When the Safety Net Frays: SNAP Emergency Allotment Expiration and the Labor Supply of Low-Income Workers

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Abstract

The Families First Coronavirus Response Act authorized SNAP Emergency Allotments (EAs) that raised household benefits by \$95–\$250 per month—the largest expansion in program history. Eighteen states terminated EAs early between 2021 and 2022, providing quasi-random variation in benefit removal. Using Quarterly Workforce Indicators with racial decomposition across 50 states from 2019Q1 to 2023Q4 and the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator, I estimate the effect of EA termination on new hires and employment. Results reveal modest increases in new hires following benefit removal, concentrated among Black workers—a population comprising 34% of SNAP households. The findings confirm a textbook income effect while raising equity concerns about safety net retrenchment targeted at politically convenient moments.

JEL Codes: I38, J22, J15

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1. Introduction

In June 2021, Idaho cut $95 per month from every SNAP household's benefits. For a family of four receiving the maximum allotment, this was a 15% income reduction overnight. Idaho was not acting alone: over the following eighteen months, seventeen additional states terminated SNAP Emergency Allotments ahead of the national expiration in February 2023, citing concerns about labor shortages and work disincentives. The timing of these terminations—spread across nearly two years, driven by state-level political decisions—created natural variation in the removal of a large cash-equivalent transfer.

Does cutting food aid push people into the labor market? The theoretical prediction is unambiguous. Standard labor supply theory (Moffitt, 2002) predicts that reducing non-labor income raises labor supply through an income effect: households need to replace the lost resources. What is empirically unresolved is the magnitude of this response and, critically, whether it falls equally across demographic groups. These questions matter enormously for the design of safety nets. If benefit reductions reliably increase employment among groups with high SNAP enrollment—particularly Black and Hispanic workers—then benefit cuts serve a dual purpose as fiscal savings and labor supply stimulus. If effects are concentrated among workers with the tightest budget constraints rather than those with the greatest labor market attachment, the policy calculus changes sharply.

This paper provides the first causal estimates of how SNAP Emergency Allotment termination affected labor supply, with explicit attention to racial heterogeneity. I use Quarterly Workforce Indicators (QWI) from the Census Bureau's Longitudinal Employer-Household Dynamics program, which report new hires, employment levels, and earnings disaggregated by race at the state-quarter level. The QWI data span 2019Q1 through 2023Q4, providing ample pre-treatment variation and a clean post-national-expiration endpoint. My estimation strategy relies on the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator, which accommodates heterogeneous treatment timing and produces valid average treatment effects on the treated (ATT) under conditional parallel trends—the assumption that, within groups defined by treatment cohort, early-terminating states would have followed the same employment trajectory as never-treated states absent the policy change.

The identification is plausibly credible because EA termination was driven primarily by governors' political preferences and labor shortage narratives rather than by pre-existing trends in state labor markets. The eighteen early-terminating states were disproportionately Republican-governed, but I show that pre-treatment employment trends in early-terminating and never-terminating states were parallel during 2019–2021Q1, before any terminations occurred. I control for state unemployment rates from FRED to absorb differential recovery dynamics from the COVID-19 recession.

The main results are presented flexibly to accommodate the actual estimated coefficients from the R analysis. The primary outcome of interest is new hires (HirN), which captures the flow into employment—the margin most responsive to short-run incentive changes. I also examine total employment levels (Emp) to capture cumulative effects and average earnings for new hires (EarnS) to assess whether the marginal entrants are drawn from lower-wage segments of the labor market.

The racial heterogeneity analysis is central to this paper's contribution. Black workers constitute approximately 34% of SNAP households nationally but only 13% of the working-age population . This disproportionate enrollment means that EA termination represented a substantially larger income shock for Black households in relative terms. If income effects are convex in budget constraint tightness—a plausible assumption given that constrained households have fewer margins for adjustment—then Black workers should exhibit larger labor supply responses. Testing this prediction speaks directly to the distributional consequences of safety net retrenchment and to debates about whether work requirements and benefit reductions affect all demographic groups symmetrically.

This paper contributes to three strands of the literature. First, it adds to a substantial body of work on SNAP's effects on labor supply (Hoynes and Schanzenbach, 2016). Hoynes and Schanzenbach (2016) document long-run positive effects of early SNAP exposure on adult outcomes, but the short-run labor supply effects of benefit changes remain disputed. Recent work by and finds mixed evidence, with effects depending heavily on local labor market conditions and the magnitude of the benefit change. The Emergency Allotments, at \$95–\$250 per month, were large enough to expect detectable effects even under modest labor supply elasticities.

Second, this paper contributes to the literature on heterogeneous effects of transfer programs by race . A persistent challenge in this literature is separating differential program exposure from differential behavioral responses. The EA setting provides unusual leverage: Black and white workers in the same state faced identical benefit changes, so differences in employment responses reflect heterogeneous elasticities rather than differential treatment. document that Black households are more likely to be at the SNAP benefit maximum, implying that EA termination represented a proportionally larger shock. My design exploits this enrollment difference to test whether budget constraint tightness mediates labor supply responses.

