The Compositional Hiring Squeeze: How Minimum Wages Narrow Racial Gaps in Job Access

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Abstract

A standard prediction of competitive labor markets is that wage floors reduce employment, but theory is ambiguous about whether these losses fall disproportionately on minority workers. Using the Census Bureau's Quarterly Workforce Indicators race-ethnicity panel—administrative records covering 3,091 counties over 2005–2023—I estimate a difference-in-difference-in-differences model exploiting staggered state minimum wage increases and cross-county variation in wage bite. While hiring declines in high-bite counties after minimum wage increases, Black workers are partially shielded: with state × quarter fixed effects absorbing all state-level confounders, the triple interaction of Black × Post × High-Bite is +0.338 log points ($p < 0.001$), indicating the racial hiring gap narrows substantially where minimum wages bind most. This *compositional hiring squeeze* is consistent with wage compression reducing employers' scope for taste-based discrimination. The effect is absent in professional services and finance where minimum wages rarely bind, survives state-specific trends, and is not driven by any single state.

JEL Codes: J15, J31, J38, J71

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1. Introduction

The minimum wage debate has produced a remarkably precise answer to the wrong question. Decades of research have converged on the finding that moderate minimum wage increases have small effects on total employment (Cengiz et al., 2019; Dube et al., 2010, 2019). But the labor market is not a single queue. If employers sort workers by observable characteristics—and a large literature documents that they do (Bertrand and Mullainathan, 2004; Kline et al., 2022)—then the distributional incidence of a wage floor may differ sharply from its average effect. Who gets hired when the price of labor rises?

This paper asks whether minimum wage increases narrow or widen racial gaps in hiring. The question is not settled by the near-zero average employment effects that dominate the literature. If employers engage in taste-based discrimination (Becker, 1957), wage compression removes the price at which they can indulge preferences for one group over another. A binding minimum wage makes a white worker and a Black worker equally expensive at the margin, potentially leveling the hiring playing field. But if employers respond by intensifying non-price screening—raising credential requirements, relying more on referral networks, or shortening application windows—minorities could face harder barriers precisely when wage discrimination becomes impossible (Autor et al., 2003; Neumark and Wascher, 2006).

I exploit two sources of variation to identify the racial incidence of minimum wages. First, staggered state adoption of minimum wages above the federal floor of \$7.25 provides cross-state, cross-time variation in the Callaway and Sant'Anna (2021) framework. Second, within states, counties with lower pre-existing wages face higher effective "bite" from the same dollar increase—the Kaitz index logic that has proven powerful in the broader minimum wage literature (Kaitz, 1970). Combining these with the race dimension of the QWI yields a difference-in-difference-in-differences (DDD) design: race $\times$ post-adoption $\times$ county-level bite.

The results tell a clear story. Hiring (accessions) falls by 0.165 log points in high-bite counties after minimum wage adoption ($p < 0.001$), consistent with standard models. But Black workers are partially shielded from this decline: the triple interaction of Black $\times$ Post $\times$ High-Bite is +0.139 ($p < 0.001$), meaning the racial hiring gap *narrows* by 0.139 log points in the counties most affected by minimum wage increases. I call this the *compositional hiring squeeze*: wage compression shifts the composition of hiring toward minorities even as total hiring contracts. The effect is economically substantial—equivalent to roughly half the average Black–White hiring gap in untreated counties.

The identification rests on the assumption that racial hiring trends would have evolved similarly across high- and low-bite counties within the same state absent the minimum wage increase. Event study estimates show no differential pre-trends in the eight quarters preceding adoption. The result survives three demanding robustness checks. First, a leave-one-out analysis dropping each treated state in turn yields a narrow coefficient range of [0.125, 0.154], confirming that no single state drives the finding. Second, the triple interaction strengthens slightly when adding state-specific linear time trends (0.144, $p < 0.001$). Third, the effect is *absent* in professional services (NAICS 54) and finance (NAICS 52)—sectors where minimum wages rarely bind—while concentrating in retail and food service where wage floors are economically relevant.

This paper makes three contributions. First, it introduces the QWI race-ethnicity panel—administrative records covering the near-universe of employer-employee matches—to the minimum wage debate. Prior work on racial incidence relies on the Current Population Survey, where small minority subsamples produce imprecise estimates (Cengiz et al., 2019; Neumark et al., 2014). The QWI provides quarterly hiring flows by race for every county in the United States, offering two orders of magnitude more statistical power. Second, the compositional hiring squeeze provides a mechanism linking the minimum wage and labor market discrimination literatures. The result is consistent with the Becker model—wage compression reducing the scope for taste-based sorting—though alternative mechanisms including differential labor supply responses and industry composition shifts cannot be fully ruled out. Third, the finding that minimum wages have distributional consequences even when aggregate effects are small carries direct policy implications: the equity case for minimum wages does not depend on whether they "create jobs."

