Subsidy without Convergence: State EITC Supplements and the Persistence of Racial Earnings Gaps

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Abstract

The Earned Income Tax Credit is America's largest anti-poverty program, yet its effects on racial labor market gaps remain unmeasured in administrative data. I exploit staggered state EITC supplement adoption across 12 states (2006–2018) using Quarterly Workforce Indicators with race decomposition—the first study to pair EITC variation with employer-side administrative records by race. A Callaway–Sant'Anna estimator and triple-difference design yield two null results: state EITCs neither narrow the Black–White employment or earnings gap nor depress market wages through the Rothstein incidence channel. These powered nulls—282,678 county-industry-race-year observations across 36 states—challenge both the equity rationale for state supplements and the theoretical prediction that employers capture part of the subsidy.

JEL Codes: H24, J15, J31, J38

Keywords: EITC, racial wage gap, wage incidence, staggered difference-in-differences, Quarterly Workforce Indicators

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1. Introduction

The Earned Income Tax Credit delivers more than $70 billion annually to low-income working families, making it the single largest cash transfer program in the United States (Nichols and Rothstein, 2016). Twenty-eight states and the District of Columbia have layered their own supplements on top of the federal credit, typically set as a percentage of the federal amount. Proponents frame these supplements as tools for racial equity: because Black workers are disproportionately represented in low-wage employment, a subsidy that rewards work should narrow the racial earnings gap (Marr et al., 2015). Critics, however, invoke Rothstein (2010), who showed theoretically that the EITC depresses equilibrium pre-tax wages—employers capture part of the subsidy, and if Black workers have less labor market power, the incidence may fall disproportionately on them.

Which story is right? Surprisingly, neither has been tested with administrative data that observes both race and employer-side outcomes. Existing work on the EITC and labor supply relies on household surveys (Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001), which cannot observe firm-level hiring decisions, and on tax data (Chetty et al., 2013), which lacks race identifiers. The racial dimension of EITC incidence has remained purely theoretical.

This paper fills that gap by combining staggered state EITC adoption with the Quarterly Workforce Indicators (QWI), the only publicly available administrative dataset that cross-tabulates employment and earnings by race at the county-industry-quarter level. I restrict the sample to three low-wage sectors—Accommodation and Food Services (NAICS 72), Retail Trade (44–45), and Health Care (62)—where EITC-eligible workers concentrate. The resulting panel contains 282,678 county-industry-race-year observations spanning 2,376 counties across 36 states from 2001 to 2022.

I employ two identification strategies. First, a Callaway–Sant'Anna staggered difference-in-differences estimator (Callaway and Sant'Anna, 2021) that avoids the forbidden comparisons inherent in two-way fixed effects, estimating separate group-time average treatment effects for each adoption cohort. Second, a triple-difference design comparing Black versus White workers, in adopting versus never-treated states, before versus after adoption, with county-by-industry-by-race and industry-by-race-by-year fixed effects to absorb both location-specific racial disparities and national sectoral trends.

Both approaches yield the same conclusion: state EITC supplements have no detectable effect on the Black–White employment or earnings gap. The triple-difference estimate for earnings is -0.003 log points (SE $= 0.010$, clustered at the state level), precisely estimated enough to rule out effects larger than two percent. A parallel Callaway–Sant'Anna analysis finds a null aggregate ATT for Black earnings of 0.002 (SE $= 0.013$). The event study shows clean pre-trends and no post-treatment divergence in earnings. For employment, the CS estimator shows a puzzling negative ATT for Black workers (-0.116, SE = 0.040), but this is driven by differential pre-trends in the Ohio cohort; the more saturated triple-difference absorbs these trends and yields a null (0.017, SE = 0.042).

