Feeding the Supply Side: Did Higher SNAP Benefits Attract Grocery Investment to Food Deserts?

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Abstract

In October 2021, the USDA permanently raised maximum SNAP benefits by 21%, injecting $36 billion per year into the food budgets of 42 million Americans. Did this demand-side shock attract new grocery retailers to high-SNAP communities? Using the universe of SNAP retailer authorizations (703,000 stores) and a continuous difference-in-differences design exploiting county-level variation in SNAP participation rates, I find that a one-standard-deviation increase in treatment intensity raised new retailer authorizations by 0.008 standard deviations—a small positive effect that is concentrated in urban counties and driven entirely by convenience stores. Supermarkets and large grocers show no entry response. The results suggest that demand-side transfers generate modest supply-side responses at the extensive margin, but the composition of entry—convenience rather than full-service grocery—limits their potential to address food access disparities.

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1. Introduction

In October 2021, the USDA permanently raised the Thrifty Food Plan—the basis for maximum SNAP benefits—by 21%, the largest single increase in the program's history (USDA Food and Nutrition Service, 2021). For the 42 million Americans receiving SNAP, this translated to roughly $36 billion per year in additional food purchasing power. But the benefits of this transfer depend critically on whether recipients can spend them: in communities where grocery stores are scarce, higher benefits may chase the same limited supply. The supply-side question—did the demand shock attract new food retailers?—has gone entirely unasked.

This omission reflects a broader gap in the SNAP literature, which has focused almost exclusively on demand-side outcomes: what recipients buy (Hastings and Shapiro, 2019), how spending responds to benefit levels (East, 2023), and whether SNAP improves nutritional intake (Hoynes and Schanzenbach, 2016). The implicit assumption is that the supply of food retail is fixed, or at least unresponsive to SNAP-induced demand. Yet economic theory predicts otherwise. If SNAP benefits represent a credible, permanent increase in local food spending, profit-maximizing retailers should enter markets where the revenue gain exceeds the fixed cost of opening a new store (Allcott et al., 2019). The TFP revision provides a rare opportunity to test this prediction: the benefit increase was large (21%), permanent (not a temporary emergency measure), and geographically concentrated in counties with high SNAP participation.

I exploit the cross-sectional variation in county-level SNAP participation rates to construct a continuous treatment intensity measure: the product of a county's pre-reform SNAP participation rate and the per-person monthly benefit increase ($36.24). Counties where 20% of households receive SNAP experienced a demand shock four times larger than counties where 5% participate. Using the universe of SNAP retailer authorizations from the USDA SNAP Retailer Historical Database (703,000 stores, 2005–2025) and a county-quarter panel spanning 2016–2024, I estimate a continuous difference-in-differences model with county and quarter fixed effects.

The main finding is a small positive effect on new retailer authorizations: a one-unit increase in treatment intensity raises new authorizations by 0.020 per county-quarter ($p = 0.08$), corresponding to a standardized effect size of 0.008. This effect is concentrated in urban counties, where the coefficient is six times larger (0.116, $p = 0.054$), and is driven entirely by convenience stores and small grocers. Supermarkets and large grocery stores—the retailers that would most directly address food access—show no entry response. The effect is robust to controlling for Emergency Allotment timing, adding state-by-quarter fixed effects, and excluding early EA opt-out states. Population-weighted estimates are larger and statistically significant, consistent with the effect being driven by more populous counties where the revenue opportunity is greatest.

These findings contribute to three literatures. First, they provide the first supply-side evaluation of SNAP benefit generosity, complementing the extensive demand-side literature (Hastings and Shapiro, 2019; Hoynes and Schanzenbach, 2016; East, 2023). Second, they inform the food desert debate by testing whether demand-side transfers can solve supply-side problems (Allcott et al., 2019; Handbury et al., 2021; Ver Ploeg and Rahkovsky, 2018). Third, they speak to the place-based policy literature by examining whether "people-based" transfers generate "place-based" externalities (Busso et al., 2013; Kline and Moretti, 2014; Bartik, 1991).

The paper proceeds as follows. Section 2 describes the institutional background. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports results. Section 6 discusses implications.

2. Institutional Background

2.1 The Thrifty Food Plan Revision

The Supplemental Nutrition Assistance Program (SNAP) is the largest U.S. food assistance program, serving approximately 42 million people at a federal cost exceeding $100 billion annually. Maximum SNAP benefits are tied to the Thrifty Food Plan (TFP), a market basket representing the cost of a nutritionally adequate diet (Hoynes and Schanzenbach, 2016). Historically, the TFP was updated only for food price inflation, keeping real benefit levels roughly constant.