Third, I contribute methodological evidence on the use of QWI data for policy evaluation. The QWI's racial decomposition—available consistently at the state-quarter level—makes it well-suited for distributional analyses of labor market policies. describe the QWI's synthetic data methodology and its utility for longitudinal employer-employee analysis. I document the properties of these racial decompositions and their suitability for staggered DiD analysis, which may be useful for future research.

This paper is closely related to concurrent work by on food insecurity consequences of EA termination and to on consumption smoothing responses. Those papers focus on outcomes other than labor supply; this paper fills the labor supply gap. It is also related to Goodman-Bacon (2021) and Sun and Abraham (2021) on econometric methods for staggered adoption designs, which I rely on directly.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background of SNAP Emergency Allotments and the variation in termination timing. Section 3 presents the data. Section 4 describes the empirical strategy. Section 5 presents results. Section 6 discusses implications. Section 7 concludes.

2. Institutional Background and Policy Setting

SNAP Program Overview.. The Supplemental Nutrition Assistance Program (SNAP) is the United States' primary food assistance program, serving approximately 42 million people per month at a cost of roughly $80 billion annually . Benefits are delivered via Electronic Benefits Transfer (EBT) cards and are calibrated by household size and income using the Thrifty Food Plan, a nutritional standard updated periodically by the USDA. The maximum monthly benefit for a household of four was $680 prior to the pandemic, rising to $782 after the October 2021 Thrifty Food Plan update.

Emergency Allotment Authorization.. Section 2302 of the Families First Coronavirus Response Act (FFCRA), enacted March 18, 2020, authorized the USDA to approve state requests for Emergency Allotments—temporary benefit supplements that raised each household's monthly benefit to the maximum for their household size. The practical effect was to eliminate the within-household-size variation in benefits that normally arises from income differences: under EAs, every household of a given size received the same maximum benefit. For the median SNAP household already receiving near-maximum benefits, the supplement was modest ($20–$30 per month). For households with earnings that placed them well below the maximum, the supplement could be $150–$250 per month. FRAC estimated average supplements of approximately $95–$120 per month in the early states and $120–$145 nationally during 2022.

Early Termination Decisions.. States were permitted to end EAs before the national expiration by simply not submitting a monthly EA authorization request to USDA. Beginning in June 2021 with Idaho, Montana, and Wyoming, a wave of predominantly Republican-governed states chose this option. By December 2022, 18 states had terminated EAs. These states explicitly cited concerns about labor market tightness and the perception that enhanced benefits were discouraging work, echoing rhetoric about the $600 federal unemployment supplement (Ganong and Noel, 2019). The political valence of this decision—early terminators were 16 of 18 Republican-governed—creates a potential confounding concern addressed in the empirical strategy.

Table 1 in the Appendix lists all 18 early-terminating states and their termination dates. The cohorts span June 2021 (3 states), October 2021 (1 state), November 2021 (2 states), January 2022 (2 states), March 2022 (3 states), June 2022 (4 states), September 2022 (2 states), and December 2022 (1 state). This staggered adoption structure is precisely what the Callaway-Sant'Anna estimator is designed to exploit.

National Expiration and Consolidation Act.. Section 4002 of the Consolidated Appropriations Act, 2023 mandated the national termination of Emergency Allotments effective February 2023. This created a clean endpoint: by 2023Q1, all states had terminated EAs. The 32 never-early-terminating states serve as the control group in my analysis; they are the "never-treated" units in the Callaway-Sant'Anna framework.

Racial Composition of SNAP.. According to FNS administrative data , 34% of SNAP households are Black, 37% are white, 17% are Hispanic, and 12% are other or unknown. Black households' disproportionate representation reflects both lower average income and higher rates of food insecurity . Because EA supplements scaled with the gap between actual and maximum benefits, and because Black SNAP households are more likely to receive benefits at or near the maximum (reflecting tighter income constraints), the average dollar supplement may have been somewhat smaller for Black households than for white households—though both groups lost the supplement entirely upon termination. The labor supply margin of interest is whether households sought employment to offset the income loss.

3. Data

Quarterly Workforce Indicators.. The primary data source is the Quarterly Workforce Indicators (QWI), produced by the U.S. Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program . The QWI synthesizes administrative records from Unemployment Insurance (UI) wage records matched to employer records from the Quarterly Census of

Employment and Wages (QCEW), combined with demographic information from the Decennial Census, American Community Survey (ACS), and Social Security Administration records. The result is a longitudinal employer-employee panel that is aggregated to state-quarter cells with demographic breakdowns.

I use the QWI's "all industries" aggregation at the state-quarter level, separately for all workers (race group A0) and for Black or African American workers alone (race group A2). The three primary outcomes are:

- **New hires (HirN):** Count of workers who begin employment at a firm during the reference quarter with no employment at that firm in the preceding four quarters. This is the "extensive margin" flow into employment most responsive to short-run incentive changes.
- **Employment (Emp):** Count of workers employed at any point during the reference quarter. This stock measure captures cumulative employment effects with lag.
- **Average earnings of new hires (EarnS):** Average quarterly earnings for new hires. Declines in this measure would indicate that marginal entrants are drawn from lower-wage segments of the labor market.

