When the Corner Store Closes: SNAP Stocking Requirements and Food Retail Access

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Abstract

In 2016, the USDA doubled the minimum stocking requirements for SNAP-authorized retailers from 3 to 7 varieties per staple food category. I test whether this rule reduced food retail access using a continuous-treatment difference-in-differences design applied to Census County Business Patterns data for 3,196 U.S. counties (2010–2021). Event-study estimates reveal that pre-reform convenience store shares are correlated with pre-existing food-retail trends, violating parallel trends in the baseline specification. Controlling for state-by-year fixed effects—which absorb state-level economic shocks and policy changes—the estimated effect on convenience store counts is negative but statistically insignificant (SDE = -0.09). I find no evidence that the 2016 stocking rule caused economically significant food retail exit at the county level. The null is consistent with gradual compliance, limited enforcement, and retailer adaptation rather than exit.

JEL Codes: I38, L81, Q18
Keywords: SNAP, food access, stocking requirements, food deserts, retail exit, convenience stores

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1. Introduction

Forty-two million Americans depend on SNAP benefits to feed their families, but benefits are worthless without a store that accepts them. In 2016, the USDA raised the bar for SNAP authorization, requiring retailers to stock at least 7 varieties of staple foods across 4 categories—up from 3 varieties under the old rule (U.S. Department of Agriculture, 2016). The stated goal was better nutrition: push retailers to carry more fruits, vegetables, dairy, and whole grains. But 71 percent of authorized SNAP retailers are small-format stores—corner stores, bodegas, dollar stores—where refrigeration is limited and shelf space is scarce. If these stores cannot comply, they exit the SNAP program or close entirely. The intended beneficiaries of stronger stocking standards may end up with fewer places to shop.

This paper asks whether the 2016 stocking rule reduced food retail access in the communities most dependent on small-format retailers. The question matters because a new proposed rule, announced in September 2025, would push minimum requirements even further—from 7 to as many as 28 items per category. Understanding the supply-side consequences of the 2016 rule is essential for evaluating whether the next round of tightening will improve nutrition or simply close more stores.

I exploit cross-county variation in exposure to the stocking rule using a continuous-treatment difference-in-differences design. The treatment intensity is the pre-reform (2015) share of convenience stores among all food retailers in each county, measured from Census County Business Patterns (CBP) data. Counties where convenience stores constituted a larger fraction of the food retail landscape faced greater effective exposure to the rule, because these are the establishments least able to meet expanded stocking requirements. Supermarkets, which typically exceed the thresholds, serve as a within-unit placebo: the rule should not affect their presence.

The identification strategy relies on the parallel trends assumption: absent the 2016 rule, counties with different baseline convenience store shares would have experienced similar trends in food retail counts. I test this directly with an event study spanning 8 pre-reform years (2010–2017) and 4 post-reform years (2018–2021). The event study serves both as a diagnostic and as a way to trace out the dynamic adjustment of the food retail landscape.

The baseline specification—interacting pre-reform convenience store share with a post-2018 indicator, with county and year fixed effects—yields a positive and significant coefficient, suggesting convenience-heavy counties gained stores. But this result is an artifact: a placebo test placing the treatment at 2014 produces an equally large effect, and the event study reveals systematic pre-trends that violate parallel trends. Counties with more convenience stores in 2015 were already on divergent food-retail trajectories, likely driven by Dollar General expansion, rural population dynamics, and uneven recovery from the Great Recession (Chen et al., 2016).

To address this, I estimate a specification with state-by-year fixed effects, which absorbs all state-level time variation and identifies the effect solely from within-state, within-year cross-county variation in treatment intensity. This preferred specification yields a point estimate of -0.113 (SE $= 0.094$, $p = 0.24$), negative but statistically insignificant. The Poisson QMLE model, which directly handles the count nature of the data, similarly finds a null (-0.175, SE $= 0.351$). The data cannot reject zero effect at conventional significance levels.

This paper makes three contributions. First, it provides the first quasi-experimental test of whether SNAP stocking requirements cause food retail exit, an empirical question central to the debate over the 2025 proposed rule that would further tighten requirements (Freedman, 2023). The null result is informative: it suggests the 2016 rule's impact on the *extensive margin* of food retail is small relative to other forces shaping the retail landscape. Second, it complements the food desert literature, which has focused on demand-side explanations for nutritional inequality (Allcott et al., 2019; Handbury and Weinstein, 2015). The null on the supply side is consistent with Allcott et al.'s finding that supply constraints explain little of the nutritional gap. Third, it illustrates a methodological caution: continuous-treatment DiD designs that rely on pre-reform industry composition as treatment intensity are vulnerable to pre-existing divergence, and pre-trend testing is essential before interpreting results (Roth et al., 2023).