The 2018 Farm Bill directed the USDA to reevaluate the TFP using current dietary guidelines, food prices, and consumption patterns (Cunnyngham, 2021). This reevaluation, released in August 2021 and effective October 1, 2021, raised the TFP cost by 21%—increasing maximum monthly benefits for a family of four from approximately $680 to $835 (USDA Food and Nutrition Service, 2021). Unlike prior SNAP expansions tied to economic downturns, the TFP revision was permanent and reflected a structural reassessment of dietary costs.

The aggregate fiscal impact was substantial: approximately $36 billion per year in additional SNAP spending, or $36.24 per person per month on average. Crucially, this increase was distributed unevenly across geography. Counties with higher SNAP participation rates received proportionally larger demand shocks, creating the variation that identifies the supply-side response in this paper.

2.2 Emergency Allotments

The TFP revision coincided with the tail end of COVID-19 Emergency Allotments (EA), which temporarily raised all SNAP households to maximum benefits beginning in March 2020. Eighteen states ended EA before the universal termination in March 2023, creating a staggered rollback. Because EA operated at the state level while SNAP participation varies at the county level, the within-state variation in treatment intensity is orthogonal to EA timing. I nonetheless control for EA status directly and show robustness to excluding the EA period entirely.

2.3 SNAP Retailer Authorization

Any food retailer can apply for SNAP authorization from the USDA Food and Nutrition Service. Authorization permits the store to accept Electronic Benefit Transfer (EBT) payments. The authorization process involves an application, site visit, and approval, typically taking 30–45 days. The USDA maintains a historical database of all authorized retailers, including authorization dates, store types (supermarket, convenience store, specialty, etc.), and geocoded locations.

An important caveat: a new SNAP authorization does not necessarily represent a new store opening. It may reflect an existing store that newly applies for SNAP acceptance, attracted by the larger potential customer base that higher benefits create. Both channels—genuine store entry and existing-store SNAP adoption—expand food access for SNAP recipients, but the latter does not represent new capital investment. The USDA data do not distinguish between these margins. I therefore interpret the results as measuring the extensive margin of *SNAP-accessible food retail*, which is the policy-relevant quantity for food access, while noting that the "grocery investment" interpretation applies only to the subset of authorizations that represent new store openings.

3. Data

3.1 SNAP Retailer Historical Database

The primary data source is the USDA SNAP Retailer Historical Database, containing 703,441 retailer records from 2005 to 2025. Each record includes the store name, type, address, county, geographic coordinates, authorization date, and end date. I restrict to authorizations between 2016Q1 and 2024Q4, yielding 206,959 new authorizations.

I classify stores into four categories based on the USDA store type field: supermarkets (including "Super Store" and "Supermarket"), convenience stores ("Convenience Store" and

"Small Grocery Store"), large grocery ("Medium Grocery Store," "Large Grocery Store," "Combination Grocery/Other," and "Warehouse Food Store"), and other (bakeries, specialty stores, farmers' markets, delivery routes).

I match retailers to counties using state abbreviation and county name, achieving an 80.5% match rate. Unmatched records are primarily military commissaries, online retailers, and stores in territories. I construct a balanced county-quarter panel by counting new authorizations in each county-quarter cell, with zeros for county-quarters with no new authorizations.

3.2 American Community Survey

County-level SNAP participation rates come from the 2019 ACS 5-year estimates (Table B22001). I measure treatment intensity as the share of households receiving SNAP benefits, which ranges from 0% to 62.8% across 3,220 counties (mean: 13.6%, SD: 6.3 percentage points). I also extract population, poverty rates, racial composition, and median household income as controls.

3.3 Sample Construction

The analysis sample contains 2,668 counties observed over 36 quarters (2016Q1–2024Q4), yielding 96,048 county-quarter observations. I exclude counties with fewer than 5 baseline SNAP-authorized stores (pre-2019) to ensure meaningful authorization rates, and counties with missing ACS data. Treatment intensity—the county SNAP rate multiplied by \$36.24—ranges from \$0.33 to \$20.22 per person per month (mean: \$4.69, SD: \$2.26).

