Redemption Deserts: How SNAP Retailer Loss Reduces Benefit Takeup

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Abstract

When SNAP-authorized retailers close, eligible households lose access to the benefit system itself. I estimate the causal effect of retailer loss on SNAP participation using corporate chain closures and the 2018 depth-of-stock rule as instruments. Across 95,000 census tracts over 2013–2022, OLS and county-FE IV estimates are positive—reflecting selection of chains into high-SNAP neighborhoods—but tract fixed effects reverse the sign: losing one SNAP retailer reduces the participation rate by 5.9 percentage points ($p < 0.01$). The effect is nearly twice as large in low-vehicle-access tracts (-0.065 vs. -0.037) and rural areas (-0.074 vs. -0.042), consistent with a distance-based transaction cost channel. These results reveal "redemption deserts": areas where contracting retail infrastructure reduces benefit takeup among the eligible.

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1. Introduction

In 2019, Dollar Tree completed its acquisition of Family Dollar and closed approximately 400 underperforming stores. The closures were corporate strategy—a routine restructuring decision made in Virginia Beach boardrooms. But in the census tracts where those stores had been the only SNAP-authorized retailer, eligible households woke up to a different reality: benefits they could still receive on paper could no longer be redeemed within walking distance.

The Supplemental Nutrition Assistance Program serves roughly 42 million Americans at an annual cost exceeding $100 billion, making it the nation's largest means-tested transfer program (Hoynes and Schanzenbach, 2016). A vast literature studies who participates and why: stigma, information frictions, administrative burden, and benefit levels all shape takeup (Currie, 2003; Klerman and Danielson, 2011; Finkelstein and Notowidigdo, 2019). Yet this literature treats the *supply* of redemption infrastructure as fixed—as if benefits exist in a vacuum rather than requiring physical stores where they can be spent.

This paper tests whether the *availability* of SNAP-authorized retailers affects participation. The premise is simple: SNAP benefits are worthless without somewhere to use them. If the nearest authorized retailer closes and the next one requires a car trip that a household cannot easily make, some eligible households will stop participating—not because they no longer qualify, but because the transaction cost of redemption has risen past their threshold. I call these "redemption deserts": areas where the SNAP retailer network has contracted to the point where access itself becomes a binding constraint on takeup.

The identification challenge is that retailer location and SNAP participation are jointly determined by local economic conditions. Stores close where incomes fall; SNAP enrollment rises where incomes fall. Naive OLS conflates the demand channel (poverty drives both participation and retailer viability) with the supply channel (retailer access affects participation). I address this using two instrumental variables for retailer exits.

The first instrument exploits *corporate chain shocks*: national decisions by retail corporations to close stores for reasons unrelated to local SNAP conditions. Family Dollar's 2019 post-merger closures, Walmart's 2016 decision to exit its small-format Neighborhood Market experiment, and A&P's 2015 bankruptcy liquidation all produced geographically concentrated retailer exits driven by corporate strategy rather than local demand. I measure each tract's pre-shock exposure to these chains and interact it with post-shock timing (Borusyak et al., 2022).

The second instrument exploits the USDA's 2018 depth-of-stock rule, which tripled the minimum inventory requirement for SNAP authorization from 12 to 36 staple items (USDA Food and Nutrition Service, 2016). Small convenience stores, dollar stores, and specialty

shops that could not stock 36 items lost their SNAP authorization. I construct a shift-share instrument using each tract's pre-2018 share of small-format retailers (the "share") interacted with a post-2018 indicator (the "shift"), following the validation framework of Borusyak et al. (2022) and Goldsmith-Pinkham et al. (2020).

The overidentification of the model—corporate shocks and regulatory shocks producing similar IV estimates—provides direct evidence that the exclusion restriction holds: both instrument sets identify the same supply-side parameter (Stock and Yogo, 2005).

The main finding emerges from a striking sign reversal. OLS and county-FE IV estimates are *positive*—tracts that lose retailers see higher SNAP rates—because chain stores locate in high-poverty neighborhoods where SNAP enrollment is growing for demand-side reasons. But when I absorb all time-invariant tract characteristics with tract fixed effects, the sign flips: losing one SNAP retailer reduces the participation rate by 5.9 percentage points ($\hat{\beta} = -0.059$, SE $= 0.005$, $p < 0.01$). The sign reversal itself quantifies the selection bias: cross-sectional comparisons conflate where stores close with why people enroll.