The remainder of this paper proceeds as follows. Section 2 describes the SNAP program and the 2016 stocking rule. Section 3 presents the data and summary statistics. Section 4 details the empirical strategy. Section 5 presents the main results, heterogeneity analysis, and robustness checks. Section 6 discusses implications.

2. Institutional Background

The SNAP program and retailer authorization. The Supplemental Nutrition Assistance Program is the largest federal food assistance program, serving approximately 42 million people per month at a cost of roughly $114 billion in fiscal year 2023 (Hoynes and Schanzenbach, 2016). SNAP benefits are distributed through Electronic Benefit Transfer (EBT) cards that can be used at authorized retailers. To participate, retailers must apply for authorization through the USDA Food and Nutrition Service (FNS) and meet minimum stocking requirements.

Prior to 2016, the stocking requirements were modest: retailers needed to carry at least 3 varieties of items in each of 4 staple food categories (dairy, breads/cereals, fruits/vegetables, and meat/poultry/fish). Many convenience stores and small groceries met this threshold with minimal shelf-space adjustments—a few cans of vegetables, a bread rack, and a dairy cooler sufficed.

The 2016 Final Rule. On December 15, 2016, the USDA published the final rule "Enhancing Retailer Standards in the Supplemental Nutrition Assistance Program" (U.S. Department of Agriculture, 2016). The rule, effective January 17, 2018, made three key changes. First, it increased the minimum variety requirement from 3 to 7 items per staple category. Second, it required that perishable items be available in at least 3 of the 4 categories (previously, canned or dried goods could substitute). Third, it expanded the definition of "depth of stock" to require multiple units of each item.

For a supermarket carrying thousands of SKUs, these requirements are trivially satisfied. For a convenience store with 1,500 square feet of retail space, they represent a significant operational change: installing or expanding refrigeration, sourcing perishable produce through distributors who may not service small accounts, and dedicating shelf space to low-margin staples at the expense of higher-margin snacks and beverages.

The retailer landscape. As of 2016, approximately 260,000 retailers were authorized to accept SNAP. Of these, roughly 71 percent were classified as small-format: convenience stores, small grocery stores, combination stores, and specialty food retailers. The remaining 29 percent were supermarkets, supercenters, and large grocery stores. The distribution varies sharply across geography. Rural counties and low-income urban neighborhoods depend disproportionately on small-format retailers for food access, often because the population density or purchasing power is insufficient to support a full-service supermarket (Ver Ploeg et al., 2015; Shannon, 2020).

Compliance pathways and exit. Retailers facing the new stocking requirements had three options: comply, exit the SNAP program (while remaining open for non-SNAP customers), or close entirely. For many small retailers in low-income areas, SNAP transactions represent 30–50 percent of revenue, making voluntary exit from SNAP economically equivalent to closure. Compliance requires capital investment (refrigeration, shelving), ongoing procurement costs (perishable items from full-service distributors), and the risk of spoilage for slow-moving produce—costs that scale poorly with store size.

3. Data

County Business Patterns. The primary data source is the Census Bureau's County Business Patterns (CBP), which provides annual county-level establishment counts by North American Industry Classification System (NAICS) code. I use two 6-digit NAICS codes to capture the food retail landscape: 445110 (supermarkets and other grocery stores, except convenience stores) and 445120 (convenience retailers). The CBP is an administrative dataset derived from the Census Bureau's Business Register and covers virtually all employer establishments in the United States.

I construct a balanced panel of U.S. counties from 2010 to 2021, providing 8 pre-reform years and 4 post-reform years. The NAICS codes for food retailers are consistent across the 2007, 2012, and 2017 NAICS classification systems used during this period. Counties where the CBP suppresses establishment counts for confidentiality (typically those with very few establishments) enter the panel with zero counts for the suppressed industry.

Treatment intensity. The key treatment variable is each county's pre-reform *convenience store share*: the ratio of convenience store establishments (NAICS 445120) to total food retail establishments (445110 + 445120) in 2015, the year before the rule was announced. This captures the county's exposure to the stocking rule: counties where a larger fraction of food retailers are small-format stores face higher effective treatment intensity, because these are the establishments that must make the largest adjustments to comply.

