The Retail Floor: SNAP Emergency Allotment Expiration
and the Resilience of Food Retail Infrastructure

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Abstract

The end of SNAP Emergency Allotments removed $46 billion in annual food purchasing power from 41 million American households. Policymakers warned of a "hunger cliff" — but did the demand shock also trigger a retail cliff? Using the universe of 703,441 SNAP-authorized retailers and staggered state-level EA expirations across 18 early opt-out states, I find no evidence that EA expiration increased retailer exit rates. Callaway-Sant'Anna estimates show convenience store exit rates *declined* by 4.2 per 1,000 per quarter after EA expiration, while supermarket exit rates were unchanged. A store-type triple-difference confirms that SNAP-dependent retailers were not differentially harmed. The food retail infrastructure proved surprisingly resilient to one of the largest demand shocks in the history of the U.S. safety net.

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1. Introduction

In March 2023, 41 million Americans lost an average of \$95 per month in food benefits overnight. The expiration of SNAP Emergency Allotments — pandemic-era supplements that had boosted benefits by 30–40% since April 2020 — removed approximately \$46 billion in annual purchasing power from households who spent the vast majority of these funds at local food retailers (Rosenbaum and Keith-Jennings, 2023). Health researchers documented a sharp increase in food insufficiency: an estimated 2 million additional Americans reported not having enough to eat (Lakhani et al., 2024). Grocery spending fell 12% in affected states (Bitler et al., 2023a). The policy world called it the "hunger cliff."

But the hunger cliff narrative focused exclusively on the demand side — on households losing purchasing power. The supply side remained unexamined. If SNAP benefits represent a meaningful share of retailer revenue, then a sudden 30–40% reduction in benefit flows could push marginal retailers below viability, triggering store closures. The resulting loss of physical food access would compound the direct nutritional harm: households would face both lower benefits *and* fewer places to redeem them. This paper tests whether the hunger cliff also created a *retail cliff*.

The answer is no. Using the universe of SNAP-authorized retailers from the USDA Food and Nutrition Service's historical database — 703,441 stores with precise authorization and deauthorization dates spanning 2005–2025 — I exploit the staggered expiration of Emergency Allotments across 18 early opt-out states to estimate the effect of EA termination on retailer exit rates. The identification strategy relies on the fact that opt-out decisions were made by Republican governors for political rather than economic reasons, creating plausibly exogenous variation in the timing of a massive demand shock to the food retail sector.

The results are surprising. Callaway-Sant'Anna estimates for convenience stores — the dominant SNAP store type (53% of authorized retailers) and the most SNAP-revenue-dependent category — show an average treatment effect of -4.19 per 1,000 stores per quarter (SE = 3.04), representing a 15% *decline* relative to the pre-treatment mean exit rate of 28.5 per 1,000. The dynamic event-study aggregation yields -5.08 (SE = 2.42, $p = 0.036$), with pre-treatment coefficients centered around zero and a gradual decline in exit rates after EA expiration. Supermarket exit rates show no significant change (+1.18, SE = 1.65). A store-type triple-difference comparing convenience stores to supermarkets confirms that SNAP-dependent retailers were not differentially harmed by EA expiration.

These findings speak to a growing literature on the infrastructure of safety net delivery. The participation literature has focused almost exclusively on demand-side barriers: information costs (Finkelstein and Notowidigdo, 2019), transaction costs (Currie, 2006), and stigma

(Klerman and Danielson, 2012). Yet SNAP benefits can only be redeemed at authorized retailers, making the physical retail network a *supply-side* constraint on effective program access. If the retail network contracted in response to demand shocks, it would create a perverse feedback loop: benefit cuts reduce retailer viability, retailer exits reduce food access, and reduced access further depresses participation — even among households whose benefits were not directly cut. My results rule out this "doom loop" for the largest demand shock in SNAP's history.

The paper also contributes to the food desert literature. Allcott et al. (2019) showed that food access explains at most 10% of nutritional inequality, suggesting demand-side preferences dominate. My results complement this finding from the supply side: even a massive, sudden reduction in subsidized demand did not cause significant retailer exit, suggesting that SNAP revenue represents a small enough share of total revenue that the retail network can absorb large benefit fluctuations. This is consistent with evidence that convenience stores — while authorized to accept SNAP — earn the majority of their revenue from non-SNAP products including tobacco, beverages, and lottery tickets.

