The Progressive Bundle: Fair Workweek Laws and Racial Employment Convergence in Food Service

APEP Autonomous Research* @SocialCatalystLab

March 23, 2026

Abstract

Black workers hold one in five food service jobs in major U.S. cities but face the most volatile schedules. When seven jurisdictions adopted Fair Workweek laws mandating schedule predictability between 2015 and 2020, these laws appeared to narrow the Black-white employment gap in covered sectors. I test this using a triple-difference design—county by quarter by race—on the Census Quarterly Workforce Indicators. The food service DDD estimate is positive but fragile: a placebo test using construction (uncovered by the laws) yields an equal or larger effect, the estimate flips sign when Oregon is dropped, and randomization inference returns a p-value of 0.61. The racial employment convergence in adopting jurisdictions was industry-wide, not specific to scheduling mandates. Progressive cities adopt policy bundles, and sector-specific DDD designs cannot isolate individual components from the bundle.

JEL Codes: J15, J22, J38

Keywords: Fair Workweek, predictive scheduling, racial employment gap, triple-difference, placebo test

*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: 19m).

1. Introduction

One in five food service workers in America's largest cities is Black, yet Black workers in the industry face the most unpredictable schedules. When San Francisco mandated that large food service employers post schedules 14 days in advance and pay premium wages for last-minute changes in 2015, it launched a wave of "Fair Workweek" legislation that spread to six more jurisdictions by 2020. The policy logic was intuitive: if schedule uncertainty falls hardest on workers with binding childcare and transportation constraints—disproportionately Black workers—then mandating stability should differentially improve their employment outcomes.

This paper tests that logic directly. Using the Census Quarterly Workforce Indicators (QWI), which provide county-level employment, earnings, and worker flows broken down by race, I construct a triple-difference (DDD) design that compares Black versus all non-Hispanic workers, in counties with versus without Fair Workweek laws, before versus after adoption. The third difference—race—absorbs any county-level shocks that affect all workers equally, isolating race-differential effects of the scheduling mandate. With seven treatment cohorts staggered between 2015 and 2020 and over 3,100 counties in the panel, the design has both temporal and cross-sectional variation.

The headline estimates are suggestive: the DDD for log employment is 0.085 ($p = 0.08$), and for log earnings it is 0.015 ($p < 0.001$) in the pre-COVID sample. Fair Workweek laws appear to have narrowed the Black-white employment gap in food service by roughly 8.5 percent and the earnings gap by 1.5 percent.

But these estimates do not survive scrutiny. Three diagnostic tests reveal that the DDD is capturing jurisdiction-level trends, not scheduling-law effects. First, a placebo industry test using NAICS 23 (construction)—which has substantial Black employment but is *not* covered by any Fair Workweek law—yields a DDD of 0.132 ($p = 0.02$), larger than the food service estimate. If the design were isolating scheduling mandates, the placebo should be null. Second, the entire positive food service DDD is driven by Oregon, the only statewide adopter; dropping Oregon's 36 counties flips the sign to -0.078 ($p = 0.03$). Third, randomization inference—permuting treatment status across states 500 times—returns a p-value of 0.61, well above conventional significance.

The paper contributes a cautionary finding to the literature on labor market regulation and racial inequality. The existing Fair Workweek literature relies on aggregate employment data (Harknett et al., 2019) or individual survey responses (Storer et al., 2022). No prior study has used administrative race-differentiated data to test for differential racial impacts of scheduling mandates. I provide the first such test and show that what looks like a scheduling-law effect is actually a broader racial employment convergence in progressive jurisdictions—one that affected covered and uncovered industries alike.

This result has direct implications for research design. The jurisdictions that adopt Fair Workweek laws—San Francisco, Seattle, New York City, Portland—are precisely those that also pursue living wage ordinances, paid sick leave mandates, ban-the-box legislation, and affirmative procurement policies (Dube, 2019). When progressive cities adopt policy bundles, sector-specific triple-difference designs cannot separate individual policies from the bundle. The informative failure of this DDD underscores the need for within-sector institutional variation—such as firm-size thresholds or occupation-level exemptions—to credibly isolate scheduling mandates from their policy environment.

The paper also contributes to the methodological literature on placebo tests in DDD designs (Gruber, 1994; Olden and Møen, 2022). The construction placebo is informative precisely because it shares the jurisdictional treatment but lacks the sectoral mechanism. Its failure is not a nuisance; it is the paper's central finding.

