Does Raising the Floor Change Who Gets Hired? Minimum Wage Increases and the Racial Composition of Worker Flows

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Abstract

If employers hire minority workers at a discount, a binding minimum wage should narrow racial hiring gaps by eliminating this wedge. I test this Becker (1957) prediction using Quarterly Workforce Indicators data, exploiting staggered state minimum wage increases above 110% of the federal floor across 28 states. Callaway–Sant'Anna estimates reveal a precisely estimated null effect on the Black share of new hires in low-wage industries. However, Black separation rates rise significantly, suggesting wage compression operates on the exit rather than entry margin. A triple-difference comparing exposed to non-exposed industries within the same county-year confirms the null hiring result. These findings constitute the first direct test of the Becker discrimination channel using administrative hiring flows, rejecting the prediction that wage floors equalize entry-level hiring across racial groups.

JEL Codes: J15, J31, J38, J71

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1. Introduction

A central prediction of Becker's (1957) model of taste-based discrimination is that competitive pressure should erode discriminatory hiring. When employers pay minority workers less than their marginal product to indulge a "taste" against them, a binding minimum wage eliminates this wage wedge — forcing employers to pay Black and White workers the same floor. If the discount was sustaining discriminatory hiring patterns, compressing the wage distribution should change *who* gets hired, not just what they earn.

Despite decades of minimum wage research, this prediction has never been directly tested with hiring flow data. The literature has extensively studied employment effects (Card and Krueger, 1994; Neumark and Wascher, 2000; Dube et al., 2010; Cengiz et al., 2019), earnings distributions (Autor et al., 2016; Dustmann et al., 2022), and firm responses (Harasztosi and Lindner, 2019; Draca et al., 2011). Yet the question of whether minimum wage increases alter the *racial composition* of who enters and exits jobs has remained largely unexamined, primarily because the data to decompose employment into race-specific worker flows did not exist at the necessary scale.

This paper fills that gap using administrative worker-flow data from the Census Bureau's Quarterly Workforce Indicators (QWI), which provide county-quarter-level counts of new hires, separations, employment, and earnings by race for detailed industry sectors. I exploit the staggered adoption of state minimum wages above 110% of the federal floor ($7.98) across 28 treated states between 2005 and 2024, with approximately 20 states that remained at or near the federal minimum of $7.25 serving as never-treated controls.

The main specification uses the Callaway–Sant'Anna (2021) staggered difference-in-differences estimator, which avoids the well-documented bias of two-way fixed effects estimators under treatment effect heterogeneity (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021). Focusing on low-wage industries where the minimum wage binds most tightly — Accommodation and Food Services (NAICS 72) and Retail Trade (NAICS 44–45) — I find a precisely estimated null effect on the Black share of new hires. The point estimate can rule out effects larger than approximately 0.5 percentage points, against a baseline Black hire share of 13.7%. However, Black separation rates increase significantly, suggesting the wage floor may differentially affect minority workers' job stability rather than their entry into employment.

I strengthen identification in three ways. First, a triple-difference compares the change in racial hiring composition within minimum-wage-exposed industries (NAICS 72, 44–45) to that in non-exposed industries (Finance, Professional Services) within the same county-year. This absorbs any county-level shocks correlated with minimum wage adoption that affect all industries equally. Second, a placebo test on the Healthcare sector (NAICS 62) — where wages are well above the minimum — confirms that the pattern is absent in non-binding sectors. Third, I show that the effects concentrate in counties with high pre-treatment Black employment shares, where the scope for taste-based discrimination is greatest, and that larger minimum wage "bites" produce stronger compositional shifts.

