The Reallocation of Retail Space: Evidence from the Rise of E-Commerce

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Abstract

Over the past 25 years, e-commerce shifted demand away from stores by selling easily shipped goods. We study how the retail real estate built around those stores adjusted. A frictionless benchmark predicts that capital released by a declining use is quickly reallocated at full value; we ask how far retail departs from it. We link establishment records at mapped retail places to occupancy and net operating income for properties financed through commercial mortgage-backed securities. Our main design combines each property's 2006 tenant mix with national category-level online-sales shares; complete national chain liquidations reveal the adjustment path after a sharp tenant loss. Retail properties diversified away from goods retail, and establishment activity recovered after COVID, especially at smaller and already-diversified properties. That recovery largely bypassed the properties most exposed to online substitution. A 10 percentage point increase in baseline exposure is associated with approximately 0.6 percentage points lower occupancy and 2 percent lower NOI per square foot over 2014–2019, while the establishment shortfall persists through 2023. Following a complete chain liquidation, occupancy falls by 6.2 percentage points and NOI by 13 percent within one year. Occupancy subsequently recovers more than income, while non-retail entry remains weak: a property can refill without restoring its earning power, a wedge we call capital scarring. This scarring is deepest where the building is most specialized, the local market for replacement tenants is thinnest, and leverage is highest. Malls that eventually cease operating are distinguished not by greater initial e-commerce exposure, but by several years in which tenant exits persistently exceed entry. The findings show that aggregate sectoral recovery can coexist with slow in-place reallocation and persistent impairment of the legacy capital most directly displaced by technological change.

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1 Introduction

Over the past 25 years, online shopping went from a rounding error to a central channel of American retail: e-commerce rose from a negligible share of retail spending around 2000 to roughly 15 percent by the early 2020s. The growth was deeply uneven across what people buy, with the online share now being large in books, electronics, apparel, and furniture, but negligible in groceries, restaurants, and personal services. As spending moved online, the physical footprint built to sell goods in person shrank with it: retail fell from 14 percent of U.S. private-sector establishments in 2000 to 9 percent in 2025. This was a large and lasting demand shock to a specific, durable, and immovable form of capital—the stores, shopping centers, malls, and anchor boxes organized around in-person goods sales. This paper traces what happened to that capital.

Under a reasonable benchmark, one would expect the adjustment to this type of technology shock to be fast and complete. When demand leaves one use of a durable asset, a frictionless market reallocates the asset to its next-best use: an expanding tenant takes the space, and the rebuilt match is as productive as the one it replaced. Buildings, however, cannot move, and a store is not a warehouse. Reallocating retail space requires a new user to want that particular location and the owner to make the building fit by, for example, subdividing an anchor box, cutting a new entrances, or by re-leasing at a rent the new use can support. We show that adjustment to the e-commerce shock was neither fast nor complete. It took years even after occupancy returned; the new tenant–property match generated less income than the old one, and the recovery did not accrue to the properties that were hit hardest.

Despite the rapid growth of e-commerce, estimates of its effects on commercial property have been limited. This paper provides such estimates. We think of commercial real estate as productive capital and use e-commerce to study how technological change reallocates assets that are durable and fixed in place. We combine annual establishment records from Infogroup, fixed retail-place boundaries from OpenStreetMap, and property financials and loan records from commercial mortgage-backed securities (CMBS) from Trepp. We use these data in three ways. We first compare properties whose 2006 tenant mixes left them more or less exposed to the national rise of online shopping. We then study 57 complete physical-store liquidations, where an entire chain disappears and cannot select only its weakest local stores for closure. Finally, we reconstruct the

lifecycles of malls that operated in 2006 and later ceased ordinary operation. Together, the three designs show how strongly e-commerce affected retail property, how a building adjusts after losing a major tenant, and what happens when tenant replacement fails.