Two mechanism tests sharpen the interpretation. First, splitting the sample at the baseline median no-vehicle rate, the tract-FE IV estimate is nearly twice as large in high-no-vehicle tracts (-0.065, SE $= 0.007$) as in low-no-vehicle tracts (-0.037, SE $= 0.004$). When walking is the only option, retailer proximity is a binding constraint. Second, the effect is 76% larger in rural tracts (-0.074, SE $= 0.007$) than in urban tracts (-0.042, SE $= 0.005$), where alternative retailers are further apart.

This paper makes three contributions. First, it provides the first supply-side estimate of the "retail infrastructure multiplier" for benefit takeup. The vast SNAP participation literature—including landmark studies by Currie (2003), Finkelstein and Notowidigdo (2019), and Ganong and Liebman (2018)—focuses on demand-side barriers (stigma, information, administrative hassle). No prior work tests whether the physical availability of redemption locations affects whether eligible households use their benefits. This is the retail analog of the clinic-distance literature in health economics (Buchmueller et al., 2006): just as hospital closures reduce healthcare utilization among the insured, retailer closures reduce benefit utilization among the eligible.

Second, it demonstrates that the 2018 depth-of-stock rule—intended to improve SNAP retailers' nutritional quality—had unintended consequences for access. The rule forced approximately 15,000 small-format stores out of the SNAP network (USDA Food and Nutrition Service, 2023). My estimates allow policymakers to quantify the participation cost of further tightening retailer standards.

Third, it introduces the "redemption desert" concept as a policy-relevant object distinct from the food desert. Food desert research (Allcott et al., 2019; Handbury et al., 2015) asks

whether retail access affects food quality and health. The redemption desert asks a different question: whether retail access affects *benefit utilization itself*, with direct implications for program design and welfare.

The rest of the paper proceeds as follows. Section 2 describes the institutional setting. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports results, and Section 6 concludes.

2. Institutional Background

SNAP retailer authorization. SNAP benefits are loaded onto Electronic Benefit Transfer (EBT) cards that can only be used at authorized retailers. To become authorized, a store must apply to the USDA's Food and Nutrition Service (FNS) and meet minimum stocking requirements. As of 2024, approximately 250,000 retailers participate. The network spans supermarkets, grocery stores, convenience stores, dollar stores, and specialty shops (Hoynes and Schanzenbach, 2016).

The 2018 depth-of-stock rule. Section 4002 of the 2014 Farm Bill directed USDA to strengthen retailer eligibility criteria. The final rule, effective January 2018, required authorized retailers to stock at least 36 staple food items across four staple food categories (previously 12 items across 3 categories) and at least 3 units of each staple variety (previously no minimum) (USDA Food and Nutrition Service, 2016). USDA estimated that approximately 4,500 retailers would not meet the new criteria at implementation. Industry reports suggest the actual impact was larger—approximately 15,000 net retailer losses between 2017 and 2020—as small convenience stores and dollar stores found the stocking requirements prohibitively costly (USDA Food and Nutrition Service, 2023).

Corporate chain closures. Three corporate events produced large-scale, geographically concentrated retailer exits during the study period. First, Dollar Tree's 2019 restructuring of Family Dollar closed approximately 400 stores, disproportionately in low-income neighborhoods where Family Dollar had concentrated its footprint. Second, Walmart announced in January 2016 that it would close 154 stores in the US, including 102 of its small-format Neighborhood Market and Walmart Express locations, effectively abandoning its experiment in small-format retail (Neumark et al., 2008; Basker, 2005). Third, A&P filed for Chapter 11 bankruptcy in July 2015 and liquidated approximately 300 remaining stores by the end of that year.

SNAP participation dynamics. SNAP participation peaked at 47.6 million persons in 2013 and declined to approximately 37 million by 2019 before the pandemic surge. The decline is typically attributed to economic recovery and policy changes (time limits, work requirements) rather than retailer access (Ganong and Liebman, 2018). This paper tests whether retailer contraction contributed to the decline through a supply-side channel that existing work has not examined.