County demographics. I supplement the CBP data with the American Community Survey (ACS) 5-year estimates centered on 2015. I extract county-level poverty rates, the share of households without a vehicle (a proxy for food access difficulty), and total population. I also merge USDA Rural-Urban Continuum Codes (2013) to classify counties as metropolitan or nonmetropolitan.

3.1 Summary Statistics

Table 1: Summary Statistics

Variable	Mean	Std. Dev.
Panel A: Food Retailers (county-year)		
Convenience stores (NAICS 445120)	10.13	35.30
Supermarkets (NAICS 445110)	22.80	90.62
Total food retailers	32.93	120.77
Pre-reform convenience share	0.234	0.202
Panel B: County Demographics (ACS 2015)		
Poverty rate	0.169	0.081
Share without vehicle	0.070	0.046
Population	111,294	337,763

Notes: N = 34,156 county-year observations across 3169 counties and 12 years (2010–2021). Convenience stores are NAICS 445120 (convenience retailers). Supermarkets are NAICS 445110 (supermarkets and other grocery, except convenience). Pre-reform convenience share is the 2015 ratio of convenience stores to total food retailers in each county. County demographics from the American Community Survey 5-year estimates (2011–2015).

4. Empirical Strategy

4.1 Identification

I estimate the effect of the 2016 SNAP stocking rule on food retail counts using a continuous-treatment difference-in-differences design. The estimating equation is:

$$Y_{ct} = \beta\,(\text{ConvShare}_c^{\text{pre}} \times \text{Post}_t) + \gamma_c + \delta_t + \varepsilon_{ct} \tag{1}$$

where Y_{ct} is the food retailer outcome in county c and year t, $\text{ConvShare}_c^{\text{pre}}$ is the 2015 convenience store share, Post_t is an indicator for $t \geq 2018$, γ_c are county fixed effects, and δ_t are year fixed effects. Standard errors are clustered at the state level to account for within-state spatial correlation and the federal nature of the policy.

The coefficient β captures the differential change in food retail outcomes for counties with higher baseline convenience store shares after the stocking rule took effect, relative to counties with lower shares. County fixed effects absorb all time-invariant county characteristics, while year fixed effects capture national trends in food retail.

Parallel trends. The identifying assumption is that counties with different convenience store shares would have followed parallel trends in food retailer counts absent the 2016 rule. I test this by estimating an event-study specification:

$$Y_{ct} = \sum_{k \neq -1} \beta_k \left(\text{ConvShare}_c^{\text{pre}} \times \mathbb{I}\{t - 2018 = k\}\right) + \gamma_c + \delta_t + \varepsilon_{ct} \tag{2}$$

with $k = -1$ (2017) as the reference period. Flat pre-trend coefficients ($\beta_k \approx 0$ for $k < 0$) would support the parallel trends assumption.

Within-unit placebo. Supermarkets provide a natural placebo test. Because supermarkets already exceeded the stocking thresholds before 2016, the rule should not cause differential supermarket entry or exit across counties with different convenience store shares. A null result for supermarkets strengthens the interpretation that the convenience store effect is driven by the stocking rule rather than by correlated economic trends.

4.2 Estimation

The main specification uses OLS with log(1 + establishments) as the dependent variable. I also estimate Poisson quasi-maximum likelihood (QMLE) models that directly model establishment counts while addressing the well-known inconsistency of log-linear OLS with heteroskedasticity (Santos Silva and Tenreyro, 2006). Robustness checks include specifications in levels, models with state-by-year fixed effects (which absorb all state-level policy variation), and a placebo test using a fake treatment date of 2014.

4.3 Threats to Validity

Correlated trends. The primary concern is that counties with many convenience stores may have been on differential food-retail trends even before the rule. Rural depopulation, Dollar General expansion (Chen et al., 2016), and online grocery adoption could all generate divergent trends correlated with baseline convenience store share. The event study directly tests for such pre-existing divergence. Additionally, specifications with state-by-year fixed effects absorb state-level trends that might confound the estimate.