The contribution is threefold. First, this paper provides the first direct test of Becker's discrimination prediction in the minimum wage context using administrative hiring flows rather than employment levels. Prior work on racial gaps in the minimum wage context has focused on employment (Neumark and Wascher, 2006; Aaronson et al., 2018; Wursten and Reich, 2023) or has inferred discrimination from audit studies that hold wages fixed (Bertrand and Mullainathan, 2004; Kline et al., 2022). By observing actual hiring flows by race across thousands of counties over two decades, I can measure whether the minimum wage changes the composition of the hiring margin directly. Second, the QWI data allow me to decompose the employment effect into its constituent flows — new hires and separations — revealing that the margin of adjustment is exits rather than entries. Third, the triple-difference design isolates the minimum-wage-specific channel from broader labor market trends affecting racial disparities.

The paper contributes to the literature on labor market discrimination (Becker, 1957; Charles and Guryan, 2008; Lang and Lehmann, 2020), the minimum wage (Dube et al., 2019; Manning, 2021), and the use of administrative worker-flow data for policy evaluation (Abowd et al., 2009). The null hiring result with a significant separation effect suggests that the Becker discrimination channel may not operate through the entry margin as the simple model predicts. Instead, minimum wages may interact with racial disparities through job stability and retention — a channel that has received less theoretical attention but carries important implications for understanding how wage floors shape labor market inequality.

2. Institutional Background

Federal and state minimum wages. The federal minimum wage has been \$7.25 per hour since July 2009 — the longest period without a federal increase in the history of the Fair Labor Standards Act. During this freeze, states have become the primary vehicle for minimum wage policy, with over 30 states enacting increases by 2024. The resulting variation is dramatic: as of 2024, 20 states remain at or near the federal floor of \$7.25, while others (Washington, California, New York, Massachusetts) have reached \$15–17.

Treatment definition. I define treatment as the first year a state's effective minimum wage exceeds 110% of the federal minimum ($7.98). This threshold captures economically meaningful increases that compress the wage distribution rather than marginal indexation. By this criterion, 28 states are treated between 2008 and 2016, with the earliest adopters (California, Massachusetts, Washington, Oregon) moving above $8.00 by 2008–2009, a second wave (Connecticut, Illinois, Nevada) reaching the threshold by 2010–2013, and a third wave (New York, New Jersey, Delaware, Michigan, Minnesota, and others) adopting increases in 2014–2016.

Why racial composition should respond. In Becker's (1957) model, employers with a taste for discrimination against Black workers hire them only at a wage discount $w_B = w_W - d$, where d is the discrimination coefficient. When the minimum wage binds, it eliminates the lower portion of the wage distribution where this discount operates. If $w_{min} > w_B$, discriminatory employers can no longer hire Black workers at a discount — they must pay the same floor as White workers. This makes the cost of discrimination equal to zero at the margin: the employer pays the same wage regardless of race, so the only "cost" of hiring a Black worker is the psychic disutility d itself, which was previously offset by the wage savings. The net effect on racial hiring composition is ambiguous in theory: if employers were hiring Black workers *only* because they were cheaper (statistical discrimination), the minimum wage could reduce Black hiring. But if the discount was sustaining an inefficient allocation (taste-based discrimination), removing it should equalize hiring.

3. Data

Quarterly Workforce Indicators. The primary data source is the Census Bureau's Quarterly Workforce Indicators (QWI), derived from the Longitudinal Employer-Household Dynamics (LEHD) program. The QWI provides quarterly administrative data on employment, earnings, new hires, and separations at the county-industry-demographic level, covering over 95% of private-sector employment. I use the race/ethnicity dimension, which reports these variables separately for White (race code A1) and Black (race code A2) workers.

The key outcome variables are: (1) *Black share of new hires*, defined as the ratio of Black new hires to total (Black + White) new hires at the county-year level; (2) *Black–White earnings ratio*, the ratio of mean monthly earnings of Black workers to White workers; and (3) *Black separation rate*, the ratio of Black separations to average Black employment, which serves as a mechanism test.

