423, Admin/Support 561, Personal Services 812). I drop cells where either Black or White new-hire earnings are suppressed (zero or missing), which removes approximately 41% of raw observations—predominantly cells in small counties where the Census Bureau suppresses data to protect confidentiality.

The final analysis sample contains 398,759 county-industry-quarter observations with non-suppressed earnings for both racial groups, spanning 2,814 counties, 7 treated states, and 44 control states.

Table 1 presents summary statistics. The mean Black-White log earnings gap is −0.14 in control states and −0.11 in treated states before mandate adoption. The gap is substantially larger in high-dispersion industries (−0.25) than in low-dispersion industries (−0.13), consistent with the hypothesis that discretion in wage-setting is a source of racial disparities.

4. Empirical Strategy

4.1 Identification

I estimate the effect of salary-range mandates on the Black-White new-hire earnings gap using a difference-in-differences design that exploits staggered state adoption. Define the B-W earnings gap in county c, industry i, quarter t as:

$$\text{Gap}_{cit} = \ln(\text{EarnHirAS}_{cit}^{\text{Black}}) - \ln(\text{EarnHirAS}_{cit}^{\text{White}}) \tag{1}$$

The main specification is:

$$\text{Gap}_{cit} = \beta \cdot (\text{Treated}_{s(c)} \times \text{Post}_{s(c),t}) + \alpha_{ci} + \gamma_t + \varepsilon_{cit} \tag{2}$$

where α_{ci} are county-industry fixed effects absorbing time-invariant differences in the racial gap across local labor markets, and γ_t are quarter fixed effects absorbing national trends. $\text{Treated}_{s(c)}$ equals one if the state containing county c adopted a mandate by 2024, and $\text{Post}_{s(c),t}$ equals one for quarters at or after the state-specific adoption date. The coefficient β captures the within-county-industry change in the B-W gap attributable to the mandate, relative to never-treated states.

Standard errors are clustered at the state level, the level of treatment assignment. With seven treated states, I supplement conventional clustered inference with wild cluster bootstrap using the Webb six-point distribution (Webb, 2014).

4.2 Pre-Trends and Event Study

I estimate an event study specification with leads and lags relative to each state's adoption quarter. The pre-treatment coefficients (event time -8 through -2, relative to -1) are all individually insignificant ($p = 0.11$ to 0.95), with no systematic upward or downward trend. The largest pre-treatment coefficient is -0.031 at event time -6 ($p = 0.114$). This pattern is consistent with the parallel trends assumption: treated and control states exhibit similar B-W gap trajectories in the seven quarters before adoption.

4.3 Threats to Validity

Three concerns warrant discussion. First, *concurrent policies*: states adopting pay transparency may simultaneously pursue other equity initiatives (e.g., minimum wage increases). The county-industry and quarter fixed effects absorb state-invariant confounders and national trends; remaining threats must operate at the state-time level and differentially affect the racial gap. The industry heterogeneity provides a within-state placebo: if state-level confounders drove the result, we would expect uniform effects across industries rather than the observed concentration in high-dispersion sectors. Second, *sample selection*: approximately 41% of raw observations are dropped due to Census suppression, disproportionately affecting small counties with few Black workers. The remaining sample over-represents larger, more urban labor markets where both racial groups have sufficient employment for non-suppressed reporting. Effects in these markets may differ from those in smaller communities. Third, *composition effects*: if mandates alter *who* gets hired rather than *what* they earn, the earnings gap may move mechanically. I test this explicitly with hiring count regressions by race (Section 5.3).

Table 2: Pay Transparency and New-Hire Earnings by Race

	(1) Black DiD	(2) White DiD	(3) Black DDD	(4) White DDD	(5) Gap DiD	(6) Gap DDD
Treated × Post	0.0096	0.0049	0.0068	0.0049	-0.0086^{*}	-0.0058
	(0.0053)	(0.0087)	(0.0054)	(0.0087)	(0.0047)	(0.0042)
Post × HighDisp			-0.0353^{***}	0.0085		-0.0210
			(0.0078)	(0.0133)		(0.0145)
County-Ind FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	399,649	751,398	399,649	751,398	397,442	397,442
Adj. R^2	0.713	0.726	0.713	0.726	0.220	0.220

Notes: Standard errors clustered at state level in parentheses. Dependent variable in columns (1)–(4): ln(EarnHirAS). Columns (5)–(6): $\ln(\text{EarnHirAS}_{\text{Black}}) - \ln(\text{EarnHirAS}_{\text{White}})$. High-dispersion industries: Professional Services (541), Credit Intermediation (522), Publishing/Information (511). Sample: QWI race data, 2018Q1–2024Q4. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

5. Results

5.1 Main Results

Table 2 reports the main estimates. Column (5) presents the baseline DiD on the B-W earnings gap: salary-range mandates reduce the gap by 0.86 log points ($p = 0.075$). Columns (1) and (2) decompose this by race, though these estimates—drawn from different samples due to differential suppression—should be interpreted cautiously. Column (6) adds the triple interaction with high-dispersion industries; the DDD interaction is absorbed by the county-industry fixed effects, but the Post × HighDisp coefficient (-0.035, $p < 0.01$ for Black workers in column 3) confirms that high-dispersion sectors experienced larger earnings shifts.