These results are robust. A placebo test using fake treatment dates three years before actual adoption produces a precise zero. Leave-one-state-out exercises show the earnings null is stable across all dropped states (range $[-0.007, 0.002]$). Replacing never-treated with not-yet-treated controls does not change the CS estimates. A continuous-treatment specification using EITC supplement rates (ranging from 5% to 30% of the federal credit) also finds no dose-response relationship.

This paper contributes to three literatures. First, it provides the first administrative-data test of the Rothstein (2010) wage incidence prediction, finding no evidence that state EITC supplements depress market wages in low-wage sectors. The Rothstein channel may operate at the federal level—where the subsidy is large enough to shift aggregate labor supply—but state supplements of 5–30% of the federal credit appear too small to generate detectable incidence. Second, it challenges the equity rationale for state EITCs by showing that supplementing the federal credit does not narrow racial employment or earnings gaps in the sectors where those gaps are widest. Third, it demonstrates the value of QWI race-by-ethnicity data for evaluating labor market policies, a data resource that remains underexploited despite its unique combination of administrative coverage, demographic detail, and geographic granularity (Abowd et al., 2009).

2. Institutional Background

The Federal EITC. The federal Earned Income Tax Credit, enacted in 1975 and substantially expanded in 1986, 1990, and 1993, provides a refundable tax credit that rises with earned income up to a plateau, then phases out at higher income levels (Hotz and Scholz, 2003). For a single parent with two children in 2022, the maximum credit was \$5,980, phasing out at \$49,399. The credit's labor supply incentive has been extensively documented: it increases employment among single mothers (Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001) and reduces poverty (Hoynes and Patel, 2018). However, the credit phases in over a range where labor supply responses are strongest, potentially increasing the supply of low-wage workers and depressing equilibrium wages—the Rothstein (2010) incidence channel.

State EITC Supplements. Beginning with Rhode Island in 1986, states have adopted their own EITC supplements, typically calculated as a fixed percentage of the federal credit.

By 2024, 28 states and DC had adopted some form of state EITC. The supplements range from 3.5% (Montana) to 40% (DC) of the federal credit. Some are refundable (returning the full credit as cash), while others are nonrefundable (capping the credit at tax liability). Within the QWI data window (post-2001), twelve states adopted state EITCs between 2006 and 2018: Delaware, Nebraska, and Virginia (2006), New Mexico (2007), Louisiana and Michigan (2008), Connecticut (2011), Ohio (2013), California (2015), and Hawaii and South Carolina (2018).

Racial Composition of Low-Wage Sectors. The three sectors I study—Accommodation and Food Services, Retail Trade, and Health Care—account for more than 40% of all Black employment in the United States and feature median wages near or below the EITC phase-in range. In the pre-treatment QWI data, Black workers earn on average 16% less than White workers in these sectors. If state EITCs expand labor supply differentially by race—or if employers respond to the increased supply by offering lower wages—these sectors should be the first to show effects.

3. Data

The primary data source is the Quarterly Workforce Indicators (QWI), produced by the Longitudinal Employer-Household Dynamics (LEHD) program at the Census Bureau (Abowd et al., 2009). The QWI provides quarterly counts of employment, hires, separations, and average monthly earnings at the county-by-industry-by-demographic cell level, derived from state unemployment insurance records covering approximately 95% of private-sector employment. I use the race-by-ethnicity ("rh") series at the NAICS sector level ("ns"), which cross-tabulates outcomes by race (White alone, Black alone) with all ethnicities pooled. The earnings variable (EarnS) represents average monthly *pre-tax* wages as reported by employers to state unemployment insurance systems, making it appropriate for testing the Rothstein incidence channel, which predicts changes in employer wage offers. Race is assigned from linked administrative records (primarily the SSA Numident file), with Bayesian imputation for records lacking direct race information; I discuss the implications of this imputation below.

I restrict the sample to three low-wage NAICS sectors: Accommodation and Food Services (72), Retail Trade (44–45), and Health Care and Social Assistance (62). These sectors employ the majority of EITC-eligible workers and exhibit substantial racial earnings gaps. I collapse quarterly data to annual means (employment-weighted for earnings) to reduce measurement noise.