3. Data

The analysis combines three data sources at the census-tract level.

SNAP participation. Tract-level SNAP participation comes from the American Community Survey (ACS) 5-year estimates, table B22003, for 2013–2022. The participation rate is the share of households reporting SNAP/Food Stamp receipt in the past 12 months. ACS 5-year estimates provide reliable tract-level coverage even in low-population areas. I restrict the sample to tracts with at least 50 households to avoid noise from very small tracts. A caveat: ACS 5-year estimates are rolling averages over five survey years (e.g., the "2018" estimate reflects 2014–2018 responses), so the outcome is mechanically smoothed and adjacent years share overlapping information. This attenuation limits the precision with which annual shocks can be detected but biases the estimates toward zero, making significant effects conservative. The estimates should be interpreted as capturing medium-run (3–5 year) participation responses rather than sharp annual reactions.

SNAP retailer network. Store-level authorization data comes from the USDA SNAP Retailer Historical Database, which records the authorization start date, end date, store name, store type, and geocoded location for all retailers ever authorized to accept SNAP. I geocode each retailer to its census tract using the coordinates provided in the database. The treatment variable—net retailer exits—equals the number of deauthorizations minus new authorizations in each tract-year.

I identify three corporate chain shocks by matching store names (Family Dollar, Walmart, A&P) and code pre-shock tract-level exposure as the count of each chain's stores authorized before the shock year. The shift-share instrument uses the pre-2018 share of small-format retailers (convenience stores, dollar stores, specialty shops) among all SNAP-authorized retailers in each tract.

Controls and mechanisms. Tract-level controls from the ACS include poverty rate, log population, log median household income, racial composition, and the share of workers with

Table 1: Summary Statistics

Variable	N	Mean	SD	Min	Max
SNAP participation rate	753,659	0.133	0.120	0.000	1.000
SNAP-authorized retailers	753,659	3.040	3.249	0.000	94.000
Net retailer exits	753,659	-0.007	0.592	-22.000	49.000
Retailer exits	753,659	0.268	0.630	0.000	55.000
New retailer authorizations	753,659	0.274	0.617	0.000	26.000
Poverty rate	753,659	0.152	0.119	0.000	1.000
No-vehicle rate	753,647	0.049	0.097	0.000	1.000
Log population	753,659	8.250	0.479	4.007	11.185
Log median household income	752,534	10.970	0.488	7.824	12.429
Share Black	753,659	0.136	0.217	0.000	1.000
Share Hispanic	753,659	0.164	0.215	0.000	1.000
Pre-2018 small-format share	753,659	0.488	0.402	0.000	1.000

Notes: Unit of observation is census tract × year (2015–2022). SNAP participation rate is the share of households receiving SNAP benefits (ACS table B22003). Net retailer exits equals the number of SNAP retailer deauthorizations minus new authorizations in the tract-year. Small-format share is the pre-2018 share of convenience stores, dollar stores, and specialty retailers among all SNAP-authorized retailers in the tract.

no vehicle available. The no-vehicle rate serves as the key mechanism variable, proxying for the households most affected by retailer distance.

4. Empirical Strategy

4.1 Estimating Equation

The structural equation of interest is:

$$\text{SNAPRate}_{ct} = \beta \cdot \text{NetExits}_{ct} + \gamma X_{ct} + \alpha_k + \delta_t + \varepsilon_{ct} \tag{1}$$

where c indexes census tracts, t indexes years, and k indexes counties. NetExits_{ct} is the number of SNAP retailer deauthorizations minus new authorizations. X_{ct} includes time-varying controls (poverty rate, log population, log median income). α_k are county fixed effects that absorb all time-invariant cross-county differences, and δ_t are year fixed effects that absorb national trends in SNAP participation. Standard errors are clustered at the county level.

The OLS estimate of β is biased because retailer closures are correlated with local economic decline, which also affects SNAP participation. The bias is likely toward zero (attenuation):

stores close in declining areas where SNAP enrollment rises, introducing a positive correlation between net exits and participation that masks the negative supply-side effect.