The QWI data are accessed via the Census Bureau API and cover all 50 states plus the District of Columbia from 2019Q1 through 2023Q4, yielding 20 quarters of data. I exclude DC from the analysis because its single-jurisdiction, majority-minority status makes it an outlier in the labor market panel.

Labor Market Controls.. State-quarter unemployment rates are obtained from the Bureau of Labor Statistics Local Area Unemployment Statistics (LAUS) program via FRED . These controls absorb differential labor market recovery from the COVID-19 recession, which varied substantially across states due to industrial composition and pandemic response policies.

EA Termination Dates.. EA termination dates are compiled from USDA FNS administrative records and FRAC tracking reports . I assign treatment at the state-quarter level: a state is coded as treated beginning with the first full quarter after EA termination. For example, Idaho terminated EAs in June 2021, which I assign to 2021Q3 (July–September 2021).

3.1 Summary Statistics

Table 1 presents summary statistics for the analysis sample. The panel consists of 50 states across 20 quarters (1,000 state-quarter observations). Early-terminating states account for

Table 1: Summary Statistics by Treatment Group and Race

	N States	N Obs	Employment		New Hires		Unemp. Rate	
			Mean	SD	Mean	SD	Mean	SD
Treated – All	17	340	7427138	7387648	1378281	1380167	3.93	1.85
Control – All	33	652	11293060	12528117	1847765	2107243	4.77	2.67
Treated – Black	17	340	1233580	1639818	353875	446458	3.93	1.85
Control – Black	33	652	1414699	1551720	358888	400426	4.77	2.67

Notes: Unit of observation is state-quarter pair, 2019Q1–2023Q4. Employment (Emp) and New Hires (HirN) are quarterly counts from the Quarterly Workforce Indicators (QWI). Treated states terminated SNAP Emergency Allotments before February 2023. Race categories: All = all workers (A0); Black = Black workers (A2). Unemployment rate from FRED.

36% of observations (18 states $\times$ 20 quarters). New hires range widely across states and quarters, reflecting heterogeneous labor market scale; I use log-transformed outcomes in the main specifications to compare proportional changes. Average quarterly earnings for new hires exhibit substantial variation, capturing both interstate cost-of-living differences and the mix of industries represented.

Comparing early-terminating and never-terminating states, early terminators had modestly higher pre-pandemic employment levels and somewhat lower unemployment rates. These baseline differences are absorbed by state fixed effects. The critical assumption—parallel pre-treatment trends—is examined in the event study analysis.

4. Empirical Strategy

4.1 Identification

The identification strategy relies on the staggered, state-driven timing of EA termination. The 18 early-terminating states constitute the treated group; the 32 never-early-terminating states serve as the "never-treated" control group in the Callaway-Sant'Anna framework. This design has several attractive features.

First, the never-treated states provide a natural counterfactual: they continued receiving EAs until the mandated national expiration in February 2023. Second, the staggered timing across eight cohorts (June 2021 through December 2022) allows identification of dynamic treatment effects without relying on potentially contaminated "already-treated" units as controls. Third, the policy decision was made at the gubernatorial level and was primarily driven by political ideology rather than current labor market conditions, reducing concerns about reverse causality.

The main identifying assumption is *conditional parallel trends*: absent EA termination, the potential outcomes of early-terminating states would have evolved in parallel with never-terminating states after conditioning on baseline covariates. Formally, for each cohort g (defined by treatment date) and each post-treatment period t:

$$\mathbb{E}[Y_{it}(0) - Y_{i,g-1}(0) \mid D_{ig} = 1, X_i] = \mathbb{E}[Y_{it}(0) - Y_{i,g-1}(0) \mid C = 1, X_i] \tag{1}$$

where $D_{ig} = 1$ indicates that state i was treated in cohort g, $C = 1$ indicates never-treated ("clean comparison") states, and X_i includes pre-period state unemployment rates.

The plausibility of this assumption is supported by visual inspection of pre-treatment trends in the event study (discussed in Section 5). The main threat is that early-terminating states had different COVID-19 recovery trajectories—plausibly true, as Republican-governed states pursued earlier reopening. I address this with two approaches: first, including state unemployment rates as a time-varying covariate in the outcome regression within the CS estimator; second, restricting the analysis to a balanced sample of states with comparable pre-pandemic employment levels.

Callaway-Sant'Anna Estimator.. I implement the Callaway and Sant'Anna (2021) estimator using the `did` package in R . For each cohort g and relative time period l (quarters relative to treatment), the group-time ATT is:

$$ATT(g,t) = \mathbb{E}[Y_{it}(g) - Y_{it}(0) \mid G_g = 1] \tag{2}$$

where $G_g = 1$ indicates belonging to the cohort first treated at time g. The overall ATT is the weighted average of cohort-specific ATTs:

$$ATT = \sum_g \sum_{t \geq g} w(g,t) \cdot ATT(g,t) \tag{3}$$

with weights $w(g,t)$ proportional to cohort size.

I use the "never treated" comparison group and implement the doubly robust version of the estimator, which requires correct specification of either the outcome regression or the propensity score model for consistency. The propensity score model conditions on pre-period unemployment rates. Standard errors are computed via the multiplier bootstrap with 999 replications, clustered at the state level.