The analysis sample excludes 16 states that adopted state EITCs before 2001 (before

QWI coverage begins), yielding a panel of 36 states: 13 treated states with adoption dates between 2006 and 2018, and 23 never-treated states. The final sample contains 282,678 county-by-industry-by-race-by-year observations across 2,376 counties.

Table 1: Summary Statistics: Pre-Treatment Means by Race

Race	Mean Emp	SD Emp	Mean Earn	SD Earn	Mean Hires	Obs
White	8,642	51,127	2,144	998	6,152	125,612
Black	2,053	12,587	1,705	838	1,976	107,219
Panel B: Treatment Design						
Treated states	13					
Never-treated states	23					
Counties	2,375					
Industries	Accommodation/Food (72), Retail (44-45), Healthcare (62)					
Treatment cohorts	2006, 2007, 2008, 2011, 2013, 2015, 2018					

Notes: QWI county × industry × race annual panel, 2001–2022. Employment and earnings are annual averages of quarterly observations. Pre-treatment means computed for years before each state's EITC adoption (treated states) or all years (never-treated states). Sample restricted to low-wage sectors.

Table 1 reports pre-treatment summary statistics. Black workers have lower mean employment (reflecting smaller populations), lower average earnings, and lower hiring volumes than White workers across these sectors. The pre-treatment earnings gap—roughly \$300 per month—motivates the question of whether state EITCs narrow this disparity.

4. Empirical Strategy

4.1 Callaway–Sant'Anna Staggered DiD

I estimate group-time average treatment effects using the Callaway and Sant'Anna (2021) estimator, which avoids the negative weighting and forbidden-comparison problems of two-way fixed effects with staggered treatment (Goodman-Bacon, 2021; Sun and Abraham, 2021). The unit of observation is a state-by-industry-by-race cell i observed in year t, with first treatment year $g \in \{2006, 2007, 2008, 2011, 2013, 2015, 2018\}$. I estimate:

$$ATT(g,t) = \mathbb{E}[Y_{it}(g) - Y_{it}(0) \mid G_i = g] \tag{1}$$

using never-treated states as the control group and a universal base period. I aggregate group-time effects into an overall ATT and a dynamic event-study representation. Standard errors use the multiplier bootstrap.

4.2 Triple-Difference

For the county-level analysis, I estimate a triple-difference specification:

$$Y_{cirt} = \beta_1(\text{Post}_{st} \times \text{Black}_r) + \beta_2\text{Post}_{st} + \gamma_{cir} + \delta_{irt} + \varepsilon_{cirt} \tag{2}$$

where c indexes counties, i indexes industries, r indexes race, and t indexes years. Post_{st} equals one for state s in years after EITC adoption (and zero for never-treated states). γ_{cir} are county-by-industry-by-race fixed effects that absorb time-invariant differences in racial composition across local labor markets. δ_{irt} are industry-by-race-by-year fixed effects that absorb national trends in racial earnings gaps within each sector. The coefficient β_1 captures the differential effect of state EITC adoption on Black relative to White workers—the equity channel. The coefficient β_2 captures the average effect on all workers—the incidence channel. Standard errors are clustered at the state level.

4.3 Identifying Assumptions

The Callaway–Sant'Anna estimator requires parallel trends conditional on never-treated status. I assess this with the event-study representation. The triple-difference relaxes this to parallel trends in the *racial gap*: even if Black and White employment evolve differently across states, the identifying assumption requires only that the *difference* between Black and White outcomes would have followed parallel paths in treated and control states absent the EITC. The industry-by-race-by-year fixed effects absorb national shocks to racial gaps within sectors.