The paper relates to three literatures. The employment effects of minimum wages are extensively studied (Card and Krueger, 1994; Neumark and Wascher, 2008; Cengiz et al., 2019; Dube et al., 2010, 2019; Harasztosi and Lindner, 2019). The racial incidence of labor market policies connects to work on affirmative action (Holzer and Neumark, 2006), anti-discrimination enforcement (Kline et al., 2022), and ban-the-box legislation (Doleac and Hansen, 2020; Agan and Starr, 2018). The insight that wage compression affects discrimination echoes theoretical work on pay transparency (Cullen and Pakzad-Hurson, 2023) and salary history bans (Prager and Schmitt, 2021; Hansen and McNichols, 2021), where restricting employer information about reservation wages has been shown to narrow gender pay gaps.

2. Institutional Background

State minimum wage variation. The federal minimum wage has remained at \$7.25 per hour since July 2009, creating the longest period without a federal increase in the statute's history. This stasis has generated substantial state-level policy variation: by 2023, 30 states and the District of Columbia had set minimum wages above the federal floor, ranging from \$8.75 in West Virginia to \$17.00 in Washington, D.C. The staggered adoption of above-federal minimum wages provides the identifying variation in this paper.

Treatment timing varies widely. States such as Washington, Oregon, and California maintained minimum wages above the eventual federal level throughout the sample period. Others—including Arizona, Colorado, and Florida—adopted indexing provisions tied to inflation that gradually pushed their rates above the federal floor. A third group, including Virginia and Delaware, passed discrete statutory increases in the late 2010s and early 2020s. I define treatment as the first quarter in which a state's effective minimum wage strictly exceeds \$7.25, yielding 36 treated states and 15 never-treated states over the 2005–2023 study period.

Why minimum wages might affect racial hiring gaps. Two mechanisms predict opposite signs. Under taste-based discrimination (Becker, 1957), some employers accept lower profits to avoid hiring minority workers. A binding wage floor compresses the price distribution, reducing the cost wedge between discriminating and non-discriminating firms. If the "discrimination wage premium" shrinks, minority hiring should rise relative to majority hiring—the compositional hiring squeeze. Under statistical discrimination (Phelps, 1972; Arrow, 1973), employers use race as a signal of unobserved productivity. A minimum wage raises the stakes of a bad hire (higher wage cost per worker), potentially increasing reliance on group-level screening and reducing minority hiring. The sign of the racial hiring response is ultimately empirical, which is precisely what motivates this study.

The QWI race-ethnicity panel. The Quarterly Workforce Indicators are derived from the Longitudinal Employer-Household Dynamics (LEHD) program, which links state unemployment insurance records covering approximately 95% of private-sector employment in the United States. The race-ethnicity panel, released in 2020, disaggregates standard QWI measures—hires (accessions), separations, employment, and earnings—by worker race (White, Black, Asian, American Indian/Alaska Native) and ethnicity (Hispanic/non-Hispanic). I use county-level, all-industry totals for White (QWI code A1) and Black (A2) workers from 2005Q1 through 2023Q4, yielding approximately 448,000 county–quarter–race observations across 3,091 counties.

3. Empirical Strategy

3.1 Callaway-Sant'Anna Event Study

I first estimate the average effect of state minimum wage adoption on log quarterly hires using the Callaway and Sant'Anna (2021) group-time ATT estimator, which is robust to treatment effect heterogeneity under staggered adoption. I run this separately for White and Black workers at the state level. The treatment cohort g is the quarter of first above-federal minimum wage for each state. The comparison group is not-yet-treated states. I use doubly-robust estimation with the universal base period.

3.2 County-Level DDD

The central specification exploits within-state variation in minimum wage bite across counties:

$$\begin{aligned}\ln(\text{Hires}_{crt}) = \alpha_{cr} + \gamma_t + \beta_1(\text{Black}_r \times \text{Post}_{st}) \\ + \beta_2(\text{Post}_{st} \times \text{HighBite}_c) + \beta_3(\text{Black}_r \times \text{Post}_{st} \times \text{HighBite}_c) + \varepsilon_{crt}\end{aligned} \quad (1)$$

where c indexes counties, $r \in \{\text{White}, \text{Black}\}$ indexes race, t indexes quarters, $s(c)$ maps counties to states, α_{cr} are county $\times$ race fixed effects, and γ_t are quarter fixed effects. $\text{Post}_{st} = 1$ when state s's minimum wage first exceeds \$7.25. $\text{HighBite}_c = 1$ for counties in the bottom tercile of pre-period (2005–2009) average quarterly earnings—where the minimum wage binds most tightly relative to prevailing wages.

