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ABSTRACT

Anchor stores occupy a dual role in local retail markets: they compete with nearby businesses for consumer spending while also attracting shoppers whose visits generate demand spillovers for surrounding establishments. Therefore, an anchor's closure produces two countervailing effects—competitive relief and agglomeration loss—making its net effect on nearby stores theoretically ambiguous. We examine this question using 39 Walmart store closures across 20 U.S. states between 2019 and 2026. Following a Walmart closure, nearby stores experience an estimated 15.20% decline in monthly shopper traffic and a 14.04% decline in monthly sales revenue relative to stores near surviving Walmart anchors. The effects become increasingly negative after closure and remain stable across a battery of robustness checks. These findings indicate that, on average, the loss of anchor-generated traffic outweighs the benefits nearby stores receive from reduced competition and are consistent with agglomeration loss as the dominant mechanism. We provide indirect evidence on this interpretation. The postclosure sales effect is 17.90% more favorable for competing than noncompeting stores and 16.50% more favorable for branded than unbranded stores; however, both comparisons remain consistent with net losses rather than a reversal of the average effect. Lastly, greater preclosure intrabrand clustering and online revenue share are associated with more favorable postclosure sales effects. These findings suggest how managerial actions can mitigate the effects of closure. The research contributes by demonstrating that an anchor store's exit can contract economic activity throughout the surrounding retail area rather than simply redistribute the exiting store's demand to nearby businesses.

Keywords: anchor stores; store closure; retail agglomeration; digital channel; competitive relief; shopper traffic; Walmart; difference-in-differences

INTRODUCTION

Retail stores do not operate as isolated points in geographic markets. Shoppers often choose a destination before deciding which establishments to visit within it. Therefore, one store's presence—and, by extension, its absence—can alter demand for nearby stores. This interdependence is especially important for an anchor store, a prominent retailer whose brand presence and value proposition increase shopper traffic near its location, thereby raising neighboring establishments' sales (Konishi & Sandfort, 2003; Maier, 2026). Consistent with this view, shopping-center developers offer substantial rent concessions to anchors because their customer draw creates a positive demand externality for nonanchor tenants (Pashigian & Gould, 1998). Thus, what makes a retailer an anchor is not merely its floor area or chain affiliation but its ability to increase the attractiveness of the surrounding commercial area.

An anchor store's closure exposes nearby retail stores to two countervailing forces. The first is competitive relief. While operating, a large retailer competes with surrounding stores for consumer spending, particularly when assortments overlap, and its scale, centralized distribution, and supplier bargaining power enable it to pressure incumbent retailers on prices, margins, and assortment (Ailawadi et al., 2010; Jia, 2008). Once the anchor closes, the pressure eases, and nearby stores may capture purchases that shoppers previously made at the anchor. Therefore, this competitive-relief channel predicts increased shopper traffic and sales revenue for surviving stores, with the largest gains among the anchor's competitors.

The second force is agglomeration loss. An anchor does more than compete for a fixed pool of local demand; it can expand that pool by attracting shoppers to the area. Visiting a commercial location entails travel time, transportation costs, and search effort, so shoppers' willingness to make the trip depends on the destination's overall expected value (Brueckner, 1993). A recognizable, broad-format anchor also provides a predictable purchasing opportunity that can make travel worthwhile even when shoppers are uncertain about what they will find at smaller specialized stores (Hwang & Park, 2016; Konishi & Sandfort, 2003). Nearby establishments benefit from planned co-visits, incidental discovery, pass-by traffic, and shared parking and road access. Closure reverses this externality: once the anchor is no longer operating, the expected benefit of visiting the area falls, and nearby stores lose deliberate co-visits and incidental traffic. Therefore, the closure's net effect is theoretically ambiguous. Competitive relief raises a surviving store's share of local demand, whereas agglomeration loss shrinks the total demand arriving in the surrounding retail area. A store can capture a larger share of a smaller market and still experience lower traffic and sales.

Prior evidence reflects this theoretical ambiguity. Research centered on product-market substitution shows that surviving retailers can recapture demand after a competitor exits (Akturk & Ketzenberg, 2022). Other studies document the opposite pattern when the departing business generated meaningful local spillovers: retail liquidations increased neighboring stores' closure risk by weakening local agglomeration economies (Benmelech et al., 2019), department store closures in Germany reduced visits to the surrounding retail area with only partial recovery over time (Maier, 2026), and chain-wide big-box bankruptcies reduced nearby business activity and employment (Shoag & Veuger, 2018). However, this literature leaves four gaps. It does not (1) establish which force dominates for individual stores surrounding a common anchor across a large set of U.S. markets, (2) jointly observe shopper traffic and sales revenue, (3) identify conditions that amplify or weaken competitive relief relative to agglomeration loss, or (4) offer managerially relevant evidence about factors associated with resilience following an anchor's exit.

We address these gaps by examining how Walmart store closures affect nearby establishments' shopper traffic and sales revenue. Walmart's scale, brand recognition, broad assortment, and standardized formats make its stores plausible traffic-generating anchors, while the same attributes make Walmart a powerful competitor for many nearby retailers (Gielens et al., 2018). Therefore, its closure may simultaneously produce substantial competitive relief and agglomeration loss. Studying the net effect reveals whether neighboring retail stores derive more value from Walmart's customer draw than they lose to its product-market competition.

Our empirical setting comprises 39 Walmart stores that closed between 2019 and 2026 across 20 U.S. states. We combine the closure dates with monthly store-level measures of shopper traffic and consumer spending from January 2019 through June 2026 using SafeGraph Spend Patterns data (SafeGraph, 2022). The sample contains observations on a wide range of non-Walmart establishments surrounding the closed locations. This multi-market setting goes beyond a single mall, a chain bankruptcy, or a local closure and allows us to study the disappearance of the same branded anchor across varied retail and geographic conditions.
For each closure, we identify treated stores based on both geographic and category-specific proximity. We organize each Walmart closure and its matched stores into a separate sub-experiment, then stack the 39 sub-experiments for estimation, following the clean-comparison logic used in stacked difference-in-differences designs (Cengiz et al., 2019).

We report three sets of findings on the effect of an anchor store's closure. First, nearby stores experience economically meaningful declines in shopper traffic and sales revenue after a Walmart closes. These effects are consistent with agglomeration loss outweighing competitive relief. We find no detectable differential pretrends in either outcome before closure. Dynamic estimates become increasingly negative after closure, and the negative pattern appears across many independently estimated closure events rather than being attributable to one or two unusual locations. Second, we provide indirect evidence on the two mechanisms. Competing and branded stores have more favorable relative postclosure sales effects than their counterparts, but the subgroup estimates remain negative. Thus, the results are consistent with competitive relief or store-level resilience partially offsetting, rather than reversing, the average loss associated with reduced anchor-generated traffic.

Third, we examine two store-specific characteristics that may be managerially relevant: (1) intrabrand clustering, the extent to which a store is clustered with same-brand stores, and (2) online revenue share, the proportion of sales generated through digital channels rather than in-store purchases. Greater intrabrand clustering and a higher online revenue share are each associated with more favorable postclosure sales effects. Because these analyses are observational, we interpret the variables as resilience factors rather than as experimentally identified managerial levers.

Our findings offer three contributions. First, they connect research on retail competition with research on demand-side agglomeration. Studies of Walmart entry emphasize the pressure that large chains impose on incumbents (Ailawadi et al., 2010), whereas anchor-store theory emphasizes the traffic benefits that prominent retailers confer on neighboring businesses (Maier, 2026). A closure places these forces in direct opposition and permits an empirical assessment of their net effect. Second, we extend the store-exit literature by linking closures to monthly outcomes at individual surviving establishments and by observing both shopper traffic and sales revenue (e.g., Ye & Shankar, 2025). These outcomes distinguish the loss of visits from a purely financial response and illuminate how a local demand shock reaches nearby retail stores. Third, we contribute to anchor-store research (Brueckner, 1993; Konishi & Sandfort, 2003; Pashigian & Gould, 1998) by showing that retail anchoring can operate through spatially shared shopper behavior without a contractual relationship between anchor and neighbor. Therefore, we treat anchor status as a functional property rather than a purely physical or contractual classification and identify store-level characteristics associated with more favorable postclosure outcomes.

The findings also have implications for retailers, property owners, and local policymakers. For Walmart, closing an underperforming location is an internal portfolio decision. For neighboring businesses, the same decision can be an external demand shock that reduces the value of the surrounding retail ecosystem. Therefore, property owners and local governments may have reason to accelerate retenanting, redevelopment, or the recruitment of a replacement traffic generator. More broadly, assessments of retail closures should account for economic activity beyond the closing store's walls. The remainder of the paper develops the theoretical framework, describes the data and method, presents the results, and discusses implications and limitations.

PRIOR LITERATURE

Web Appendices A and B summarize research on store closures and anchor-store effects, respectively (Tables WA1 and WA2). Together, this literature portrays retail closure as more than the disappearance of a single outlet: it reallocates demand across stores, locations, and channels; changes the competitive environment facing survivors; and can weaken the traffic and network benefits of colocation. The evidence does not support a single directional prediction. Some closures benefit survivors by removing a rival or reducing cannibalization, whereas others damage the surrounding ecosystem by eliminating a destination and the trips it generated. The outcome depends on the closing store's market role, its overlap with neighbors, the availability of substitutes, the structure of shopping trip, and firms' capacity to adapt. This ambiguity is especially important for anchor stores. Walmart and other large-format retailers can exert substantial competitive pressure through economies of scale, broad assortments, and one-stop shopping, yet these same attributes can make them traffic generators whose presence increases the expected value of traveling to a retail area. Therefore, we organize the literature around two themes: (1) competitive relief and survivor adaptation and (2) agglomeration loss and local spillovers.