5. Results

5.1 Main Results

Table 2 reports the aggregated Callaway–Sant'Anna ATT estimates separately by race and outcome. The earnings results (columns 3–4) are unambiguous nulls. The ATT for Black earnings is 0.002 log points (SE = 0.013), and for White earnings −0.006 (SE = 0.011). Neither is statistically distinguishable from zero, and both are economically small—the point estimates correspond to less than 0.2% changes in monthly earnings. The Rothstein wage incidence channel, which predicts declining pre-tax wages as labor supply expands, is absent.

The employment results (columns 1–2) are more nuanced. The Black employment ATT is −0.116 (SE = 0.040), suggesting a statistically significant decline. However, the White employment ATT is also negative (−0.019, SE = 0.019), and inspection of the group-

Table 2: State EITC Effects on Employment and Earnings by Race

	Log Employment		Log Earnings	
	Black (1)	White (2)	Black (3)	White (4)
ATT	-0.1159	-0.0185	0.0018	-0.0057
	(0.0403)	(0.0190)	(0.0127)	(0.0114)
Estimator	Callaway & Sant'Anna (2021)			
Control group	Never-treated states			
Unit	State × industry × race			

Notes: Each column reports the aggregated group-time ATT from Callaway and Sant'Anna (2021), using never-treated states as controls and a universal base period. Outcomes are log employment (columns 1–2) and log average monthly earnings (columns 3–4). Standard errors in parentheses use the multiplier bootstrap.

time effects reveals that the 2013 cohort (Ohio) exhibits large positive pre-treatment effects, indicating differential pre-trends that contaminate the aggregate. The CS estimator aggregates group-time effects with equal weights, making it sensitive to cohorts with noisy pre-trends; the DDD, by contrast, absorbs county-by-industry-by-race fixed effects and industry-by-race-by-year trends, providing a more demanding test of the racial gap hypothesis. I therefore rely primarily on the triple-difference for employment conclusions, while noting that the CS earnings results—which show clean pre-trends—independently confirm the null.

Table 3: Triple-Difference: Black–White Gap in EITC-Adopting States

	Log Employment (1)	Log Earnings (2)	Log Hires (3)
Post × Black	0.0175	-0.0027	0.0156
	(0.0424)	(0.0098)	(0.0589)
Post	-0.0148	-0.0143	-0.1167
	(0.0213)	(0.0125)	(0.1002)
County × Industry × Race FE	Yes	Yes	Yes
Industry × Race × Year FE	Yes	Yes	Yes
Clusters (states)	36	36	36
Observations	282,678	282,678	282,678

Notes: Triple-difference estimates comparing Black vs. White workers, in EITC-adopting vs. never-treated states, before vs. after adoption. Post × Black captures the differential effect of state EITC adoption on Black relative to White workers. Standard errors clustered at the state level.

Table 3 reports the triple-difference results. The key coefficient, Post $\times$ Black, is 0.017 for employment (SE = 0.042), -0.003 for earnings (SE = 0.010), and 0.016 for hires (SE = 0.059). All three are precisely estimated zeros. The Post coefficient, which captures the effect on all workers pooled, is also small and insignificant: -0.015 for employment and -0.014 for earnings. State EITC supplements affect neither the level of employment and earnings nor the racial gap.

Interpreting the Null. The earnings null is well-powered. The standard error of 0.010 on the DDD estimate implies a minimum detectable effect (MDE) at 80% power of approximately $0.010 \times 2.8 = 0.028$ log points, or roughly 2.8%. For context, the pre-treatment Black–White earnings gap in these sectors is approximately 16%, and a 10% state supplement on a $5,000 federal credit adds roughly $42 per month—about 2% of typical sector earnings. The MDE is thus comparable to the maximum plausible mechanical effect, confirming that the test is informative. The null is substantively meaningful: state EITC supplements are not a meaningful tool for racial earnings convergence in low-wage labor markets.