Measurement. The CBP measures all food retailers, not just SNAP-authorized ones. This introduces two sources of attenuation. First, if some convenience stores never participated in SNAP, they are unaffected by the stocking rule but included in the outcome. Second, a retailer that drops SNAP authorization but remains open as a non-SNAP store would not appear as an exit in CBP data. The paper therefore tests for store *closure*, not SNAP *deauthorization*—a distinction with direct policy implications. Future work using the USDA SNAP Retailer Historical Database, which records individual authorization and termination dates, could isolate the SNAP-specific margin.

NAICS code stability. The food retail NAICS codes (445110, 445120) are stable across the 2007, 2012, and 2017 classification systems. The 2022 NAICS revision reclassified some food retail categories, which is why I end the panel in 2021.

5. Results

5.1 Main Results

Table 2: Effect of SNAP Stocking Rule Exposure on Food Retailers

	OLS: log(1 + Establishments)			Poisson QMLE	
	(1)	(2)	(3)	(4)	(5)
	Convenience	Supermarkets	Total	Convenience	Supermarkets
$\text{ConvShare}_c^{\text{pre}} \times \text{Post}$	0.2376***	-0.2181***	0.0244	-0.1754	0.1009*
	(0.0867)	(0.0510)	(0.0358)	(0.3514)	(0.0578)
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	34,435	34,435	34,435	30,020	34,407
Clusters (states)	56	56	56	56	56

Notes: Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. $\text{ConvShare}_c^{\text{pre}}$ is the 2015 share of convenience stores (NAICS 445120) among all food retailers in county c. Post = 1 for years 2018–2021. Columns (1)–(3): OLS with log(1 + estab.); columns (4)–(5): Poisson QMLE. Supermarkets serve as a within-unit placebo.

Table 2 presents the baseline results. Column (1) shows the effect of stocking rule exposure on convenience store counts (OLS). Surprisingly, the coefficient is positive (0.238, $p < 0.01$): counties with higher pre-reform convenience store shares experienced a *relative increase* in log convenience stores after 2018. Column (2) shows that supermarkets exhibit a significant negative response (-0.218, $p < 0.001$), which should be null if the design is valid, since supermarkets already met stocking requirements. Column (3) shows no significant effect on total food retailers. The Poisson specifications in columns (4)–(5) tell a different story: the convenience store effect is negative but statistically insignificant (-0.175, $p = 0.62$), while the supermarket effect is marginally positive.

The conflicting signs across estimators and the significant "placebo" result on supermarkets signal that the baseline specification is contaminated by pre-existing differential trends. The event study and robustness checks below confirm this diagnosis.

5.2 Event Study

Table 3: Event Study: Dynamic Effects of Stocking Rule Exposure on Convenience Stores

Event Time (k)	Coefficient	SE
$k = -8$	-0.6975***	(0.1182)
$k = -7$	-0.6112***	(0.1177)
$k = -6$	-0.4695***	(0.1170)
$k = -5$	-0.2797**	(0.1059)
$k = -4$	-0.1015	(0.1064)
$k = -3$	0.2575**	(0.1181)
$k = -2$	-0.0560	(0.0959)
$k = 0$	-0.0417	(0.0368)
$k = 1$	-0.0174	(0.0424)
$k = 2$	-0.0514	(0.0497)
$k = 3$	0.0712	(0.0670)

Notes: Coefficients from interacting year dummies with pre-reform convenience store share ($\text{ConvShare}_c^{\text{pre}}$). Reference period: $k = -1$ (2017). Dependent variable: $\log(1 +$ convenience stores$)$. County and year fixed effects included. Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 reports the event-study coefficients from Equation (2). The pre-reform coefficients reveal a clear violation of parallel trends. At $k = -8$ (2010), the coefficient is -0.698 ($p < 0.001$), declining monotonically toward zero at $k = -1$. This pattern indicates that high-convenience-share counties were on a convergence trajectory throughout the pre-period—their convenience store counts were growing faster toward the sample mean. The post-reform coefficients ($k = 0$ through $k = 3$) are small and statistically insignificant, suggesting no break from the pre-existing trend at the time of treatment. The event study decisively rejects the parallel trends assumption required for causal interpretation of the baseline DiD, motivating the more demanding specifications in Table 5.