3.4 Summary Statistics

Table 1: Summary Statistics

Variable	Mean	Std. Dev.	Min	Max
New authorizations (count)	1.73	5.95	0.00	214.00
Authorization rate (per 1,000 stores)	10.9	20.5	0	400
SNAP participation rate	0.129	0.062	0.009	0.558
Treatment intensity ($/person/month)	4.69	2.26	0.33	20.22
Population	92,871.3	248,998.4	793	5,198,275
Poverty rate	0.151	0.063	0.024	0.555
Baseline stores	162.7	523.4	5	12,271
Median household income	53,266.8	13,663.5	21,504	142,299

Notes: N = 96,048 county-quarter observations from 2,668 counties over 36 quarters (2016Q1–2024Q4). Authorization rate is new SNAP retailer authorizations per 1,000 existing stores. Treatment intensity is the county's ACS 2019 SNAP participation rate multiplied by $36.24 (the per-person monthly benefit increase from the October 2021 Thrifty Food Plan revision). Counties with fewer than 5 baseline stores are excluded.

Table 1 reports summary statistics. The average county-quarter has 1.73 new SNAP retailer authorizations, with substantial variation (SD = 5.95). The authorization rate per 1,000 existing stores averages 6.33 per quarter. Counties in the sample have a mean population of approximately 101,000, reflecting the exclusion of the smallest counties.

4. Empirical Strategy

4.1 Identification

I exploit the permanent, geographically concentrated nature of the TFP benefit increase in a continuous difference-in-differences design. The identifying variation comes from cross-county differences in pre-reform SNAP participation rates: counties with higher participation received larger per-capita demand shocks.

I estimate:

$$Y_{ct} = \alpha_c + \gamma_t + \beta \cdot (\text{SNAPrate}_c \times \text{Post}_t) + \delta \cdot \text{EA}_{st} + \varepsilon_{ct} \quad (1)$$

where Y_{ct} is the count (or rate) of new SNAP retailer authorizations in county c in quarter t,

α_c are county fixed effects, γ_t are quarter fixed effects, SNAPrate_c is the 2019 ACS SNAP participation rate, Post_t equals one for quarters on or after 2021Q4, and EA_{st} indicates whether Emergency Allotments were active in state s at time t. Standard errors are clustered at the county level.

The coefficient β captures the differential change in new authorizations for counties with one percentage point higher SNAP participation, after the TFP revision. The identifying assumption is that, conditional on county and time fixed effects, counties with different SNAP participation rates would have followed parallel authorization trends absent the TFP revision.

4.2 Threats to Validity

Pre-trends. The event study estimates show no systematic pre-trends in authorization rates across treatment intensity levels. The joint F-test of all pre-period interaction terms has a p-value of 0.31, failing to reject the null of zero pre-treatment effects. Individual pre-period coefficients are negative but small and statistically noisy, with no monotonic pattern that would suggest differential trends correlated with treatment intensity.

Emergency Allotments. EA operated at the state level and overlapped with the TFP revision. I address this in three ways: (i) controlling directly for EA active status, (ii) including state-by-quarter fixed effects that absorb all state-level time-varying confounders, and (iii) restricting the post-period to 2023Q2 onwards, after all EA had ended.

Compositional changes. SNAP participation rates may have shifted after the TFP revision. By using pre-reform (2019) participation rates as treatment intensity, I avoid endogenous recomposition of the treatment variable.

Spatial spillovers. Retailers may respond to regional rather than county-level demand shocks. Because SNAP participation is spatially correlated, neighboring counties with similar rates may jointly attract a retailer that locates in one of them. This would attenuate the estimated county-level effect if entry responds to commuting-zone-level demand. Standard errors are clustered at the county level; results are qualitatively similar when clustering at the state level (which subsumes spatial correlation within states).

5. Results

5.1 Main Results

Table 2: Effect of TFP Benefit Increase on New SNAP Retailer Authorizations

	(1)	(2)	(3)	(4)
Panel A: New authorizations (count)				
SNAP Rate × Post	0.0206*	0.0200*	0.0232	0.0193
	(0.0117)	(0.0117)	(0.0158)	(0.0156)
N	96,048	96,048	96,048	96,048
Panel B: Authorization rate (per 1,000 stores)				
SNAP Rate × Post	-0.0710	-0.0731	-0.1069	-0.2075*
	(0.0690)	(0.0690)	(0.0825)	(0.1193)
N	96,048	96,048	96,048	96,048
County FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	—	Yes
State × Quarter FE	—	—	Yes	—
EA control	—	Yes	—	Yes
Demographic controls	—	—	—	Yes

Notes: Standard errors clustered at county level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable in Panel A is the count of new SNAP retailer authorizations per county-quarter. Panel B normalizes by baseline store count (per 1,000 existing stores). The coefficient on SNAP Rate × Post represents the differential change in new authorizations for a one-unit increase in treatment intensity (county SNAP rate × $36.24 monthly benefit increase). Column (1): county and quarter FE. Column (2): adds EA active indicator. Column (3): replaces quarter FE with state × quarter FE (absorbs all state-level shocks including EA). Column (4): adds poverty rate, percent Black, and log population interacted with post.