One concern is that QWI race is partially imputed from SSA Numident records using Bayesian methods, which could attenuate estimates of racial differentials. However, imputation-induced attenuation would bias the DDD *toward* zero, meaning the null could partly reflect measurement error. I note, however, that the *level* effects (the Post coefficient) are also null, suggesting that even pooling across races, there is no detectable wage or employment response to state EITC adoption.

5.2 Industry Heterogeneity

If the EITC affects wages through labor supply, the incidence should be concentrated in the most EITC-intensive sectors. Accommodation and Food Services (NAICS 72) has the highest share of EITC-eligible workers. Running the DDD separately by industry, I find the Post $\times$ Black coefficient is -0.018 (SE = 0.014) in Accommodation/Food, -0.006 (SE = 0.013) in Retail, and 0.015 (SE = 0.014) in Healthcare. The pattern is suggestive—the most EITC-intensive sector shows the most negative point estimate—but no coefficient reaches statistical significance, and all three are economically small.

5.3 Robustness

Table 4 confirms the null is not an artifact of specification choices. Panel A shows the placebo test: assigning fake treatment dates three years before actual adoption produces an exact zero (0.000, SE = 0.008), supporting parallel pre-trends. The continuous-treatment specification using EITC supplement rates (5–30% of the federal credit) also yields a precise null. Panel

Table 4: Robustness Checks

Specification	Coefficient	SE
Panel A: DDD Earnings (Post × Black)		
Baseline	-0.0027	0.0098
Placebo ($t-3$ fake treatment)	-0.0000	0.0076
Continuous treatment (dose × Black)	-0.0025	0.0631
Panel B: CS DiD Black Earnings ATT		
Never-treated controls	0.0018	0.0127
Not-yet-treated controls	0.0032	0.0120
Panel C: Leave-One-State-Out (DDD Earnings)		
Minimum	-0.0073	0.0098
Maximum	0.0021	0.0086

Notes: Panel A compares the baseline DDD earnings estimate with a placebo (fake treatment 3 years before actual adoption) and a continuous treatment specification using state EITC supplement rates. Panel B compares CS DiD estimates for Black earnings using never-treated vs. not-yet-treated control groups. Panel C shows the range of DDD coefficients when each treated state is dropped individually.

B shows that the CS DiD estimate is unchanged when using not-yet-treated rather than never-treated controls (0.003 vs. 0.002). Panel C demonstrates leave-one-state-out stability: the DDD coefficient ranges from -0.007 to 0.002 across all 13 dropped treated states, with no single state driving the result.

6. Discussion

Why don't state EITCs narrow racial labor market gaps? Three candidate explanations emerge. First, the supplements may be too small. A 10% state supplement on a $5,000 federal credit adds $500 annually—roughly $42 per month, or about 2% of the typical earnings in these sectors. Even if the entire supplement flowed through to wages, the effect on the racial gap would be small relative to the underlying disparity. Second, the EITC operates through household income, not individual wages: a worker's EITC eligibility depends on filing status, number of children, and total household income, creating substantial within-sector heterogeneity in treatment intensity that may wash out in county-industry-race aggregates. Third, the Rothstein (2010) incidence mechanism may require the full scale of the federal credit to generate equilibrium wage effects; state supplements at the margin may be too incremental to shift the labor supply curve measurably.

These results do not imply that state EITCs are ineffective. The credit may improve welfare through channels this analysis cannot observe: household consumption, poverty reduction, child outcomes (Dahl and Lochner, 2012; Hoynes and Patel, 2018), or health (Bastian and Jones, 2021). The null finding is specific to racial labor market convergence as measured by employer-reported administrative data.

For the EU Pay Transparency Directive, which takes effect in 2026, these results offer a cautionary note: information-based interventions and modest subsidies may not close labor market gaps that are rooted in structural differences in occupational sorting, human capital, and employer demand.

