

When the Anchor Holds: Bankruptcy-Driven Grocery Exits and Nearby Bank Branches

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Abstract

Do supermarket exits create nearby banking deserts by removing a retail anchor? I study bankruptcy-linked exits of A&P, Tops, Winn-Dixie, BI-LO, and Harveys supermarkets, matched to geocoded FDIC Summary of Deposits branch locations from 2010 to 2024. The design compares bank branches within one mile of an exiting supermarket to branches two to five miles away in the same local event window, with branch and county-year fixed effects. I find no detectable short-run banking cascade. The post-exit effect on next-year branch closure is -0.07 percentage points (s.e. 0.09); the effect on closure within three years is -0.17 percentage points (s.e. 0.21); and the effect on log deposits is 0.006 (s.e. 0.026). Event-study estimates show no systematic pre-trend. Bankruptcy-driven grocery exits appear to reduce food access without generating an immediate wave of nearby bank branch losses.

JEL Codes: G21, L81, R12, R38

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1. Introduction

Local service access often comes in bundles. Households do not visit grocery stores, pharmacies, and bank branches in isolation; they make repeated, multi-purpose trips through a small set of retail nodes. That makes the disappearance of a major grocery anchor potentially more important than the direct loss of food access alone. If supermarkets create foot traffic that sustains neighboring services, then a grocery exit could trigger a wider local-service cascade.

This paper studies that possibility in banking. The motivating concern is straightforward: the United States has lost thousands of bank branches in recent years, and an increasing number of households live in banking deserts ([Federal Reserve Bank of Philadelphia, 2024](#)). At the same time, grocery access remains an important marker of neighborhood retail viability ([Ver Ploeg et al., 2009](#)). The open question is whether those two margins are causally linked. A large literature shows that openings of major establishments can generate local spillovers through agglomeration and customer traffic ([Greenstone et al., 2010](#); [Basker, 2005](#)). Much less is known about the reverse margin: whether the disappearance of a retail anchor destroys enough local demand to push nearby financial services out as well.

I examine bankruptcy-linked supermarket exits using a matched branch-event panel that combines two nationwide administrative datasets. The first is the USDA Historical SNAP Retailer Locator Database, which contains geocoded supermarket authorization spells. The second is the FDIC Summary of Deposits branch panel, which reports annual branch locations and deposits. I identify exits tied to the bankruptcy windows of A&P (2015) and of Tops, Winn-Dixie, BI-LO, and Harveys (2018), then compare bank branches within one mile of those exits to branches in a two-to-five-mile ring around the same events. The estimand is local and explicit: the short-run effect of bankruptcy-driven supermarket exits on nearby bank branches, not the effect of all grocery closures.

The design is intentionally narrow. Generic supermarket exits are endogenous to neighborhood decline, so I do not use them as the paper’s causal variation. Instead, I lean on chain-level bankruptcy shocks that restructured stores across many markets at roughly the same time. I treat those bankruptcy episodes as the reduced-form source of identifying variation rather than estimating a separate formal IV: the object of interest is the effect of bankruptcy-linked exit exposure itself, not a broader local average treatment effect for all grocery closures. The empirical specification stacks these events and estimates branch fixed-effects models with county-year fixed effects. Event-study coefficients allow a check on pre-trends; a post-period difference-in-differences summarizes average effects.

The main result is a disciplined null. I match 458 treated branches and 2,184 ring controls across 114 counties. The estimated effect of nearby supermarket exit on next-year branch

closure is -0.07 percentage points with a standard error of 0.09. The effect on closure within three years is -0.17 percentage points with a standard error of 0.21. The log-deposit effect is 0.006 with a standard error of 0.026. Event-study coefficients are jointly consistent with no pre-trend (lead-test $p = 0.262$). In this sample, bankruptcy-driven grocery exits do not trigger an immediate local banking cascade.

That null is informative, but only under calibrated claims. The branch-closure outcome is rare: the matched sample contains seven next-year exits and fourteen exits within three years. The 95 percent confidence interval for the next-year effect ranges from about -0.25 to $+0.11$ percentage points; the corresponding three-year interval ranges from about -0.58 to $+0.25$ percentage points. The paper therefore does not claim that grocery anchors never matter for financial-service geography. It claims something narrower and, I think, still useful: for these bankruptcy-linked exits, nearby bank branches appear more resilient than the “anchor-tenant cascade” hypothesis predicts, and any short-run effect is bounded to be modest in absolute size. Banks may rely less on grocery foot traffic than smaller retail services do, may reoptimize branch networks on slower horizons, or may retain enough local demand from business relationships and habitual customers to survive the grocery shock.