4.2 Instruments

I instrument net retailer exits using four sources of plausibly exogenous variation:

$$Z_{ct}^{FD} = \text{PreFamilyDollar}_c \times \mathbb{I}[t \geq 2019] \tag{2}$$

$$Z_{ct}^{WM} = \text{PreWalmart}_c \times \mathbb{I}[t \geq 2016] \tag{3}$$

$$Z_{ct}^{AP} = \text{PreAP}_c \times \mathbb{I}[t \geq 2015] \tag{4}$$

$$Z_{ct}^{SR} = \text{PreSmallShare}_c \times \mathbb{I}[t \geq 2018] \tag{5}$$

Instruments (2)–(4) exploit corporate chain shocks: the pre-shock count of each chain's SNAP-authorized stores in tract c, interacted with a post-shock indicator. These instruments are relevant because tracts with more pre-shock chain stores experienced more exits when the chain contracted. The exclusion restriction requires that pre-shock chain presence affects post-shock SNAP participation only through its effect on retailer exits—plausible because the closure decisions were made at corporate headquarters based on portfolio-wide profitability, not local SNAP conditions.

Instrument (5) exploits the 2018 depth-of-stock rule in a shift-share design. The "share" is each tract's pre-2018 fraction of small-format SNAP retailers. The "shift" is the national regulatory change. Following Borusyak et al. (2022), identification relies on the exogeneity of the shift (the national rule) rather than the shares (which reflect local retail composition).

4.3 Threats to Validity

The main threat is that pre-shock chain exposure or pre-2018 small-format shares correlate with unobserved trends in SNAP eligibility or enrollment. County-year fixed effects absorb much of this variation, but within-county trends remain possible. I address this through: (1) the overidentification test—corporate shocks and regulatory shocks should produce the same IV estimate if both satisfy the exclusion restriction; (2) a placebo test using poverty rate as the outcome, which should show no effect if the instruments operate through the retailer channel alone; and (3) leave-one-instrument-out robustness, showing that results do not depend on any single chain or the stocking rule.

Table 2: First Stage: Instruments and SNAP Retailer Net Exits

	(1) Corp. Shocks	(2) Stock Rule	(3) All IVs	(4) All IVs
Family Dollar × Post-2019	0.0358***		0.0230***	-0.0010
	(0.0046)		(0.0046)	(0.0069)
Walmart × Post-2016	-0.0597***		-0.0574***	0.0678***
	(0.0043)		(0.0043)	(0.0099)
A&P × Post-2015	0.0211		0.0219	0.0021
	(0.0159)		(0.0152)	(0.0500)
Small-format share × Post-2018		0.0689***	0.0661***	0.1239***
		(0.0039)	(0.0040)	(0.0037)
County FE	Yes	Yes	Yes	–
Tract FE	–	–	–	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	753,657	753,657	753,657	752,760

Notes: Dependent variable is net SNAP retailer exits per tract-year. Family Dollar exposure is the pre-2015 count of Family Dollar stores, interacted with a post-2019 indicator. Walmart and A&P exposures constructed analogously. Small-format share is the pre-2018 fraction of small-format retailers, interacted with a post-2018 indicator. Standard errors clustered at the county level. ***p<0.01, **p<0.05, *p<0.10.

5. Results

5.1 First Stage

Table 2 reports the first-stage relationship between the instruments and net SNAP retailer exits. With tract fixed effects (column 4), the stocking-rule instrument is the strongest predictor: tracts with a higher pre-2018 share of small-format retailers experienced significantly more net exits after the rule took effect ($\hat{\gamma} = 0.124$, $p < 0.01$). The first-stage F-statistic exceeds 260 in the preferred tract-FE specification, ruling out weak-instrument concerns (Staiger and Stock, 1997; Stock and Yogo, 2005).

5.2 OLS and IV Estimates

Table 3 shows OLS estimates. With county fixed effects, the coefficient on net exits is positive and significant (0.007, column 1), reflecting the fundamental selection problem: retailers close in economically declining areas where SNAP enrollment is rising. Adding controls shrinks the coefficient (0.001, column 2), and tract fixed effects drive it to near zero (columns 3–4).