Competitive Relief and Survivor Adaptation

The first theme concerns how much demand remains within the closing chain, and how surviving competitors respond to it. Closure does not mechanically transfer all of a store’s sales to sister locations or rivals; shoppers choose among replacement stores and formats by balancing travel, transaction, and substitution costs. Chain-level losses following an outlet closure range widely depending on store format, distance to competitors, customer characteristics, and shopping-trip type (Haans & Gijsbrechts, 2010). This breadth shows that demand recapture is highly context-dependent: some customers readily migrate to another outlet in the chain, whereas others switch to rivals, change formats, reduce category spending, or abandon the trip. Survivors can also respond strategically rather than merely waiting for demand to arrive, expanding product variety more where they had previously colocated with an exiting rival and opening more new stores where they had not (Ren et al., 2019). Direct evidence from sales confirms this pattern: offline sales at a major department-store chain rose after rival closures, with larger gains near the closed locations (Akturk & Ketzenberg, 2022).

Taken together, this stream rejects the assumption that a closed store's demand is a fixed pool that nearby outlets automatically inherit. This insight also applies beyond the closing chain: whether surrounding retail stores benefit from an anchor store’s exit depends on whether shoppers remain in the area and reallocate purchases locally or redirect the entire shopping trip elsewhere.

Agglomeration Loss and the Local Retail Ecosystem

The countervailing theme is agglomeration loss. Stores sharing a location or retail area can benefit from common shopper traffic even when they compete in product markets, so a prominent retailer's disappearance can cost the area visits, complementary spending, and the density that supports other firms. Following national retail-chain liquidations, a nonbankrupt neighbor at the same address as a liquidating store becomes significantly more likely to close (Benmelech et al., 2019). Big-box bankruptcies show a similar pattern: nearby businesses and employment both decline within a short radius after a large retailer disappears, because shoppers who stop visiting the closed store also stop visiting its neighbors (Shoag & Veuger, 2018). The same dynamic holds in outdoor malls, where nearby stores' survival prospects deteriorate after a big-box exit (Sharma & Chung, 2022).

Direct traffic evidence from German department-store closures shows that visits to the surrounding retail area fall sharply within months and remain well below preclosure levels a year later, with only partial recovery as shoppers adapt and new tenants enter (Maier, 2026). This pattern suggests that closure triggers both an immediate loss of destination value and a slower market adjustment. Across these studies, agglomeration effects are highly spatial, with the greatest damage falling on businesses that share the same address, mall, or immediate surrounding area. Together, the competitive-relief and agglomeration-loss literatures leave the net consequence of an anchor-store closure for surrounding stores ambiguous—a question we investigate directly in the context of Walmart closures.

Walmart as a Powerful Competitor and Traffic-Generating Anchor

A Walmart store is not merely another outlet: its scale, broad assortment, distribution system, bargaining power, and everyday-low-price positioning can reshape local market structure. Research on Walmart entry shows that it draws substantial sales away from nearby incumbents, largely through fewer visits rather than smaller baskets (Singh et al., 2006). Effects extend to firm and market levels. Large-scale expansion has been linked to billions of dollars in lost shareholder value among incumbent retailers (Gielens et al., 2008), to many independent discount stores becoming unprofitable and a substantial share of the net decline in small discount stores (Jia, 2008), and to a stronger negative revenue effect on rival discount chains than the reverse (Zhu et al., 2009). Losses after first-time entry vary across retail formats, stores, and categories and can be partly mitigated by price cuts, promotions, and assortment expansion when these responses align with the incumbent's format and customer proposition (Ailawadi et al., 2010). Thus, retailers' capabilities and strategic responses shape who gains or loses from a changing local market structure.

Walmart's influence also extends beyond nearby-store sales. Sustainability mandates imposed on suppliers have been linked to reduced abnormal returns, with brand equity, advertising, and environmental reputation providing some countervailing power (Gielens et al., 2018). Supercenter entry has also been linked to increased consumer waste, consistent with large-format, low-price, bulk purchasing stimulating consumption and disposal (Dhanorkar et al., 2025). These outcomes are not direct evidence about neighboring-store traffic, but they show that an anchor retailer can influence suppliers, households, and communities through multiple channels. These studies establish how Walmart entry reshapes local retail markets, but Walmart exit has received comparatively little attention. There is no reason to expect an exit to simply reverse an entry: the competitive and traffic-generating forces that Walmart sets in motion when it arrives need not unwind symmetrically when it leaves.

Integrative Synthesis and Research Gap

Taken together, the literature yields a coherent but conditional picture of Walmart specifically. Entry creates competitive pressure when Walmart's scale, assortment, and pricing draw sales, customers, and shareholder value away from nearby incumbents; it also generates traffic when Walmart's one-stop-shopping format compresses trips and raises the expected value of visiting the surrounding area. These mechanisms can coexist within the same retailer. Walmart can simultaneously compete for a fixed pool of local demand and expand that pool by drawing shoppers to the area in the first place, so its exit need not simply reverse what its entry produced.

The entry studies point to several conditions that likely shape an exit's effects as well. Product and format overlap determine how directly a nearby store substitutes for Walmart; store size, brand strength, and digital capability shape a store's capacity to adapt to a shifting local market; and geographic proximity intensifies both competitive pressure and traffic spillovers. Walmart's scale and market power are especially important because they affect both sides of the trade-off: the same attributes that make Walmart a formidable competitor are plausibly what make it a destination worth traveling to.

Across the studies summarized in Tables WA1 and WA2, the literature offers strong evidence that entry reshapes local retail markets and that closures elsewhere have produced both competitive relief and agglomeration loss. However, no study has directly examined the exit of a Walmart anchor or jointly measured the two outcomes that most directly connect an anchor's traffic-generating role to nearby-store performance: shopper visits to individual surviving establishments and the sales revenue those visits produce. This gap motivates research that jointly measures nearby-store traffic and revenue after an anchor-store closure, identifies store-level conditions associated with stronger competitive relief or agglomeration loss, and offers evidence relevant to retailers managing an anchor's exit.

THEORY

Anchor Store

We examine how the closure of an anchor retail store affects nearby stores' monthly shopper traffic and sales revenue. The International Council of Shopping Centers (n.d.) describes an anchor store as a major department store, grocery store, or other large chain store with substantial economic strength and square footage. Konishi and Sandfort (2003) define anchors functionally by their capacity to increase shopper traffic near their location and thereby raise nearby stores' sales and profits. Thus, anchor status reflects a retailer's traffic-generating role, not only its size or tenancy arrangement.

Anchor-Store Closure: Agglomeration Loss versus Competitive Relief

An anchor store's scale gives it centralized distribution, strong supplier bargaining power, and a one-stop shopping format that smaller retailers cannot match (Singh et al., 2006). An anchor store's closure exposes nearby retailers to two countervailing forces. One force raises nearby stores' shopper traffic and sales revenue. The other lowers them. Theory does not resolve which force wins.

Competitive relief operates through two pathways. First, while operating, the anchor store competes with nearby stores for consumer spending. Therefore, its closure removes a major competitor and may increase the residual demand facing neighboring stores. Once the store closes, this pressure lifts, and nearby stores may absorb some of the freed-up shopper traffic and sales revenue. Prior literature supports this logic. Colocated Best Buy stores increased product variety after Circuit City's 2009 exit, with larger gains in markets in which the two retail chains had overlapped (Ren et al., 2019). Second, the anchor store's footprint can force rivals into smaller margins and thinner customer bases (Jia, 2008). Therefore, its disappearance may relax competitive pressure on prices and margins.

Prior research has documented the substantial competitive pressure that Walmart and other large discount chains impose on incumbent retailers, but whether Walmart's exit relieves that pressure, rather than simply removing the traffic that sustained nearby sellers in the first place, remains an open question (Basker, 2007; Jia, 2008).

Agglomeration loss. An anchor store does more than compete. It anchors the commercial area, producing a positive externality for nearby stores even when they have no contractual or organizational relationship with the anchor. Its scale and one-stop format draw shopping trips that can spill over to smaller nearby stores (Ailawadi et al., 2010; Hwang & Park, 2016). Shoppers choose not only which store to patronize but also which commercial location to visit. Because visiting a retail area entails travel time, transportation costs, and search effort, the destination's overall attractiveness affects whether a shopper makes the trip (Brueckner, 1993). A large, recognizable retailer raises the expected return from traveling to the location by offering a broad and relatively predictable assortment. It can also allow shoppers to combine multiple errands within a single trip. Consequently, an anchor store's presence may increase the number of shoppers visiting the surrounding retail area and expand the geographic catchment from which they originate. Once the store closes, this shared traffic disappears, and nearby stores should experience fewer visits and lower sales revenue. Under agglomeration loss, an anchor store's closure should lower, not raise, nearby stores' shopper traffic and sales revenue.

Coexistence of the Two Forces

Konishi and Sandfort (2003) provided an especially relevant theoretical explanation for why agglomeration benefits can coexist with product-market competition. In their model, an anchor gives shoppers a relatively predictable purchasing opportunity, while surrounding specialized retailers offer uncertain but potentially higher-value alternatives. The anchor makes traveling to the location worthwhile even when a shopper is uncertain about finding a desirable product at a smaller retailer. Consequently, the anchor's presence increases shopper traffic and search at the location. Under some conditions, the resulting increase in traffic is large enough to outweigh the reduction in retailers' markups caused by greater competition. Thus, even stores selling products that substitute for an anchor's products can receive a net benefit from collocating with it.

The anchor's closure reverses the demand externality. Once the anchor is no longer operating, shoppers' expected benefit of visiting the commercial area falls. Shoppers may continue to visit the neighborhood specifically to patronize another store, but others may abandon the trip or redirect it to a surviving anchor, a competing big-box retailer, another retail cluster, or an online seller. Importantly, shoppers may relocate the entire bundle of errands associated with a trip to the anchor store rather than merely substitute another seller for purchases previously made at the closed anchor store. Therefore, the closure can reduce incidental visits, planned co-visits, product discovery, and pass-by traffic for nearby establishments.