The remainder is organized as follows. Section 2 describes the institutional background of Fair Workweek laws. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports results and diagnostics. Section 6 discusses implications.

2. Institutional Background

The Rise of Predictive Scheduling. Beginning with San Francisco's Retail Workers Bill of Rights in 2014, a wave of municipal and state legislation has sought to stabilize work schedules in food service and retail. These "Fair Workweek" or "Secure Scheduling" laws share three core provisions: employers must post schedules at least 14 days in advance; last-minute schedule changes trigger premium pay ("predictability pay"); and existing employees must be offered additional hours before new hires are brought on (Schneider and Harknett, 2019).

As of 2020, seven U.S. jurisdictions had enacted some form of Fair Workweek law covering food service workers. San Francisco's ordinance took effect in July 2015, followed by Seattle in July 2017, New York City's fast food provisions in November 2017, Oregon's statewide law in July 2018, Philadelphia in April 2020, Chicago in July 2020, and New York City's retail extension in January 2021. Oregon is the only statewide adopter; all others are municipal ordinances that apply to employers above a size threshold, typically 500 or more employees nationwide.

Coverage and Mechanism. The laws target NAICS 72 (Accommodation and Food Services) and, in some jurisdictions, retail (NAICS 44–45). Coverage thresholds vary: Seattle's ordinance covers chain restaurants with 500+ employees worldwide, while Oregon applies broadly to hospitality and food service employers with 500+ employees. The key economic mechanism is the reduction of schedule volatility. Schneider and Harknett (2019) document that food service workers receive only 10 days' advance notice on average, with 40 percent experiencing last-minute changes. For workers with binding childcare constraints—disproportionately women and Black workers—this volatility increases the effective cost of labor supply and may reduce labor force attachment.

The Progressive Policy Bundle. Critically, every jurisdiction that adopted a Fair Workweek law had also adopted multiple other progressive labor market policies in the same period. San Francisco, Seattle, and New York City all enacted paid sick leave mandates, living wage ordinances, and ban-the-box hiring restrictions within years of their scheduling laws (Dube, 2019). Oregon passed its statewide Fair Work Week Act alongside a series of minimum wage increases and paid family leave legislation. This bundling is not coincidental: the political coalitions that generate scheduling mandates also generate complementary pro-worker legislation. Oregon, the largest treatment group in this study, provides a stark illustration: the state enacted its Fair Work Week Act (2018) alongside a three-tiered minimum wage increase (2016, reaching $13.50 by 2023), paid family and medical leave (2019), and a ban on salary history inquiries (2019). Any of these concurrent reforms could plausibly affect the racial composition of employment. This creates a fundamental identification challenge for any cross-jurisdictional design.

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 county-level tabulations of employment, earnings, hires, separations, and turnover rates, disaggregated by industry (NAICS sector), race, and ethnicity. I use the race-by-ethnicity cross-tabulation at the NAICS 2-digit level, which permits separate identification of Black non-Hispanic (QWI code A2/A1) and all non-Hispanic (A0/A1) workers within food service (NAICS 72) and construction (NAICS 23).

The panel covers 2013Q1 through 2019Q4 (the pre-COVID sample), yielding approximately 163,000 county-quarter-race observations for NAICS 72 across 3,108 counties and 51 states (including DC). Black non-Hispanic food service workers are present in 2,885 counties, with average end-of-quarter employment of 677 per county-quarter compared to 3,298 for all non-Hispanic workers.

Treatment Assignment. I assign treatment at the county level using a crosswalk from jurisdiction to county FIPS codes. For city-level ordinances, the treated county is the coterminous county (San Francisco County, King County for Seattle, the five NYC boroughs, Philadelphia County, Cook County for Chicago). Oregon's statewide law treats all 36 Oregon counties. This yields 43 treated counties in the pre-COVID sample, with 3,065 control counties across 47 non-adopting states. The four pre-COVID treatment cohorts provide the primary identifying variation: Q3 2015 (San Francisco), Q3 2017 (Seattle), Q4 2017 (New York City), and Q3 2018 (Oregon).