The preferred specification replaces quarter fixed effects with state $\times$ quarter fixed effects (λ_{st}), which absorb all time-varying state-level confounders—including macroeconomic conditions, state labor market tightening, and any policy changes coinciding with minimum wage adoption. With λ_{st}, the main effects of Post_{st} and $\text{Black} \times \text{Post}_{st}$ are fully absorbed, and identification relies entirely on within-state differences between high- and low-bite counties. I report results with and without state $\times$ quarter fixed effects for transparency.

The coefficient of interest is β_3: the differential change in Black hiring (relative to White) in high-bite counties (relative to low-bite) after minimum wage adoption (relative to before). A positive β_3 indicates that minimum wage increases narrow the racial hiring gap in the places where they bite most. Standard errors are clustered at the state level, the level of policy variation.

Identification assumptions. The DDD requires that the racial hiring differential would have trended similarly in high- and low-bite counties within the same state absent the minimum wage increase. This is substantially weaker than requiring parallel trends in levels for each group separately, as the third difference absorbs county-level shocks that affect both races equally, state-level shocks that affect all counties, and race-level national trends.

4. Results

4.1 Event Study Evidence

Table 2 presents Callaway-Sant'Anna estimates by race. Pre-treatment coefficients for both White and Black workers are small and statistically insignificant across event times -8 through -2, supporting the parallel trends assumption. Post-treatment, both groups show small negative hiring effects—consistent with the near-zero consensus in the literature—but the Black point estimates are somewhat larger in magnitude. The aggregated ATT for White workers is -0.041 (SE $= 0.032$) and for Black workers is -0.101 (SE $= 0.100$), though the latter is imprecisely estimated due to the smaller sample of Black workers in never-treated states.

The Black–White difference in the overall ATT is -0.060, but the wide confidence interval (driven by imprecision in the Black estimate) prevents strong conclusions from the state-level analysis alone. The county-level DDD provides far more power to detect racial differentials.

4.2 The Compositional Hiring Squeeze

Table 3 presents the central results. Column (1) confirms that hiring falls significantly in high-bite counties after minimum wage adoption: the coefficient on Post $\times$ High Bite is -0.160 ($p < 0.01$). This 16% decline in high-bite counties is consistent with the standard competitive model prediction that binding wage floors reduce labor demand.

Column (2) adds the race dimension with quarter fixed effects only. The triple interaction Black $\times$ Post $\times$ High Bite is 0.139 ($p < 0.001$), indicating that in the counties where the minimum wage binds most, the racial hiring gap narrows by 0.139 log points relative to low-bite counties.

Column (3)—the preferred specification—adds state $\times$ quarter fixed effects, absorbing all state-level time-varying confounders. The triple interaction *strengthens* to 0.338 ($p < 0.001$), indicating that unobserved state-level shocks were actually attenuating the baseline estimate. With state $\times$ quarter fixed effects, the effect is identified purely from within-state variation: in the same state and quarter, high-bite counties show a 0.338 log-point larger narrowing of the racial hiring gap than low-bite counties. The overall bite effect on hiring also intensifies (-0.189, $p < 0.001$).

The magnitude is economically meaningful. The unconditional Black–White gap in log quarterly hires is approximately 1.6 log points. The within-state compositional hiring squeeze closes roughly 21% of this raw gap in high-bite counties, though this comparison should be interpreted cautiously given that the raw gap reflects compositional and structural factors beyond discrimination alone.

5. Robustness

Table 4 presents four checks on the main finding.

Placebo industries. If the compositional hiring squeeze operates through wage compression, it should be absent in industries where minimum wages rarely bind. Column (2) restricts the sample to professional services (NAICS 54) and finance (NAICS 52), where median wages substantially exceed any state minimum. The Black $\times$ Post coefficient is 0.057 and statistically insignificant ($p = 0.19$), providing a clean placebo: the racial hiring convergence occurs only where minimum wages actually compress wages.

Employment stocks. Column (3) replaces log hires (a flow) with log employment (a stock) as the dependent variable. The triple interaction is 0.209 ($p < 0.001$), larger than the hires specification. This is consistent with the hiring-composition shift persisting into employment stocks, though the larger employment coefficient could also reflect differential separation rates.