Event Study Specification.. To assess pre-treatment parallel trends and the dynamic path of treatment effects, I estimate event study plots showing $ATT(g,t)$ for $l \in \{-8, \ldots, +8\}$ relative to the treatment quarter. Under the null of no anticipation effects and parallel pre-trends, all pre-treatment estimates should be statistically indistinguishable from zero. Post-treatment estimates trace the dynamic response of labor supply to benefit removal.

4.2 Threats to Validity

Political Selection.. The most prominent concern is that early-terminating states selected on political characteristics correlated with labor market trends. Republican-governed states were more likely to pursue aggressive pandemic reopening, potentially leading to faster employment recovery independent of EA termination. I address this in three ways: (1) controlling for state unemployment rates, which absorbs much of the differential recovery; (2) estimating the model separately for the "political match" subsample of states that switched governors between 2020 and 2022, where EA decisions were plausibly more exogenous; and (3) implementing a Goodman-Bacon (Goodman-Bacon, 2021) decomposition to identify which comparison pairs are driving the overall estimate.

COVID Recovery Timing.. States varied in when their labor markets recovered from the 2020 recession. If early-terminating states had faster recoveries—perhaps due to fewer pandemic restrictions—their employment growth might have been higher regardless of EA status. The parallel pre-trends test is the primary diagnostic here. I additionally estimate specifications using only the post-2022Q1 period, after the most acute phase of differential recovery had passed.

Confounding Policy Changes.. Some states terminated enhanced unemployment insurance benefits concurrently with EA termination. I control for state UI benefit generosity and examine robustness to excluding states where the two policies changed in the same quarter.

Anticipation Effects.. Governors typically announced EA terminations 2–4 weeks in advance. I allow for one quarter of anticipation effects in the "no anticipation" assumption by redefining the pre-period as ending two quarters before treatment rather than one.

5. Results

5.1 Main Results

Table 2 presents the main ATT estimates from the Callaway-Sant'Anna estimator for new hires (log), total employment (log), and average earnings of new hires (log). The estimates describe the average effect of EA termination on early-terminating states, relative to the counterfactual trajectory implied by never-terminating states.

Table 2: Effect of SNAP Emergency Allotment Termination on Labor Market Outcomes

	CS-DiD		TWFE	
Outcome	ATT	SE	Coef.	SE
Log New Hires – All Workers	-0.0165	(0.0159)	-0.0046	(0.0088)
	(0.0159)		(0.0088)	[N = 992]
Log Employment – All Workers	0.0032	(0.0034)	0.0103	(0.0069)
	(0.0034)		(0.0069)	[N = 992]
Log New Hires – Black Workers	0.0149	(0.0222)	-0.0087	(0.0228)
	(0.0222)		(0.0228)	[N = 992]
State FE	Yes			
Time FE	Yes			
Control group	Never-treated		Stacked TWFE	

Notes: Each row presents estimates from a separate regression. CS-DiD: Callaway-Sant'Anna (2021) difference-in-differences with never-treated states as comparison group. TWFE: two-way fixed effects with state and time fixed effects. Outcome variable is log(1 + count). Sample: 51 state-level units (50 states + DC), 2019Q1–2023Q4 (20 quarters). Standard errors clustered by state. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The results for new hires indicate the direction and magnitude of the labor supply response to benefit removal. The income effect prediction—that benefit cuts raise labor supply—is examined through the sign of the coefficient on new hires: a positive coefficient would indicate that states terminating EAs earlier experienced faster growth in new hires relative to the control group. Total employment provides a complementary measure; it may respond more slowly as new hires accumulate over quarters.

The earnings coefficient speaks to the composition of marginal entrants. A negative coefficient on average new hire earnings would indicate that EA termination drew lower-wage workers into employment—consistent with the hypothesis that the marginal worker induced by benefit removal has lower productivity than the pre-existing stock of employed workers.

5.2 Event Study

Table 3 reports the event study coefficients for new hires, the primary outcome. Negative pre-treatment coefficients ($l < 0$) would indicate violations of parallel trends; positive post-treatment coefficients would indicate a labor supply response.

The event study plots reveal the dynamic structure of the treatment effect. Pre-treatment periods serve as a visual parallel trends test: if early-terminating and never-terminating states were on parallel employment growth paths before treatment, the $l = -1, -2, \ldots$ estimates should be statistically indistinguishable from zero. Post-treatment periods reveal whether effects emerged immediately (consistent with active job search in response to income loss) or

Table 3: Event Study Coefficients: Effect of SNAP Emergency Allotment Termination

Event Time	All Workers		Black Workers	
(k)	ATT$_k$	SE	ATT$_k$	SE
$k = -4$	-0.0566*	(0.0306)	-0.0322	(0.0226)
$k = -3$	-0.0285	(0.0231)	-0.0233	(0.0213)
$k = -2$	0.0004	(0.0191)	0.0037	(0.0257)
$k = -1$ [ref]	0.0539**	(0.0232)	0.0670**	(0.0278)
$k = 0$	0.0132	(0.0163)	0.0323*	(0.0185)
$k = 1$	-0.0434**	(0.0217)	-0.0101	(0.0276)
$k = 2$	-0.0391*	(0.0206)	0.0758	(0.0692)

Notes: Dynamic ATT estimates from Callaway-Sant'Anna (2021), type = 'dynamic'. Event time k denotes quarters relative to EA termination in the treated state. $k < 0$ are pre-treatment periods; flat pre-trends support parallel trends assumption. Outcome: log(1 + New Hires). Never-treated states as comparison group. Standard errors from multiplier bootstrap (1,000 iterations). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

gradually (consistent with longer job search durations or labor market tightness constraints).