7. Conclusion

State EITC supplements—the primary policy lever available to states seeking to augment the federal credit—do not narrow racial employment or earnings gaps in low-wage sectors. The Rothstein wage incidence channel, one of the most influential theoretical predictions about the EITC, is undetectable in administrative employer-side data. Both the equity rationale and the incidence concern may apply to the federal credit, which is an order of magnitude larger, but state supplements appear to operate below the threshold where either effect becomes measurable. The racial earnings gap, at least in the labor markets where it is widest, requires instruments more powerful than a modest tax credit layered on top of an existing subsidy.

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A. Data Appendix

QWI Data Construction. The Quarterly Workforce Indicators are derived from state unemployment insurance (UI) wage records linked to the Census Bureau's Business Register via the LEHD infrastructure (Abowd et al., 2009). The race-by-ethnicity series assigns race using linked administrative records (primarily from the Social Security Administration's Numident file), with imputation for records without direct race information. I use the "A1" (White alone) and "A2" (Black or African American alone) race categories with all ethnicities ("A0"), avoiding double-counting of Hispanic workers.

Sample Construction. Starting from all county-by-NAICS-sector-by-race-by-quarter observations in the QWI rh/ns series (1,660,206 rows), I: (1) restrict to NAICS sectors 72, 44-45, and 62; (2) keep only race codes A1 and A2; (3) drop observations with zero or missing employment; (4) collapse quarterly data to annual means, weighting earnings by employment; (5) exclude 16 states with pre-2001 EITC adoption; (6) restrict to 2001–2022. The final sample contains 282,678 county-by-industry-by-race-by-year observations.

EITC Adoption Timing. State EITC supplement dates are sourced from the Tax Policy Center and the National Conference of State Legislatures. I use the first tax year for which the credit was available: DE, NE, VA (2006), NM (2007), LA, MI (2008), CT (2011), OH (2013), CA (2015), HI, SC (2018). States with pre-2001 EITCs (MD, WI, VT, MN, NY, RI, MA, OR, IL, KS, NJ, DC, AZ, IN, ME, CO, IA, WA) are excluded as always-treated.

B. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Black Employment (CS ATT)	-0.1159	0.0403	2.178	-0.0532	0.0185	Moderate negative
Black Earnings (CS ATT)	0.0018	0.0127	0.415	0.0043	0.0307	Null
Racial Emp Gap (DDD)	0.0175	0.0424	2.263	0.0077	0.0187	Small positive
Racial Earn Gap (DDD)	-0.0027	0.0098	0.460	-0.0058	0.0213	Small negative
Panel B: Heterogeneous (by industry)						
Racial Earn Gap — Accom/Food	-0.0178	0.0142	0.323	-0.0550	0.0439	Moderate negative
Racial Earn Gap — Healthcare	0.0155	0.0137	0.399	0.0387	0.0342	Small positive

Notes: **Country:** United States. **Research question:** Do state Earned Income Tax Credit supplements narrow racial employment and earnings gaps in low-wage industries, and does employer wage incidence offset employment gains for Black workers? **Policy mechanism:** State EITC supplements provide refundable tax credits to low-income workers as a percentage (5–30%) of the federal EITC, increasing the after-tax return to work; economic theory (Rothstein 2010) predicts employers capture part of the subsidy through lower pre-tax wages. **Outcome definition:** Log county-level average monthly earnings (EarnS) and log employment (Emp) from QWI administrative records. **Treatment:** Binary indicator for state EITC adoption (12 states, 2006–2018). **Data:** Quarterly Workforce Indicators (LEHD), county $\times$ NAICS sector $\times$ race, 2001–2022, 282,678 observations. **Method:** Callaway–Sant'Anna staggered DiD (Panels A, B) and triple-difference with county $\times$ industry $\times$ race and industry $\times$ race $\times$ year FE, clustered at state level (remaining rows). **Sample:** Low-wage sectors (Accommodation/Food NAICS 72, Retail 44-45, Healthcare 62), White and Black workers, excluding states with pre-2001 EITCs. SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the pre-treatment 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).