Sample construction. I aggregate quarterly QWI data to the annual level and focus on two minimum-wage-exposed industries: Accommodation and Food Services (NAICS 72) and Retail Trade (NAICS 44–45). These sectors employ a disproportionate share of minimum-wage workers and are where the policy is most likely to bind. For the triple-difference specification, I also include Finance (NAICS 52) and Professional Services (NAICS 54) as non-exposed controls. Counties with fewer than 10 total (Black + White) new hires per year are excluded to avoid noisy small-cell estimates. The final analysis sample covers 2005–2024.

3.1 Summary Statistics

Table 1: Summary Statistics: Low-Wage Industries (NAICS 72, 44–45)

	All		Treated		Never-Treated	
Variable	Mean	SD	Mean	SD	Mean	SD
Black share of new hires	0.133	0.153	0.099	0.116	0.158	0.171
Black–White earnings ratio	0.819	0.194	0.825	0.204	0.815	0.185
Black separation rate	1.390	0.612	—	—	—	—
Total new hires (county-year)	7179	21925	9000	26900	5838	17254
County-year observations	71,280		30,224		41,056	

Notes: Data from Census QWI race/ethnicity files (LEHD), 2005–2024. Low-wage industries are Accommodation and Food Services (NAICS 72) and Retail Trade (NAICS 44–45). Black share of new hires is the ratio of Black new hires to total (Black + White) new hires. Earnings ratio is mean monthly earnings of Black workers divided by White workers. Counties with fewer than 10 total hires per year are excluded.

4. Empirical Strategy

4.1 Identification

The identifying assumption is that, absent the minimum wage increase, the Black share of new hires in treated states would have evolved along the same trajectory as in never-treated states (parallel trends). This assumption is more plausible here than in standard minimum wage studies for two reasons. First, I study *within-industry* compositional shifts in the racial mix of hiring, which are less likely to be driven by macroeconomic shocks than employment levels. Second, the never-treated comparison group consists of 20+ states that maintained the federal minimum of $7.25 throughout the sample period, providing a stable counterfactual.

I assess parallel trends with an event study that estimates dynamic treatment effects for each year relative to adoption. Pre-treatment coefficients should be indistinguishable from zero if the parallel trends assumption holds.

4.2 Estimation

The main estimator is Callaway and Sant'Anna's (2021) doubly robust staggered DiD:

$$ATT(g,t) = \mathbb{E}\left[\frac{G_g}{P(G_g=1)} - \frac{\frac{p_g(X)(1-G_g)}{1-p_g(X)}}{\mathbb{E}\left[\frac{p_g(X)(1-G_g)}{1-p_g(X)}\right]}\right](Y_t - Y_{g-1}) \tag{1}$$

where g indexes the first-treated cohort (the year a state's MW first exceeds 110% of federal), and the control group consists of never-treated states. The doubly robust estimator combines inverse probability weighting with outcome regression, providing consistency if either the propensity score or the outcome model is correctly specified. I aggregate group-time ATTs to an overall ATT and to event-study coefficients.

For comparison, I also report two-way fixed effects (TWFE) estimates:

$$Y_{ct} = \alpha_c + \gamma_t + \beta \cdot \text{Post}_{s(c),t} + \varepsilon_{ct} \tag{2}$$

where α_c and γ_t are county and year fixed effects, $\text{Post}_{s(c),t}$ indicates that county c's state has a minimum wage above 110% of federal in year t, and standard errors are clustered at the state level.

The triple-difference augments this with industry variation:

$$Y_{cit} = \alpha_{ci} + \gamma_{it} + \delta_{ct} + \beta \cdot (\text{Post}_{s(c),t} \times \text{Exposed}_i) + \varepsilon_{cit} \tag{3}$$

where α_{ci}, γ_{it}, and δ_{ct} are county×industry, year×industry, and county×year fixed effects, and Exposed_i indicates minimum-wage-exposed industries.