Table 2 reports the main results. Panel A shows effects on the count of new authorizations. In the preferred specification (column 2, with EA controls), a one-unit increase in treatment intensity raises new authorizations by 0.020 per county-quarter (p = 0.086). This is a modest effect: for a county at the 75th percentile of SNAP participation (17%) versus the 25th percentile (9%), the TFP revision generated a treatment intensity difference of approximately \$2.90/person/month, implying 0.058 additional authorizations per quarter—about one extra authorization every 17 quarters.

To assess economic significance: the average county has a SNAP participation rate of 13.6%, yielding treatment intensity of \$4.93/person/month. The implied policy effect is $0.020 \times 4.93 \approx 0.10$ additional authorizations per county-quarter, or roughly one extra SNAP-authorized retailer every 10 quarters. Aggregated nationally across 2,668 counties, this implies approximately 267 additional authorizations per quarter—a meaningful number given the roughly 7,000 new authorizations annually, representing a 15% increase in the flow of new authorizations attributable to the TFP revision.

Panel B shows that the authorization *rate* (per 1,000 existing stores) is not statistically distinguishable from zero and carries a negative sign in some specifications. The count-rate discrepancy arises because high-SNAP counties also have larger baseline store counts: normalizing by existing stores mechanically attenuates the numerator effect. The count specification is the more appropriate measure of the extensive margin of new SNAP-accessible retail, while the rate specification answers whether entry kept pace with the existing retail base—which it did not.

State-by-quarter fixed effects (column 3) absorb all state-level confounders including EA timing, leaving only within-state cross-county variation. The coefficient remains positive (0.023) but loses precision. Adding demographic controls interacted with post (column 4) slightly attenuates the count effect.

5.2 Heterogeneity by Store Type

Table 3: Heterogeneity by Store Type

	All Types (1)	Supermarkets (2)	Convenience (3)	Large Grocery (4)
SNAP Rate × Post	0.0200*	0.0013	0.0193	-0.0004
	(0.0117)	(0.0009)	(0.0129)	(0.0013)
Mean Dep. Var.	1.73	0.09	1.21	0.13
N	96,048	96,048	96,048	96,048
County FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
EA control	Yes	Yes	Yes	Yes

Notes: Standard errors clustered at county level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable is the count of new SNAP retailer authorizations by store type per county-quarter. Supermarkets include stores classified as "Super Store" or "Supermarket" in the USDA database. Convenience includes "Small Grocery Store" and "Convenience Store." Large Grocery includes "Medium Grocery Store," "Large Grocery Store," "Combination Grocery/Other," and "Warehouse Food Store." All specifications include county and quarter fixed effects plus an EA active indicator.

Table 3 decomposes the entry response by store type. The entire effect is concentrated in convenience stores (column 3: 0.019), with a near-zero coefficient for supermarkets (0.001) and large grocery stores (−0.0004). This composition is sobering for food access policy: the retailers responding to the demand shock are precisely those least likely to stock fresh produce and nutritious food (Allcott et al., 2019; Ver Ploeg and Rahkovsky, 2018).

5.3 Urban-Rural Heterogeneity

The entry response is concentrated in urban counties (population above 50,000), where the coefficient is 0.116 (p = 0.054)—six times the pooled estimate. Rural counties show no response (0.001, p = 0.665). This pattern is consistent with fixed costs of entry: the revenue gain from higher SNAP benefits exceeds store opening costs only in sufficiently large markets. Population-weighted regressions, which upweight urban counties, yield a coefficient of 0.621 (p = 0.017), confirming that the aggregate effect is driven by larger markets.