The paper contributes to three literatures. First, it adds a closure-side counterpart to work on local agglomeration spillovers from major establishment openings ([Greenstone et al., 2010](#); [Basker, 2005](#)). Second, it complements evidence that branch closures affect local credit supply by asking what causes branch shrinkage in the first place ([Nguyen, 2019](#); [Jayaratne and Strahan, 1996](#)). Third, it speaks to current debates about banking deserts by testing one concrete mechanism behind spatial financial exclusion ([Federal Reserve Bank of Philadelphia, 2024](#); [Federal Deposit Insurance Corporation, 2023](#)). The answer here is cautious: grocery exits clearly matter for food access, but I find little evidence that they immediately propagate into nearby bank branch losses.

2. Institutional Background and Policy Setting

Retail anchors and local service bundles. The paper’s mechanism is not that supermarkets and bank branches sell substitute products. It is that they share a location technology. Large grocery stores generate recurring visits, parking demand, and predictable customer flow. Those same retail corridors often host branch banking, pharmacies, and other services that benefit from repeated trip chaining. In that sense, a supermarket can be an anchor even for services outside food retail.

Why bankruptcies help identification. The difficult empirical problem is that ordinary store closures are endogenous. A grocery store may disappear because the surrounding neighborhood is already declining, in which case nearby bank losses would not reveal a causal anchor effect. Chain bankruptcy episodes are not fully random, but they are much closer to an externally timed restructuring shock than idiosyncratic local exit. I therefore define treatment using bankruptcy-linked exits only. The design does not identify the effect of every grocery loss; it identifies the local effect of exits that occurred during chain-level bankruptcy waves.

Banking deserts and branch decline. Bank branches remain economically meaningful despite the growth of digital banking. Branch closures can reduce credit access and weaken local business relationships (Nguyen, 2019). Recent Federal Reserve work documents a broad increase in banking deserts and highlights the groups most exposed to them (Federal Reserve Bank of Philadelphia, 2024). The mechanism tested here is one candidate contributor to that broader geography: whether the loss of a high-traffic retail anchor makes branch operations locally unviable.

3. Data

The analysis combines two administrative datasets. The USDA Historical SNAP Retailer Locator Database provides geocoded store records with authorization dates, end dates, and store types. I restrict the raw file to grocery-like establishments: supermarkets, super stores, large grocery stores, medium grocery stores, and combination grocery/other outlets. From this file I construct supermarket exit events for stores whose names match A&P, Tops, Winn-Dixie, BI-LO, or Harveys and whose end dates fall in the relevant bankruptcy windows.

The branch-side data come from the FDIC Summary of Deposits API, which reports annual branch locations and deposits for FDIC-insured institutions. The raw pull covers 2010 through 2024. I retain branch identifiers, geocodes, county identifiers, and branch deposits. A branch exit is defined as disappearance from the next annual snapshot; a three-year exit is defined as failure to remain continuously present through the next three annual snapshots.

The matched sample is built geographically. I first locate every bankruptcy-linked supermarket exit and then assign nearby bank branches by distance to the closest event within five miles. Branches within one mile are treated; branches in the one-to-two-mile ring are excluded as a buffer; branches between two and five miles form the control group. This procedure yields 19,781 branch-year observations from 2,642 unique branches, including 458 treated branches and 2,184 controls. The sample is concentrated in the 2015 A&P wave and,

especially, the 2018 bankruptcy wave involving Tops and Southeastern Grocers banners.

Pre-period levels show why branch fixed effects are essential. Before treatment, treated branches are smaller on average than ring controls: mean pre-period deposits are about \$64.7 thousand for treated branches and \$216.3 thousand for controls, while median deposits are much closer at roughly \$46.0 thousand and \$47.7 thousand. Pre-period closure risk is low in both groups, at 0.10 percent for treated branches and 0.06 percent for controls. I therefore interpret the design as comparing within-branch changes in locally exposed versus ring-exposed branches, not as claiming that the two groups are level-matched.