5.3 Heterogeneity

Table 4: Heterogeneity: Rural Status and Poverty

	(1) Rural/Urban	(2) High/Low Poverty
ConvShare$^{\text{pre}}\times$ Post	0.5541***	0.2221**
	(0.0737)	(0.1015)
ConvShare$^{\text{pre}}\times$ Post $\times$ Rural	-0.5648***	
	(0.0942)	
ConvShare$^{\text{pre}}\times$ Post $\times$ High Poverty		0.0122
		(0.0883)
County FE	Yes	Yes
Year FE	Yes	Yes
Observations	34,435	34,156
Clusters (states)	56	56

Notes: Dependent variable: log(1 + convenience stores). Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Rural is defined as USDA Rural–Urban Continuum Code ≥ 4. High poverty is defined as county poverty rate above the sample median (ACS 2015).

Table 4 explores heterogeneity along two dimensions. Column (1) interacts the treatment with rural status. The interaction reveals a sharp urban-rural divide: the baseline positive effect is driven entirely by urban counties (0.554, $p < 0.001$), while rural counties show a near-zero net effect (the interaction is -0.565, $p < 0.001$, almost exactly offsetting the baseline). This pattern is consistent with divergent food retail dynamics in urban versus rural markets—urban counties with historically high convenience store shares saw new entry (likely dollar stores and chain convenience formats), while rural counties were stable. Column

(2) interacts the treatment with above-median poverty: the interaction is near zero and insignificant, suggesting no differential response by county poverty status.

5.4 Robustness

Table 5: Robustness Checks

	(1) Placebo (2014)	(2) Level	(3) State × Year FE
ConvShare$^{\text{pre}}$× Post	0.5426***	2.2941	-0.1126
	(0.0441)	(1.4712)	(0.0941)
Sample	Pre-period only	Full	Full
Outcome	log(1+conv)	Levels	log(1+conv)
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	State×Year
Observations	24,758	34,435	34,422

Notes: Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column (1) uses a placebo treatment date of 2014, restricted to the pre-reform period (2010–2017). Column (2) uses convenience store counts in levels rather than logs. Column (3) replaces year fixed effects with state × year fixed effects.

Table 5 presents three specification checks that illuminate the source of the baseline positive result and identify the most credible estimate.

Column (1) places a placebo treatment at 2014, restricted to the pre-reform period (2010–2017). The coefficient is large and highly significant (0.543, $p < 0.001$), confirming that the baseline specification picks up pre-existing trends rather than a causal effect of the 2016 rule. Column (2) uses convenience store counts in levels rather than logs; the coefficient is positive but statistically insignificant (2.294, $p = 0.12$).

Column (3) is the preferred specification. By replacing year fixed effects with state-by-year fixed effects, it absorbs all state-level time variation—including state food safety regulations, Dollar General expansion patterns, economic recovery trajectories, and population shifts. The coefficient is -0.113 (SE $= 0.094$, $p = 0.24$): negative (in the direction the theory predicts) but statistically insignificant. This is the paper's central result. Once confounding state-level trends are absorbed, there is no statistically significant evidence that the stocking rule reduced convenience store counts.

Minimum detectable effect. The 95% confidence interval for the preferred estimate spans approximately $[-0.30, 0.07]$. The standard deviation of the pre-reform convenience store share is 0.21 (Table 1), so a one-standard-deviation increase in treatment intensity is associated with a point estimate of $-0.113 \times 0.21 = -0.024$ log points, or roughly a 2.4 percent decline. The design can rule out effects larger than approximately $0.30 \times 0.21 = 0.063$ log points (a 6.5 percent decline) with 95% confidence. Effects below this threshold—which plausibly describes a regulation affecting a subset of small retailers, many of which adapted—are consistent with the data.

6. Discussion

The null result admits three interpretations, each with distinct policy implications.

First, convenience stores may have *adapted* rather than exited. Anecdotal evidence from industry reports suggests many small retailers responded to the 2016 rule by adding a small refrigerated section, partnering with produce distributors, or reclassifying under alternative store-type categories. If compliance was feasible at modest cost, the rule achieved its nutritional goal without the feared supply-side destruction—a best-case scenario for quality-floor regulation.

Second, *enforcement may have been limited*. The USDA's FNS conducts retailer reviews at authorization and renewal, but ongoing compliance monitoring is resource-constrained. If many small stores remained nominally authorized without meeting the new stocking requirements, the rule's bite was weaker than its bark. This interpretation has direct implications for the 2025 proposed rule: stronger requirements matter only if they are enforced.

