The Vanishing Aisle That Didn't Matter: Supermarket Closures and Birth Outcomes

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Abstract

When a supermarket disappears, do pregnant women pay the price? I link the USDA SNAP Retailer Historical Database—recording every authorization and closure of 703,000 SNAP retailers from 2005 to 2025—to CDC natality microdata covering 25 million births across 51 states (2016–2023). Using five national chain bankruptcies as instruments for supermarket closures, I find no detectable effect of state-level supermarket exit rates on low birth weight, preterm birth, or gestational diabetes. The null holds across OLS, IV, event-study, and population-weighted specifications, and across states with high and low Medicaid birth shares. These results extend the Allcott et al. (2019) finding that food access changes don't reshape nutrition: at the state level, the supermarket-to-birth-outcome channel is a phantom. The evidence suggests that if grocery closures harm infant health, the margin of action is hyper-local—neighborhood, not state.

JEL Codes: I12, I18, Q18, J13

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1. Introduction

Every year, hundreds of American supermarkets close their doors—not because demand has vanished, but because a corporate parent filed for bankruptcy in a distant city. Between 2015 and 2020, five major chain collapses shuttered over 1,100 grocery stores, from A&P's 300 northeastern locations to Southeastern Grocers' 580 stores across the South. For the pregnant women in affected neighborhoods, these closures eliminated the nearest source of affordable fresh produce during the nine months when maternal nutrition matters most.

The question of whether food access affects health has generated an influential but ambiguous literature. Allcott et al. (2019) found that food desert residents' diets are largely explained by demand factors rather than supply, leading some to conclude that supermarket closures are nutritionally inconsequential. But purchasing behavior is not the same as health. The epidemiological evidence is unambiguous: maternal nutrition during pregnancy—particularly intake of folate, iron, and fresh produce—directly affects birth weight and gestational length (Abu-Saad and Fraser, 2010). The puzzle, then, is whether the loss of a supermarket transmits through to birth outcomes, or whether pregnant women substitute toward alternative food sources quickly enough to prevent harm.

This paper provides the first causal estimates of the effect of supermarket closures on birth outcomes. I construct a state-year panel linking the USDA SNAP Retailer Historical Database—which records the authorization and end dates of every SNAP-participating retailer in the United States from 2005 to 2025—to CDC natality microdata covering the universe of U.S. births from 2016 to 2023. My identification strategy exploits five national chain bankruptcies as instruments for local supermarket exit rates. The key insight is that these corporate failures were driven by financial distress at the parent company level—leveraged buyouts, pension obligations, and competitive pressure from discount retailers—and are therefore orthogonal to local health trends conditional on state and year fixed effects.

The instrument is strong: pre-existing chain presence interacted with post-bankruptcy timing powerfully predicts supermarket closures, with first-stage F-statistics well above conventional thresholds. The exclusion restriction requires that chain bankruptcy exposure affects birth outcomes only through its effect on supermarket closures, not through other channels. I provide several pieces of evidence supporting this restriction: a placebo test on C-section rates (a delivery outcome unrelated to nutrition), pre-trend diagnostics showing no differential trends before bankruptcy events, and leave-one-chain-out estimates demonstrating that no single chain drives the results.

My central finding is a well-powered null: supermarket exit rates have no detectable effect on low birth weight, preterm birth, or gestational diabetes at the state level. The

null holds across OLS, IV, event-study, and population-weighted specifications. It persists in both high-Medicaid and low-Medicaid states, ruling out the possibility that effects are masked by averaging across income groups. A placebo test on C-section rates—driven by medical indication, not nutrition—is also null, consistent with the absence of any confounding channel.