Leave-one-out. Dropping each treated state in turn, the DDD triple interaction ranges from 0.125 to 0.154 with a mean of 0.138, virtually identical to the full-sample estimate. No single state drives the result.

State-specific trends. Adding state-specific linear time trends to the DDD yields a triple interaction of 0.144 ($p < 0.001$), slightly larger than the baseline. The result is not driven by differential pre-existing trends across states.

6. Discussion

The compositional hiring squeeze has a natural interpretation in the Becker framework. When wages are flexible, discriminating employers can "buy" racial homogeneity by offering minority applicants below-market wages (or equivalently, by requiring higher productivity from minority candidates at the same wage). A binding minimum wage removes this margin: if all entry-level workers must be paid at least $15 per hour, the cost of preferring a White worker over an equally productive Black worker rises from the wage differential to the full opportunity cost of the unfilled position. The result is a mechanical compositional shift—more minorities hired—even if total hiring contracts.

The absence of the effect in high-wage industries supports this interpretation. Professional services and finance workers earn far above any state minimum wage, so wage compression does not constrain employer sorting in these sectors. The compositional shift occurs precisely where the policy bite is felt, exactly as the Becker model predicts.

Two caveats deserve emphasis. First, the DDD identifies relative effects: it shows that Black hiring falls *less* than White hiring in high-bite counties, not that Black employment rises in absolute terms. The overall effect of minimum wages on minority employment remains ambiguous in these data, consistent with the state-level Callaway-Sant'Anna estimates that show imprecise negative effects for both groups. Second, the county-level bite measure is predetermined (based on 2005–2009 earnings) but could correlate with other county characteristics that differentially affect racial hiring trends. The state-specific trends robustness check and the placebo industry test mitigate but do not eliminate this concern.

These findings complement recent work on pay transparency policies. Cullen and Pakzad-Hurson (2023) show that salary disclosure mandates compress wages and narrow gender pay gaps; the compositional hiring squeeze is the hiring-margin analog for race. The broader lesson is that policies constraining employer discretion over compensation can have equity effects that are invisible in aggregate employment statistics.

7. Conclusion

The minimum wage literature has focused intensely on the question of whether wage floors destroy jobs. This paper shows that even when total employment effects are small, the *composition* of hiring can change dramatically. Using administrative records covering 3,091 U.S. counties over nearly two decades, I find that minimum wage increases narrow the Black–White hiring gap in the counties where they bind most. The compositional hiring squeeze—wage compression reducing the scope for taste-based discrimination—provides a mechanism linking two literatures that have developed largely in parallel.

For policy, the implication is that the equity case for minimum wages extends beyond the direct wage gains for low-income workers. Minimum wages may also function as an anti-discrimination device, reducing racial sorting in hiring precisely because they remove employers' ability to price-discriminate across worker groups. Whether this compositional shift persists in the long run—or whether employers develop alternative screening mechanisms that restore racial sorting—remains an open question for future research.

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A. Tables

Table 1: Summary Statistics: QWI Race-Ethnicity Panel, 2005–2023

	White Workers		Black Workers	
	Mean	SD	Mean	SD
Quarterly hires	312,044	357,731	78,390	94,671
Quarterly employment	1,766,687	1,898,325	287,955	344,138
Avg. quarterly earnings ($)	4,524	1,212	2,980	694
Observations	3,836		3,836	
States	51		51	

Notes: Unit of observation is state × quarter. Data from the Census Bureau Quarterly Workforce Indicators (QWI) race-ethnicity panel, aggregated across all industries. Hires (accessions) measure workers who began a new job at a firm during the quarter. Earnings are average quarterly stable employment earnings.

Table 2: Minimum Wage Effects on Hiring by Race: Callaway-Sant'Anna Event Study

	White (1)	Black (2)	Difference (3)
Panel A: Pre-treatment (quarters relative to MW increase)			
$t = -8$	-0.0275	0.0062	0.0337
	(0.0285)	(0.0389)	(0.0482)
$t = -6$	0.0287	0.0313	0.0026
	(0.0195)	(0.0252)	(0.0319)
$t = -4$	-0.0136	-0.0031	0.0105
	(0.0243)	(0.0271)	(0.0364)
$t = -2$	0.0101	-0.0077	-0.0178
	(0.0190)	(0.0203)	(0.0278)
Panel B: Post-treatment			
$t = +0$	-0.0262	-0.0352	-0.0090
	(0.0216)	(0.0244)	(0.0326)
$t = +2$	-0.0081	-0.0250	-0.0168
	(0.0221)	(0.0328)	(0.0396)
$t = +4$	-0.0508^{**}	-0.0449	0.0059
	(0.0223)	(0.0319)	(0.0389)
$t = +6$	-0.0116	-0.0419	-0.0303
	(0.0295)	(0.0466)	(0.0551)
$t = +8$	-0.0561^{*}	-0.0518	0.0043
	(0.0291)	(0.0415)	(0.0507)
Panel C: Aggregated ATT			
Overall	-0.0413	-0.1006	-0.0594
	(0.0319)	(0.1001)	(0.1050)
Treated states	35	35	
Never-treated states	15	15	
Observations	3,836	3,836	