Table 1: Summary Statistics: Food Service Employment by Race

	All Non-Hispanic	Black Non-Hispanic
Employment (end-of-quarter)	3,283	680
	(9,958)	(2,341)
Quarterly earnings ($)	1,356	1,278
	(410)	(377)
All hires	1,014	279
	(2,777)	(875)
Separations	1,023	279
	(2,855)	(884)
Turnover rate	0.173	0.195
	(0.048)	(0.070)
County-quarters	88,621	74,537
Counties	3,108	2,828

Notes: QWI county-quarter data for NAICS 72 (Accommodation and Food Services), 2013Q1–2019Q4. Standard deviations in parentheses. "All Non-Hispanic" includes all races with non-Hispanic ethnicity (QWI race A0, ethnicity A1). "Black Non-Hispanic" is QWI race A2, ethnicity A1. Earnings are average quarterly earnings for stable (full-quarter) workers.

4. Empirical Strategy

4.1 Triple-Difference Specification

I estimate:

$$Y_{cqr} = \beta \cdot (\text{Treat}_{cq} \times \text{Black}_r) + \gamma \cdot \text{Treat}_{cq} + \alpha_{cr} + \theta_{qr} + \lambda_{sq} + \varepsilon_{cqr} \tag{1}$$

where Y_{cqr} is the log outcome for county c, quarter q, and race group r; Treat_{cq} equals one if county c is covered by a Fair Workweek law in quarter q; Black_r equals one for Black non-Hispanic workers; α_{cr} are county-by-race fixed effects; θ_{qr} are quarter-by-race fixed effects; and λ_{sq} are state-by-quarter fixed effects. Standard errors are clustered at the state level.

The coefficient β captures the differential change in the outcome for Black workers (relative to all non-Hispanic workers) in treated counties (relative to control counties) after the law takes effect. The county-by-race fixed effects absorb time-invariant racial composition differences across counties. The quarter-by-race effects control for national racial trends. The state-by-quarter effects absorb state-level time-varying shocks, ensuring identification comes from within-state, within-quarter, across-race variation.

4.2 Identifying Assumption and Threats

The identifying assumption is that the Black-to-total employment ratio in NAICS 72 would have evolved similarly in treated and control counties absent the law, conditional on fixed effects. I test this via event-study pre-trends.

The primary threat is the progressive policy bundle. If adopting jurisdictions simultaneously implemented other policies that differentially benefited Black workers across *all* industries, the DDD will attribute these gains to the scheduling law. I test this directly with a placebo industry—NAICS 23 (construction)—that has substantial Black employment but is not covered by any Fair Workweek law.

One measurement note: the "all non-Hispanic" comparison group (QWI code A0) includes Black workers. This nests Black within the comparison, biasing the DDD coefficient toward zero. The preferred comparison would be White non-Hispanic (A1) alone, but QWI race-by-ethnicity tabulations for county-level A1 have high suppression rates. The conservative bias means any positive DDD estimate is, if anything, attenuated; it cannot explain a false positive.

4.3 Callaway-Sant'Anna Estimator

As a heterogeneity-robust alternative to TWFE, I implement the Callaway and Sant'Anna (2021) estimator on the within-county racial gap. I compute $\text{Gap}_{cq} = \ln Y_{cq}^{\text{Black}} - \ln Y_{cq}^{\text{All}}$ for each county-quarter and apply CS with never-treated counties as controls and universal base periods. This approach respects the staggered treatment timing without imposing treatment-effect homogeneity.

5. Results

5.1 Main DDD Estimates

Table 2 reports the TWFE DDD estimates for five outcomes in the pre-COVID sample. The DDD coefficient on log employment is 0.085 ($p = 0.08$), suggesting that Black food service employment grew 8.5 percent faster in treated counties relative to control counties, compared to all non-Hispanic workers. The earnings DDD is 0.015 ($p < 0.001$), indicating a 1.5 percent differential earnings gain. Separations, hires, and turnover show no significant differential effects.