The contribution to the literature on pandemic fiscal policy is direct. Bitler et al. (2023b) and Schanzenbach (2023) document the demand-side effects of pandemic safety net expansions. East et al. (2023) study labor supply responses to the end of enhanced unemployment benefits. This paper adds the supply-side dimension: when pandemic-era fiscal supports are withdrawn, does the commercial infrastructure that channeled those supports survive? The answer is yes — and this resilience has implications for the design of temporary benefit expansions. Policymakers can temporarily increase SNAP generosity without permanently distorting the retail landscape.

The rest of the paper proceeds as follows. Section 2 describes the institutional background of SNAP Emergency Allotments and the staggered opt-out process. Section 3 describes the data. Section 4 presents the empirical strategy. Section 5 reports the results, including robustness checks and heterogeneity analysis. Section 6 discusses mechanisms and implications. Section 7 concludes.

2. Institutional Background

SNAP and the Retail Network. The Supplemental Nutrition Assistance Program provides food benefits to approximately 41 million Americans in 21 million households, distributing roughly $114 billion annually through Electronic Benefits Transfer (EBT) cards redeemable at authorized retailers (USDA Food and Nutrition Service, 2023). As of 2024, approximately 260,000 retailers were authorized to accept SNAP, dominated by convenience stores

(53%), small grocery stores (9%), and supermarkets (11%). Retailers must apply for SNAP authorization and meet stocking requirements specifying minimum varieties of staple foods.

Emergency Allotments. The Families First Coronavirus Response Act of 2020 authorized states to issue Emergency Allotments (EA) — supplemental SNAP benefits that brought all households to at least the maximum benefit level for their household size. For a family of four, the maximum monthly benefit was $835 (FY2022), so a household already receiving $600 would get an additional $235 in EA. On average, EA added approximately $95 per household per month, boosting total SNAP outlays by roughly 30–40% and channeling an additional $46 billion annually through the SNAP retail network (Bitler et al., 2023a).

Staggered Expiration. EA issuance required a state or federal public health emergency declaration. Beginning in April 2021, eighteen states terminated their emergency declarations, ending EA for their SNAP populations. Idaho was first (April 2021), followed by a cluster of ten states in July–August 2021 (Montana, Nebraska, South Dakota, Wyoming, Tennessee, Florida, Iowa, Mississippi, Missouri, and Indiana). Alaska and Arizona followed in early 2022, and Arkansas, Georgia, and Kentucky in mid-2022. South Carolina was the last early opt-out in January 2023. The Consolidated Appropriations Act of 2023 ended EA nationwide, with the remaining 32 states plus DC issuing their final EA in February 2023.

The opt-out decisions were driven by political considerations — all 18 early opt-out states had Republican governors, and the decisions were typically announced alongside broader messaging about ending pandemic-era government programs. This political selection, while non-random, is plausibly orthogonal to local retail market conditions. Governors did not end EA because convenience stores in their states were unusually healthy or struggling; they ended EA as part of a broader political project of rolling back pandemic-era expansions.

3. Data

The primary dataset is the USDA Food and Nutrition Service's SNAP Retailer Historical Database, a comprehensive panel of all retailers ever authorized to accept SNAP benefits between 2005 and 2025. The database contains 703,441 unique store records with store name, type, address, geographic coordinates, authorization date, and — crucially — end date (deauthorization date) for stores that exited the SNAP program.

I classify stores into four categories: convenience stores (375,809 stores, 53%), small grocery stores (63,193, 9%), supermarkets including large grocery stores and super stores (79,266, 11%), and other SNAP retailers (184,173, 26%). Convenience stores are the most relevant store type for this analysis because they combine high SNAP authorization rates with relatively thin margins, making them most vulnerable to demand shocks.

I construct a balanced panel of 51 states (including DC) $\times$ 24 quarters (2019Q1–2024Q4) $\times$ 4 store types = 4,896 observations. For each state-quarter-type cell, I compute: (a) the number of active authorized stores at the beginning of the quarter, (b) the number of deauthorizations (exits) during the quarter, and (c) the exit rate per 1,000 active stores.

An important measurement caveat: SNAP deauthorization captures both business closures and voluntary withdrawal from the SNAP program. Some stores may lose authorization through administrative actions (failure to comply with stocking requirements) rather than economic failure. The outcome therefore captures the broader margin of SNAP retail infrastructure availability — whether through closure, voluntary exit, or administrative removal — rather than strictly measuring business survival.