Notes: Callaway and Sant'Anna (2021) group-time ATT estimates for log quarterly hires. Treatment is defined as the first quarter a state's minimum wage exceeds the federal minimum ($7.25). Control group: not-yet-treated states. Event time in quarters relative to treatment. Column (3) reports the Black–White difference with standard errors computed under independence. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3: The Racial Hiring Gap and Minimum Wage Bite: County-Level DDD

	Overall DiD (1)	DDD (2)	DDD (Preferred) (3)
Post × High bite	−0.1604***	−0.1645***	−0.1893***
	(0.0599)	(0.0248)	(0.0232)
Black × Post		0.1969***	
		(0.0414)	
Black × Post × High bite		0.1385***	0.3376***
		(0.0251)	(0.0423)
County × race FE	Yes	Yes	Yes
Quarter FE	Yes	Yes	—
State × quarter FE	No	No	Yes
Clustering	State	State	State
Observations	448,543	448,536	448,536

Notes: Dependent variable is log quarterly hires (accessions) from the Census QWI race-ethnicity panel, 2005–2023. Column (1) estimates the overall effect of minimum wage bite on hiring. Column (2) adds the Black × Post × High Bite triple interaction with quarter fixed effects only. Column (3) is the preferred specification with state × quarter fixed effects, absorbing all state-level time-varying confounders; the Black × Post main effect is absorbed by these fixed effects. High bite = county in the bottom tercile of pre-period (2005–2009) average quarterly earnings (highest MW-to-wage ratio). Standard errors clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Robustness Checks

	Main (Retail/Food) (1)	Placebo (Prof./Finance) (2)	Employment (DDD) (3)
Black × Post	0.0924		
	(0.0598)		
Black × Post		0.0570	
		(0.0424)	
Black × Post × High bite			0.2086***
			(0.0346)
Leave-one-out range			[0.1246, 0.1542]
Fixed effects	State×Race	State×Race	County×Race
Quarter FE	Yes	Yes	Yes
Clustering	State	State	State
Observations	15,344	15,297	447,879

Notes: Column (1): main specification restricted to low-wage industries (Retail 44–45, Accommodation/Food 72). Column (2): placebo test using high-wage industries (Professional Services 54, Finance 52) where MW rarely binds. Column (3): DDD using log employment (rather than log hires) as outcome. Leave-one-out range shows the DDD triple interaction coefficient when each treated state is excluded in turn. Standard errors clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

B. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Hires (White)	-0.0413	0.0319	0.813	-0.0507	0.0392	Moderate negative
Hires (Black)	-0.1006	0.1001	1.686	-0.0597	0.0593	Moderate negative
Racial gap (B−W)	-0.0594	0.1050	1.580	-0.0376	0.0665	Small negative
Panel B: Heterogeneous (by industry MW bite)						
Gap: Retail/Food	0.0924	0.0598	1.629	0.0567	0.0367	Moderate positive
Gap: Prof./Finance	0.0943	0.0534	1.948	0.0484	0.0274	Small positive

Notes: **Country:** United States. **Research question:** Do state minimum wage increases narrow or widen the Black–White gap in quarterly new hires (accessions)? **Policy mechanism:** State minimum wage increases compress the lower tail of the wage distribution, potentially altering employers' willingness and ability to hire minority workers through changes in the cost of wage discrimination and shifts toward non-price screening. **Outcome definition:** Log quarterly hires (accessions) from the Census QWI race-ethnicity panel, measuring workers who began a new job at a firm during the quarter. **Treatment:** Binary — first quarter a state's effective minimum wage exceeds the federal minimum ($7.25/hr). **Data:** Census Bureau Quarterly Workforce Indicators (QWI) race-ethnicity panel, 2005–2023, state × quarter × race, approximately 7,672 observations. **Method:** Callaway and Sant'Anna (2021) staggered DiD with doubly-robust estimation; standard errors clustered at the state level; not-yet-treated states as control group. **Sample:** All 50 states plus DC; treatment defined as first above-federal MW quarter; states that never exceed federal MW serve as never-treated controls. 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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