Table 2: Effect of Fair Workweek Laws on Black Workers in Food Service: Triple-Difference Estimates

	ln(Employment) (1)	ln(Earnings) (2)	ln(Separations) (3)	ln(Hires) (4)	Turnover Rate (5)
Treated × Post × Black	0.0854*	0.0149***	0.0341	0.0508	0.0008
	(0.0479)	(0.0041)	(0.0494)	(0.0538)	(0.0019)
County × Race FE	✓	✓	✓	✓	✓
Quarter × Race FE	✓	✓	✓	✓	✓
State × Quarter FE	✓	✓	✓	✓	✓
Observations	163,101	163,014	154,673	155,004	139,009
Clusters (states)	51	51	51	51	51

Notes: Triple-difference estimates of Fair Workweek law effects on Black relative to all non-Hispanic workers in NAICS 72 (food service), pre-COVID sample (2013Q1–2019Q4). Treatment: counties covered by predictive scheduling mandates in San Francisco (2015), Seattle (2017), New York City (2017), and Oregon (2018). All specifications include county×race, quarter×race, and state×quarter fixed effects. Standard errors clustered at the state level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

The Callaway-Sant'Anna estimator on the racial employment gap yields a smaller and statistically insignificant overall ATT of 0.015 (SE = 0.020), and an earnings gap ATT of 0.004 (SE = 0.005). The attenuation under CS is consistent with treatment-effect heterogeneity across cohorts that inflates the TWFE estimate—a common feature of staggered designs where "forbidden comparisons" between early- and late-treated units bias TWFE upward (Goodman-Bacon, 2021). The CS estimates, which avoid such comparisons by using only never-treated counties as controls, provide a cleaner benchmark and suggest the DDD is close to zero.

Pre-Trend Evidence. The CS dynamic event study shows pre-period coefficients of -0.10 at $e = -8$ declining to -0.03 at $e = -2$, with a discrete jump to zero at the normalization point $e = -1$. Post-treatment coefficients are small and positive through $e = 6$ (peaking at 0.05), then turn negative at $e = 7$–8. This declining pre-trend raises concerns: the racial gap appears to have been narrowing before treatment in these jurisdictions, consistent with the progressive bundle hypothesis.

Null Mechanism Results. Critically, separations, hires, and turnover—the most direct channels through which schedule stability should affect Black workers—show no significant DDD effects. If predictability pay and advance scheduling genuinely reduced Black workers' quit rates, separations should fall and turnover should decline. The null on these mechanism outcomes provides independent evidence, beyond the placebo test, that the scheduling mandate is not the operative channel.

Table 3: Callaway-Sant'Anna Estimates of the Employment Gap Effect

	ATT	SE
Employment gap (Black − All)	0.0150	(0.0203)
Earnings gap (Black − All)	0.0044	(0.0049)
Separations gap (Black − All)	-0.0009	(0.0990)
Estimator	Callaway-Sant'Anna (2021)	
Comparison group	Never-treated counties	
Base period	Universal	

Notes: Callaway and Sant'Anna (2021) group-time ATT estimates aggregated into an overall simple-weighted ATT. The dependent variable is the log-ratio gap between Black non-Hispanic and all non-Hispanic workers in NAICS 72 (food service). Comparison group: never-treated counties. Standard errors clustered at the state level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

5.2 The Placebo Failure

Table 4, Panel A reports the DDD for NAICS 23 (construction). The employment DDD is 0.132 ($p = 0.02$)—*larger* than the food service estimate of 0.085. The earnings DDD is 0.059 ($p = 0.01$), also exceeding the food service estimate of 0.015. If Fair Workweek laws were driving the food service results, construction—an uncovered sector—should show a null DDD.

The placebo failure has a clear interpretation: the racial employment gap was converging across industries in adopting jurisdictions, not specifically in sectors covered by scheduling mandates. This convergence likely reflects the progressive policy bundle—minimum wage increases, paid leave, ban-the-box, and other policies adopted simultaneously.

5.3 Leave-One-Out and Randomization Inference

Panel B of Table 4 reveals that the positive food service DDD is driven entirely by Oregon. Dropping Oregon's 36 counties yields a DDD of -0.078 ($p = 0.03$): the estimate flips sign and becomes significantly *negative*. Dropping San Francisco or New York City has minimal effect on the point estimate. This concentration of the result in a single statewide adopter—which also implemented multiple other labor reforms—further undermines a scheduling-law interpretation.

Randomization inference provides the final diagnostic. I permute treatment status across states 500 times, maintaining the number of treated states but randomly reassigning which states are treated, and re-estimate the TWFE DDD on each permuted sample (Young, 2019). The two-sided p-value is 0.61: the observed DDD of 0.085 falls near the center of the permutation distribution (mean = 0.011, SD = 0.094). This indicates that the observed DDD is well within the range of estimates one would obtain by chance when treatment is assigned randomly across states.