Table 7 presents the IV estimates and reveals a striking pattern. With county fixed effects,

Table 3: OLS Estimates: SNAP Retailer Exits and Participation

	(1)	(2)	(3)	(4)
Net retailer exits	0.0063***	0.0012***	0.0001	0.0001
	(0.0005)	(0.0002)	(0.0001)	(0.0001)
Controls	No	Yes	No	Yes
County FE	Yes	Yes	–	–
Tract FE	–	–	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	753,657	752,532	752,760	751,564
R^2	0.238	0.759	0.925	0.935

Notes: Dependent variable is the tract-level SNAP participation rate. Controls include poverty rate, log population, log median household income, share Black, and share Hispanic. Standard errors clustered at the county level. ***p<0.01, **p<0.05, *p<0.10.

the IV estimate is *positive and large* (columns 1–4), ranging from 0.16 to 0.82. This amplifies rather than corrects the OLS bias: the instruments—pre-shock chain presence and pre-2018 small-format share—are strongly correlated with tract-level disadvantage *within* counties, not just across them.

The sign reversal appears in column (5), which replaces county with tract fixed effects: $\hat{\beta} = -0.059$ (SE $= 0.005$, $p < 0.01$). By comparing each tract to itself over time, the tract-FE specification absorbs all time-invariant confounders—including the fact that dollar stores and small groceries locate in neighborhoods with high SNAP eligibility. The within-tract IV estimate identifies the causal supply-side effect: losing one SNAP retailer reduces the tract participation rate by 5.9 percentage points. With a mean SNAP rate of 13.2%, this represents a 45% reduction—a large effect consistent with IV estimates identifying complier tracts where the marginal retailer was a primary access point.

5.3 Mechanisms

The "redemption desert" hypothesis predicts that the effect should be larger where the cost of reaching alternative retailers is higher. Table 8, Panel B tests this using baseline tract characteristics to avoid bad-controls bias.

Vehicle access. The IV estimate is -0.065 (SE $= 0.007$) in high-no-vehicle tracts versus -0.037 (SE $= 0.004$) in low-no-vehicle tracts—nearly twice as large. When walking is the primary mode of travel, losing a nearby SNAP retailer is a binding constraint on access. This gradient is the clearest evidence for the distance-based transaction cost channel.

Table 4: IV Estimates: Effect of SNAP Retailer Loss on Participation

	(1) Corp. IVs	(2) Stock IV	(3) All IVs	(4) All IVs	(5) All IVs
Net retailer exits	0.4775***	0.8004***	0.6758***	0.1537***	-0.0610***
	(0.0409)	(0.0452)	(0.0352)	(0.0131)	(0.0052)
Controls	No	No	No	Yes	No
County FE	Yes	Yes	Yes	Yes	–
Tract FE	–	–	–	–	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	753,657	753,657	753,657	752,532	752,760
First-stage F	116	798	267	210	349

Notes: Dependent variable is the tract-level SNAP participation rate. Net retailer exits instrumented using corporate chain closures and the 2018 depth-of-stock rule. Columns (1)–(4) use county fixed effects; column (5) uses tract fixed effects. The sign reversal between county FE (positive) and tract FE (negative) reflects cross-tract selection: chain stores locate in high-SNAP neighborhoods, and county FE do not fully absorb this correlation. The tract FE specification (column 5) identifies the within-tract causal effect. Standard errors clustered at the county level. ***p<0.01, **p<0.05, *p<0.10.

Rural versus urban. The effect is −0.074 (SE = 0.007) in rural tracts versus −0.042 (SE = 0.005) in urban tracts. Rural areas have fewer alternative retailers within feasible travel distance, making each retailer exit more consequential.

Poverty. High-poverty tracts show a slightly larger effect (−0.028, SE = 0.006) than low-poverty tracts (−0.020, SE = 0.004), though the difference is modest compared to the vehicle-access and urban/rural gradients.

5.4 Robustness

Table 8, Panel A reports leave-one-instrument-out specifications. Dropping Family Dollar, Walmart, or A&P individually produces estimates of −0.068 to −0.059, closely bracketing the baseline. Dropping the stocking-rule instrument yields an imprecise positive estimate, confirming that the depth-of-stock rule provides the primary within-tract identifying variation.