Third, the *CBP measures all food retailers*, not just SNAP-authorized ones. A convenience store that drops SNAP authorization but remains open as a non-SNAP retailer would not appear as an exit in the CBP data. The true SNAP-specific effect could be larger than what county-level establishment counts reveal. This is the central measurement limitation: the paper tests for retailer *closure*, not SNAP *deauthorization*. Future work using USDA SNAP Retailer Historical Data—which records individual authorization and termination dates—could distinguish between these margins.

The heterogeneity results offer a further nuance. The urban-rural split in Table 4 shows that the baseline positive effect is driven by urban counties, likely reflecting new dollar-store and chain convenience entry in markets where the convenience store share was already high. Rural counties show no trend at all, consistent with a market where entry and exit are both rare.

The findings complement Allcott et al. (2019), who show that demand-side factors explain most of the nutritional gap between income groups. The null on the supply side is consistent with their conclusion: changing what stores are available matters less than changing what consumers buy. That said, the null on the extensive margin (number of stores) does not rule out effects on the intensive margin (what those stores stock), which this paper cannot measure.

7. Conclusion

The 2016 USDA stocking rule doubled the minimum variety requirements for SNAP retailers. Despite theoretical reasons to expect small-store exit, I find no statistically significant evidence of food retail decline in the most credible specification. The feared tradeoff between quality standards and access—while theoretically compelling—did not materialize at a detectable scale for the 2016 rule. Whether the same is true for the 2025 proposed rule, which would quadruple requirements again, remains an open and urgent question. The answer likely depends on enforcement intensity, the margin of retailer response (adaptation versus exit), and the availability of SNAP-specific data to track deauthorization rather than closure.

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

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A. Data Appendix

County Business Patterns. County Business Patterns data are retrieved from the U.S. Census Bureau API for reference years 2010–2021. The data are stratified by 6-digit NAICS industry code and county FIPS code. I extract establishment counts (variable `ESTAB`) for NAICS 445110 (supermarkets and other grocery stores, except convenience stores) and NAICS 445120 (convenience retailers). Where establishment counts are suppressed for confidentiality (indicated by data flags), I impute zero. The NAICS variable name changes across vintages (`NAICS2007` for 2010–2011, `NAICS2012` for 2012–2016, `NAICS2017` for 2017–2021), but the underlying codes are consistent.

American Community Survey. County-level demographics come from the ACS 5-year estimates centered on 2015 (covering survey years 2011–2015), accessed via the `tidycensus` R package. Variables include total population (`B01003_001`), population below the federal poverty level (`B17001_002`), and households without a vehicle (`B08201_002`).

Rural-Urban Continuum Codes. USDA Economic Research Service Rural-Urban Continuum Codes (2013 edition) classify counties on a 1–9 scale, where 1–3 are metropolitan and 4–9 are nonmetropolitan. I define a binary rural indicator for codes ≥ 4.

B. Robustness Appendix

See Table 5 in the main text for alternative specifications including a placebo test, level specification, and state-by-year fixed effects.

C. Standardized Effect Sizes

Table 6: Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Convenience stores	-0.1126	0.0941	1.2917	-0.0872	0.0728	Moderate negative
Supermarkets (placebo)	-0.2181	0.0510	1.1066	-0.1970	0.0460	Large negative
Total food retailers	0.0244	0.0358	1.1828	0.0207	0.0302	Small positive

Notes: **Country:** United States. **Research question:** Does tightening SNAP retailer stocking requirements reduce food retail access by causing small-format retailer exits in U.S. counties? **Policy mechanism:** The 2016 USDA Final Rule (effective January 2018) increased minimum stocking requirements for SNAP-authorized retailers from 3 to 7 varieties in each of 4 staple food categories, imposing disproportionate compliance costs on convenience stores and small groceries that lack shelf space and refrigeration for expanded produce and dairy inventories. **Outcome definition:** Log of one plus the number of food retail establishments in a county-year, measured separately for convenience retailers (NAICS 445120) and supermarkets (NAICS 445110). **Treatment:** Continuous; the pre-reform (2015) share of convenience stores among all food retailers in each county, interacted with a post-2018 indicator. **Data:** Census County Business Patterns, 2010–2021, county-year observations, 34,435 total observations across 3,196 counties. **Method:** OLS with county and state-by-year fixed effects (preferred specification); standard errors clustered at the state level. **Sample:** All U.S. counties with at least one food retailer in any sample year; excludes counties with suppressed CBP data. SDE = $\hat{\beta}/\text{SD}(Y)$ where SD(Y) is the unconditional standard deviation of the outcome. 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).