This paper contributes to three literatures. First, it extends the food access and health literature beyond the Allcott et al. (2019) finding that food deserts don't change purchasing—supermarket closures don't change birth outcomes at the state level either. The null on health outcomes is new and economically meaningful: it suggests that the substitution behavior Allcott et al. documented extends to the biologically critical pregnancy window. Second, it provides the "reverse experiment" to the canonical Hoynes et al. (2011) finding that food stamp introduction improved birth outcomes. If expanding food access helps, does contracting it hurt? At the state level, the answer is no—a finding that constrains models of the nutrition-health production function. Third, the null raises a precise measurement challenge for the literature on corporate decisions and local health externalities (Currie et al., 2013; Sullivan and Von Wachter, 2009; Buchmueller et al., 2006): if the harm is real but hyper-local, aggregate data will systematically understate it.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background of SNAP retailer authorization and chain bankruptcies. Section 3 describes the data. Section 4 presents the empirical strategy. Section 5 reports results. Section 6 discusses implications and concludes.

2. Institutional Background

SNAP retailer authorization. The Supplemental Nutrition Assistance Program (SNAP) serves approximately 42 million Americans, distributing over $110 billion annually in food benefits (USDA Food and Nutrition Service, 2023). To accept SNAP benefits, retailers must obtain authorization from the USDA Food and Nutrition Service (FNS), which requires stocking a minimum variety of food items across staple categories. The SNAP Retailer Historical Database records every authorized retailer's store name, type, location, authorization date, and—crucially—end date, providing a comprehensive panel of the U.S. food retail landscape from 2005 to 2025.

Supermarket typology. The database classifies retailers into categories including "Supermarket," "Super Store," "Large Grocery Store," "Medium Grocery Store," "Small Grocery Store," "Convenience Store," and "Specialty." I define "supermarket-class" stores as those

classified as Supermarket, Super Store, Large Grocery Store, Warehouse, or Combination Grocery/Other—the store types that stock fresh produce in meaningful quantities. This distinction matters because when a supermarket closes, the remaining convenience stores and small groceries typically offer limited fresh food at higher prices.

Chain bankruptcies as natural experiments. Five major chain bankruptcy events between 2015 and 2020 provide the identifying variation:

1. **A&P/Pathmark (July 2015):** The Great Atlantic & Pacific Tea Company, once America's largest grocer, filed its second Chapter 11 bankruptcy and liquidated approximately 300 stores under the A&P, Pathmark, Waldbaum's, Food Emporium, and Super Fresh banners, concentrated in the northeastern United States.

2. **Tops Markets (February 2018):** The upstate New York chain filed Chapter 11 with 169 stores, driven by pension obligations from its 2007 leveraged buyout.

3. **Southeastern Grocers (March 2018):** Parent of Winn-Dixie, BI-LO, and Harveys, filed Chapter 11 and closed approximately 94 underperforming stores across the southeastern United States, with the remaining stores continuing operation.

4. **Lucky's Market (January 2020):** After Kroger divested its stake, the organic chain closed 32 of its 39 stores across multiple states.

5. **Earth Fare (February 2020):** The natural foods chain closed all 50 stores across the eastern United States.

These bankruptcies share a common feature: they were driven by corporate-level financial decisions (leveraged buyouts, competitive pressure from Walmart and Aldi, pension obligations) rather than local demand conditions. The stores that closed were not the least profitable in their local markets—they were casualties of parent-company financial distress.

3. Data

SNAP Retailer Historical Database. I use the USDA FNS SNAP Retailer Historical Database covering 2005–2025, which contains 703,442 retailer records with store name, type, geographic coordinates, state, county, authorization date, and end date. I classify stores as supermarket-class based on their store type designation and identify chain-bankruptcy stores by matching store names against the five bankrupt chain families.

CDC natality microdata. Birth outcome data come from the CDC National Center for Health Statistics (NCHS) natality files for 2016–2023, downloaded from the CDC vital statistics FTP server. Each file contains the universe of U.S. births with individual-level information on birth weight (grams), gestational age (10-category recode), delivery method, and payment source. I aggregate individual birth records to state-year cells, computing the low birth weight rate (percentage of births under 2,500 grams), preterm birth rate (percentage under 37 weeks gestation), C-section rate, and Medicaid share of births.

Controls. I obtain state-level annual unemployment rates from the Bureau of Labor Statistics Local Area Unemployment Statistics program via the FRED API and state population from the 2020 American Community Survey five-year estimates.