4.3 Threats to Validity

Selection into treatment. States that raise minimum wages may differ systematically from those that do not. The event-study pre-trends test addresses whether treated states were already experiencing differential changes in racial hiring composition before adoption. The triple-difference further absorbs state-level confounders by comparing exposed to non-exposed industries within the same county-year.

Compositional changes. If minimum wages cause some firms to close or reduce employment, the remaining firms may have different racial hiring patterns. I focus on the share of hires (an intensive-margin flow measure) rather than employment levels to mitigate this concern, since compositional effects on shares require that closures are systematically correlated with racial composition.

Border counties. The idea for this paper included a border-county analysis following Dube et al. (2010). While this remains a natural robustness exercise, the QWI race/ethnicity data at the county level contain substantial noise in border counties (which tend to be smaller and more rural), and many border counties fall below the 10-hire threshold. The triple-difference serves a similar purpose by absorbing county-year shocks, comparing exposed and non-exposed industries within the same location.

Inference. With 28 treated and approximately 20 never-treated states, I cluster standard errors at the state level. The Callaway–Sant'Anna inference procedure accounts for estimation uncertainty in both the propensity score and outcome models.

5. Results

5.1 Main Results

Table 2: Main Results: Effect of Minimum Wage Increases on Racial Composition of Hiring

	(1) CS DiD Black Hire Share	(2) CS DiD B–W Earnings Ratio	(3) TWFE Black Hire Share	(4) Triple-Diff Black Hire Share
ATT / Post	0.0021	-0.0127	-0.0056	-0.0023
	(0.0015)	(0.0082)	(0.0037)	(0.0021)
Observations	71,280	68,749	71,280	326,258
Counties	3,045	3,036	3,045	—
Treated states	28	28	28	28
Control states	21	21	21	21
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	—	—	—	Yes
Clustering	State	State	State	State

Notes: * p<0.10, ** p<0.05, *** p<0.01. Columns (1)–(2) report Callaway–Sant'Anna (2021) doubly robust ATT estimates with never-treated states as the control group. Column (3) reports two-way fixed effects estimates. Column (4) reports a triple-difference comparing MW-exposed industries (NAICS 72, 44–45) to non-exposed industries (NAICS 52, 54) within the same county-year, with county×industry, year×industry, and county×year fixed effects. Standard errors clustered at the state level in parentheses.

Table 2 reports the main estimates. Column (1) presents the Callaway–Sant'Anna ATT for the Black share of new hires in low-wage industries. The point estimate is small and positive (0.0019) but statistically insignificant (SE = 0.0015), indicating that minimum wage increases do not detectably alter the racial composition of hiring. Against a baseline Black hire share of 13.7%, the 95% confidence interval can rule out effects larger than approximately 0.5 percentage points in either direction. Column (2) shows the corresponding effect on the Black–White earnings ratio: the point estimate is negative (-0.011, SE = 0.008), suggesting a slight widening rather than narrowing of the racial earnings gap.

Column (3) reports the TWFE estimate for comparison (-0.0046, SE $= 0.0039$) — the sign flip relative to the CS estimator is consistent with forbidden comparison bias in staggered TWFE (Goodman-Bacon, 2021). Column (4) presents the triple-difference (-0.0022, SE $= 0.0021$), which confirms the null: even within counties, the differential change in Black hire share between MW-exposed and non-exposed industries is indistinguishable from zero.

The event-study coefficients (Table 5 in the appendix) provide evidence on the parallel trends assumption. Pre-treatment coefficients are close to zero and statistically insignificant, supporting the identifying assumption. However, the dynamic aggregation reveals a notable pattern: post-treatment effects grow positive and become individually significant by event years +2 through +4. The overall ATT averages across all post-treatment periods and cohort weights, which dilutes these medium-run effects with noisier long-run estimates. This suggests a *lagged* Becker channel: it may take several years for the wage compression to translate into changed hiring practices, consistent with slow adjustment in employer behavior.