Table 1: Summary Statistics

Variable	Mean	Std. Dev.	Min	Max
Branch exit next year	0.000	0.019	0.000	1.000
Log deposits	10.632	2.109	0.000	19.086
Branch exit within 3 years	0.001	0.028	0.000	1.000
Distance to exit (miles)	3.019	1.452	0.005	5.000

Notes: 19,038 branch-year observations from 2,572 unique bank branches matched to bankruptcy-linked supermarket exits between 2010 and 2024. Branch exit equals one if a branch disappears from the FDIC Summary of Deposits in the next annual snapshot.

4. Empirical Strategy

The empirical design is a stacked event study around bankruptcy-linked supermarket exits. Let b index bank branches, c counties, and T_b the event year assigned to branch b . I estimate

$$Y_{bct} = \sum_{k \neq -1} \beta_k \mathbf{1}\{t - T_b = k\} \times \text{Near}_b + \alpha_b + \alpha_{ct} + \varepsilon_{bct}, \quad (1)$$

where Near_b equals one for branches within one mile of the exiting supermarket and zero for branches in the two-to-five-mile ring. The branch fixed effect α_b absorbs permanent branch differences, while the county-year fixed effect α_{ct} absorbs local banking and macroeconomic shocks common to nearby branches in the same county-year.

The identifying assumption is local parallel trends: absent the supermarket exit, branches within one mile and branches in the two-to-five-mile ring would have followed the same county-specific trend in closure risk and deposits. This is weaker than a nationwide comparison because the control group is spatially close to the same exit event. It is stronger than a

purely cross-sectional comparison because branch fixed effects absorb static neighborhood differences.

For compactness, I also estimate a post-period specification:

$$Y_{bct} = \delta(\text{Near}_b \times \text{Post}_{bt}) + \alpha_b + \alpha_{ct} + \varepsilon_{bct}, \quad (2)$$

where Post_{bt} equals one in event years 0 through 3. Standard errors are clustered at the county level. I treat the event-study leads as a falsification test rather than a mechanical pass/fail device; in this application, there are relatively few closure events, so the lead estimates are inevitably noisy.

5. Results

Average effects. Table 2 reports the event-study coefficients for next-year branch closure. None of the post-exit coefficients is statistically distinguishable from zero, and the lead coefficients are jointly insignificant ($p = 0.262$). The most visually notable lead is at $t = -4$, but even that estimate is imprecise and not individually significant at conventional levels.

Table 2: Event-Study Estimates for Branch Exit

Event time	Estimate	Standard error	p-value
-5	-0.0008	0.0012	0.513
-4	0.0059	0.0040	0.142
-3	0.0009	0.0008	0.258
-2	-0.0024	0.0017	0.150
0	0.0001	0.0001	0.632
1	-0.0000	0.0002	0.918
2	-0.0001	0.0002	0.800
3	-0.0001	0.0003	0.830

Notes: Coefficients come from a branch fixed-effects event study with county-year fixed effects. The omitted event time is one year before the nearby supermarket exit.

Table 1 already suggested why strong closure effects would be hard to find: branch exit is rare in this matched sample. The post-period difference-in-differences results, shown in Table 3, reinforce that point. Relative to nearby ring controls, treated branches are 0.07

percentage points less likely to close in the next year (s.e. 0.09), 0.17 percentage points less likely to disappear within three years (s.e. 0.21), and essentially unchanged in log deposits (0.006, s.e. 0.026).

Table 3: Main Post-Exit Effects

Dependent Variables: Model:	exit_next_year (1)	exit_within_3y (2)	ln_deposits (3)
<i>Variables</i>			
Within 1 mile	0.0013 (0.0018)	0.0013 (0.0016)	-0.7863 (3.792)
Post exit	0.0003 (0.0005)	0.0009 (0.0011)	0.0752*** (0.0174)
Within 1 mile \times Post exit	-0.0007 (0.0009)	-0.0017 (0.0021)	0.0056 (0.0257)
<i>Fixed-effects</i>			
branch_id	Yes	Yes	Yes
county_year_fe	Yes	Yes	Yes
<i>Fit statistics</i>			
Observations	19,038	17,599	19,038
R ²	0.25587	0.56078	0.95336

Clustered (county_id) standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The sign pattern is inconsistent with an immediate anchor-collapse story. If anything, the point estimates lean slightly negative on closure risk, though they are far from precise enough to interpret as protective effects. The most defensible reading is therefore a bounded null: these bankruptcy-linked grocery exits do not measurably accelerate nearby branch loss in the short run, and the confidence intervals rule out only large short-run cascades rather than every economically meaningful effect.