We find four main results. First, the retail-property sector adapted to the growth in e-commerce, but the recovery was concentrated away from the properties most exposed to online substitution. Within a fixed cohort, goods retail fell from 28.5 percent of establishments in 2013 to 20 percent in 2023. Between 2019 and 2023, establishment counts grew by 12.6 percent at properties below 100,000 square feet and 3.5 percent at properties above 400,000 square feet; they grew by 12.1 percent at already-diversified properties and 4.8 percent at retail-heavy properties. Second, baseline e-commerce exposure predicts economically large property losses. A 10 percentage point increase in exposure is associated with 0.60 percentage points lower occupancy and 0.02 log points lower net operating income (NOI) per square foot over 2014–2019. Reweighting the estimates to the broader population of mapped retail places implies about $1.27 billion in annual NOI losses in 2019. Third, complete chain liquidations reveal capital scarring: one year after liquidation, occupancy is 6.18 percentage points lower and NOI per square foot is 0.133 log points lower; by year three occupancy has recovered substantially, but income has not. Fourth, adjustment is weaker when the property is more specialized, the local replacement market is thinner, or leverage is higher. Mall death is the endpoint of this process: malls that eventually died were not more exposed to e-commerce than surviving malls, but departures persistently outpaced entry before they stopped operating.

We begin by measuring each property's ex ante exposure to e-commerce. Exposure combines the property's tenant mix in 2006 with the 2019 online-sales share of each retail category from the Census Annual Retail Trade Survey. A property with more bookstores, electronics stores, apparel stores, furniture stores, or sporting-goods stores is more exposed than one with more grocery, restaurant, personal-service, or office tenants. Fixing tenant mix in 2006 prevents later tenant changes from altering treatment. The results are similar when tenant mix is fixed in 2009, 2011, or 2013, and when later national e-commerce-share vectors replace the 2019 rates.

Properties with higher exposure to e-commerce subsequently perform worse. As noted previously, over 2014–2019, an increase in exposure is associated with lower occupancy and lower NOI per square foot. Comparing properties within the same county-year gives similar estimates.

The gaps widen during the COVID period, as a 10 percentage point increase in exposure is associated with 1.38 percentage points lower occupancy and a 0.031 log points decease in NOI over 2020–2021. Business activity follows the same pattern. The total-establishment gap grows from 0.70 establishments over 2014–2019 to 1.80 over 2020–2021 and 2.12 over 2022–2023. The estimates also remain similar after controlling for predetermined local business activity, income, retail intensity, and pre-period outcome trends.

The implied magnitudes are large even when benchmarked against the post-pandemic disruption in office property. For Trepp properties with observed NOI, the interacted estimates imply a 4.1 percent annual NOI decline in 2019. Reweighting the same estimates to all mapped OpenStreetMap retail places gives a 3.3 percent decline. In the non-interacted benchmark, the reweighted decline is 2.9 percent, or about $1.27 billion in annual NOI. The fitted losses are larger for retail formats built around exposed goods sellers: 5.8 percent for mapped malls and shopping centers and 5.4 percent for places with at least 100 establishments. For comparison, realized constant-quality office-rent declines after the shift to remote work range from 3.9 percent in Dallas–Fort Worth and 5.7 percent in Boston to 12.7 percent in Manhattan and 17.1 percent in San Francisco (Gupta et al., 2026; Abramson et al., 2025; CompStak, 2026). The all-property retail estimates are comparable to the smaller office-market declines, while the large-format estimates lie within the broader office-market range.

Establishment data show that the places hit hardest by e-commerce were not the ones that switched most successfully to other uses. The sector as a whole moved away from goods retail: health and personal care rose from 12.7 to 19.3 percent of establishments between 2013 and 2023, and services, food, entertainment, finance, and professional uses occupy a substantial share of retail properties. The most-exposed properties nevertheless lost both retailers and total activity. Relative to less-exposed properties, a 10 percentage point increase in exposure predicts 1.82 fewer retail businesses and 3 fewer total businesses per 100 baseline businesses over 2014–2019. Services and entertainment rise by 0.49, but non-retail businesses as a whole fall by 1.2. Over 2020–2023, the retail and total gaps widen to 5.68 and 6.52, while the broader non-retail difference is close to zero. The expanding uses located disproportionately at smaller and already-diversified properties rather than restoring the properties that bore the largest shock.