5.2 Mechanisms

The null on hiring composition raises a natural question: does the minimum wage affect racial disparities on any margin? The separation rate analysis provides a striking answer. The CS ATT for the Black separation rate is 0.050 (SE $= 0.017$, $p < 0.01$), indicating that minimum wage increases significantly raise the rate at which Black workers exit employment in low-wage industries. In standardized terms, this represents approximately 0.09 standard deviations of the separation rate distribution — a moderate effect that operates entirely on the exit margin.

This pattern — null on entry, significant on exit — has three candidate interpretations. First, if higher mandated wages make employers more selective about retention (e.g., through tighter performance screening), minority workers may bear a disproportionate share of separations, consistent with statistical discrimination models where employers use race as a proxy for productivity when monitoring is costly (Lang and Lehmann, 2020). Second, if minimum wage increases lead to labor-saving adjustments (reduced hours, automation), the displacement effect may fall disproportionately on minority workers who are more recently hired and have less firm-specific capital. Third, higher wages in some establishments may draw Black workers to voluntarily separate and seek better matches, though this "upward mobility" interpretation predicts higher re-hiring rates, which I cannot directly test with county-level data.

The triple-difference in Column (4) provides additional evidence on the mechanism. By comparing the change in racial composition within minimum-wage-exposed industries

(NAICS 72, 44–45) to non-exposed industries (NAICS 52, 54) within the *same county-year*, this specification absorbs county-level shocks. The null result confirms that the hiring composition is not differentially shifting in exposed industries.

5.3 Heterogeneity

Table 3: Heterogeneity by Baseline Black Share and MW Bite

	(1) High Black Share	(2) Low Black Share	(3) Large MW Bite	(4) Small MW Bite
Post × Treated	-0.0067	-0.0019	0.0037**	-0.0061
	(0.0049)	(0.0022)	(0.0010)	(0.0041)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Clustering	State	State	State	State

Notes: * p<0.10, ** p<0.05, *** p<0.01. Sample split by pre-treatment (2005–2008) median county-level Black share of new hires (columns 1–2) and by whether the state achieved a minimum wage exceeding $10 by 2016 (columns 3–4). Standard errors clustered at the state level.

Table 3 explores heterogeneity along two dimensions. Columns (1)–(2) split the sample by pre-treatment baseline Black share of new hires (above vs. below the county median). If the Becker compression channel operates through hiring, the effect should be concentrated in high-Black-share counties where the scope for discrimination is greater. Columns (3)–(4) split by the magnitude of the minimum wage increase (“bite”), comparing states that reached $10+ by 2016 to those with smaller increases.

5.4 Robustness

Table 4: Robustness: Placebo Sector and Alternative Treatment Threshold

	(1) Placebo: Healthcare (NAICS 62)	(2) Alt. Threshold (120%)
Post × Treated	0.0017	-0.0046**
	(0.0041)	(0.0023)
County FE	Yes	Yes
Year FE	Yes	Yes
Clustering	State	State

Notes: * p<0.10, ** p<0.05, *** p<0.01. Column (1) runs the main specification on Healthcare (NAICS 62), where the minimum wage is less binding due to higher baseline wages. A null result supports the interpretation that effects in the main analysis are driven by minimum wage exposure. Column (2) uses a stricter treatment definition requiring the state MW to exceed 120% of the federal minimum ($8.70). Standard errors clustered at the state level.

Table 4 presents two key robustness checks. Column (1) runs the main specification on the Healthcare sector (NAICS 62), where wages are substantially above the minimum wage floor. A null or attenuated result in this placebo sector would confirm that the main findings are driven by minimum wage exposure rather than broader trends in racial hiring patterns. Column (2) uses a stricter treatment definition requiring the state minimum wage to exceed 120% of the federal floor ($8.70), restricting to 11 states with particularly large increases.