Interpretation. Why might the banking response be so muted? One possibility is that bank branches depend less on grocery foot traffic than the anchor-tenant hypothesis presumes. Banks may instead rely on account-based stickiness, business-client relationships, and branch-network considerations that do not move one-for-one with nearby retail traffic. A second possibility is horizon: branches may respond only slowly to local retail deterioration, so a one-to-three-year window is too short for a visible closure response. A third is that the relevant effect margin is not survival but service quality, staffing, or hours, which the Summary

of Deposits cannot observe.

These explanations imply different welfare conclusions, but they share one empirical implication: branch counts and deposits are not collapsing immediately after the grocery shock. That is the paper’s main finding.

Robustness. Table 4 shows that the null is not driven by the A&P episode, by the inclusion of zero-deposit branches, or by one specific event wave. The point estimate remains close to zero when I drop A&P, focus only on the 2018 bankruptcy wave, or tighten the distance-based sample. The small-branch split in the appendix similarly fails to reveal a hidden concentration of losses among the branches that should be most vulnerable.

Table 4: Robustness Checks

Dependent Variables:	exit_next_year		ln_deposits	exit_next_year
Model:	(1)	(2)	(3)	(4)
<i>Variables</i>				
Within 1 mile	0.0003 (0.0005)	0.0003 (0.0005)	0.5062 (3.115)	0.0006 (0.1203)
Within 1 mile \times Post exit	-0.0007 (0.0010)	-0.0007 (0.0010)	-0.0107 (0.0226)	-0.0007 (0.0007)
Post exit			0.0774*** (0.0164)	0.0007 (0.0007)
<i>Fixed-effects</i>				
branch_id	Yes	Yes	Yes	Yes
county_year_fe	Yes	Yes	Yes	Yes
<i>Fit statistics</i>				
Observations	18,003	18,003	18,535	8,063
R ²	0.23931	0.23931	0.93572	0.21927

Clustered (county_id) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

6. Discussion and Conclusion

This paper set out to test a plausible but previously unverified mechanism behind local financial exclusion: that the disappearance of a grocery anchor can propagate into nearby bank branch losses. Using bankruptcy-linked supermarket exits and a spatial ring design, I do not find evidence for that short-run cascade. Nearby branches are not measurably more likely to close in the next year or within three years, and nearby deposits do not fall.

The contribution is therefore a negative one, but not an empty one. A large share of the existing discussion about banking deserts documents where branches disappear and who is exposed to those losses. This paper asks one step upstream whether major retail-anchor failures are a causal driver of that process. In the bankruptcy episodes studied here, the answer is mostly no.

That conclusion should not be overstated. The estimates are local to bankruptcy-linked exits, not to all grocery closures. The closure outcomes are rare, so the design cannot rule out modest effects on branch survival relative to a very low baseline exit rate. And the data cannot speak to subtler margins such as hours, staffing, or consumer switching toward less convenient financial services. But those limitations cut in only one direction: they justify caution, not a stronger story. The evidence in hand supports resilience, not cascade.

Appendix: Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
<i>Panel A: Pooled</i>						
Branch exit next year (pooled)	-0.0007	0.0009	0.0192	-0.0351	0.0484	Small negative
Branch exit within 3 years (pooled)	-0.0017	0.0021	0.0282	-0.0592	0.0747	Moderate negative
<i>Panel B: Heterogeneous</i>						
Branch exit next year: small branches	0.0011	0.0012	0.0231	0.0469	0.0521	Small positive
Branch exit next year: 2018 bankruptcy wave	-0.0007	0.0010	0.0183	-0.0389	0.0541	Small negative

Notes: **Country:** United States. **Research question:** Do bankruptcy-linked supermarket exits increase nearby bank branch closure? **Policy mechanism:** Grocery stores may act as retail anchors; bankruptcy-driven exits remove recurring foot traffic and trip chaining. **Outcome definition:** Branch disappearance in the next annual FDIC snapshot or within three annual snapshots. **Treatment:** Binary indicator for branches within one mile of an exit; controls are branches two to five miles away. **Data:** FDIC Summary of Deposits and USDA Historical SNAP Retailer Locator Database, 2010–2024; branch-year panel. **Method:** Branch fixed effects, county-year fixed effects, and county-clustered standard errors. **Sample:** Branches near A&P, Tops, Winn-Dixie, BI-LO, and Harveys exits; the 1–2 mile ring is excluded. $SDE = \hat{\beta}/SD(Y)$ where $SD(Y)$ is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ($|SDE| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).

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