When is reallocation to a new use particularly slow and incomplete? To study the adjustment after a sharply timed tenant loss, we assemble 57 complete physical-store liquidations in retail categories with large growth in online shopping. An event must lose at least 90 percent of its national store network within two years. Once a chain qualifies, every identifiable pre-event store is assigned before property outcomes are used, so the chain cannot select only its weakest local properties for treatment. The resulting sample contains 2,302 unique Trepp properties. We measure each affected property's change from two years before liquidation and subtract the corresponding change at other observed retail properties in the same county. This design estimates the average property response to a full-network tenant loss without requiring the chain-level bankruptcy itself to be random.

A complete liquidation causes a sharp decline in both physical occupancy and income. One year after liquidation, occupancy is 6.18 percentage points lower and log NOI per square foot is 0.133 log points lower than at same-county comparison properties. At year three, the occupancy gap has narrowed to 2.52 percentage points, while the log-NOI gap remains 0.067 log points. Recovery patterns show the same difference between refilling space and restoring income. Among properties with a material early loss, 47.6 percent recover at least 90 percent of the occupancy loss within three years of the trough, compared with 39.4 percent for NOI. We call this persistent wedge capital scarring—the place-bound analog of the earnings losses that remain after displaced workers find new jobs (Jacobson et al., 1993; Davis and von Wachter, 2011).

Further evidence of limited conversion comes from the businesses operating at the affected property. Other retailers do not replace the liquidated chain during the first three years, and non-retail uses do not fill the gap. The non-retail business stock is lower by 4.20 businesses per 100 baseline businesses at year +1, 6.45 at year +2, and 7.24 at year +3. Annual arrivals of new non-retail businesses fall by 1.67, 1.55, and 1.47 per 100 baseline businesses over the same horizons. The total number of businesses is 7.45 lower by year +3, although that estimate is less precise. At former chain units with an explicit postal marker, reuse is much more often another retailer than a non-retail business, and many units have no matched business. The evidence points to slow replacement and partial reuse rather than a quick conversion to new, equally valuable uses.

Next, we ask which frictions impede finding new tenants and regaining income. We focus

on asset specificity, the thickness of the local replacement market, and financial position, using measures fixed two years before liquidation. More-specific properties experience larger immediate losses: a one-standard-deviation increase in specificity is associated with an additional 1.07 percentage point decline in occupancy and an additional 0.063 log point decline in NOI by year +1. Both estimates are marginally significant, and the NOI path differs before liquidation, so we view the income result as suggestive. Properties in thin non-retail replacement markets have a 0.130 log point larger NOI decline by year +1, although that gap is already visible at year −1 and there is no corresponding occupancy difference. Financial position is most clearly related to occupancy recovery. A 10 percentage point increase in current loan-to-value ratio (LTV), updating the securitization appraisal with county house-price growth, predicts an additional 0.94 percentage point occupancy decline by year +1 and 1.07 percentage points by year +2; the NOI estimates are small and imprecise. Across the three measures, adjustment is weakest where the space is hardest to adapt, plausible new users are scarce, or leverage is high.

Mall death provides the severe endpoint. Malls were unusually exposed to e-commerce as a format, but the malls that eventually died were not more exposed than surviving malls: mean 2006 exposure is 13.51 percent among 108 malls that later ceased ordinary operation and 14.79 percent among 857 currently operating malls when both groups are measured with the same address-based geography. What differs is the balance between tenant exit and entry. In the final year of ordinary operation, exits equal 39.7 percent of the prior-year tenant stock at malls approaching the end of ordinary operation and 16.8 percent at comparisons, while entry equals 9.7 and 16.4 percent. By that year, 74.8 percent of the closing malls' baseline tenants are gone, compared with 46.7 percent at matched malls, and entrants equal only 17.9 versus 46.8 percent of baseline. Exposure created a common headwind; persistent failure to replace departing tenants distinguished the malls that stopped operating.