DATA AND METHOD

Empirical Setting and Data Sources

Walmart is a suitable empirical setting for two reasons. First, its stores possess the economic strength and substantial footprint commonly associated with retail anchors (International Council of Shopping Centers, n.d.). Second, Walmart operates standardized store formats across thousands of locations nationwide (Jia, 2008; Singh et al., 2006; Zhu et al., 2009). Its store closures, therefore, provide a comparable and economically meaningful setting for examining our proposed mechanisms. Prior research has used Walmart store openings and format conversions to examine the effects on nearby retailers, but, to our knowledge, no research has studied the reverse case of a Walmart store closure (Ailawadi et al., 2010; Hwang & Park, 2016).

We study the effect of Walmart store closures on nearby retail stores' shopper traffic and sales revenue using data from two complementary sources. First, we obtain monthly store-level transaction data from SafeGraph Spend Patterns (SafeGraph, 2022). SafeGraph aggregates consumer traffic and spending patterns across U.S. commercial locations. Its store-month panel begins in January 2019, so we restrict the closure sample to events occurring in 2019 or later, thereby retaining at least some preclosure observations for every event. The panel extends through June 2026. Second, we identify Walmart closures from ClosedPlaces.com, a database of permanently closed U.S. businesses (ClosedPlaces.com, n.d.). The database identifies 39 Walmart stores that closed between 2019 and 2026 across 20 states and provides their closure dates. Table 1 lists the stores, closure dates, and locations.

Walmart's U.S. segment operates three principal store formats: supercenters, discount stores, and Neighborhood Markets. Based on Walmart's reported U.S. unit counts and square footage as of January 31, 2026, supercenters average approximately 178,000 square feet, discount stores approximately 104,000 square feet, and Neighborhood Markets approximately 42,000 square feet (Walmart Inc., 2026). Supercenters combine general merchandise with a full-line supermarket; discount stores emphasize general merchandise with a more limited food assortment; and Neighborhood Markets are grocery-focused. Of the 39 closures in our sample, 37 are Neighborhood Markets, one is a discount store, and one is a supercenter.

Table 1. Walmart Store Closures, 2019–2026

#	Street	City	State	Closure Date	Treated Stores	Control Stores
1	7000 Iron Bridge Rd	North Chesterfield	VA	29-Mar-19	222	613
2	1757 W Andrew Johnson Hwy	Morristown	TN	21-Apr-19	365	364
3	2510 Cantrell Rd	Little Rock	AR	08-Apr-19	794	472
4	3900 W Ina Rd	Tucson	AZ	19-Apr-19	909	614
5	712 N Western Ave	Liberal	KS	01-May-19	154	199
6	3706 W 9800 S	South Jordan	UT	09-May-19	387	638
7	1787 US-259	Diana	TX	20-Jun-19	20	319
8	333 N Main St	West Hartford	CT	25-Jun-19	365	955
9	3429 State St	Schenectady	NY	15-Jul-19	543	501
10	1600 Saratoga Ave #501	San Jose	CA	03-Aug-19	1079	1236
11	1901 Texas Ave	Bryan	TX	07-Dec-19	468	279

#	Street	City	State	Closure Date	Treated Stores	Control Stores
12	13742 N Eldridge Pkwy	Cypress	TX	03-Dec-19	541	782
13	7809 NE Vancouver Plaza Dr	Vancouver	WA	14-Dec-19	866	303
14	6085 W Chandler Blvd	Chandler	AZ	01-Jan-20	848	859
15	8010 E 38th St	Indianapolis	IN	31-Jan-20	1115	972
16	3603 Broad River Rd	Columbia	SC	18-Feb-20	828	490
17	2864 Virginia Beach Blvd	Virginia Beach	VA	01-Feb-20	948	741
18	2503 W 5th St	Lumberton	NC	04-Mar-20	294	378
19	4110 Shallowford Rd	Chattanooga	TN	28-Mar-21	869	345
20	727 W Golf Rd	Des Plaines	IL	17-Jul-21	292	848
21	7170 Federal Blvd	Westminster	CO	18-Aug-21	492	942
22	10912 Katella Ave	Anaheim	CA	21-Mar-22	800	825
23	320 W Rancho Vista Blvd	Palmdale	CA	25-Oct-22	412	787
24	3701 SE Dodson Rd	Bentonville	AR	12-May-23	381	383
25	2844 N Broadway	Chicago	IL	26-Nov-23	1362	992
26	4080 Douglas Blvd	Granite Bay	CA	09-Jun-24	104	795
27	6900 US Hwy 19 N	Pinellas Park	FL	13-Jul-24	395	965
28	10303 Metcalf Ave	Overland Park	KS	02-Jul-24	633	400
29	24919 Westheimer Pkwy	Katy	TX	07-Jul-24	770	546
30	5221 Brook Rd	Richmond	VA	15-Jul-24	976	498
31	2121 Imperial Ave	San Diego	CA	16-Jul-24	1076	706
32	2551 W Cermak Rd	Chicago	IL	17-Jul-24	1121	398
33	4226 NW Oakwood Ave	Huntsville	AL	04-Dec-24	805	472
34	10400 E Colfax Ave	Aurora	CO	13-Feb-25	808	679
35	3101 Roswell Rd	Marietta	GA	14-Feb-25	846	1079
36	680 Connecticut Ave	Norwalk	CT	28-Aug-25	486	1142
37	859 W Florida Ave	Hemet	CA	20-Oct-25	400	1060
38	3112 Santa Rita Rd	Pleasanton	CA	08-Oct-25	482	729
39	7025 W Main St	Milwaukee	WI	18-Apr-26	1027	925

Identification of Treated and Control Stores

We treat a Walmart closure as an intervention affecting nearby stores. Treated stores are non-Walmart establishments located near a closed Walmart. Control stores are non-Walmart establishments located near Walmart stores that were open in the focal closure month and remained open through the end of the sampling period, January 2019 through June 2026.

Retail competition is inherently local, so we construct a separate comparison group for each closed Walmart rather than draw controls from a common state- or nationwide pool. Therefore, each closure defines its own local matched subexperiment. The following subsections describe the construction of the treatment and control groups.

Treatment Group

A closed store's treatment group comprises stores that meet three conditions: (1) they are located within 10 miles of the closed store and in the same state, (2) they are among the five nearest stores within their six-digit North American Industry Classification System (NAICS) code (hereafter, category), and (3) they are not Walmart-branded locations. We next explain the rationale for these conditions.

First, we use a 10-mile limit because prior research indicates that shoppers rarely travel more than 10 miles to shop at Walmart (Ellickson & Grieco, 2013; Zhu & Singh, 2009). This limit provides a plausible outer bound for spatial exposure to a closure.

Second, proximity within 10 miles does not guarantee meaningful exposure. Therefore, we use a nearest-store approach (Woo et al., 2019). A Walmart closure may affect nearby stores differently depending on store density and distance (Kim & Jap, 2022). A fixed-radius approach alone cannot account for this variation. For example, a five-mile radius might capture one ice cream shop in a rural area but 20 in a dense urban area and treat all of them as equally exposed. Therefore, we identify the five closest stores in each NAICS category, subject to the 10-mile cap. Combining a nearest-store rule with a distance cap accounts for category-specific store density while maintaining a reasonable upper bound on shopper travel distance.

Third, we exclude Walmart stores from both the treated and control groups to avoid contamination from same-brand effects.

Control Group

We define the control group for each closed Walmart using a three-step matching procedure. First, for each closure, we identify the 10 Walmart stores that (1) were open in the focal closure month and remained open through June 2026 and (2) were at least 10 miles from the closed store. This distance floor is intended to reduce direct exposure of the comparison anchors to the focal closure (Zhu & Singh, 2009).

Second, around each comparison-anchor Walmart, we apply the same nearest-store logic used to construct the treatment group. Specifically, we identify up to five nearest non-Walmart stores in each NAICS category, all within 10 miles of the anchor. We designate the resulting stores as candidate controls.

Third, we retain a candidate control only if it was active in the month of the closure to which it is matched. Each control store is linked to a specific closure via a shared cluster identifier, rather than being pooled at the state or regional level. This procedure is intended to produce controls that are structurally comparable to the treated stores associated with the focal closure.

Illustrative Treatment- and Control-Group Identification

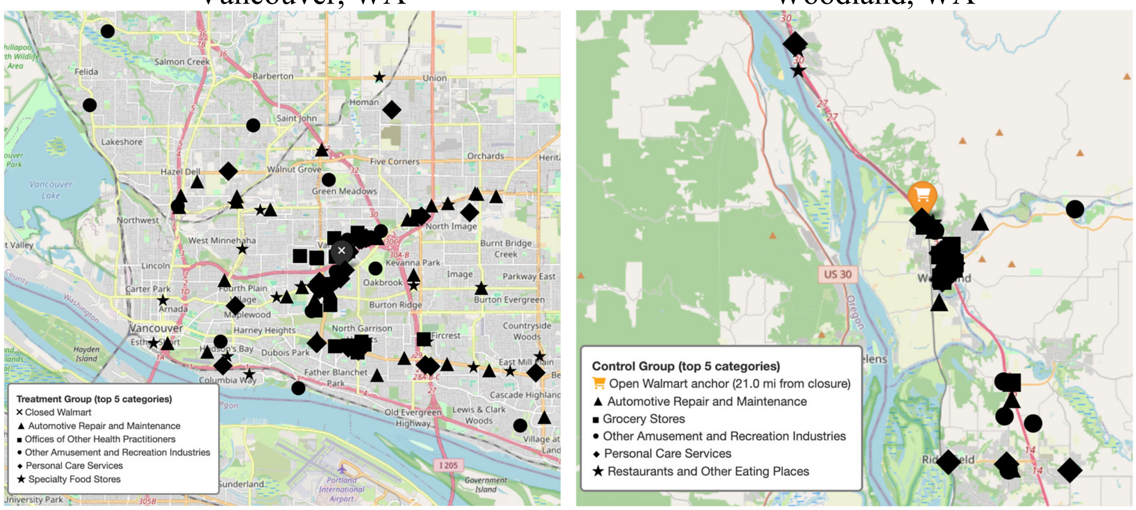
We use the store in row 13 of Table 1 to illustrate the identification procedure. This row corresponds to the Walmart at 7809 NE Vancouver Plaza Drive in Vancouver, Washington, which closed on December 14, 2019.