Treatment timing comes from official USDA and CBPP documentation of EA expiration dates (Rosenbaum and Keith-Jennings, 2023; Lakhani et al., 2024). Each state is assigned to the first quarter in which EA was no longer available.

Table 1: Summary Statistics: SNAP Retailer Panel, 2019Q1–2024Q4

	Mean	SD	N (state-quarters)
Panel A: Convenience Stores			
Exit rate (per 1,000)	28.5	14.9	732
Active stores	2261	2435	
Quarterly exits	71.1	91.1	
Panel B: Small Grocery Stores			
Exit rate (per 1,000)	48.7	35.3	732
Active stores	164	267	
Quarterly exits	8.8	17.0	
Panel C: Supermarkets			
Exit rate (per 1,000)	8.4	8.6	732
Active stores	867	947	
Quarterly exits	7.2	10.2	

Notes: Pre-treatment (before EA expiration) statistics for SNAP-authorized retailers. Exit rate is quarterly deauthorizations per 1,000 active stores. Active stores counted at start of quarter. Data from USDA FNS SNAP Retailer Historical Database, 2005–2025.

Table 1 reports pre-treatment summary statistics. The mean quarterly exit rate for convenience stores is 28.5 per 1,000 (roughly 2.85% per quarter), with a standard deviation of 14.9. Pre-treatment exit rates are similar across early opt-out states (27.7) and non-opt-out states (28.8), supporting the parallel trends assumption.

4. Empirical Strategy

I exploit the staggered expiration of Emergency Allotments across states using the Callaway and Sant'Anna (2021) difference-in-differences estimator, which produces heterogeneity-robust estimates under staggered treatment timing. The estimand is the average treatment effect on the treated (ATT): the change in exit rates caused by EA expiration, comparing treated states to not-yet-treated states.

For each treatment cohort g (the quarter in which EA expired) and time period t, I estimate:

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

where $Y_{it}(g)$ is the potential exit rate for state i in period t given treatment at g, $Y_{it}(\infty)$ is the potential outcome under no treatment, and G_i is the cohort indicator. I use doubly-robust estimation with not-yet-treated states as the comparison group, following Callaway and Sant'Anna (2021)'s recommendation for designs where all units are eventually treated.

The key identifying assumption is parallel trends: absent EA expiration, exit rates in early opt-out states would have followed the same trajectory as in states still receiving EA. I assess this with pre-treatment event-study coefficients, which should be centered around zero. The political nature of opt-out decisions supports this assumption: governors ended EA to make an ideological statement, not in response to retail market conditions in their states.

As a complement, I estimate a store-type triple-difference:

$$Y_{ist} = \alpha_s + \gamma_t + \delta_k + \beta_1 \text{Treated}_{st} + \beta_2(\text{Treated}_{st} \times \text{Convenience}_{ik}) + \varepsilon_{ist} \tag{2}$$

where α_s, γ_t, and δ_k are state, time, and store-type fixed effects. The coefficient β_2 captures the *differential* effect of EA expiration on SNAP-dependent convenience stores relative to supermarkets — a within-state, within-time placebo that absorbs any state-level confounds affecting all store types equally. If EA expiration specifically harms stores that depend on SNAP revenue, β_2 should be positive (higher exit rates for convenience stores).

I also estimate standard TWFE as a diagnostic and conduct a Goodman-Bacon decomposition (Goodman-Bacon, 2021) to verify that estimates are not driven by problematic timing comparisons.

Threats to Validity. The main concern is that EA opt-out states differ systematically in ways correlated with retail market dynamics. Republican-governed states may have experienced faster economic recovery, stronger labor markets, or different COVID trajectories. The store-type DDD addresses this directly: state-level confounds affect convenience stores and supermarkets equally, so β_2 is identified even if state-level trends differ. Additionally, the pre-treatment event study provides a direct test of differential trends.

An ideal placebo would compare SNAP-authorized retailers to non-SNAP businesses in the same tracts, since non-SNAP retailers are unaffected by EA expiration. The SNAP Historical Database does not include non-SNAP establishments, so I cannot implement this test directly. The store-type DDD (convenience vs. supermarkets within the SNAP network) serves as a partial substitute, exploiting differential SNAP revenue dependence across authorized store types.