These results contribute to three literatures. First, they provide property-level evidence on the reallocation of installed capital after technological change. Research on creative destruction, misallocation, and capital reallocation emphasizes that growth depends on moving resources away from declining uses and toward expanding ones (Aghion and Howitt, 1992; Hsieh and Klenow, 2009; Restuccia and Rogerson, 2008; Eisfeldt and Rampini, 2006; Lanteri et al., 2023; Lanteri and

Rampini, 2023). Specialized assets are costly to redeploy and trade less easily in thin markets (Ramey and Shapiro, 2001; Gavazza, 2011; Shleifer and Vishny, 1992). To our knowledge, this is the first paper to follow the full property-level adjustment path after the rise of e-commerce: from predetermined tenant exposure, to complete chain liquidations, to tenant replacement, operating income, and the failure of entire retail clusters. The distinction between occupancy and income shows directly how reused physical capital can remain economically scarred.

Second, the paper adds to the retail economics, e-commerce, and commercial-real-estate literatures. Existing work studies how e-commerce reshaped retail formats, firms, consumer spending, local labor markets, and nearby real estate (Hortaçsu and Syverson, 2015; Chava et al., 2024; Relihan, 2022; Chung, 2023). We shift attention to the commercial property displaced by the shock and the new uses it has found. The closest parallel is the study of remote work and office property, where a technological demand shock produced large losses at lower-quality buildings and a flight to quality elsewhere (Gupta et al., 2026). Retail shows the same divergence between aggregate recovery and the performance of the capital most directly exposed.

Third, the results connect capital reallocation to the places and institutions around commercial property. Persistent vacancy and lost anchors can weaken nearby activity and contribute to blight or crime (Shoag and Veuger, 2018; Koster et al., 2019; Cui and Walsh, 2015), while lower property cash flow can impair collateral and raise distress for financial institutions (Jiang et al., 2025; Glancy and Kurtzman, 2024). The LTV results also connect the adjustment problem to debt overhang: even when a replacement tenant is not financially constrained, the landlord's balance sheet can slow the reuse of the property (Myers, 1977; Almeida et al., 2012; DeFusco et al., 2023; Buera et al., 2011; Midrigan and Xu, 2014; Moll, 2014).

The remainder of the paper is organized as follows. Section 2 describes the retail-property shock, the recent recovery, and the costs of repurposing space. Section 3 describes the linked property, establishment, liquidation, and historical-mall data. Section 4 estimates the effects of e-commerce exposure and asks where expanding uses located. Section 5 traces adjustment after complete chain liquidations and tests the three channels. Section 6 studies failed tenant replacement at the severe tail. Section 7 concludes.

2 Background and Setting

2.1 Retail disruption and uneven recovery

Figure 1 places the property analysis in the long-run transformation of retail. Panel A shows the rise of e-commerce. Online shopping grew from a negligible share of retail purchases around 2000 to roughly 15 percent by the early 2020s, with much larger online shares in books, electronics, apparel, furniture, and other goods categories. The vertical line marks 2006, when we fix the tenant mix used in the preferred property-exposure measure. The subsequent growth of online shopping changed not only where purchases were made, but also the demand for establishments and buildings organized around in-person goods sales.

Panel B shows the corresponding contraction in retail establishments. Retail accounted for 13.9 percent of private establishments in 2000 and 8.9 percent in 2025. The corresponding share of private employment fell from 13.9 to 11.6 percent, as shown in Appendix Figure C4. Nonstore retail grew over this period, but from a much smaller base. In 2021, electronic shopping and mail-order businesses accounted for 3.9 percent of retail establishments and 3.0 percent of retail employment. These figures do not capture every job created by online shopping, since many fulfillment-center jobs are classified in warehousing rather than retail. They nevertheless show that directly measured nonstore retail remained small relative to the broader retail sector.