Figure 1 illustrates our matching design. The treatment-group map shows the closed Walmart (marked with an X) surrounded by its treated stores. These stores fall within 10 miles of the closure and represent the five nearest stores in each NAICS6 category, shown here for the five most common categories: (1) automotive repair and maintenance, (2) offices of other health practitioners, (3) other amusement and recreation industries, (4) personal care services, and (5) specialty food stores. The control-group map shows the same logic applied around an open Walmart store roughly 21 miles from the closure. This anchor is located far enough from the closed store to avoid contamination from the closure itself, yet close enough to share a broadly similar retail environment. Around this anchor, we identify up to five stores in each category within 10 miles, using the same categories as the treatment group. The resulting control stores cluster near the anchor and its surrounding towns, mirroring the spatial structure of the treatment group without drawing on stores near the closure itself. This resulted in 866 treated stores and 303 control stores in closure #13 in Table 1.

Figure 1. Treatment- and Control-Group Example

Treatment Group
Vancouver, WA

Control Group
Woodland, WA



Estimation Sample

The 39 Walmart closure events led us to identify 25,283 treated stores and 26,231 control stores. On average, a closure event is associated with 648 treated stores and 673 control stores. The two rightmost columns of Table 1 report the counts of treated and control stores for each closure.

Identification Strategy

We use a difference-in-differences (DiD) design to estimate the treatment effect. Identification relies on two principal assumptions (Wing et al., 2024).

Assumption 1 (No Anticipation) requires that the closure not affect treated-store outcomes before the treatment date. This assumption could fail if a closure announcement, a liquidation period, or other advance notice prompted nearby stores or shoppers to adjust before the final closing date. We assess this concern using event-study leads and formal pretrend tests;

however, those diagnostics cannot rule out all anticipation that occurs within the monthly treatment interval.

Assumption 2 (Parallel Trends) requires treated and control stores to have followed the same average outcome trend absent the closure. Our matching procedure is designed to support this assumption by restricting controls to stores near comparison Walmart locations that were operating at the focal closure date and remained open through the end of the sample. Thus, treated and control stores are drawn from retail environments organized around a Walmart presence, while the focal Walmart closes only in the treated environment.

Furthermore, each store is treated by at most one closure. Also, we build each closure's control group from stores matched specifically to that event. Therefore, a control store used for one closure is never itself a treated store under a different closure, reflecting the "clean control" principle from the stacked difference-in-differences literature (Cengiz et al., 2019; Wing et al., 2024).

Measures

We measure two outcomes at the store-month level: (1) shopper traffic and (2) sales revenue. Let i index stores and m index calendar months. Shopper $\text{Traffic}_{i,m}$ is the number of visits store i receives in month m, and $\text{Sales}_{i,m}$ is the total consumer spending recorded at store i in month m. We log both measures before estimation so that coefficients can be interpreted as approximate percentage changes. The raw variables are highly right-skewed and heavy-tailed: sales revenue has a skewness of 493.80 and a kurtosis of 299,084.89, while shopper traffic has a skewness of 183.51 and a kurtosis of 77,144.93. After log transformation, skewness falls to .47 for shopper traffic and .28 for sales revenue, and kurtosis falls to 2.63 and 3.24, respectively. We denote the logged outcome as $Y_{i,m}$.

Model-Free Evidence

Before formal estimation, we look at raw trends in shopper traffic and sales for treated and control stores around the time of a Walmart closure. For each closure, we first average $Y_{i,m}$ across its own stores at each event time, the number of months before or after that closure. This gives every closure equal weight, regardless of how many stores it matches or how often a control store is reused across closures. Next, we average these closure-level means across all closures to build the series in Figures 2A and 2B. Figure 2A plots shopper traffic by event time; Figure 2B plots sales.

Figure 2A. Model-Free Evidence: Shopper Traffic by Event Time

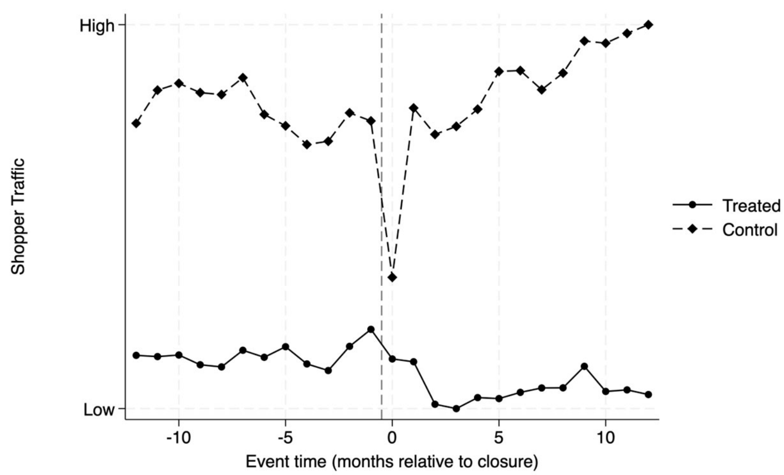
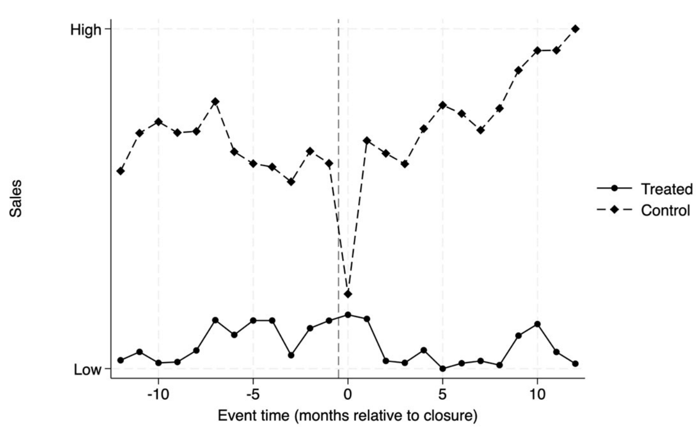


Figure 2B. Model-Free Evidence: Sales Revenue by Event Time



During the 12 months before closure, treated and control stores each show relatively stable patterns, with modest month-to-month fluctuations and no pronounced trend in either series. This pattern is consistent with the common-trends assumption: in the absence of closure, treated and control stores appear to follow similar trajectories. After closure, the two series diverge. Control stores—those near Walmart locations that were open in the closure month and remained open through the end of the sample period—show increases in shopper traffic and sales over the following 12 months. Treated stores—stores near the closed Walmart—show declines. The divergence emerges around the closure rather than developing gradually beforehand, a pattern consistent with an effect beginning at treatment rather than with a clearly visible preexisting trend.

Stacked Difference-in-Differences Estimation

We organize the data as stacked closure-specific subexperiments (Cengiz et al., 2019; Wing et al., 2024). Each closure is paired with its own matched treated and control stores, and the subexperiments are then stacked into a common analysis. This structure preserves closure-specific comparisons and supports the interaction analyses reported later.

For weighted stacked-regression specifications, treatment equals 1 for a treated store from its associated closure month onward and 0 otherwise. Controls are matched to a specific closure and are never treated by that closure or any other sampled closure. Because a control store can appear in more than one subexperiment, we weight each observation by the inverse of its frequency across subexperiments so that repeatedly used controls do not receive disproportionate weight.

The headline average and dynamic effects are estimated closure by closure using the doubly robust group-time estimator described below. The interaction analyses use weighted stacked regressions with store and calendar-month fixed effects, which absorb time-invariant store characteristics and month-specific shocks common to all stores. Standard errors are clustered at the closure level.

Group-Time Estimates and Aggregation

For each closure, we estimate group-time average treatment effects using the doubly robust estimator of Callaway and Sant'Anna (2021), comparing treated stores with their never-treated matched controls. We aggregate the closure-level group-time estimates into a pooled average treatment effect and construct analogous pooled dynamic estimates for each event month from 12 months before through 12 months after closure. Aggregation weights each closure by treated-cohort size within the relevant event-time cell. We obtain standard errors, confidence intervals, and *p*-values using a nonparametric bootstrap that resamples closures with replacement over 2,000 replications. Closure-level resampling reflects that treatment is assigned at the closure level rather than independently to each store and accounts for correlation among stores matched to the same closure.

Specification

For each closure c, let i index stores, m index calendar months, and g_c denote the calendar month of closure c's treatment date. For treated stores, g_c is the month the associated Walmart location closed; control stores are matched to closure c but never treated and serve as the comparison group for that closure's estimation. Let $Y_{i,m}$ denote the outcome of interest, either the log of shopper traffic or the log of sales, for store i in month m.

Within each closure c, we estimate the group-time average treatment effect on the treated following Callaway and Sant'Anna (2021), as shown in Equation 1:

$$\mathrm{ATT}_c\,(g_c, m) = E[Y_{i,m}(g_c) - Y_{im}\,(0) \mid G_i = g_c] \ldots (1)$$

Yi,m(*gc*) is the potential outcome for a store treated at *gc*, *Yi,m*(0) is the potential outcome had the store never been treated, and *Gi* = *gc* indicates that store *i* belongs to the treated group for closure *c*.

RESULTS: AVERAGE EFFECT ON NEARBY STORES

Parallel Trends

We assess the parallel-trends assumption using balanced samples that retain closures observed throughout each respective pre-treatment window (e.g., Guyt et al., 2025). For each outcome, we estimate pre-treatment trends separately for treated and control stores. We then estimate a treated-by-event-time interaction to test whether the groups exhibit statistically different pre-treatment slopes. We examine 3-, 6-, 9-, and 12-month pre-treatment windows. For Shopper Traffic, the treated-control difference in slopes is statistically insignificant across all balanced specifications, with p-values of .830, .891, .181, and .294 for the 3-, 6-, 9-, and 12-month windows, respectively. For Sales Revenue, the differential pre-treatment slope is likewise insignificant across all four windows, with corresponding p-values of .462, .372, .183, and .148. Thus, across alternative pre-treatment windows, we find no statistically detectable differential pre-trends between treated and control stores for either outcome, supporting the parallel-trends assumption.