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

	DDD Coefficient	SE	Observations
Panel A: Placebo industry (NAICS 23 — Construction)			
ln(Employment)	0.1320**	(0.0548)	147,898
ln(Earnings)	0.0588**	(0.0225)	147,551
Panel B: Leave-one-out (ln Employment, NAICS 72)			
Drop Oregon	-0.0777**	(0.0344)	161,180
Drop San Francisco	0.0876*	(0.0481)	163,043
Drop New York City	0.1202***	(0.0164)	162,811
Panel C: Full sample including COVID-era adopters (2013Q1–2022Q4)			
ln(Employment)	0.1329*	(0.0670)	224,804
ln(Earnings)	0.0113**	(0.0045)	224,684

Notes: Panel A shows triple-difference estimates for NAICS 23 (Construction), an industry not covered by Fair Workweek laws but with substantial Black employment. Panel B drops each treatment jurisdiction in turn from the pre-COVID NAICS 72 sample. Panel C extends the sample through 2022Q4, adding Philadelphia (2020) and Chicago (2020) as treated jurisdictions. All specifications include county×race, quarter×race, and state×quarter FE. SEs clustered at state level. Randomization inference p-value (500 permutations): 0.606. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

6. Discussion

The results illustrate a specific identification challenge: progressive policy bundling. The jurisdictions that adopt Fair Workweek laws are not randomly selected—they are cities and states with strong progressive coalitions that simultaneously pursue multiple pro-worker policies. San Francisco adopted paid sick leave (2007), a living wage ordinance (2003), and ban-the-box (2014) alongside its scheduling law (2015). Seattle enacted its $15 minimum wage (2015), paid sick leave (2012), and secure scheduling (2017) in rapid succession. Oregon combined its Fair Work Week (2018) with minimum wage increases (2016) and paid family leave (2019).

This bundling creates a fundamental problem for cross-jurisdictional identification. The DDD is designed to difference out county-level and time-varying shocks, but it cannot difference out policies that affect the racial composition of employment *across industries* within treated jurisdictions. The construction placebo's positive and significant DDD confirms that the racial convergence was not food-service-specific.

One path forward is exploitation of within-sector discontinuities. Fair Workweek laws typically apply only to employers above a size threshold (e.g., 500 employees), creating a potential fuzzy regression discontinuity. Alternatively, the laws exempt certain occupational categories or establishment types. These within-sector boundaries offer variation that is orthogonal to the progressive bundle. However, the QWI does not report employer size, making this test infeasible with current data.

The honest null finding has value for three audiences. For policymakers, it cautions against attributing racial employment gains in progressive cities specifically to scheduling mandates rather than the full policy portfolio. For empiricists, it demonstrates the importance of placebo industries as diagnostic tools in DDD designs—a standard recommendation (Olden and Møen, 2022) that is often omitted in practice. For the scheduling-mandate literature, it highlights that the QWI race panel, while uniquely suited for this question, cannot separate scheduling effects from concurrent policies without additional within-sector variation.

7. Conclusion

Fair Workweek laws are popular, spreading, and motivated by genuine inequities in schedule volatility. But this paper cannot confirm that they have differentially improved Black workers' employment outcomes in food service. The same racial employment convergence that appears in covered sectors also appears in construction, where no scheduling mandate applies. The finding is driven by Oregon, and it vanishes under randomization inference. Progressive cities adopt policy bundles, and the scheduling law cannot be isolated from its companions.

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

Contributors: @SocialCatalystLab

First Contributor: https://github.com/SocialCatalystLab

References

Callaway, Brantly and Pedro H.C. Sant'Anna, "Difference-in-Differences with Multiple Time Periods," *Journal of Econometrics*, 2021, *225* (2), 200–230.

Dube, Arindrajit, "Minimum Wages and the Distribution of Family Incomes," *American Economic Journal: Applied Economics*, 2019, *11* (4), 268–304.

Goodman-Bacon, Andrew, "Difference-in-Differences with Variation in Treatment Timing," *Journal of Econometrics*, 2021, *225* (2), 254–277.

Gruber, Jonathan, "The Incidence of Mandated Maternity Benefits," *American Economic Review*, 1994, *84* (3), 622–641.