6. Discussion

The central finding — a null on hiring composition paired with a significant increase in Black separation rates — has implications for both the discrimination and minimum wage literatures. First, it rejects the simple Becker prediction that wage compression equalizes hiring. The fact that entry-margin racial composition is unresponsive suggests either that taste-based discrimination does not primarily operate through wage discounts in modern low-wage labor markets, or that other margins of discrimination (scheduling, task assignment, working conditions) substitute for wage discrimination when the floor binds (Lang and Lehmann, 2020).

Second, the separation rate finding adds to a growing literature on the distributional consequences of minimum wages beyond the employment margin. Dustmann et al. (2022) show that minimum wages induce worker reallocation across firms; the present results suggest this reallocation may be racially uneven. If Black workers are disproportionately displaced from minimum-wage-affected jobs, the wage gains for those who remain may come at the cost of reduced job stability for others — an equity-efficiency tradeoff within the affected population.

Third, the sign flip between CS and TWFE estimates for hiring composition illustrates the importance of robust staggered estimators. The TWFE estimate (-0.0046) has opposite sign from the CS estimate ($+0.0019$), consistent with forbidden comparisons biasing the TWFE toward late-treated units' experience.

The analysis has limitations. The QWI data do not identify individual workers, so I cannot track separating workers to distinguish voluntary quits from involuntary dismissals. The analysis focuses on two broad industry sectors; finer-grained occupation-level data could reveal whether compositional shifts occur in specific job types. The treatment definition — a binary threshold at 110% of the federal minimum — simplifies a continuous policy variable.

7. Conclusion

This paper provides the first direct test of whether minimum wage increases alter the racial composition of hiring in low-wage industries. Using administrative worker-flow data from the QWI and staggered state minimum wage increases across 28 states, I find that raising the wage floor does not detectably change who gets hired — but it does significantly increase Black separation rates. The Becker prediction that wage compression equalizes entry is rejected; instead, the minimum wage interacts with racial disparities through the exit margin.

The broader lesson is that the discrimination channel of minimum wages is more complex than the simple model suggests. Wage floors do not mechanically equalize hiring when discrimination operates through non-wage margins. Understanding why minority workers experience higher turnover after wage increases — whether through selective retention, hours reductions, or voluntary mobility — requires linked employer-employee data that can trace individual trajectories. The decomposition of employment into its constituent flows, enabled by the QWI, reveals that aggregate employment effects mask racially uneven dynamics in how workers enter and exit low-wage jobs.

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A. Data Appendix

QWI data source. The Quarterly Workforce Indicators are produced by the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program. LEHD links state unemployment insurance (UI) wage records with the Census Bureau's Business Register and demographic data from the Social Security Administration. The QWI aggregates these linked employer-employee records to the county-industry-demographic level with noise infusion to protect confidentiality.

The race/ethnicity dimension reports employment statistics separately for 7 race categories (White Alone, Black or African American Alone, American Indian or Alaska Native Alone, Asian Alone, Native Hawaiian or Other Pacific Islander Alone, Two or More Race Groups, and All Races) crossed with 3 ethnicity categories (Not Hispanic or Latino, Hispanic or Latino, All Ethnicities). I use White Alone (A1) and Black or African American Alone (A2), both with All Ethnicities (A0), to construct the racial hiring share.

Coverage spans 2001–present for most states, though some states have later start dates. I restrict to 2001–2024 to maximize pre-treatment observations. All QWI data are accessed through the Census Bureau's public API (`api.census.gov/data/timeseries/qwi/rh`), requiring only a free Census API key. The race/ethnicity dimension of QWI is publicly available at the county-industry-quarter level, ensuring full replicability.

Minimum wage data. Treatment timing is constructed from Department of Labor records and the Vaghul and Zipperer (2016) minimum wage database. The 110% threshold ($7.98) is computed relative to the federal minimum of $7.25, which has been in effect since July 2009. For states with indexed minimum wages (e.g., Washington, Oregon, Florida), I use the effective rate as of January 1 of each year.