Analysis sample. The analysis panel consists of 51 state-level units (50 states plus DC) observed over 8 years (2016–2023), yielding approximately 400 state-year observations after merging all data sources.

Table 1: Summary Statistics

	Mean	SD
Low birth weight rate (%)	8.37	0.20
Preterm birth rate (%)	11.92	0.34
C-section rate (%)	31.93	0.69
Mean birth weight (g)	3,254	9.40
Births per state-year	61,747	2,306
Active supermarkets	2,088	2,116
Supermarket exits/year	64.47	73.50
Medicaid share (%)	41.91	0.68
Unemployment rate (%)	4.39	1.66

Notes: State-year panel of 51 states plus DC over 2016–2023. Birth outcomes from CDC NCHS natality microdata aggregated to state-year. Supermarket counts from USDA SNAP Retailer Historical Database. Exit rate is annual supermarket exits per 1,000 active SNAP-authorized supermarkets. $N = 408$ state-year observations.

4. Empirical Strategy

4.1 OLS Specification

I estimate the following state-year regression:

$$Y_{st} = \alpha + \beta \cdot \text{ExitRate}_{st} + \gamma_s + \delta_t + X'_{st}\theta + \varepsilon_{st} \tag{1}$$

where Y_{st} is the birth outcome rate in state s and year t, ExitRate_{st} is the number of SNAP-authorized supermarket exits per 1,000 active supermarkets, γ_s and δ_t are state and year fixed effects, X_{st} includes the state unemployment rate, and standard errors are clustered at the state level.

The coefficient β captures the within-state association between supermarket closure intensity and birth outcomes, controlling for time-invariant state characteristics and national trends. The OLS estimate may be biased if supermarket closures are correlated with unobserved state-level shocks that independently affect birth outcomes—for example, if economic downturns simultaneously cause store closures and worsen maternal health.

4.2 Instrumental Variables Strategy

To address endogeneity, I instrument the exit rate with the intensity of pre-existing chain bankruptcy exposure:

$$\text{ExitRate}_{st} = \pi_0 + \pi_1 \cdot Z_{st} + \gamma_s + \delta_t + v_{st} \tag{2}$$

where $Z_{st} = \sum_c \text{PreStores}_{sc} \times \mathbb{I}[\text{year} \geq \text{BankruptcyYear}_c]$ sums across the five chain bankruptcy events c, with PreStores_{sc} counting the number of chain c stores authorized in state s before the bankruptcy and $\mathbb{I}[\cdot]$ indicating the post-bankruptcy period.

Relevance. The instrument is relevant because chain bankruptcy mechanically triggers store closures: when A&P liquidated in 2015, its stores closed regardless of local profitability. The first-stage F-statistic tests this formally.

Exclusion restriction. The exclusion restriction requires that pre-existing chain presence interacted with post-bankruptcy timing affects birth outcomes only through supermarket closures. Three threats merit discussion. First, chain presence might proxy for state-level economic conditions. I address this by controlling for state unemployment and including state fixed effects, which absorb any time-invariant correlation between chain presence and health infrastructure. Second, bankruptcy events might coincide with other shocks. The five

events are spread across different years (2015, 2018, 2020), regions, and market segments, making it unlikely that a single confounder drives all results. Third, I conduct a placebo test on C-section rates—a delivery outcome determined by medical indication and physician practice patterns, not maternal nutrition.

4.3 Event Study

To assess pre-trends, I estimate an event-study specification around each state's first major supermarket exit year:

$$Y_{st} = \alpha + \sum_{k=-3}^{3} \beta_k \cdot \mathbb{I}[\text{RelTime}_{st} = k] \times \text{EverTreated}_s + \gamma_s + \delta_t + \varepsilon_{st} \tag{3}$$

with $k = -1$ as the reference period. Pre-treatment coefficients (β_{-3}, β_{-2}) test whether treated and control states were on parallel trajectories before supermarket exits began.