5. Results

5.1 Main Results

Table 2 presents the main results. Column (1) reports the Callaway-Sant'Anna ATT for convenience store exit rates. The point estimate is -4.188 per 1,000 per quarter with a standard error of 3.038, representing a 15% decline relative to the pre-treatment mean of 28.47. While the simple aggregation falls short of conventional significance ($p = 0.17$), the dynamic aggregation — which weights by the number of post-treatment periods — yields -5.076 (SE $= 2.417$, $p = 0.036$).

Column (2) shows the TWFE estimate of -3.112 (SE $= 1.471$, $p = 0.039$), confirming the direction and magnitude. Column (3) reports the triple-difference: the interaction term (Treated $\times$ Convenience) is -1.589 (SE $= 0.822$, $p = 0.059$), indicating that convenience stores experienced a *larger* decline in exit rates than supermarkets after EA expiration. This is the opposite of what the "retail cliff" hypothesis predicts.

Event Study. Table 3 reports the full event-study coefficients from the Callaway-Sant'Anna dynamic aggregation. Pre-treatment estimates (event times -8 through -1) are centered near zero with no systematic pattern, supporting the parallel trends assumption. Post-treatment, exit rates decline gradually: -2.1 at event time 0, -3.5 at $+1$, -5.2 at $+2$, stabilizing around -5 per 1,000 through event time $+5$, with the largest effect at $+6$ (-9.7, $p < 0.01$).

Statistical Power. A post-hoc power calculation indicates a minimum detectable effect (MDE) of 8.5 per 1,000 at 80% power ($\alpha = 0.05$), corresponding to 30% of the pre-treatment mean exit rate. The data can therefore rule out increases of economically meaningful magnitude, though modest positive effects (below 30% of baseline) remain within the confidence interval. The upper bound of the 95% CI for the CS ATT is $+1.77$ per 1,000, or 6% of the pre-treatment mean — meaning even the most unfavorable interpretation of the results

Table 2: Effect of SNAP Emergency Allotment Expiration on Convenience Store Exit Rates

	(1) CS ATT	(2) TWFE	(3) DDD
EA Expired	-4.188	-3.112**	-1.263
	(3.038)	(1.471)	(1.089)
	[-10.143, 1.767]		
EA Expired × Convenience			-1.589*
			(0.822)
Observations	1,224	1,224	2,448
States	51	51	51
Quarters	24	24	24
Estimator	Callaway-Sant'Anna	TWFE	TWFE
Control group	Not-yet-treated	—	—
Clustering	State	State	State
State FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Store-type FE	No	No	Yes
Pre-treatment mean	28.47		

Notes: Dependent variable is quarterly exit rate (deauthorizations per 1,000 active stores). Column (1) estimates the average treatment effect on the treated using Callaway and Sant'Anna (2021) with doubly-robust estimation and not-yet-treated states as controls. Column (2) reports two-way fixed effects. Column (3) is a triple-difference comparing convenience stores (high SNAP revenue share) to supermarkets (low share); the interaction term captures the differential effect. Standard errors clustered at the state level in parentheses. 95% confidence interval in brackets (Column 1). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Event-Study Estimates: Convenience Store Exit Rates

Event time	Estimate	SE	95% CI
-8	-15.044	(10.048)	[-34.74, 4.65]
-7	1.467	(2.225)	[-2.89, 5.83]
-6	3.687	(4.083)	[-4.32, 11.69]
-5	-1.421	(3.064)	[-7.43, 4.59]
-4	0.751	(2.656)	[-4.45, 5.96]
-3	0.479	(4.255)	[-7.86, 8.82]
-2	-4.472	(3.687)	[-11.70, 2.75]
-1	5.431	(3.989)	[-2.39, 13.25]
+0	-2.057	(2.720)	[-7.39, 3.27]
+1	-3.472	(3.579)	[-10.49, 3.54]
+2	-5.191	(3.261)	[-11.58, 1.20]
+3	-5.111	(3.207)	[-11.40, 1.17]
+4	-4.210	(4.770)	[-13.56, 5.14]
+5	-5.779*	(3.109)	[-11.87, 0.31]
+6	-9.709***	(2.241)	[-14.10, -5.32]

Notes: Callaway and Sant'Anna (2021) dynamic ATT estimates for convenience store exit rates (per 1,000 active stores). Event time 0 is the first quarter after EA expiration. Pre-treatment periods (< 0) test the parallel trends assumption. Standard errors in parentheses; 95% confidence intervals in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

implies an effect far smaller than what a "retail cliff" narrative would suggest.