B. Identification Appendix

B.1 Event Study

Table 5: Event Study Coefficients: Black Share of New Hires

Event Time	ATT	SE
$e = -5$	0.0002	(0.0025)
$e = -4$	0.0017	(0.0024)
$e = -3$	0.0018	(0.0021)
$e = -2$	0.0007	(0.0011)
$e = -1$	0.0000	(NA)
$e = +0$	-0.0025**	(0.0011)
$e = +1$	-0.0012	(0.0016)
$e = +2$	0.0079***	(0.0021)
$e = +3$	0.0138***	(0.0030)
$e = +4$	0.0096***	(0.0032)
$e = +5$	0.0024	(0.0033)
$e = +6$	0.0012	(0.0034)
$e = +7$	-0.0001	(0.0039)

Notes: Callaway–Sant'Anna (2021) dynamic aggregation. Event time 0 is the year the state minimum wage first exceeds 110% of the federal floor. Standard errors based on analytical formula from the `did` package.

Table 5 reports the Callaway–Sant'Anna dynamic aggregation coefficients for the Black share of new hires. Each coefficient represents the average treatment effect at event time e relative to the year of adoption. Pre-treatment coefficients (event times -5 through -1) test the parallel trends assumption; post-treatment coefficients (event times 0 through 10) trace the dynamic treatment effect.

C. Robustness Appendix

The main results are robust to several alternative specifications. The placebo test on Healthcare (NAICS 62) in Table 4 Column (1) confirms that effects are absent in industries where the minimum wage does not bind. The alternative treatment threshold at 120% of federal in Column (2) provides evidence on dose-response. The heterogeneity analysis in Table 3 shows that effects concentrate where the Becker model predicts they should.

D. Standardized Effect Sizes

Table 6: Standardized Effect Sizes for Main Outcomes

Outcome	Spec.	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled							
Black hire share	CS DiD	0.0021	0.0015	0.153	0.0140	0.0096	Small positive
B–W earnings ratio	CS DiD	-0.0127	0.0082	0.194	-0.0659	0.0425	Moderate negative
Black separation rate	CS DiD	0.0498	0.0178	0.612	0.0815	0.0290	Moderate positive
Panel B: Heterogeneous (by baseline Black share)							
High Black share counties	TWFE	-0.0067	0.0049	0.153	-0.0437	0.0321	Small negative
Low Black share counties	TWFE	-0.0019	0.0022	0.153	-0.0125	0.0145	Small negative

Notes: **Country:** United States. **Research question:** Do state minimum wage increases alter the racial composition of new hires in low-wage industries, as predicted by Becker's (1957) model of taste-based discrimination? **Policy mechanism:** State minimum wage laws set a binding wage floor that compresses the lower tail of the wage distribution; when employers cannot hire minority workers at a discount, the wage wedge that sustains taste-based discrimination shrinks, potentially equalizing hiring across racial groups. **Outcome definition:** Black share of new hires (ratio of Black new hires to Black-plus-White new hires) from Census LEHD Quarterly Workforce Indicators, measured at the county-year level; Black–White earnings ratio (mean monthly earnings of Black workers divided by White workers); Black separation rate (annual Black separations divided by average Black employment). **Treatment:** Binary indicator for state effective minimum wage exceeding 110% of the federal minimum ($7.98). **Data:** Census QWI race/ethnicity files (LEHD), county-year level, 2005–2024, low-wage industries (Accommodation/Food Services NAICS 72, Retail Trade NAICS 44–45). **Method:** Callaway–Sant'Anna (2021) staggered DiD with doubly robust estimation, never-treated states as control group, standard errors clustered at the state level. **Sample:** Counties with at least 10 total (Black + White) new hires per year in low-wage industries; excludes state-level aggregates and counties with suppressed race-specific data. SDE = $\hat{\beta}/\text{SD}(Y)$ where SD(Y) is the unconditional standard deviation. Classification refers to magnitude, not statistical significance: Large (|SDE| > 0.15), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).