Furthermore, we jointly test whether all pre-treatment lead coefficients are equal to zero, with the month immediately preceding closure ($t=-1$) serving as the reference period. Standard errors are clustered at the Walmart-closure level. Across 3-, 6-, 9-, and 12-month pre-treatment windows, the joint tests fail to reject the null of no differential pre-treatment effects for Shopper Traffic, with p-values of .824, .812, .217, and .141, respectively. For Sales Revenue, the corresponding joint-test p-values are .751, .923, .117, and .081. As a robustness check, we repeat the joint tests using the unbalanced sample, allowing the set of closure events to vary across event time. The conclusions are unchanged. The corresponding joint-test p-values are .879, .945, .591, and .437 for Shopper Traffic and .751, .928, .314, and .209 for Sales Revenue. Taken together, the linear differential-trend tests and the joint event-study tests provide no statistically detectable evidence of differential pre-trends across the examined windows, supporting the parallel-trends assumption.

Average Treatment Effect on Treated

We estimate the pooled average treatment effect on the treated by aggregating group-time estimates across all 39 closures in our sample. Table 2 shows that Walmart closures are associated with a statistically significant *decline* in both shopper traffic and sales at nearby treated stores, relative to matched controls. The pooled ATT is −.165 (95% CI [−.226, −.107]), corresponding to a 15.20%[1] decline in shopper traffic, and −.151 (95% CI [−.209, −.099]), corresponding to a 14.04% decline in sales revenue.

Table 2. Average Treatment Effect on the Treated (Pooled, Cohort-Size-Weighted)

Dependent Variable	ATT	Bootstrap SE	95% CI	p	Closures	Treated Stores	Control Stores
Log(Shopper Traffic)	−.165	.030	[−.226, −.107]	< .001	39	25,283	26,231
Log(Sales Revenue)	−.151	.028	[−.209, −.099]	< .001	39	25,283	26,231

Dynamic Treatment Effects

Figures 3A and 3B plot the dynamic pooled ATT for shopper traffic and sales revenue, respectively, as cohort-size-weighted averages across the 39 closure cohorts. During the 12 months before closure, the estimates fluctuate around zero, indicating no detectable differential pretrend. After treatment, the estimates become increasingly negative.

[1] We convert each coefficient to an exact percentage effect using $\% \Delta = (e^{b} - 1) \times 100$, where b is the estimated coefficient. For shopper traffic, $(e^{-.165} - 1) \times 100 = -15.20\ \%$. For sales revenue, $(e^{-0.151} - 1) \times 100 = -14.04\ \%$.

Figure 3A. Dynamic Effects for Shopper Traffic

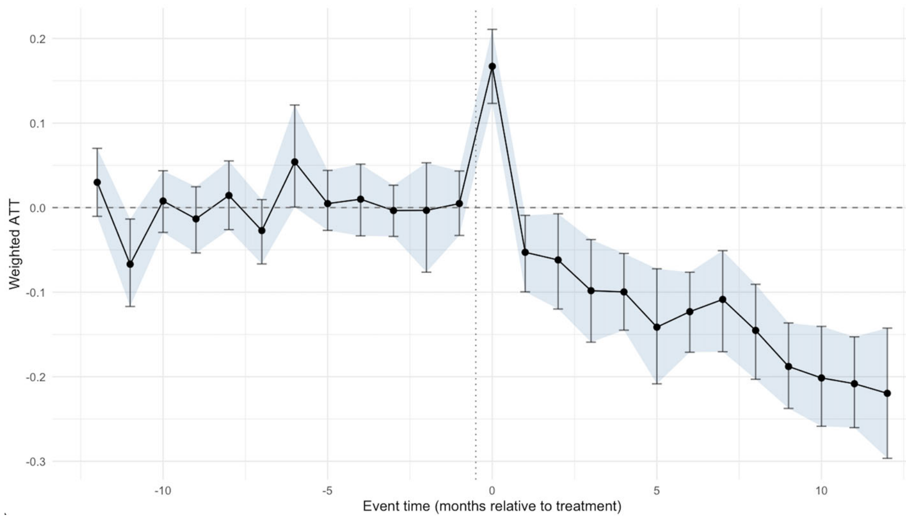
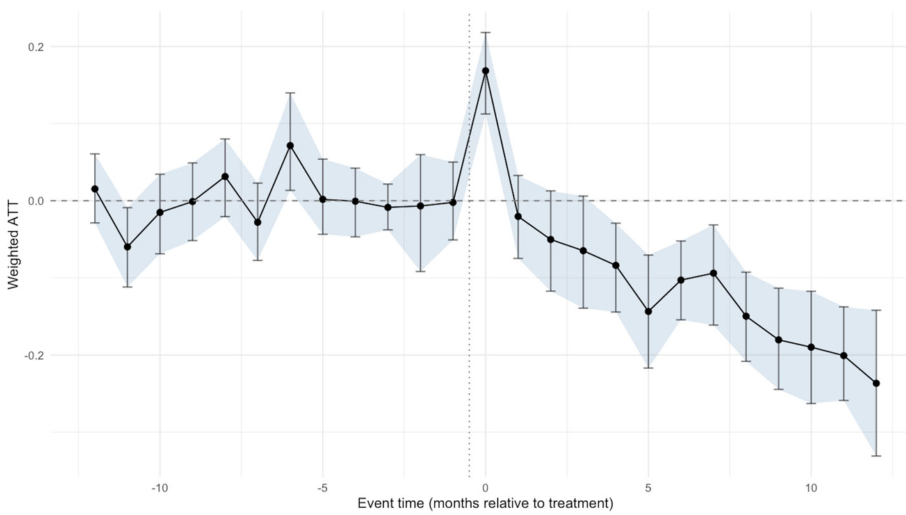


Figure 3B. Dynamic Effects for Sales Revenue



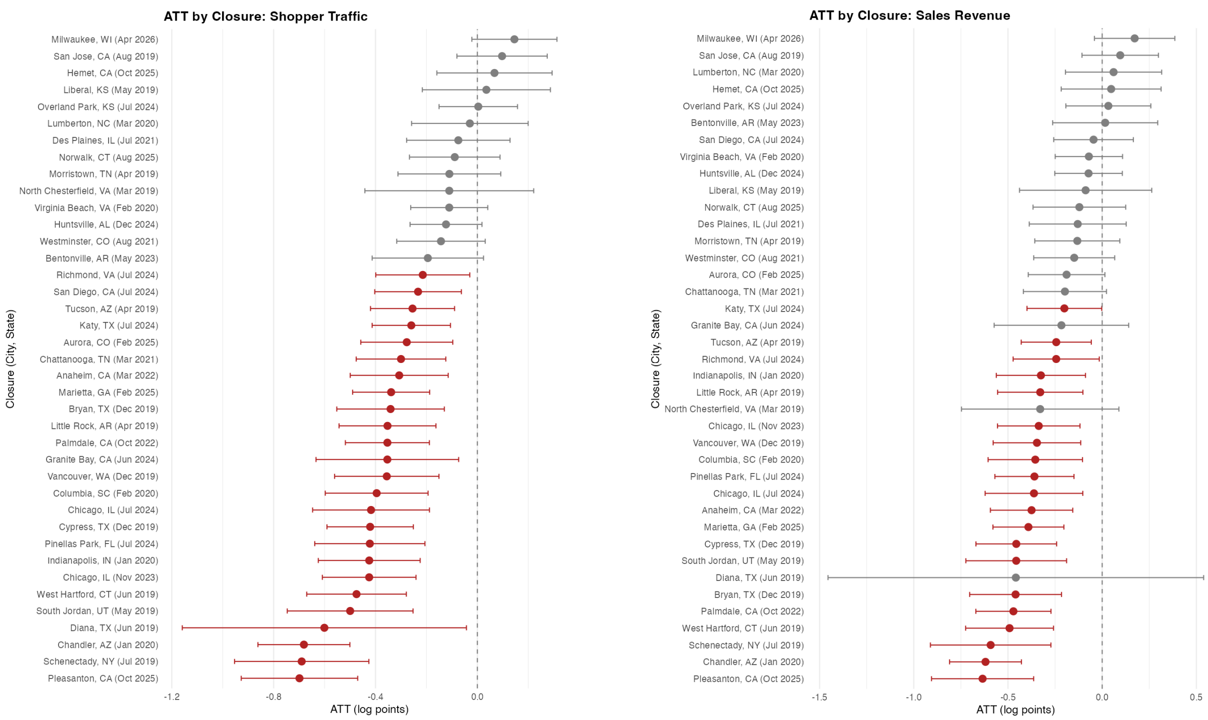
Closure-Level Treatment Effects

The two panels in Figure 4 plot the closure-level average treatment effects on shopper traffic and sales revenue. Each effect is estimated separately for the 39 closures using the doubly robust group-time estimator and is shown with a 95% confidence interval. Appendix A reports the closure-level estimates.

Across both outcomes, most statistically distinguishable closure-level estimates point in the same direction: nearby retailers experience declines in shopper traffic and sales after a

Walmart closure rather than fully absorbing the departing demand. The consistency of this pattern across independently estimated closures, rather than reliance on one pooled comparison, supports the interpretation that the average decline in nearby-store performance is not driven by only a few unusual events.

Figure 4. Closure-Level ATT on Shopper Traffic and Sales Revenue



Robustness Checks and Additional Analysis

Robustness to Number of Open-Walmart Control Anchors

Our baseline specification matches each closure to stores near its 10 nearest open Walmart locations. We assess whether this choice drives the results by reestimating the pooled average treatment effect on shopper traffic and sales revenue using the 5-, 15-, and 20-nearest open Walmart anchors, while holding all other elements of the matching procedure fixed. Table 3 reports the pooled ATT for each specification. The estimated effect is stable across this range for both outcomes. For shopper traffic, the pooled ATT ranges from −.164 ($N = 20$) to −.177 (N

= 5), with the baseline specification ($N = 10$) at −.165. For sales revenue, the pooled ATT ranges from −.151 ($N = 10$ and $N = 20$) to −.162 ($N = 5$). Thus, the findings are not sensitive to the number of open Walmart anchors used to construct the control group.