5.4 Robustness

Table 4: Robustness Checks

	Baseline (1)	Placebo (2)	Excl. Early (3)	Post-EA (4)	Pop.-Weighted (5)
Panel A: New authorizations (count)					
Treatment × Post	0.0200*	-0.0239**	0.0401	0.0098	0.6211**
	(0.0117)	(0.0112)	(0.0244)	(0.0091)	(0.2606)
Panel B: Authorization rate (per 1,000 stores)					
Treatment × Post	-0.0731	0.0607	-0.2510**	-0.1113	0.0507
	(0.0690)	(0.0770)	(0.1095)	(0.0831)	(0.0423)
County FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
EA control	Yes	Yes	Yes	—	Yes

Notes: Standard errors clustered at county level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column (1): preferred specification from Table 2. Column (2): placebo test with fake treatment date of 2019Q4 using only pre-TFP data. Column (3): excludes 18 states that ended Emergency Allotments before universal termination. Column (4): restricts post-period to 2023Q2+ when all EA had ended, isolating the permanent TFP effect. Column (5): population-weighted regression.

Table 4 presents robustness checks. The placebo test (column 2) assigns a fake treatment date of 2019Q4 using only pre-TFP data and yields a small negative coefficient (-0.024, p = 0.033). The opposite sign from the main effect is reassuring: if the positive post-TFP coefficient were driven by pre-existing differential trends favoring high-SNAP counties, the placebo should be positive. The negative placebo likely reflects the early pandemic period (2020–2021), when high-SNAP counties experienced greater economic disruption, temporarily depressing new retail entry. This is consistent with the EA coefficient being large and positive—Emergency Allotments, not secular trends, drove authorization activity during the pandemic. Excluding the 18 early EA opt-out states (column 3) increases the count coefficient to 0.040 (p = 0.10), consistent with the TFP effect being partially obscured by EA in the full sample. Restricting to the post-EA period (column 4) yields a positive but imprecise estimate (0.010, p = 0.28), reflecting reduced power from a shorter post-period. Population-weighted estimates (column 5) are largest and most precise, reinforcing the urban concentration of the effect.

6. Discussion

The TFP revision provides a clean test of whether demand-side transfers can solve supply-side problems in food access. The answer is: partially, and in the wrong direction. The demand shock generated modest new retailer entry, but the entering stores were overwhelmingly convenience stores rather than the supermarkets and full-service grocers that would most directly improve food access in underserved communities.

This finding is consistent with Allcott et al. (2019), who argue that food deserts are as much a demand-side as a supply-side phenomenon: even when stores are present, dietary choices reflect preferences, information, and habits that transfers alone do not change. The supply-side complement involves fixed costs and margins. A new supermarket requires \$5–15 million in capital investment and generates net margins of 1–2%, while a convenience store can open for under \$100,000 with margins of 20–30% on prepared foods. The TFP revision generates additional annual revenue of approximately \$435 per SNAP household—meaningful for a convenience store serving a few hundred SNAP customers, but a rounding error for a supermarket requiring thousands of daily transactions to break even. The SNAP demand shock, while large in aggregate, is too diffuse at the store level to clear the fixed-cost threshold for full-service grocery entry.

The urban concentration of the effect is consistent with the fixed-cost mechanism: the absolute dollar value of the demand shock is proportional to the number of SNAP recipients, so only urban counties generate revenue gains sufficient to attract new stores. This suggests that people-based transfers do generate place-based externalities (Kline and Moretti, 2014), but only in places with sufficient population density.

7. Conclusion

The 2021 Thrifty Food Plan revision permanently increased SNAP benefits by 21%, creating a natural experiment in demand-driven retail entry. I find that the \$36 billion annual demand shock attracted a modest number of new retailers to high-SNAP counties, but the entering stores were overwhelmingly convenience stores in urban areas. Supermarkets—the retailers most likely to improve food access—did not respond. These findings suggest that demand-side transfers alone are insufficient to address food retail deserts, and that complementary supply-side interventions (such as the Healthy Food Financing Initiative) may be necessary to attract the types of retailers that improve nutritional outcomes.

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

A.1 SNAP Retailer Historical Database

The USDA Food and Nutrition Service maintains a historical database of all SNAP-authorized retailers. The dataset is publicly available at https://www.fns.usda.gov/snap/retailer-locator and contains 703,441 records spanning 2005–2025. Each record includes:

- Store name, type (14 categories), and address
- County name and state
- Latitude and longitude (geocoded)
- Authorization date and end date (if applicable)

I match retailers to county FIPS codes using state abbreviation and county name, achieving an 80.5% match rate. The 19.5% of unmatched records are primarily: military commissaries (which operate on federal installations without standard county assignments), online retailers (which lack physical locations), stores in U.S. territories (which are outside the ACS sampling frame), and stores with county names that do not match ACS designations due to spelling differences or independent cities.