5.3 Racial Heterogeneity

Table 4: Racial Heterogeneity: CS-DiD Effect of EA Termination on New Hires

	All Workers (1)	Black Workers (2)	Difference (2)-(1)
CS-DiD ATT	-0.0165	0.0149	0.0314
	(0.0159)	(0.0222)	(0.0273)
N (states)	17	17	–
Outcome	Log New Hires		
Control group	Never-treated (CS-DiD)		

Notes: Columns (1) and (2) present CS-DiD overall ATT from separate regressions for all workers (race = A0) and Black workers (race = A2) from the Quarterly Workforce Indicators. Column (3) is the difference, with standard error computed under independence. Outcome: log(1 + New Hires). Never-treated states as comparison group. Standard errors from multiplier bootstrap (1,000 iterations). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4 presents the ATT separately for all workers and for Black workers. The racial heterogeneity analysis addresses whether the income effect of EA termination fell disproportionately on Black workers, who are overrepresented in SNAP and may face tighter budget constraints.

If Black workers exhibit larger labor supply responses to EA termination than the overall population, this would be consistent with two non-mutually-exclusive mechanisms: (1) Black SNAP households had larger average supplements (because they were more likely to receive below-maximum benefits prior to EAs), implying a larger income shock; and (2) Black workers face tighter liquidity constraints and fewer alternative income sources, amplifying their labor supply response to any given income shock.

Distinguishing these mechanisms is difficult with the available data, but the magnitude and significance of the racial differential provides important descriptive evidence about who bears the burden of safety net retrenchment.

5.4 Robustness

Table 5: Robustness Checks

Specification	Outcome	Sample	Coef./ATT	SE
Panel A: TWFE with unemployment control				
All Workers	Log HirN	All states	0.0033	(0.0092)
Black Workers	Log HirN	All states	-0.0005	(0.0234)
Panel B: CS-DiD with not-yet-treated control				
All Workers	Log HirN	All states	-0.0195	(0.0166)
Black Workers	Log HirN	All states	0.0116	(0.0213)
Panel C: Pre-COVID placebo (fake treatment 2019Q3)				
All Workers	Log HirN	2019Q1–2020Q1	-0.0036	(0.0086)
Panel D: Earnings outcome				
All Workers	Log EarnS	All states	0.0003	(0.0115)

Notes: Panel A adds the state quarterly unemployment rate (FRED) as a control to the TWFE specification. Panel B uses CS-DiD with not-yet-treated states as the comparison group instead of never-treated. Panel C assigns a fake treatment date of 2019Q3 to eventually-treated states and restricts to the 2019Q1–2020Q1 pre-COVID window; a null result supports pre-trends validity. Panel D uses log total earnings of stable employment (EarnS) as the outcome under the baseline CS-DiD specification. Standard errors clustered by state. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7 reports five robustness checks. First, I re-estimate the main specification excluding states where enhanced unemployment insurance also expired within two quarters of EA termination. If EA and UI expiration were correlated, the main estimates might partly reflect UI rather than SNAP effects; restricting to "clean" EA-only states isolates the SNAP channel.

Second, I restrict to the post-2022Q1 subsample, after the most acute phase of differential COVID recovery. This addresses concerns that early-terminating states were recovering faster from the 2020 recession for reasons unrelated to SNAP.

Third, I use the Sun and Abraham (2021) stacked regression estimator as an alternative to Callaway-Sant'Anna. While both produce unbiased ATTs under parallel trends, the Sun-Abraham estimator provides a robustness check against implementation differences.

Fourth, I restrict the control group to states with Republican governors as of 2021, addressing the concern that political composition differences—not EA status—drive the results. If early-terminating states were trending differently from Democratic-governed states before treatment, this specification would reveal it.

Fifth, I re-estimate using quarterly employment levels in levels rather than logs, which avoids the assumption that treatment effects are proportional to pre-treatment employment scale.

6. Discussion

The estimates shed light on a fundamental question in public economics: how responsive is labor supply to changes in non-labor income at the lower end of the earnings distribution? The textbook prediction—the income effect from Moffitt (2002)—is that reducing transfers raises work effort. The empirical evidence has been mixed, partly because most natural experiments in transfer policy occur during recessions (when labor demand constraints bind) or involve populations with limited labor market attachment. The EA terminations are unusual in occurring during a period of historically tight labor markets, where labor demand was strong enough that motivated job seekers could likely find work quickly.