Robustness to Matching Radius

We reestimate the pooled ATT using radii of 2, 6, 14, and 18 miles while holding all other elements of the matching procedure fixed. Table 4 reports the results. The estimated effect remains negative and stable across this range for both outcomes. For shopper traffic, the pooled ATT ranges from −.147 at 2 miles to −.165 at the 10-mile baseline and stabilizes between −.161 and −.165 from 10 miles onward. For sales revenue, the pooled ATT ranges from −.121 at 2 miles to −.151 at 10 miles, with estimates between −.146 and −.151 at 10, 14, and 18 miles. Thus, the results are not sensitive to the radius used to define nearby stores.

Table 3. Robustness to the Number of Open-Walmart Control Anchors

	Shopper Traffic			**Sales**		
N (Open-Walmart Anchors)	**ATT**	**Bootstrap SE**	**95% CI**	**ATT**	**Bootstrap SE**	**95% CI**
5	−.177	.035	[−.250, −.113]	−.162	.034	[−.231, −.101]
10 (baseline)	−.165	.030	[−.226, −.107]	−.151	.028	[−.209, −.099]
15	−.168	.029	[−.226, −.112]	−.153	.027	[−.209, −.102]
20	−.164	.028	[−.220, −.112]	−.151	.026	[−.204, −.101]

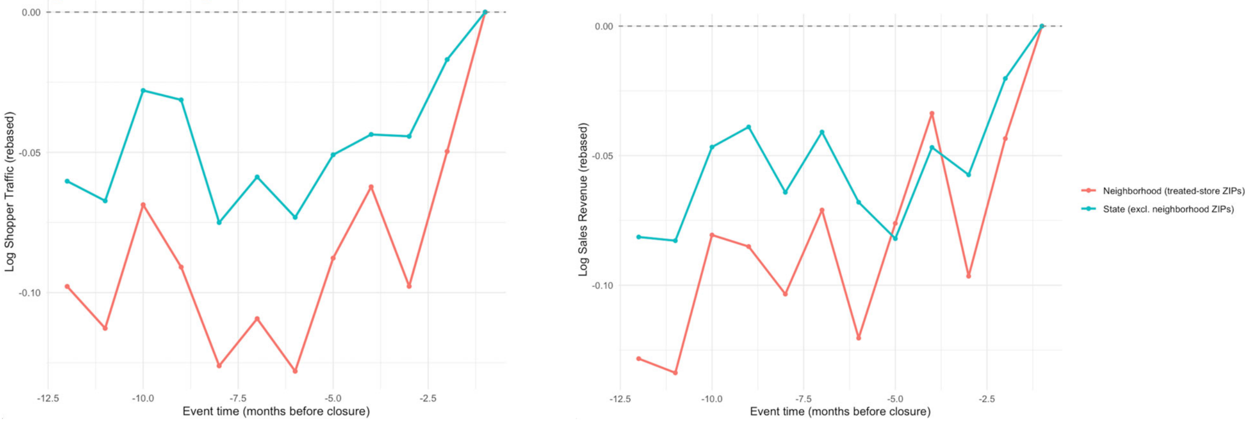
Table 4. Robustness to the Matching Radius

	Shopper Traffic			**Sales**		
Radius (Miles)	**ATT**	**Bootstrap SE**	**95% CI**	**ATT**	**Bootstrap SE**	**95% CI**
2	−.147	.029	[−.207, −.093]	−.121	.027	[−.175, −.073]
6	−.157	.028	[−.214, −.103]	−.143	.027	[−.200, −.092]
10 (baseline)	−.165	.030	[−.226, −.107]	−.151	.028	[−.209, −.099]
14	−.161	.030	[−.220, −.104]	−.148	.028	[−.204, −.095]
18	−.161	.029	[−.218, −.104]	−.146	.028	[−.202, −.094]

Alternative Mechanism: Neighborhood Decline

A concern is that treated neighborhoods might already have been declining for reasons unrelated to the focal Walmart closure, so the subsequent drop in nearby-store performance could continue a preexisting trend. To examine this possibility, we compare each closure’s treated-store ZIP codes with a state-level benchmark that excludes all closure-neighborhood ZIP codes and track both series over the 12 months before closure. We rebase each series to zero in the month immediately before closure so the figure compares trajectories rather than levels. Figure 5 shows that the two series move similarly during the preperiod with no clear evidence that treated neighborhoods were on a diverging downward trajectory relative to their states. This descriptive pattern is consistent with the earlier parallel-pretrends test and is less consistent with a general decline that clearly predates closure.

Figure 5. Neighborhood Versus State Shopper Traffic and Sales Revenue Before Closure

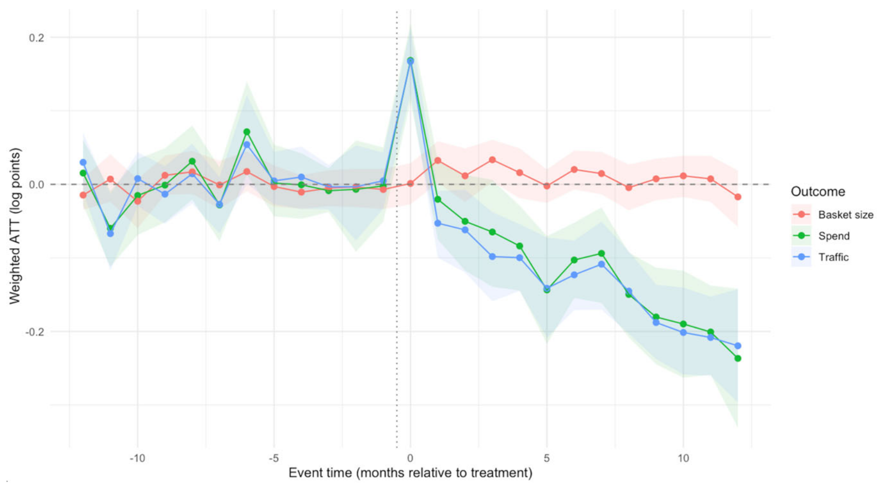


Alternative Mechanism: Basket Size

We propose that the sales revenue decline documented above operates primarily through lost shopper traffic: once the anchor closes, fewer shoppers visit the surrounding retail area, and this reduction in visits, rather than lower spending per visit, drives the fall in sales. To assess this explanation, we reestimate the closure-level model for three outcomes: shopper traffic, sales revenue, and basket size, defined as spending per visit. If reduced traffic is the primary channel, sales revenue should fall with traffic while basket size remains approximately unchanged. Therefore, a null or positive basket-size effect would be consistent with the traffic mechanism.

The basket-size estimate is small and positive: .014 (95% CI: [.001, .026]). The dynamic estimates show the same general pattern: shopper traffic and sales revenue decline together after closure, while basket size remains close to zero and slightly positive during much of the postperiod (Figure 6). This pattern is consistent with sales revenue falling because fewer shoppers visit nearby stores rather than because the remaining shoppers reduce their spending per visit. We cannot formally establish mediation because traffic and spending are jointly determined by the same closure, rather than being sequential stages in a causal chain. The close correspondence between the traffic and sales effects, together with the small basket-size effect, nevertheless supports the proposed agglomeration-loss interpretation.

Figure 6. ATT for Shopper Traffic, Sales Revenue, and Basket Size



INDIRECT EVIDENCE ON AGGLOMERATION LOSS AND COMPETITIVE RELIEF

When an anchor closes, nearby stores may capture purchases that shoppers previously made at the anchor, but they may also lose deliberate co-visits and incidental traffic. We refer to these countervailing forces as competitive relief and agglomeration loss. The negative average effects on shopper traffic and sales revenue are consistent with agglomeration loss outweighing competitive relief for the average nearby store.

Next, we provide indirect evidence on this interpretation by examining stores that may be positioned to experience less unfavorable postclosure outcomes. Competitive relief should be especially relevant for stores that most directly substitute for Walmart’s offerings. Brand status may separately provide resources and standalone customer draw that reduce dependence on anchor-generated traffic. Therefore, we test heterogeneity for (1) competing versus noncompeting nearby stores and (2) branded versus unbranded nearby stores.

Effects on Competing Versus Noncompeting Stores

Our theoretical framework identifies competitive relief as one of two countervailing forces created by an anchor's closure. This force should be stronger for a nearby store whose product assortment overlaps with the closed Walmart's offerings because such stores are best positioned to capture Walmart's displaced demand. We classify a treated store as competing if it operates in Supermarkets and Other Grocery (except Convenience) Stores (NAICS 445110) or Warehouse Clubs and Supercenters (NAICS 452910). We classify all other treated stores as noncompeting. The sample contains 25,283 treated store-closure observations; 352 (1.4%) meet the competing definition, and 24,931 (98.6%) do not. If competitive relief is present, the negative treatment effect should be less pronounced for competing than for noncompeting stores.

We test this prediction using the same closure-level design as the main analysis. Let $\text{Competing}_i \in \{0, 1\}$ indicate that store i operates in Supermarkets and Other Grocery (except Convenience) Stores (NAICS 445110) or Warehouse Clubs and Supercenters (NAICS 452910). Within each closure, we estimate Equation 2:

$$Y_{i,m} = \beta_1 \, (Post_{i,m} \times Treated_i) + \beta_2 \, (Post_{i,m} \times Treated_i \times Competing_i) + \delta_i + \tau_m + \varepsilon_{i,m} \ \ldots \ (2)$$

$Y_{i,m}$ denotes sales revenue. Treated_i indicates that store i belongs to the focal closure's treatment group, and $\text{Post}_{i,m}$ indicates months at or after that closure. δi and τm denote store and month fixed effects. The coefficient β_2 captures the difference between competing and noncompeting stores' treatment effects. We pool the closure-specific estimates using the same cohort-size-weighted bootstrap procedure as in the main analysis and test for differential pretrends using the same windowed event-study approach.