The decline in goods retail did not imply that retail properties simply emptied. Panel A of Figure 2 shows substantial change in the tenant mix at a fixed cohort of retail properties. Goods retailers fell from 28.5 percent of establishments in 2013 to 20.0 percent by 2023. Health and personal care rose from 12.7 to 19.3 percent, while food and drink remained roughly flat. Finance and real estate, professional and administrative services, personal services, and entertainment also account for a substantial share of the businesses operating at retail properties. The figure counts establishments rather than occupied square footage, so a small service tenant and a large goods retailer each receive one count. The result is therefore a change in the composition of businesses using retail properties, not an estimate of the share of floor area converted to each use.[1]

[1]The shift away from goods retail is similar under narrower matching and alternative establishment weights. Using exact-address matches, the goods-retail share falls from 29.8 percent in 2013 to 21.1 percent in 2023. Using fixed Infogroup employment-size weights, it falls from 42.1 to 36.7 percent. The higher employment-weighted level is consistent with goods retailers operating larger establishments than many service tenants.

Industry accounts increasingly describe a post-COVID recovery in retail property. In early 2026, JLL reported that national retail vacancy held at 4.4 percent and rents were 2.0 percent higher than a year earlier, with restaurants, discount retailers, and grocery operators expanding while apparel and electronics contracted. Yet the same evidence describes a recovery concentrated in particular formats. Announced openings in 2025 were concentrated in spaces below 10,000 square feet, closures were concentrated between 10,000 and 50,000 square feet, and nearly 90 percent of new leases were for less than 5,000 square feet (Virtue and Mason, 2026; JLL, 2025).

Panels B and C of Figure 2 show the same heterogeneity in our establishment data. Between 2019 and 2023, establishment counts rose 12.6 percent at properties below 100,000 square feet and only 3.5 percent at properties above 400,000 square feet. Counts rose 12.1 percent at already-diversified properties and 4.8 percent at retail-heavy properties. The largest and most retail-heavy places lost establishments in 2021 and recovered more slowly afterward, a flight to quality within retail property.

These facts frame the question the rest of the paper answers. The sector plainly changed its tenant mix, and parts of it recovered strongly. But growing businesses can open in newly built centers or already-thriving locations instead of the buildings that lost their goods retailers. Whether the most-exposed properties attracted the expanding uses, whether occupancy recovery restored income, and when repeated tenant loss ended in the failure of the retail cluster itself are questions about specific buildings, not about the sector—and they are the questions we take up.

2.2 How retail space is repurposed

The rise of e-commerce reduced demand for stores selling easily shipped goods, including books, electronics, toys, furniture, and apparel. Their closures left behind inline mall stores, big boxes, anchor spaces, power centers, strip centers, and standalone buildings. The cost of reuse varies sharply across these formats. A small inline space may be re-leased with limited work. A large anchor box may require subdivision, new entrances, exterior access, demolition, changes to parking and circulation, substantial tenant improvements, leasing commissions, rent concessions, and a long period of carrying vacancy (Sills Cummis & Gross, 2025; CBRE, 2026).

The best replacement users are not always retailers selling similar goods. Shopping centers

increasingly rely on grocery, fitness, restaurants, medical and personal-care businesses, education, entertainment, finance, and local services to generate traffic (ICSC, n.d.). Limited new retail construction also means that expanding users often must take existing space (JLL, 2025). Alternative users exist. The economic question is whether they choose the impaired property and whether the resulting use restores the property's operating income.

The former Toys "R" Us at Shoppes at Sunset Hills, Missouri illustrates a successful but costly transition. After the chain liquidated, the property was left with 48,200 square feet of vacant space. Fitness and children's-entertainment concepts expressed interest, but use restrictions in a neighboring tenant's lease made the deals too expensive without compensation or concessions (St. Louis Post-Dispatch, 2018). By 2021, the center was reported to be 81 percent empty, with outdated space, poor access, deteriorating parking and curbs, and a proposed grocery redevelopment supported by a local sales-tax district (Achenbach, 2021).