5.2 Heterogeneity by Store Type

Table 4: Effects by Store Type: Callaway-Sant'Anna ATT Estimates

	Convenience	Small Grocery	Supermarket
EA Expired (CS ATT)	-4.188	7.142	1.184
	(2.777)	(10.900)	(1.650)
	[-9.63, 1.26]	[-14.22, 28.51]	[-2.05, 4.42]
Pre-treatment mean	28.47	48.72	8.39
Pre-treatment SD	14.93	35.27	8.57
Observations	1,224	1,224	1,224
States	51	51	51
Control group	Not-yet-treated	Not-yet-treated	Not-yet-treated

Notes: Callaway and Sant'Anna (2021) ATT estimates with doubly-robust estimation. Dependent variable is quarterly exit rate (deauthorizations per 1,000 active stores) by store type. Standard errors (in parentheses) and 95% confidence intervals (in brackets). Convenience stores have the highest SNAP revenue dependence; supermarkets the lowest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 decomposes the effect by store type. The negative effect is concentrated entirely in convenience stores (-4.19, SE = 2.78). Small grocery stores show a positive but imprecise estimate (+7.14, SE = 10.90), and supermarkets show essentially zero effect (+1.18, SE = 1.65). The pattern is consistent with a market-thinning mechanism: EA expiration reduced the revenue available to support marginal stores, which deterred *new entry* rather than triggering exits among incumbents.

5.3 Robustness

Table 5 reports robustness checks. Panel A shows that the effect is similar for early opt-out states alone (-3.76, SE = 2.11, $p = 0.09$) and that there is no differential effect between early and late opt-outs (interaction: 0.04, SE = 1.40). Panel B examines alternative outcomes: new authorization rates also declined (-2.69, SE = 1.81), while the net change rate (entries minus exits) was essentially zero (0.42, SE = 1.47). This decomposition reveals the mechanism: EA expiration slowed *both* exits and entries, reducing market churn rather than selectively destroying incumbents. The absolute number of exits declined by approximately 4.2 stores per state per quarter ($p = 0.087$).

Table 5: Robustness Checks

	Estimate	SE
Panel A: Subsample		
Early opt-out states only (18 states)	-3.763*	(2.114)
Treated × Early interaction	0.036	(1.400)
Panel B: Alternative Outcomes		
Entry rate (new authorizations per 1,000)	-2.691	(1.809)
Net change rate (entries − exits)	0.420	(1.469)
Exit count (level)	-4.208*	(2.412)
Panel C: Bacon Decomposition		
Earlier vs. later treated (weight: 0.560)	-2.961	
Later vs. earlier treated (weight: 0.440)	-3.304	

Notes: All specifications include state and quarter fixed effects with standard errors clustered at the state level. Panel A restricts to 18 early opt-out states (Column 1) and tests for differential effects between early and late opt-outs (Column 2). Panel B uses alternative outcomes for convenience stores. Panel C reports the Goodman-Bacon (2021) decomposition of the TWFE estimate. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Panel C reports the Bacon decomposition. The TWFE estimate is driven roughly equally by earlier-versus-later comparisons (weight: 0.56, estimate: -2.96) and later-versus-earlier comparisons (weight: 0.44, estimate: -3.30). Both types produce similar negative estimates, ruling out concerns about contamination from problematic timing comparisons.

6. Discussion

The central finding is a non-finding: the largest demand shock in SNAP's history did not destabilize the food retail infrastructure. Three mechanisms may explain this resilience.

First, *SNAP revenue shares are modest.* Even for convenience stores — the most SNAP-dependent category — EBT redemptions represent a minority of total revenue. The average convenience store earns the bulk of its revenue from tobacco, beverages, lottery, and prepared foods. A 30–40% reduction in SNAP benefits, while meaningful to households, may represent only a 5–10% revenue shock to the store, insufficient to push viable businesses below break-even.

Second, *countervailing trends offset the demand shock.* The EA expiration occurred during a period of rising food prices and general economic recovery. Higher food prices may have increased per-transaction revenue for remaining SNAP purchases, partially cushioning the benefit reduction.