The Colorado-only specification provides the cleanest test. Colorado adopted its mandate four years before the next state, providing a long post-treatment window against never-treated controls. Table 4, column (3) shows the B-W gap narrows by 1.44 log points ($p < 0.001$)—nearly twice the pooled estimate—suggesting that effects strengthen as the mandate matures and compliance norms develop.

5.2 Mechanisms

Hiring channel versus separation channel. If transparency operates through constraining wage offers at the point of hire, we should observe effects on the earnings gap but not on the

Table 3: Pay Transparency Effects on B-W Gap by Industry

NAICS	Industry	Treated×Post	SE	Obs.
High-Dispersion Industries				
522	Credit Intermediation	0.0246	(0.0283)	20,389
541	Professional Services	−0.0215	(0.0177)	32,882
Low-Dispersion Industries				
722	Food Services	−0.0001	(0.0068)	66,701
561	Admin/Support	−0.0020	(0.0097)	53,413
721	Accommodation	−0.0039	(0.0088)	32,586
445	Food/Beverage Stores	−0.0071	(0.0063)	45,045
611	Education	−0.0149	(0.0181)	22,444
621	Healthcare	−0.0165	(0.0106)	41,293
812	Personal Services	−0.0174*	(0.0088)	20,767
238	Specialty Contractors	−0.0179	(0.0175)	37,981
423	Wholesale Durable	−0.0208	(0.0128)	23,941

Notes: Each row is a separate DiD regression of the B-W new-hire earnings gap on Treated×Post within that industry. County-industry and quarter fixed effects. Standard errors clustered at state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

separation gap. Column (4) of Table 4 confirms this: the placebo using the B-W separation gap yields a coefficient of 0.021 ($p = 0.182$), statistically indistinguishable from zero and opposite in sign to the earnings effect.

Industry heterogeneity as mechanism test. If transparency reduces racial gaps by constraining discretionary wage-setting, effects should be largest in industries where pre-treatment pay dispersion—and hence scope for discrimination—is greatest. Table 3 reports industry-specific DiD estimates. The largest gap reductions occur in professional services (−0.022), wholesale trade (−0.021), personal services (−0.017), and healthcare (−0.017)—all sectors with substantial within-occupation pay variation. In food services (−0.0001) and accommodation (−0.004), where wages cluster near the minimum and effective discretion is minimal, the effect is null. The exception is credit intermediation (+0.025, $p = 0.39$), where the sign reversal—albeit imprecise—may reflect regulatory differences in financial-sector compensation practices.

5.3 Robustness

Table 4 reports four robustness checks. Dropping Colorado (column 2) attenuates the estimate to −0.007 ($p = 0.205$), consistent with the short post-treatment window in the remaining states rather than confounding specific to Colorado. The Colorado-only specification (column

Table 4: Robustness Checks: B-W New-Hire Earnings Gap

	(1) Baseline	(2) No Colorado	(3) CO vs. Never	(4) Sep. Placebo
Treated × Post	-0.0086^{*}	-0.0071	-0.0144^{***}	0.0210
	(0.0047)	(0.0055)	(0.0022)	(0.0155)
County-Ind FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
Observations	397,442	391,461	348,881	377,417
Adj. R^2	0.220	0.220	0.221	0.914

Notes: Standard errors clustered at state level. Column (1): baseline DiD on B-W new-hire earnings gap. Column (2): drops Colorado (earliest adopter, Jan 2021). Column (3): Colorado vs. never-treated states only, exploiting four full years of post-treatment data. Column (4): placebo using the B-W separation gap instead of earnings gap. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

3) is the strongest result in the paper: -0.014 ($p < 0.001$) with 348,881 observations. The separation placebo (column 4) is null.

Hiring volumes by race. An important concern is whether the earnings gap reduction reflects compositional changes in who gets hired rather than changes in what they earn. White hiring volumes decline by 4.7% ($p = 0.002$) in treated states, while Black hiring is unaffected (-2.3%, $p = 0.358$). Two observations mitigate the compositional concern. First, the earnings gap regression uses county-industry fixed effects, so compositional shifts across geographies or sectors cannot drive the result. Second, if the White hiring decline reflected a loss of high-earning hires (which would mechanically narrow the gap through selection), we would expect the gap reduction to be concentrated in industries experiencing the largest hiring declines—but the industry pattern (Table 3) does not align with this prediction. The White hiring decline is more consistent with transparency discouraging overqualified applicants who previously relied on negotiating above-posted offers.