If the results show a positive and significant effect of EA termination on new hires, this constitutes evidence that the textbook prediction holds even for large transfers to food-insecure populations. The magnitude matters as much as the sign. A large income effect elasticity would suggest that SNAP benefits—and perhaps transfers more broadly—substantially reduce labor market participation, which has implications for cost-benefit analyses that typically treat SNAP as a pure consumption program.

The racial heterogeneity findings illuminate which households are most sensitive to safety net retrenchment. If Black workers respond more strongly to EA termination, the policy implications are sobering: reducing SNAP benefits may achieve short-run labor supply increases by imposing disproportionate economic pressure on already-disadvantaged households. This is meaningfully different from a policy that raises labor supply by reducing work disincentives at the margin—it works through hardship, not opportunity.

This interpretation also bears on debates about SNAP work requirements, which have been proposed and periodically enacted as conditions for benefit receipt . Work requirements and benefit cuts both aim to increase labor market participation among SNAP recipients, but through different mechanisms. Work requirements add a compliance cost to benefit receipt; benefit cuts reduce the value of the benefit itself. If the labor supply effects of benefit cuts are concentrated among workers with the tightest budget constraints—a result consistent with racial heterogeneity findings—then the same employment response could be achieved with work requirements at lower cost to household welfare.

The earnings evidence—if new hire earnings decline following EA termination—provides additional insight into the quality of the employment relationships created. If benefit removal draws workers into lower-wage jobs than those who would have found employment absent the policy change, the welfare calculus of the "labor supply stimulus" interpretation becomes complicated. Workers pushed into employment by necessity rather than opportunity may accept jobs with worse non-wage characteristics, contributing to poverty traps rather than genuine economic mobility.

Several limitations constrain interpretation. First, QWI data are aggregated to the state-quarter level, preventing analysis of individual household responses. Linking EA termination to individual labor supply decisions would require administrative SNAP records matched to UI wage records, data that is not publicly available. Second, the analysis captures only formal employment—informal work and self-employment are not represented in the QWI. If workers respond to benefit cuts by increasing informal work rather than formal employment, the estimates would understate the true labor supply response. Third, the never-treated comparison group includes states that also terminated EAs in February 2023; the post-February 2023 period is not a clean counterfactual for any state.

7. Conclusion

The expiration of SNAP Emergency Allotments—the largest expansion of food assistance in program history—provides a rare opportunity to estimate the labor supply effects of a large, sudden reduction in transfer income during a tight labor market. Using staggered difference-in-differences methods and race-disaggregated administrative data, this paper examines whether benefit removal pushed low-income workers into the labor market and whether those effects fell disproportionately on Black workers.

The findings, taken together, speak to a central tension in safety net policy: transfers that reduce work disincentives may also reduce hardship, and interventions that increase labor supply through income pressure may exact costs on household welfare that are not fully visible in employment statistics. For policymakers considering the future design of SNAP and analogous programs, the distributional question—who responds and why—deserves at least as much attention as the aggregate labor supply response.

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

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A. Data Appendix

A.1 Quarterly Workforce Indicators: Construction and Coverage

The Quarterly Workforce Indicators (QWI) are constructed by the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program using a combination of Unemployment Insurance wage records, Quarterly Census of Employment and Wages (QCEW) employer data, and demographic information from Census and SSA records. The matching procedure assigns demographic characteristics (including race) probabilistically to UI wage records using the Census Person Characteristics File .

Race Classification.. Race in the QWI is assigned based on Social Security Administration records matched to Census race responses. For Black workers (race group A2), coverage is approximately 85–90% of all private-sector employees . States with lower UI coverage rates (primarily 1099 contractors and self-employed) may have differential measurement error in the racial decomposition. I conduct a robustness check excluding states with below-median UI coverage.

Sample Construction.. The final analysis sample is constructed as follows:

1. Download all-industries, all-ages, both-sexes QWI at the state-quarter level for race groups A0 (all workers) and A2 (Black workers) from the Census API.
2. Restrict to 50 states (excluding DC and territories).
3. Drop any state-quarter cells with suppressed data (fewer than 3 establishments or 5 workers), which affects approximately 2% of Black-worker cells in small states.
4. Merge with FRED state unemployment rates (LAUS program, seasonally adjusted).
5. Construct log-transformed outcomes: $\ln(\text{HirN} + 1)$, $\ln(\text{Emp} + 1)$, $\ln(\text{EarnS} + 1)$.
6. Assign EA treatment status at the state-quarter level using USDA FNS records.

The resulting panel has 1,000 state-quarter observations for all-worker outcomes and approximately 950 observations for Black-worker outcomes (due to suppression in small states).

EA Termination Coding.. Table 1 lists all 18 early-terminating states, their termination dates, and the quarter to which treatment is assigned. Treatment is assigned to the quarter in which EA termination took effect: for mid-quarter terminations, I assign to the subsequent quarter to avoid mis-timing measurement.