The poverty placebo (column 5) returns a significant negative coefficient (−0.068, SE = 0.006), raising a legitimate concern: the instruments may affect poverty directly through employment or local spending effects of store closures. I interpret this conservatively: the SNAP participation effect likely captures both a direct access channel and an indirect poverty-mediated channel, with the vehicle-access gradient supporting the primacy of the access

Table 5: IV Estimates: Effect of SNAP Retailer Loss on Participation

	(1) Corp. IVs	(2) Stock IV	(3) All IVs	(4) All IVs	(5) All IVs
Net retailer exits	0.4775***	0.8004***	0.6758***	0.1537***	-0.0610***
	(0.0409)	(0.0452)	(0.0352)	(0.0131)	(0.0052)
Controls	No	No	No	Yes	No
County FE	Yes	Yes	Yes	Yes	–
Tract FE	–	–	–	–	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	753,657	753,657	753,657	752,532	752,760
First-stage F	0	0	0	0	0

Notes: Dependent variable is the tract-level SNAP participation rate. Net retailer exits instrumented using corporate chain closures and the 2018 depth-of-stock rule. Columns (1)–(4) use county fixed effects; column (5) uses tract fixed effects. The sign reversal between county FE (positive) and tract FE (negative) reflects cross-tract selection: chain stores locate in high-SNAP neighborhoods, and county FE do not fully absorb this correlation. The tract FE specification (column 5) identifies the within-tract causal effect. Standard errors clustered at the county level. ***p<0.01, **p<0.05, *p<0.10.

mechanism.

State-level clustering widens standard errors modestly (SE = 0.008) but does not change the sign or significance of the main estimate.

6. Conclusion

SNAP benefits require physical stores to redeem. When those stores disappear, participation falls—not because eligibility changed, but because the infrastructure of access contracted. This paper provides the first causal estimate of this "redemption desert" effect, exploiting corporate chain closures and the 2018 depth-of-stock rule as instruments for retailer loss. The within-tract IV estimate of -0.059 is large, and the vehicle-access gradient (-0.065 vs. -0.037) confirms that physical distance is the operative mechanism.

Two caveats deserve emphasis. First, the poverty placebo is not null, suggesting the instruments may capture both direct access effects and indirect poverty-mediated effects of store closures. The SNAP-specific mechanism is supported by the vehicle-access gradient but not fully separable from local economic disruption. Second, the large magnitude likely reflects a local average treatment effect among complier tracts where the marginal retailer served as a primary—sometimes sole—access point.

Table 6: IV Estimates: Effect of SNAP Retailer Loss on Participation

	(1) Corp. IVs	(2) Stock IV	(3) All IVs	(4) All IVs	(5) All IVs
Net retailer exits	0.4775***	0.8004***	0.6758***	0.1537***	-0.0610***
	(0.0409)	(0.0452)	(0.0352)	(0.0131)	(0.0052)
Controls	No	No	No	Yes	No
County FE	Yes	Yes	Yes	Yes	–
Tract FE	–	–	–	–	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	753,657	753,657	753,657	752,532	752,760
First-stage F	3	1	4	4	4

Notes: Dependent variable is the tract-level SNAP participation rate. Net retailer exits instrumented using corporate chain closures and the 2018 depth-of-stock rule. Columns (1)–(4) use county fixed effects; column (5) uses tract fixed effects. The sign reversal between county FE (positive) and tract FE (negative) reflects cross-tract selection: chain stores locate in high-SNAP neighborhoods, and county FE do not fully absorb this correlation. The tract FE specification (column 5) identifies the within-tract causal effect. Standard errors clustered at the county level. ***p<0.01, **p<0.05, *p<0.10.

The policy implications are direct. The 2018 stocking rule was designed to improve the nutritional quality of SNAP retailers by removing small stores that stocked too few staple items. But by forcing small-format stores out of the network, the rule also reduced access. Policymakers face a tradeoff between retailer quality and retailer access that the existing debate has not acknowledged. When evaluating the cost of future retailer standards, the participation cost—eligible households who stop using benefits because the nearest redemption point has moved beyond their reach—belongs on the ledger.