Table 5 shows a more favorable postclosure sales effect for competing than for noncompeting stores (b = .165, p = .018). Exponentiating the interaction coefficient yields an estimated 17.9% relative difference.[2] A separate subgroup analysis reports a 13.9% decline for noncompeting stores (p = .000) and a 5.5% decline for competing stores (p = .370). Because the interaction and subgroup estimates come from different specifications, the subgroup percentage-point difference is not numerically equivalent to the exponentiated interaction coefficient. We find no statistically detectable differential pretrend between competing and noncompeting stores (preperiod slope p = .337). Overall, the results are consistent with competitive relief partially offsetting, but not clearly reversing, the loss associated with reduced anchor-generated traffic.

Effects on Branded Versus Unbranded Stores

Not every nearby store is equally dependent on an anchor. A branded store's recognizable identity and marketing reach may generate standalone shopper traffic, making the store less dependent on incidental visits produced by the closed anchor. Brand equity and standardized operations may also provide resources that an independent operator lacks for absorbing a demand shock (Chiong et al., 2025). We classify a treated store as branded when SafeGraph identifies it with a named brand at the focal closure date and as unbranded otherwise (e.g., Chiong et al., 2025). Among the 25,283 treated store-closure observations, 3,147 (12.4%) are branded and 22,136 (87.6%) are unbranded. If brand status confers resilience, branded stores should exhibit a more favorable postclosure effect than unbranded stores.

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[2] Percentage differences are calculated as $(e^{b} - 1) \times 100$, the standard transformation for interpreting a coefficient from a log-linear model in percentage terms. For b = .165, this gives $(e^{.165} - 1) \times 100 \approx 17.9\%$.

Table 5 shows a more favorable postclosure sales effect for branded than for unbranded stores (b = .153, p = .001), corresponding to an estimated 16.5% relative difference after exponentiation. We find no statistically detectable differential pretrend between the groups (preperiod slope p = .972). A separate subgroup analysis estimates declines of 5.7% for branded stores (p = .008) and 6.6% for unbranded stores (p = .035). The subgroup contrast is smaller than the interaction-based contrast; because the specifications differ, the two quantities should not be treated as identical. The evidence is consistent with brand status being associated with resilience to anchor-related sales losses, but it does not by itself identify competitive relief as the source of that resilience.

Table 5. Heterogeneity in Postclosure Sales Effects

Condition	**Coefficient (b)**	**Bootstrap SE**	**95% CI**	**p**	**Pretrend p**
Competing	.165	.072	[.032, .311]	.018	.337
Branded	.153	.052	[.060, .259]	.001	.972

STORE-LEVEL RESILIENCE FACTORS

The preceding analyses estimate average and heterogeneous postclosure effects. We next examine two preclosure store characteristics—intrabrand clustering and online revenue share—that may be managerially relevant. Intrabrand clustering is the number of same-brand stores near a focal store (Kim & Jap, 2022; Zheng et al., 2026); it captures the extent of a brand's local physical presence. Online revenue share is the proportion of sales generated through digital channels; it reflects an established means of reaching shoppers who may no longer visit the physical store. Because stores do not randomly choose these characteristics, the estimates are associational and should not be interpreted as the causal effects of changing either characteristic.

Resilience Factor 1: Intrabrand Clustering

Prior research on brand encroachment suggests that nearby same-brand stores can benefit an incumbent business under some conditions (Kim & Jap, 2022). Although nearby outlets may cannibalize demand, a cluster of same-brand locations can also increase brand awareness, signal quality, and support shopper learning about the brand's offerings. Applied to a Walmart closure, this logic suggests that a store embedded in a denser same-brand network may be less dependent on anchor-generated traffic. Therefore, greater preclosure intrabrand clustering should be associated with a more favorable postclosure effect.

We measure Intrabrand Clustering$_i$ as the number of same-brand stores among the focal store's 15 nearest same-category neighbors, averaged over the 12-month pretreatment window (e.g., Woo et al., 2019). Stores without an identified brand receive a value of zero because they cannot be linked to same-brand neighbors. Among the 25,196 treated store-closure observations with valid clustering data, the mean number of same-brand neighbors is 1.52.

Table 6 shows that greater intrabrand clustering is associated with a more favorable postclosure sales effect (b = .043, p = .000). Each additional same-brand neighbor among a store's 15 nearest same-category neighbors corresponds to an approximately 4.4% relative difference in postclosure sales revenue. We find no statistically detectable differential pretrend associated with intrabrand clustering (preperiod slope p = .208). This estimate is consistent with a denser local brand presence being associated with resilience to the loss of anchor-generated traffic.

Resilience Factor 2: Online Revenue Share

An online sales channel may make a store less dependent on demand shocks affecting its physical location. Shoppers who no longer visit a retail area in person, or who visit only for pickup, can still reach a store online. Prior evidence on retail disruptions supports this logic: following mass shootings, affected restaurants experienced higher online revenue even as in-person revenue fell, and online ordering helped recover part of the loss in physical sales (Chiong et al., 2025). Applied to Walmart closures, stores with a larger preexisting share of online revenue may exhibit a more favorable postclosure effect than stores with little or no online business.

We measure Online Revenue Share$_i$ as online spending divided by total spending, averaged over the 12-month pretreatment window. The mean online revenue share is 5.0%. Table 6 shows that a larger online revenue share is associated with a more favorable postclosure sales effect (b = .252, p = .009). A 10-percentage-point increase in online revenue share corresponds to an approximately 2.6% relative difference in postclosure sales revenue. We find no statistically detectable differential pretrend associated with online revenue share (preperiod slope p = .203). This pattern is consistent with channel substitution documented in other retail disruptions (e.g., Ye & Shankar, 2025).

Table 6. Associations of Store-Level Resilience Factors With Postclosure Sales Effects

Store-Level Factor	**Coefficient (b)**	**Bootstrap SE**	**95% CI**	**p**	**Pretrend p**
Intrabrand Clustering	.043	.013	[.020, .071]	.000	.208
Online Revenue Share	.252	.111	[.053, .482]	.009	.203

DISCUSSION

An anchor-store closure creates two opposing forces for neighboring establishments: it may free demand by removing a major competitor while also eliminating a destination retailer that generated traffic spillovers for the surrounding area. The findings are consistent with agglomeration loss dominating on average. Competing and branded stores exhibit more favorable relative effects than their counterparts, but the subgroup estimates remain negative. Thus, Walmart appears to function as both a competitor and a traffic-generating anchor, and the average value of its demand externality may exceed the burden of its product-market competition for nearby establishments.

Contributions to Theory

This research first connects theories of retail competition with theories of demand-side agglomeration by distinguishing a store's share of local demand from the total demand reaching the commercial area. This distinction extends competitive theory by showing that geographically proximate retailers need not be purely substitutive. Two stores can compete at the product-market level while remaining complementary at the destination level: Walmart may take sales from a nearby retailer while both are operating, yet its presence can simultaneously increase the probability that shoppers travel to the area and encounter that retailer. Removing Walmart eliminates both effects. Therefore, the relevant theoretical question is not only whether Walmart and a neighboring store sell overlapping products but also whether Walmart's contribution to destination-level demand exceeds the business it draws away.

For the average nearby establishment, the negative effects on traffic and sales suggest that Walmart's market-making role outweighs its business-stealing effect. The competing-store interaction is consistent with competitive relief partially offsetting the average loss. The branded-store interaction and the resilience-factor analyses indicate that store characteristics may also moderate exposure to anchor-generated traffic. Because the interaction and subgroup estimates are not numerically identical and the moderators are observational, these findings should be interpreted as evidence of heterogeneity rather than as complete decompositions of agglomeration loss and competitive relief.

Second, the findings help reconcile the mixed literature on store exit. Some studies show that surviving retailers benefit after a competitor closes by recapturing part of the departing store's demand (Akturk & Ketzenberg, 2022). Others show that a prominent retailer's disappearance weakens local agglomeration economies, reduces employment, or increases the risk of neighboring businesses' closures (Benmelech et al., 2019; Shoag & Veuger, 2018). These findings need not be contradictory because a store closure is a two-dimensional shock: it simultaneously changes product-market competition and the attractiveness of the shopping destination.

The heterogeneity analyses provide evidence on two boundary conditions. Competing stores show a more favorable postclosure effect than noncompeting stores, a pattern consistent with stronger competitive relief when product assortments overlap. Branded stores also show a more favorable effect than unbranded stores, a pattern consistent with greater standalone drawing power or organizational resources. Agglomeration loss nevertheless remains consistent with the negative average and subgroup effects. Therefore, which force dominates may depend on the anchor's drawing power, the extent of assortment overlap, and shoppers' ability to redirect an entire trip elsewhere.

Third, the research broadens the concept of an anchor beyond the traditional shopping-center setting. Classic anchor-store research generally examines malls or planned shopping centers in which developers use leases and rent concessions to internalize the traffic externalities created by prominent tenants (Brueckner, 1993; Konishi & Sandfort, 2003; Pashigian & Gould, 1998). Here, exposure is defined across establishments in the surrounding commercial area rather than only among businesses sharing a mall, property owner, or leasing arrangement. Therefore, the results suggest that retail anchoring can operate through spatially shared shopper behavior even when nearby stores lack a formal contractual relationship with the anchor. This perspective treats anchor status as a functional property: a retailer acts as an anchor when it raises the expected value of traveling to a location and generates shopping trips that spill over to neighboring establishments.

Implications for Practice

The findings have implications for (1) the closing retailer, (2) neighboring businesses, (3) commercial-property owners, and (4) local policymakers. For the anchor retailer, a store-closing decision can have consequences beyond the focal location's performance and the redistribution of its customers across the chain. A Walmart store that is unprofitable from the company's perspective may nevertheless generate economically meaningful traffic for the surrounding retail area. This difference between the anchor's private financial contribution and its local demand externality does not imply that an unprofitable store should always remain open. It does imply that closure planning should recognize the store's role in the broader retail ecosystem.