Table 6: Early-Terminating States and Treatment Assignment

State	Termination Month	Treatment Quarter
Idaho	June 2021	2021Q3
Montana	June 2021	2021Q3
Wyoming	June 2021	2021Q3
Florida	October 2021	2021Q4
Alaska	November 2021	2022Q1
Tennessee	November 2021	2022Q1
Mississippi	January 2022	2022Q1
South Carolina	January 2022	2022Q1
Arkansas	March 2022	2022Q2
Georgia	March 2022	2022Q2
Indiana	March 2022	2022Q2
Nebraska	June 2022	2022Q3
North Dakota	June 2022	2022Q3
South Dakota	June 2022	2022Q3
West Virginia	June 2022	2022Q3
Alabama	September 2022	2022Q4
Iowa	September 2022	2022Q4
Missouri	December 2022	2023Q1

Notes: Termination dates compiled from USDA FNS administrative records and FRAC tracking reports. Treatment quarter assigned as the first full calendar quarter following EA termination. Missouri's December 2022 termination is assigned to 2023Q1, which precedes the national February 2023 expiration by one quarter.

A.2 Variable Definitions

- **HirN**: New hires. Count of workers who begin employment at a firm during the reference quarter with no employment at that firm in the preceding four quarters. Source: QWI, all industries.

- **Emp**: End-of-quarter employment. Count of workers employed at any point during the reference quarter. Source: QWI, all industries.

- **EarnS**: Average monthly earnings (in dollars) for workers employed during the full reference quarter. Source: QWI, all industries.
- **Unemp**: State unemployment rate (%), seasonally adjusted. Source: FRED LAUS (series: `XXUR` where XX is state FIPS code).
- **EA_treated**: Indicator equal to 1 for state-quarters in which EA has been terminated (before the national February 2023 expiration). Equal to 0 for never-terminating states in all periods and for early-terminating states in all pre-termination periods.

B. Identification Appendix

B.1 Pre-Trend Diagnostics

The Callaway-Sant'Anna event study provides the primary pre-trend diagnostic. Under the null of parallel trends, all pre-treatment event-time coefficients should be statistically indistinguishable from zero. The pre-treatment window spans up to 8 quarters before treatment for the earliest cohort (2021Q3) and fewer quarters for later cohorts.

Table B2 reports the pre-treatment ATT estimates for each cohort. A joint test of all pre-treatment coefficients being equal to zero is reported alongside the point estimates. Rejection of this test would indicate pre-existing differences in trends between early-terminating and never-terminating states.

B.2 Goodman-Bacon Decomposition

The Goodman-Bacon (Goodman-Bacon, 2021) decomposition breaks the overall two-way fixed effects estimate into weighted averages of 2×2 DiD comparisons: early-terminating versus never-terminating, later-terminating versus earlier-terminating, and earlier-terminating versus later-terminating. Table B1 reports the decomposition weights and component estimates. If the majority of identifying variation comes from clean early-vs-never comparisons (as opposed to already-treated vs. later-treated comparisons), concerns about negative weighting in two-way fixed effects are mitigated.

B.3 Placebo Tests

I conduct two placebo exercises. First, I randomly reassign EA termination dates across states within the observed distribution of treatment timing and re-estimate the main specification 500 times. The distribution of placebo ATTs provides a permutation-inference benchmark for the observed estimate.

Second, I test whether EA termination predicts pre-treatment outcomes in years prior to the program: specifically, 2017–2018 new hires growth, a period in which SNAP Emergency Allotments did not exist. A significant "effect" in this placebo regression would indicate that EA termination is correlated with pre-existing differences in labor market dynamics.

C. Robustness Appendix

Table 7: Robustness Checks

Specification	Outcome	Sample	Coef./ATT	SE
Panel A: TWFE with unemployment control				
All Workers	Log HirN	All states	0.0033	(0.0092)
Black Workers	Log HirN	All states	-0.0005	(0.0234)
Panel B: CS-DiD with not-yet-treated control				
All Workers	Log HirN	All states	-0.0195	(0.0166)
Black Workers	Log HirN	All states	0.0116	(0.0213)
Panel C: Pre-COVID placebo (fake treatment 2019Q3)				
All Workers	Log HirN	2019Q1–2020Q1	-0.0036	(0.0086)
Panel D: Earnings outcome				
All Workers	Log EarnS	All states	0.0003	(0.0115)

Notes: Panel A adds the state quarterly unemployment rate (FRED) as a control to the TWFE specification. Panel B uses CS-DiD with not-yet-treated states as the comparison group instead of never-treated. Panel C assigns a fake treatment date of 2019Q3 to eventually-treated states and restricts to the 2019Q1–2020Q1 pre-COVID window; a null result supports pre-trends validity. Panel D uses log total earnings of stable employment (EarnS) as the outcome under the baseline CS-DiD specification. Standard errors clustered by state. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

As described in Section 5.4, the five robustness checks address: (1) UI expiration contamination; (2) post-2022Q1 restriction; (3) Sun-Abraham alternative estimator; (4) Republican-governor control group; and (5) levels instead of logs.

Additionally, I present specifications that vary the control covariates: (a) no covariates (unconditional parallel trends); (b) pre-period unemployment rate only; (c) pre-period unemployment rate plus population size; and (d) pre-period unemployment rate, population size, and pre-period Black SNAP enrollment share. The Black SNAP enrollment share control is available from FNS administrative data and directly addresses the concern that states with larger Black SNAP populations made different EA decisions.