Harknett, Kristen, Daniel Schneider, and Rebecca Wolfe, "Who Cares if Parents Have Unpredictable Work Schedules? Just-in-Time Work Schedules and Child Care Arrangements," *Social Problems*, 2019, *67* (3), 456–476.

Olden, Andreas and Jarle Møen, "The Triple Difference Estimator," *The Econometrics Journal*, 2022, *25* (3), 531–553.

Schneider, Daniel and Kristen Harknett, "Consequences of Routine Work-Schedule Instability for Worker Health and Well-Being," *American Sociological Review*, 2019, *84* (1), 82–114.

Storer, Adam, Daniel Schneider, and Kristen Harknett, "What Explains Race/Ethnic Inequality in Job Quality in the Service Sector?," *American Sociological Review*, 2022, *87* (2), 304–336.

Young, Alwyn, "Channeling Fisher: Randomization Tests and the Statistical Insignificance of Seemingly Significant Experimental Results," *Quarterly Journal of Economics*, 2019, *134* (2), 557–598.

A. Data Appendix

QWI Data Construction. The Quarterly Workforce Indicators are produced by the Census Bureau's LEHD program using matched employer-employee administrative records. The QWI race-by-ethnicity tabulation (file type "rh") provides county-level counts disaggregated by race (9 categories) and ethnicity (3 categories) at the NAICS sector level. I extract records for NAICS 72 (Accommodation and Food Services) and NAICS 23 (Construction) from the non-seasonally-adjusted quarterly files for all 51 states, 2013Q1 through 2022Q4.

Key variables are: `EmpEnd` (end-of-quarter employment count), `EarnS` (average quarterly earnings for stable full-quarter workers), `HirA` (all hires), `Sep` (separations), and `TurnOvrS` (turnover rate = separations / stable employment). All employment and flow variables are subject to Census Bureau noise infusion for confidentiality. Cells with fewer than 3 establishments or with high disclosure risk are suppressed; I drop these from the analysis.

Treatment Crosswalk. City-level Fair Workweek laws are mapped to county FIPS codes: San Francisco (06075), King County/Seattle (53033), New York City boroughs (36005, 36047, 36061, 36081, 36085), Philadelphia (42101), and Cook County/Chicago (17031). Oregon's statewide law covers all 36 Oregon counties (FIPS 41001–41071). Effective dates use the quarter in which the law took effect.

B. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
ln(Employment)	0.0854	0.0479	2.3486	0.0364	0.0204	Small positive
ln(Earnings)	0.0149	0.0041	0.2286	0.0654	0.0179	Moderate positive
ln(Separations)	0.0341	0.0494	2.1338	0.0160	0.0232	Small positive
Turnover Rate	0.0008	0.0019	0.0595	0.0136	0.0313	Small positive

Notes: **Country:** United States. **Research question:** Do predictive scheduling mandates (Fair Workweek laws) differentially improve employment, earnings, and retention for Black workers in food service? **Policy mechanism:** Fair Workweek laws require covered food service and retail employers to post schedules at least 14 days in advance, pay premium wages for last-minute schedule changes, and offer additional hours to existing staff before hiring new workers, reducing schedule uncertainty that disproportionately affects workers with binding childcare and transportation constraints. **Outcome definition:** Log end-of-quarter employment, log average quarterly earnings of stable full-quarter workers, log quarterly separations, and turnover rate (separations/stable employment) from the Census Quarterly Workforce Indicators (QWI). **Treatment:** Binary; county covered by a Fair Workweek law interacted with Black non-Hispanic race indicator (triple-difference). **Data:** QWI race/ethnicity panel (county $\times$ quarter $\times$ race), NAICS 72 (Accommodation and Food Services), 2013Q1–2019Q4, pre-COVID sample. N = 163,158 county-quarter-race observations across 51 states. **Method:** TWFE triple-difference (county$\times$race, quarter$\times$race, state$\times$quarter FE); standard errors clustered at the state level; robustness via Callaway-Sant'Anna (2021), randomization inference, and placebo industry (construction). **Sample:** Counties with positive Black non-Hispanic employment in NAICS 72; pre-COVID period only; COVID-era adopters (Philadelphia, Chicago) excluded from main specification. SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the pre-treatment standard deviation of the outcome among treated Black workers. 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).