Third, the robustness analysis offers a suggestive but not causally established pattern: new store authorizations declined by a similar magnitude as exits, leaving the net stock essentially unchanged. One interpretation is that EA expiration reduced market churn — fewer entries and fewer exits — rather than selectively destroying incumbents. However, the entry-rate decline is estimated with a standard TWFE specification rather than the full CS framework, and the causal link between EA expiration and entry decisions requires further investigation.

These findings have direct implications for the current policy debate. The OBBB Act proposed $187 billion in SNAP cuts over a decade. Advocates warned that such cuts would devastate food access by triggering retailer exits in underserved communities. My results suggest this concern is overstated for the retail infrastructure margin: even the sudden, complete elimination of EA — a larger per-household shock than most proposed cuts — did not cause measurable retailer destruction. The binding constraint on food access in underserved communities may be structural (zoning, costs, consumer density) rather than revenue-driven.

7. Conclusion

The hunger cliff was real. Millions of Americans experienced food hardship when Emergency Allotments expired. But the retail cliff never materialized. The food retail network proved resilient to a $46 billion demand shock, absorbing the loss of pandemic-era SNAP supplements without measurable increases in store closures. The infrastructure through which America's largest anti-poverty program delivers benefits is more durable than the crisis narrative suggests — a finding that should inform the design of temporary benefit expansions and the evaluation of proposed benefit cuts.

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

SNAP Retailer Historical Database. The USDA Food and Nutrition Service maintains a comprehensive database of all retailers ever authorized to accept SNAP benefits. The dataset is publicly available as a bulk download from `https://www.fns.usda.gov/snap/retailer-locator/data` and covers the period 2005–2025. Each record includes: store name, store type (15 categories), full street address, latitude and longitude, authorization date, and end date (for stores that were deauthorized).

Store type classifications follow USDA FNS definitions: "Convenience Store" (single-aisle format, limited selection), "Small Grocery Store" (moderate selection, typically independently owned), "Medium Grocery Store," "Large Grocery Store," "Supermarket" (full-service, high-volume), and "Super Store" (supercenter format). I consolidate large grocery stores, supermarkets, and super stores into a single "supermarket" category. The "other" category includes combination grocery/other stores, specialty stores (bakery, meat, seafood, fruits/vegetables), delivery routes, farmers' markets, food buying co-ops, military commissaries, and wholesalers.

Emergency Allotment Dates. EA termination dates are compiled from USDA FNS administrative records, the Center on Budget and Policy Priorities state-by-state tracking (Rosenbaum and Keith-Jennings, 2023), and Lakhani et al. (2024). All 18 early opt-out states are identified and assigned their termination quarter based on the last month in which EA was issued in that state.

B. Standardized Effect Sizes

Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Conv. store exit rate	-4.188	2.777	14.93	-0.280	0.186	Large negative
Small grocery exit rate	7.142	10.900	35.27	0.202	0.309	Large positive
Supermarket exit rate	1.184	1.650	8.57	0.138	0.193	Moderate positive
All SNAP exit rate	-0.556	1.393	9.55	-0.058	0.146	Moderate negative

Notes: **Country:** United States. **Research question:** Does the expiration of SNAP Emergency Allotments cause SNAP-authorized food retailers to exit the market? **Policy mechanism:** Emergency Allotments provided an additional \$95/month per household to approximately 41 million SNAP participants during the COVID-19 pandemic. Eighteen states ended EA early (April 2021–January 2023) through gubernatorial decisions; remaining states lost EA in March 2023. Expiration reduced SNAP benefits by 30–40%, removing approximately \$46 billion in annual food purchasing power routed through authorized retailers. **Outcome definition:** Quarterly deauthorization rate per 1,000 SNAP-authorized retailers, measuring the rate at which stores lose their authorization to accept SNAP benefits (exit the SNAP market). **Treatment:** Binary; equals one in the first quarter after a state's EA expires and all subsequent quarters. **Data:** USDA FNS SNAP Retailer Historical Database (703,441 retailers, 2005–2025), state-quarter panel, 51 states $\times$ 24 quarters (2019Q1–2024Q4), 1,224 observations per store type. **Method:** Callaway and Sant'Anna (2021) doubly-robust staggered DiD with not-yet-treated states as controls. Standard errors clustered at the state level. **Sample:** All SNAP-authorized retailers in 50 states plus DC; convenience stores are the primary outcome given their dominant share (53%) of SNAP-authorized locations and high SNAP revenue dependence. SDE $= \hat{\beta}/\text{SD}(Y)$ where SD(Y) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).