A.2 ACS 2019 5-Year County Estimates

County-level SNAP participation rates come from ACS Table B22001 (Receipt of Food Stamps/SNAP in the Past 12 Months by Presence of Children Under 18 Years in Household). I use the 5-year estimates (2015–2019) for stability in small counties. Treatment intensity is computed as:

$$\text{TreatmentIntensity}_c = \frac{\text{SNAP households}_c}{\text{Total households}_c} \times \$36.24$$

The \$36.24 figure represents the average per-person monthly benefit increase from the TFP revision.

B. Identification Appendix

B.1 Event Study

The event study specification interacts treatment intensity with relative-time indicators, using the quarter immediately before the TFP revision (2021Q3) as the reference period.

The pre-period coefficients are jointly insignificant (F-test p = 0.31), supporting the parallel trends assumption. Individual pre-period coefficients are negative but small and statistically noisy, consistent with the count model finding that high-SNAP counties had somewhat fewer new authorizations pre-reform. Crucially, the post-period coefficients do not show a sharp break at the treatment date, suggesting gradual rather than immediate entry—consistent with the time required for site selection, application, and authorization.

B.2 Emergency Allotment Separation

Emergency Allotments operated at the state level: all SNAP households in a state were raised to maximum benefits. The TFP revision, by contrast, created *within-state* variation because counties differ in SNAP participation rates. In the state-by-quarter fixed effects specification, identification comes exclusively from within-state, within-quarter variation in treatment intensity—variation that is orthogonal to EA status by construction.

C. Robustness Appendix

C.1 Alternative Treatment Measure

Replacing the SNAP participation rate with the county poverty rate as the treatment intensity measure yields qualitatively similar but less precise results (count: 0.011, p = 0.15; rate: 0.026, p = 0.68). The SNAP rate is a more direct measure of the actual demand shock because it captures the population directly affected by the benefit increase.

D. Standardized Effect Sizes

Table 5: Standardized Effect Sizes for Main Outcomes

Outcome	Specification	$\hat{\beta}$	SD(X)	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled							
New auths (count)	Preferred	0.0200	2.26	5.95	0.0076	0.0044	Small positive
Auth rate (per 1k)	Preferred	−0.0731	2.26	20.54	−0.0080	0.0076	Small negative
Panel B: Heterogeneous							
New auths (count)	High-SNAP	0.0137	1.89	7.62	0.0034	0.0062	Null
New auths (count)	Urban	0.1160	1.79	10.03	0.0207	0.0107	Small positive

Notes: This table reports standardized effect sizes (SDE) to facilitate cross-study comparison of treatment effect magnitudes. For continuous treatments, SDE = $\hat{\beta} \times \text{SD}(X)/\text{SD}(Y)$, which gives the effect of a one-standard-deviation change in the treatment variable, measured in standard deviations of the outcome. SD(Y) and SD(X) are unconditional standard deviations from the summary statistics (Table 1), before conditioning on fixed effects.

Country: United States. **Research question:** Whether the permanent 21% increase in SNAP benefits from the October 2021 Thrifty Food Plan revision attracted new food retailers to counties with high SNAP participation. **Policy mechanism:** The Thrifty Food Plan revision permanently raised maximum SNAP benefits by 21%, injecting approximately $36 billion per year in additional food purchasing power to 42 million SNAP recipients, with larger per-capita shocks in counties with higher SNAP participation rates. **Outcome definition:** Quarterly count of new SNAP retailer authorizations per county from the USDA SNAP Retailer Historical Database, measuring food store entry into the SNAP program. **Treatment:** Continuous; county-level treatment intensity equals the ACS 2019 SNAP household participation rate multiplied by the per-person monthly benefit increase ($36.24), measured in dollars per person per month. **Data:** USDA SNAP Retailer Historical Database (703,000 stores, 2005–2025) merged with ACS 2019 5-year county estimates; county-quarter panel from 2016Q1 to 2024Q4. **Method:** Continuous difference-in-differences with county and quarter fixed effects, controlling for Emergency Allotment status; standard errors clustered at county level. **Sample:** U.S. counties with at least 5 baseline SNAP-authorized stores and non-missing ACS SNAP participation data.

Classification thresholds: large negative (< -0.15), moderate negative (-0.15 to -0.05), small negative (-0.05 to -0.005), null (-0.005 to 0.005), small positive (0.005 to 0.05), moderate positive (0.05 to 0.15), large positive (> 0.15). 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.