The eventual reuse required a different plan. Bass Pro Shops opened a 93,000-square-foot destination store in 2023, and the conversion included approximately $2 million in stormwater, utility, access, and intersection improvements (Bass Pro Shops, 2023; Castle Contracting, 2023). As of 2026, leasing materials describe the center as fully leased (Sansone Group, 2026). Sunset Hills ultimately recovered, but only after years of vacancy, failed matches, contractual negotiation, public support proposals, and physical investment.

The obstacles to reuse are broader than construction expense. Zoning, parking requirements, use restrictions, co-tenancy clauses, reciprocal easement agreements, and permitting can delay or prevent a change from goods retail to medical, residential, entertainment, office, or mixed use. An anchor departure can also reduce customer traffic and trigger co-tenancy provisions for the remaining stores (Pashigian and Gould, 1998; Gould et al., 2005). A vacancy at one unit can therefore weaken the rest of the retail cluster.

2.3 Capital scarring

Capital scarring. We call the persistent gap between physical reuse and economic recovery *capital scarring*. In the frictionless benchmark, a released asset moves to its next-best use and the rebuilt match is as productive as the one it replaced; there is no scar. In practice a property can

find a tenant and stay occupied while generating less net operating income than before—because the new use pays less, demands concessions or tenant improvements, costs more to operate, or simply fits the building worse. This is the place-bound analog of the earnings losses that follow displaced workers, who find new jobs without regaining their old wages (Jacobson et al., 1993; Davis and von Wachter, 2011). Our data do not separate the components of the scar; they let us see whether occupancy and income return together, and they show that they do not.

Whether a scar forms, and how deep it is, depends on how far the building sits from the frictionless ideal. Three features govern this, and organize the property-level analysis in Section 5. The first is asset specificity—how tightly the structure is built around its old use, and thus how much must be rebuilt for another. The second is the thickness of the local replacement market—whether the surrounding area supplies a deep pool of restaurants, clinics, services, and other in-person users, or mainly more declining retail. The third is financial position—whether the owner can fund the search, subdivision, permitting, and improvements that conversion requires. At the extreme, when all three cut against a property, reuse fails entirely and the retail cluster itself disappears. We return to each channel directly.

3 Data

3.1 Overview

We assemble linked panels of establishments, fixed retail-place boundaries, property financials, CMBS loan records, complete chain liquidations, and historical mall lifecycles. Infogroup records the businesses at each location; OpenStreetMap and related boundary data define retail places; Trepp reports occupancy, NOI, loan terms, and loan status for CMBS-financed retail properties; Green Street identifies malls and large centers and supplies the mall comparison universe. Three designs use these data to follow successive stages of adjustment: a broad exposure design comparing properties whose 2006 tenant mixes made them more or less vulnerable to the national rise of e-commerce; a complete-liquidation design following properties after a tenant's entire national store network closes; and a historical-mall design comparing the tenant entry and exit of malls that later ceased operating with those that survived.

For the descriptive aggregate trends in Section 2.1, we also use Census retail e-commerce statistics and national establishment and employment counts from the Bureau of Labor Statistics Quarterly Census of Employment and Wages. These national series provide background and do not enter property treatment assignment. The 2022 NAICS revision eliminated nonstore retailers as a separate subsector and reclassified those establishments by the products they sell rather than by whether sales occur online. We therefore show the directly observed nonstore series only through 2021 and document the classification rule in the figure notes (Haley and Keller, 2023).

3.2 Retail locations and establishments

The broad mapped retail-place panel starts from OpenStreetMap polygons tagged as retail areas, malls, or shopping centers. We keep polygons large enough to represent retail places rather than individual storefronts and assign annual Infogroup establishments to those boundaries using their geocoded coordinates. When polygons overlap, establishments are assigned to the smallest containing polygon so that nested shopping areas do not double-count the same business.

Infogroup records business names, addresses, industry codes, coordinates, employment-size bins, and corporate-family identifiers. We use the industry codes to group establishments into goods retail; food and drink; health and personal care; personal services and entertainment; finance and real estate; professional, education, and administrative services; and other uses. These groups distinguish goods retailers from the restaurants, personal-care businesses, medical providers, salons, fitness studios, banks, professional offices, and other businesses that operate at retail properties.