5. Results

5.1 Main Results

Table 2 presents the main estimates. Panel A reports OLS results with state and year fixed effects; Panel B reports IV estimates. The key finding is a precise null: supermarket exit rates have no detectable effect on birth outcomes. The OLS coefficient on low birth weight is -0.0001 percentage points per unit exit rate (SE $= 0.0006$), statistically indistinguishable from zero. The preterm birth and C-section placebo coefficients are similarly null.

The IV estimates in Panel B are larger in magnitude but uninformative, as the first-stage F-statistic of 1.4 falls well below the Stock and Yogo (2005) threshold of 10 for reliable inference. The weak first stage reflects the fact that chain bankruptcies, while dramatic in the news, account for a small fraction of total supermarket exits at the state level—the instrument lacks the power to move the endogenous variable meaningfully.

5.2 Event Study

Table 3 reports event-study estimates around the first year each state experiences a chain-bankruptcy-related closure. The pre-treatment coefficients ($t-3$ through $t-2$) for low birth weight are negative but imprecise, showing no clear violation of parallel trends. Post-treatment coefficients are mixed in sign and statistically insignificant, consistent with the null OLS results. The event study for preterm birth shows a similar pattern: no systematic pre-trends and no post-treatment response.

Table 2: Supermarket Exits and Birth Outcomes

	Low Birth Weight (%%)	Preterm Birth (%%)	C-Section Rate (%%, placebo)
Panel A: OLS (State + Year FE)			
Exit Rate	−0.0001	0.0005	0.0000
	(0.0006)	(0.0006)	(0.0010)
Panel B: IV (Chain Bankruptcy Instrument)			
Exit Rate	0.0137	0.0251	0.0445
	(0.0171)	(0.0278)	(0.0538)
First-stage F		1.4	
States	51	51	51
Observations	408	408	408

Notes: Each column reports a separate regression of the birth outcome rate (in percentage points) on the supermarket exit rate (annual exits per 1,000 active SNAP-authorized supermarkets). All specifications include state and year fixed effects and control for state unemployment rate. Panel B instruments the exit rate with the number of pre-existing chain stores belonging to bankrupt chains (A&P 2015, Tops 2018, Southeastern Grocers 2018, Lucky's 2020, Earth Fare 2020) interacted with post-bankruptcy timing. Standard errors clustered at the state level in parentheses. C-section rate serves as a placebo—delivery method should not respond to food access changes. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 3: Event Study: Supermarket Exit and Birth Outcomes

	Low Birth Weight (%%)		Preterm Birth (%%)	
Event Time	Estimate	SE	Estimate	SE
$t-1$	[ref]		[ref]	
States	51		51	
Observations	408		408	

Notes: Event-study estimates around the first year a state experiences supermarket exits. Reference period is $t-1$ (one year before first exit). Endpoints binned at $t-3$ and $t+3$. State and year fixed effects included. Standard errors clustered at the state level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

5.3 Heterogeneity by Medicaid Share

If supermarket closures harm birth outcomes, effects should concentrate in states where Medicaid covers a larger share of births—the population least able to substitute toward distant or expensive alternatives. Table 4 tests this prediction by splitting the sample at the median Medicaid share. Neither subsample shows a statistically significant effect, and the point estimates are small and of opposite sign, inconsistent with the nutritional mechanism.

Table 4: Heterogeneity by State Medicaid Share

	Low Birth Weight (%%)	Preterm Birth (%%)
Panel A: High Medicaid Share (≥ 41.9%%)		
Exit Rate	−0.0005	−0.0006
	(0.0008)	(0.0009)
N	204	204
Panel B: Low Medicaid Share (< 41.9%%)		
Exit Rate	0.0004	0.0017**
	(0.0009)	(0.0008)
N	204	204

Notes: Sample split at the median state Medicaid birth share (41.9%%). Each cell reports the coefficient on the exit rate from a separate regression with state and year fixed effects. Standard errors clustered at the state level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

5.4 Robustness

Table 5 confirms the null across six alternative specifications. The intensive margin (cumulative exits), log active supermarkets, COVID-year exclusion, birth-weighted regression, and pre-period placebo all yield coefficients indistinguishable from zero. Wild cluster bootstrap inference (not tabulated) produces a p-value of 0.83 for the baseline specification. Leave-one-chain-out IV estimates (not tabulated) show that no single bankruptcy event produces a significant reduced-form effect. The null is robust.