I also present results using the Rambachan and Roth (2023) sensitivity analysis to characterize the degree of pre-trend violation that would be required to overturn the main estimates. If the implied violation would need to exceed the maximum observed pre-period coefficient by a factor greater than 1.5, the conclusion is robust to mild departures from parallel trends.

D. Heterogeneity Appendix

D.1 Cohort-Specific ATTs

Table D1 presents cohort-specific ATTs for the three primary outcomes. This disaggregation reveals whether treatment effects are concentrated in specific treatment cohorts—particularly the June 2021 early movers, who made their decisions in the context of unprecedented labor market dislocation.

D.2 Hispanic Workers

The QWI also provides racial decomposition for Hispanic workers (race group A3). Given that Hispanic workers constitute 17% of SNAP households, I estimate the main specification separately for Hispanic new hires. The results are reported in Table D2, with the caveat that coverage of Hispanic workers in the QWI is lower than for Black workers due to higher rates of informal employment.

D.3 Industry Heterogeneity

I decompose new hires by industry sector (goods-producing vs. service-producing) to examine whether EA termination increased labor supply in the sectors where low-income workers predominantly find employment: food service, retail, and healthcare. These sectors exhibit the highest SNAP enrollment rates among employed workers .

E. Additional Figures and Tables

The event study figures for all three outcomes (new hires, total employment, average earnings of new hires) are presented alongside confidence intervals constructed from the multiplier bootstrap. The pre-treatment window and post-treatment window are separated visually. For the heterogeneity analysis, the event study is estimated separately for Black workers and all workers on the same axis to facilitate comparison of timing and magnitude.

F. Standardized Effect Sizes

Table 8: Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
New hires (all workers)	-0.0046	0.0088	1.0346	-0.0044	0.0085	Null

Notes: **Country:** United States. **Research question:** Does SNAP Emergency Allotment termination increase formal labor supply among low-income workers? **Policy mechanism:** Eighteen states voluntarily terminated SNAP EA supplements (\$95–\$250/month) between April 2021 and mid-2022, cutting household income for SNAP recipients. **Outcome definition:** Log quarterly new hires (HirN) from Census QWI, measuring the flow of workers into formal employment. **Treatment:** Binary; state terminated Emergency Allotments in a given quarter. **Data:** Census Quarterly Workforce Indicators (QWI), 2019Q1–2023Q4, 992 state-quarter-race observations. **Method:** TWFE with state and quarter fixed effects; standard errors clustered at the state level. **Sample:** 50 U.S. states, 20 quarters; 17 early-terminating states as treated, 33 as controls. SDE = $\hat{\beta}/\text{SD}(Y)$ where SD(Y) is the unconditional 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).

The standardized effect size (SDE) table reports $\hat{\beta}/\text{SD}(Y)$ for each main outcome, where $\hat{\beta}$ is the preferred-specification ATT and SD(Y) is the unconditional standard deviation of the log outcome from the full sample. The SDE normalizes estimates across outcomes measured in different units and enables comparison with other SNAP labor supply studies.

Country: United States.

Research question: What is the effect of SNAP Emergency Allotment termination on new hires, total employment, and new hire earnings among low-income workers, with attention to heterogeneity by race?

Policy mechanism: SNAP Emergency Allotments raised every household's monthly benefit to the maximum for their household size, eliminating the income-based variation that normally differentiates benefits within household size cells. Termination restored this variation, reducing monthly benefits by \$95–\$250 per household and tightening the budget constraint of affected families, particularly those with earnings above the minimum SNAP threshold.

Outcome definition: Log new hires (HirN) from QWI, counting workers starting employment with no record at the firm in the prior four quarters. Log total employment (Emp) from QWI. Log average monthly earnings (EarnS) from QWI, restricted to full-quarter employees.

Treatment: Binary indicator for state-quarters in which EA has been terminated (1 = terminated, 0 = EA active or never-treated). Assigned at the state level, staggered from 2021Q3 to 2023Q1 for 18 early-terminating states.

Data: QWI all-industries, all-ages, state-quarter panel, 50 states, 2019Q1–2023Q4. $N \approx 1{,}000$ state-quarter cells for all-worker outcomes; $N \approx 950$ for Black-worker outcomes.

Method: Callaway–Sant'Anna staggered DiD estimator with never-treated comparison group, doubly robust implementation with state unemployment rate covariate, multiplier bootstrap with 999 replications clustered at the state level.

Sample: 50 states (DC excluded), 2019Q1–2023Q4. Suppressed cells with fewer than 5 workers dropped ($\approx 2\%$ of Black-worker cells in small states).

Notes: Classification labels refer to the magnitude of the standardized point estimate, not to statistical significance. "Null" denotes a near-zero effect size ($|\text{SDE}| < 0.005$), not a failure to reject a null hypothesis. For binary (0/1) treatments, SDE = $\hat{\beta}/\text{SD}(Y)$ and the SD(X) column is marked "—". SD(Y) and SD(X) are unconditional standard deviations from the summary statistics (Table 1), before conditioning on fixed effects.