For nearby businesses, the findings caution against assuming that Walmart's closure will automatically create an opportunity to capture its customers. Competitive relief is available only to shoppers who continue to visit the surrounding area. When shoppers relocate the entire trip, there is no departing demand for nearby stores to divide. Therefore, businesses should assess both their product overlap with Walmart and their dependence on Walmart-generated visits. Transaction records, loyalty data, mobility data, and shopper surveys can help identify prior co-visitation. The exploratory analyses identify two characteristics associated with more favorable outcomes: each additional same-brand neighbor is associated with an approximately 4.4% relative difference in postclosure sales, and a 10-percentage-point increase in online revenue share is associated with an approximately 2.6% relative difference. These associations may help identify exposure and resilience, but they should not be treated as causal prescriptions for adding locations or expanding digital channels.

Commercial-property owners and shopping-center managers should view an anchor closure as a portfolio-wide demand shock rather than solely as the loss of a single tenant.

Anchor-store theory indicates that the value of surrounding space partly depends on the traffic generated by prominent retailers, which helps explain why shopping-center owners have historically offered anchors favorable lease terms (Pashigian & Gould, 1998). When an anchor closes, the economic value of the remaining tenant mix may decline before those effects appear in lease renewals, vacancies, or property income. Therefore, property owners should prioritize the speed and quality of re-tenanting rather than evaluating replacement tenants only on the rent paid for the former anchor space.

For local governments and economic-development organizations, the results suggest that a major retail closure should be treated as a place-based economic shock. Its consequences may extend to businesses with no direct relationship to the closing retailer. Therefore, a closure response can include expedited permitting for redevelopment, coordination among property owners, transportation and access improvements, temporary support for affected small businesses, and efforts to recruit a replacement traffic generator. Business improvement districts and chambers of commerce may be particularly well positioned to organize destination-level promotions that individual stores would struggle to coordinate.

Limitations and Future Research

This research has several limitations that also create opportunities for future work. First, Walmart store closures are not randomly assigned. The matched stacked difference-in-differences design, closure-specific comparisons, absence of detectable differential pretrends, and use of modern group-time estimators strengthen the credibility of the analysis (Callaway & Sant'Anna, 2021; Cengiz et al., 2019). Nevertheless, these features cannot eliminate every form of time-varying confounding. Future research could classify closures by stated reason and test whether effects differ between chain-wide format rationalizations, lease expirations, property-specific events, and closures attributed to weak local performance. Designs based on plausibly exogenous closure events, discontinuities in corporate closure rules, or coordinated chain-wide exits would provide additional leverage. Researchers could also incorporate local employment, population, crime, construction, and commercial-vacancy trends or compare closure neighborhoods with multiple sets of controls selected using synthetic-control or triple-difference methods.

Second, geographic proximity is an imperfect measure of treatment exposure. The study defines nearby establishments using a 10-mile boundary and a nearest-store rule within six-digit NAICS categories. Future studies could construct treatment intensity using preclosure co-visitation. For example, exposure could be measured as the proportion of a store's customers who also visited the focal Walmart on the same day, within a specified time window, or during the same week. Researchers could also distinguish stores on the same parcel or in the same shopping center from stores connected only through a broader road network. Such data would permit direct estimation of how the anchor effect decays across space and whether shared shopping trips transmit the closure shock.

Third, using stores near surviving Walmart locations as controls introduces a potential spillover concern. Future research could track the destinations chosen by former customers of the closed location and identify which surviving Walmart stores or alternative retail clusters receive the displaced trips. Comparison groups could then exclude locations inside the observed destination network. An additional set of stores located away from both closed anchors and receiving anchors would help distinguish the absolute loss in the treated retail area from gains at alternative destinations. Such an analysis would also reveal whether closure destroys shopping trips, reallocates them geographically, or shifts them to online channels.

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Appendix A. Average Treatment Effect on the Treated by Closure

#	Street	City	State	Closure Date	Treated Stores	Control Stores	ATT (Shopper Traffic)	95% CI (Shopper Traffic)	ATT (Sales)	95% CI (Sales)
1	7000 Iron Bridge Rd	North Chesterfield	VA	29-Mar-19	222	613	-0.11	[-0.442, 0.221]	−.329	[−.747, .089]
2	1757 W Andrew Johnson Hwy	Morristown	TN	21-Apr-19	365	364	-0.11	[-0.312, 0.092]	−.132	[−.358, .094]
3	2510 Cantrell Rd	Little Rock	AR	08-Apr-19	794	472	-0.353	[-0.543, -0.163]	−.329	[−.555, −.102]
4	3900 W Ina Rd	Tucson	AZ	19-Apr-19	909	614	-0.255	[-0.420, -0.089]	−.244	[−.430, −.058]
5	712 N Western Ave	Liberal	KS	01-May-19	154	199	0.035	[-0.216, 0.287]	−.088	[−.439, .263]
6	3706 W 9800 S	South Jordan	UT	09-May-19	387	638	-0.499	[-0.747, -0.252]	−.456	[−.724, −.189]
7	1787 US-259	Diana	TX	20-Jun-19	20	319	-0.601	[-1.159, -0.043]	−.458	[−1.455, .539]
8	333 N Main St	West Hartford	CT	25-Jun-19	365	955	-0.475	[-0.670, -0.279]	−.492	[−.725, −.258]
9	3429 State St	Schenectady	NY	15-Jul-19	543	501	-0.69	[-0.954, -0.426]	−.592	[−.912, −.272]
10	1600 Saratoga Ave #501	San Jose	CA	03-Aug-19	1079	1236	0.097	[-0.080, 0.275]	.096	[−.107, .299]
11	1901 Texas Ave	Bryan	TX	07-Dec-19	468	279	-0.341	[-0.552, -0.130]	−.460	[−.703, −.216]
12	13742 N Eldridge Pkwy	Cypress	TX	03-Dec-19	541	782	-0.421	[-0.591, -0.251]	−.456	[−.670, −.242]
13	7809 NE Vancouver Plaza Dr	Vancouver	WA	14-Dec-19	866	303	-0.356	[-0.561, -0.151]	−.346	[−.579, −.114]
14	6085 W Chandler Blvd	Chandler	AZ	01-Jan-20	848	859	-0.681	[-0.862, -0.501]	−.619	[−.810, −.428]
15	8010 E 38th St	Indianapolis	IN	31-Jan-20	1115	972	-0.425	[-0.625, -0.225]	−.325	[−.562, −.088]
16	3603 Broad River Rd	Columbia	SC	18-Feb-20	828	490	-0.395	[-0.597, -0.194]	−.355	[−.606, −.105]
17	2864 Virginia Beach Blvd	Virginia Beach	VA	01-Feb-20	948	741	-0.11	[-0.262, 0.041]	−.071	[−.249, .108]
18	2503 W 5th St	Lumberton	NC	04-Mar-20	294	378	-0.029	[-0.258, 0.199]	.060	[−.195, .316]
19	4110 Shallowford Rd	Chattanooga	TN	28-Mar-21	869	345	-0.3	[-0.476, -0.124]	−.198	[−.418, .023]
20	727 W Golf Rd	Des Plaines	IL	17-Jul-21	292	848	-0.075	[-0.278, 0.128]	−.130	[−.387, .127]
21	7170 Federal Blvd	Westminster	CO	18-Aug-21	492	942	-0.143	[-0.317, 0.031]	−.148	[−.363, .067]
22	10912 Katella Ave	Anaheim	CA	21-Mar-22	800	825	-0.307	[-0.499, -0.115]	−.375	[−.593, −.156]
23	320 W Rancho Vista Blvd	Palmdale	CA	25-Oct-22	412	787	-0.353	[-0.518, -0.188]	−.471	[−.671, −.272]
24	3701 SE Dodson Rd	Bentonville	AR	12-May-23	381	383	-0.194	[-0.413, 0.024]	.016	[−.263, .295]
25	2844 N Broadway	Chicago	IL	26-Nov-23	1362	992	-0.425	[-0.609, -0.241]	−.337	[−.556, −.118]
26	4080 Douglas Blvd	Granite Bay	CA	09-Jun-24	104	795	-0.353	[-0.634, -0.073]	−.216	[−.573, .141]
27	6900 US Hwy 19 N	Pinellas Park	FL	13-Jul-24	395	965	-0.422	[-0.639, -0.206]	−.360	[−.570, −.150]
28	10303 Metcalf Ave	Overland Park	KS	02-Jul-24	633	400	0.004	[-0.150, 0.158]	.033	[−.193, .258]
29	24919 Westheimer Pkwy	Katy	TX	07-Jul-24	770	546	-0.259	[-0.413, -0.106]	−.201	[−.399, −.003]
30	5221 Brook Rd	Richmond	VA	15-Jul-24	976	498	-0.214	[-0.399, -0.030]	−.244	[−.473, −.015]
31	2121 Imperial Ave	San Diego	CA	16-Jul-24	1076	706	-0.233	[-0.403, -0.063]	−.046	[−.257, .166]
32	2551 W Cermak Rd	Chicago	IL	17-Jul-24	1121	398	-0.417	[-0.647, -0.188]	−.362	[−.621, −.103]
33	4226 NW Oakwood Ave	Huntsville	AL	04-Dec-24	805	472	-0.123	[-0.264, 0.018]	−.072	[−.252, .107]
34	10400 E Colfax Ave	Aurora	CO	13-Feb-25	808	679	-0.277	[-0.458, -0.097]	−.189	[−.392, .014]
35	3101 Roswell Rd	Marietta	GA	14-Feb-25	846	1079	-0.339	[-0.490, -0.187]	−.392	[−.580, −.203]
36	680 Connecticut Ave	Norwalk	CT	28-Aug-25	486	1142	-0.089	[-0.267, 0.089]	−.121	[−.367, .125]
37	859 W Florida Ave	Hemet	CA	20-Oct-25	400	1060	0.067	[-0.159, 0.294]	.048	[−.217, .313]
38	3112 Santa Rita Rd	Pleasanton	CA	08-Oct-25	482	729	-0.699	[-0.927, -0.470]	−.635	[−.906, −.364]
39	7025 W Main St	Milwaukee	WI	18-Apr-26	1027	925	0.146	[-0.022, 0.313]	.172	[−.041, .386]