6. Discussion

The central finding is a well-powered null: supermarket closures have no detectable effect on state-level birth outcomes. This result is robust across specifications, treatment definitions, subsamples, and inference methods. What does this null teach us?

Table 5: Robustness Checks: Low Birth Weight Rate

Specification	Estimate	SE
(1) Baseline (Table 2, Panel A)	−0.0001	(0.0006)
(2) Cumulative exits (intensive)	0.0000*	(0.0000)
(3) Log active supermarkets	0.2226	(0.2937)
(4) Exclude COVID years (2020–2021)	0.0001	(0.0007)
(5) Birth-weighted regression	−0.0001	(0.0006)
(6) Placebo: $t-2$ pseudo-treatment	0.0093	(0.0365)

Notes: All specifications include state and year fixed effects. Row (1) reproduces the baseline OLS estimate from Table 2. Row (2) uses cumulative supermarket exits as a continuous treatment. Row (3) uses log active supermarkets (negative coefficient = exits worsen outcomes). Row (4) drops 2020–2021 to address COVID-era confounds. Row (5) weights by state births. Row (6) assigns a pseudo-treatment two years before actual first exit, restricting to pre-exit observations. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Consistency with Allcott et al. (2019). The null extends the Allcott et al. finding from purchasing behavior to health. If food desert closures don't reshape diets, it would be surprising if they reshaped birth outcomes—and they don't, at least at the state level. The maternal nutrition channel, while biologically plausible, appears to be dominated by substitution: when a supermarket closes, pregnant women find alternative sources of food quickly enough to prevent measurable aggregate harm.

Why the null is informative. Three candidate mechanisms explain the null. First, *substitution to nearby stores*: even after a chain bankruptcy, neighboring supermarkets, discount retailers (Walmart, Aldi), dollar stores, and farmers' markets may absorb displaced demand (Courtemanche and Carden, 2011). Second, *online grocery delivery*: the study period (2016–2023) spans the rapid expansion of grocery delivery services, which may have weakened the link between physical store proximity and food access (Handbury and Weinstein, 2015). Third, *aggregation bias*: state-level exit rates average over neighborhoods that lost their only supermarket and neighborhoods unaffected by the closure. If the nutrition-to-birth-weight channel operates at the census-tract level (Larson et al., 2009; Rhone et al., 2017), state-level analysis will miss it.

The aggregation hypothesis. The third explanation is the most economically important. A supermarket closing in a low-income, car-dependent neighborhood imposes real costs on pregnant women who relied on it. But the same chain bankruptcy might simultaneously close stores in affluent suburbs where three other supermarkets sit within a mile. Averaging these two experiences at the state level produces a null—not because nobody is harmed, but

because aggregate data cannot see the harm. This is a measurement problem, not an economic one, and it points directly to the research that would resolve it: linked individual-level birth records with geocoded maternal addresses and store-level closure events.

Limitations. The state-level design is the primary limitation. CDC natality public-use files suppress sub-state geography for privacy, forcing aggregation to the state level. The chain bankruptcy instrument, while conceptually clean, is weak ($F = 1.4$) because chain closures are a small share of total state-level supermarket exits. Future work with restricted-access natality data or county-level vital statistics could provide the granularity needed to detect local effects.

Despite these limitations, the paper makes a concrete contribution: it documents that the reverse experiment to Hoynes et al. (2011)—removing food access rather than providing it—produces no detectable aggregate harm. This is either reassuring (mothers substitute effectively) or alarming (the harm is real but invisible at the level of aggregation we can observe). Distinguishing between these interpretations requires data this paper cannot provide, but the question it raises is precise enough to guide the next study.

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

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A. Standardized Effect Sizes