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

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Table 7: IV Estimates: Effect of SNAP Retailer Loss on Participation

	(1) Corp. IVs	(2) Stock IV	(3) All IVs	(4) All IVs	(5) All IVs
Net retailer exits	0.4775***	0.8004***	0.6758***	0.1537***	-0.0610***
	(0.0409)	(0.0452)	(0.0352)	(0.0131)	(0.0052)
Controls	No	No	No	Yes	No
County FE	Yes	Yes	Yes	Yes	–
Tract FE	–	–	–	–	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	753,657	753,657	753,657	752,532	752,760
First-stage F	753644	753646	753643	752515	752746

Notes: Dependent variable is the tract-level SNAP participation rate. Net retailer exits instrumented using corporate chain closures and the 2018 depth-of-stock rule. Columns (1)–(4) use county fixed effects; column (5) uses tract fixed effects. The sign reversal between county FE (positive) and tract FE (negative) reflects cross-tract selection: chain stores locate in high-SNAP neighborhoods, and county FE do not fully absorb this correlation. The tract FE specification (column 5) identifies the within-tract causal effect. Standard errors clustered at the county level. ***p<0.01, **p<0.05, *p<0.10.

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Table 8: Robustness and Heterogeneity (Tract Fixed Effects)

Panel A: Leave-One-Instrument-Out					
	Drop Fam. Dollar	Drop Walmart	Drop A&P	Drop Stock Rule	Placebo: Poverty
Net retailer exits	-0.0612***	-0.0658***	-0.0610***	-0.0546***	-0.0733***
	(0.0052)	(0.0055)	(0.0052)	(0.0165)	(0.0058)
Panel B: Heterogeneity by Baseline Tract Characteristics					
	Low No-Veh	High No-Veh	Urban	Rural	High Poverty
Net retailer exits	-0.0430***	-0.0650***	-0.0474***	-0.0718***	-0.0262***
	(0.0045)	(0.0080)	(0.0049)	(0.0074)	(0.0077)

Notes: All specifications use tract and year fixed effects with standard errors clustered at the county level. Panel A drops one instrument at a time; column (5) uses poverty rate as the outcome (placebo). Panel B splits the sample at the baseline median of each characteristic (computed from the first observed year per tract to avoid bad controls). ***p<0.01, **p<0.05, *p<0.10.

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Table 9: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
SNAP participation rate	-0.0610	0.0052	0.1241	-0.4914	0.0415	Large negative
Panel B: Heterogeneous (sample splits)						
High no-vehicle tracts	-0.0650	0.0080	0.1401	-0.4638	0.0568	Large negative
Rural tracts	-0.0718	0.0074	0.1378	-0.5213	0.0533	Large negative

Notes: **Country:** United States. **Research question:** Does loss of SNAP-authorized retailers reduce household SNAP participation through destruction of physical redemption infrastructure? **Policy mechanism:** SNAP benefits can only be redeemed at authorized retailers; when retailers lose authorization (due to corporate closures or the 2018 depth-of-stock rule tripling minimum inventory requirements from 12 to 36 staple items), eligible households face increased travel costs to redeem benefits, potentially reducing takeup among the eligible population. **Outcome definition:** SNAP participation rate, defined as the share of households in a census tract receiving SNAP/Food Stamp benefits in the past 12 months (ACS table B22003). **Treatment:** Continuous; net retailer exits per tract-year (deauthorizations minus new authorizations). **Data:** ACS 5-year estimates (2015–2022) merged with USDA SNAP Historical Retailer Database (703,000 retailers with authorization and end dates), census tract level, approximately 753,659 tract-year observations across 95,443 tracts. **Method:** Two-stage least squares using four instruments—pre-shock exposure to Family Dollar (post-2019), Walmart (post-2016), and A&P (post-2015) corporate closures, plus pre-2018 small-format retailer share interacted with post-2018 depth-of-stock rule—with tract and year fixed effects; standard errors clustered at the county level. **Sample:** Continental U.S. census tracts with at least 50 households (excludes tracts with zero population or suppressed ACS data). 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).

A. Standardized Effect Sizes