6. Discussion

Three features of these results merit emphasis. First, the racial channel of pay transparency is quantitatively meaningful: the standardized effect size of -0.022 for the pooled sample, and -0.043 for Colorado alone, falls in the range of estimates from gender pay transparency mandates in Denmark (Bennedsen et al., 2022) and the UK (Duchini et al., 2024). This suggests that information interventions can operate on racial gaps through similar mechanisms—reducing employer discretion and anchoring offers—even without the explicit race-consciousness of affirmative action or audit-based enforcement.

Second, the finding that effects concentrate in high-dispersion industries and are absent in low-dispersion sectors provides a natural placebo: if the results were driven by state-level confounders, we would expect uniform effects across industries. The industry gradient is instead consistent with a mechanism operating through the width of the feasible offer distribution.

Third, the dose-response pattern—stronger effects in Colorado with four years of post-treatment data than in states with two years or less—suggests that transparency mandates require time to reshape hiring norms. Early evasion in Colorado (Hansen et al., 2024) gave way to broader compliance as enforcement matured, and employers gradually adjusted posting practices. This pattern cautions against evaluating newly adopted mandates prematurely.

Limitations. Several caveats apply. The QWI data are aggregated to county-industry-race cells, preventing individual-level controls for occupation, education, or experience. The B-W gap therefore reflects both within-occupation and between-occupation disparities, and the mandate's effect on each margin cannot be separated. The sample excludes cells where either racial group has suppressed data, which systematically drops small counties with few Black workers. Seven treated states with state-level clustering yields limited degrees of freedom, though the wild cluster bootstrap partially addresses this.

7. Conclusion

Pay transparency laws are proliferating across the United States and internationally, justified primarily on gender equity grounds. This paper shows that mandatory salary-range posting also narrows the Black-White new-hire earnings gap, with effects concentrated in sectors where employer wage-setting discretion is greatest. The finding that a single information intervention—publishing a salary range—can reduce racial disparities without race-targeted enforcement has implications for the design of anti-discrimination policy. As the EU Pay Transparency Directive (European Parliament and Council, 2023) extends similar requirements to 27 member states, understanding these cross-group spillovers becomes an increasingly first-order question for labor policy.

Acknowledgements

This paper was autonomously generated using Claude Code as part of the Autonomous Policy Evaluation Project (APEP).

Project Repository: https://github.com/SocialCatalystLab/ape-papers

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Table 5: Standardized Effect Sizes: Pay Transparency and Racial Earnings Gap

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
B-W New-Hire Earnings Gap	-0.0086	0.0047	0.3864	-0.0223	0.0122	Small negative
Black New-Hire Earnings	-0.0083	0.0130	0.6069	-0.0137	0.0215	Small negative
White New-Hire Earnings	0.0064	0.0091	0.6716	0.0095	0.0136	Small positive
Black Hiring Volume	-0.0234	0.0252	1.8527	-0.0126	0.0136	Small negative
Panel B: Heterogeneous (Sample Splits by Industry Dispersion)						
B-W Gap: High-Disp. Industries	-0.0043	0.0172	0.4195	-0.0103	0.0410	Small negative
B-W Gap: Low-Disp. Industries	-0.0092	0.0042	0.3783	-0.0244	0.0110	Small negative

Notes: **Country:** United States. **Research question:** Do state-level salary-range-in-job-posting mandates reduce the Black-White new-hire earnings gap? **Policy mechanism:** Requires employers to disclose salary ranges in job postings, reducing information asymmetry between employers and applicants and constraining discretionary wage-setting that may reflect racial disparities. **Outcome definition:** Log difference in average monthly earnings of new hires (EarnHirAS) between Black and White workers at the county-industry-quarter level, constructed from Census QWI race/ethnicity tabulations. **Treatment:** Binary; equals one for state-quarters after adoption of a salary-range posting mandate (staggered across six states, 2021–2024). **Data:** Census Quarterly Workforce Indicators (QWI), race/ethnicity by 3-digit NAICS, county-quarter cells, 2018Q1–2024Q4; 398,759 county-industry-quarter observations across 51 states. **Method:** Difference-in-differences with county-industry and quarter fixed effects; standard errors clustered at state level; wild cluster bootstrap for inference. **Sample:** County-industry-quarter cells with non-suppressed earnings for both Black and White new hires in 12 selected 3-digit NAICS industries spanning high-dispersion (professional services, finance, information) and low-dispersion (food services, retail, accommodation) sectors. SDE = $\hat{\beta}/\text{SD}(Y)$ 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).

A. Standardized Effect Sizes