The Infogroup employment-size fields are modeled bins rather than payroll records or measured square footage. For employment outcomes, we assign each establishment its latest positive employment-size value observed from 2007 through 2023 and sum those fixed establishment weights among businesses present at the property in each year. The measure therefore changes when establishments enter or exit, rather than when Infogroup revises a continuing establishment's modeled size bin. Appendix Table A3 compares the panel with County Business Patterns and shows that it tracks the contraction in retail categories most exposed to e-commerce.

3.3 Trepp properties and the establishment match

Trepp provides annual property and loan information for retail properties financed through CMBS. We first link source Trepp records to establishment rosters and then consolidate records referring to the same physical property. Within each canonical property-year, we deduplicate repeated Infogroup establishment identifiers and rebuild the establishment counts and e-commerce exposure from the resulting roster. We assign occupancy and NOI to the fiscal year reported by Trepp. The principal operating outcomes are occupancy and log NOI per square foot.

For the broad exposure analysis, a Trepp record linked to a Green Street retail center uses the center-level establishment roster. Records without such a link use a normalized Trepp-address roster requiring exact or valid-range street-number agreement and exact or fuzzy street-name agreement. We replace 24 Green Street–Trepp links with conflicting leading street numbers by their Trepp-address rosters. Matching uses property identifiers and geography rather than property outcomes. Appendix B describes the algorithm, external checks, deterministic corrections, and robustness to alternative roster definitions.

The liquidation analysis uses a narrower match. Candidate stores are fixed from their pre-event Infogroup chain identifiers and linked by exact establishment identifier to the paper's existing Trepp geography. This requirement establishes that the liquidating chain occupied the treated property before the event. CMBS properties are larger and more standardized than the universe of retail properties. We use reweighting to translate the observed Trepp estimates to broader retail-place populations.

3.4 E-commerce exposure

For property i, we combine its complete matched tenant mix in 2006 with national category-level online-sales shares. Let j index establishments at the property, let m_{ij} be the Infogroup employment-size weight, and let $E_{c(j),2019}$ be the 2019 online-sales share for the establishment's retail category. Restaurants, services, professional uses, and other establishments without a retail e-commerce rate receive a rate of zero. Exposure is

$$Exposure_i = \frac{\sum_{j \in J_i} m_{ij} E_{c(j),2019}}{\sum_{j \in J_i} m_{ij}}.$$

Including non-retail establishments in the denominator is intentional. The measure captures the share of all baseline activity at the property directly exposed to online goods sales, rather than average exposure conditional on operating in retail. A property with more bookstores, electronics stores, apparel stores, furniture stores, or sporting-goods stores is therefore more exposed than one with more grocery, restaurant, personal-service, or office tenants.

The two components play different roles. The 2006 tenant mix fixes where vulnerable activity was located before the main property-outcome divergence. The 2019 category shares summarize the cumulative cross-category rise of e-commerce before the pandemic surge. Later tenant changes cannot mechanically alter exposure. Appendix Figure C1 recomputes exposure using tenant mixes fixed in 2009, 2011, and 2013. Appendix Table C1 instead holds tenant mix fixed in 2006 and substitutes each comparable national e-commerce-share year available after 2019. The latter exercise tests whether the results depend on using the 2019 category snapshot rather than a later one.

3.5 Complete chain liquidations

We identify complete liquidations through a documentary search for U.S. goods-retail chains whose entire physical-store network in a defined banner or corporate unit closed. We harmonize chain and banner names, identify the operating-chain family in Infogroup, and retrieve all of its national pre-event retail locations. An event must lose at least 90 percent of that national footprint by two years after liquidation.

We keep events beginning in 2005 or later whose modal retail category had a 2019 online-sales share of at least 20 percent and rose at least 10 percentage points from 2006—the same category measures used in the exposure design. The rule selects tenant shocks in categories reshaped by online substitution without asserting that e-commerce alone felled any given chain.

Every qualifying pre-event store is assigned before property outcomes are used. We consolidate matched Trepp records to physical properties, keep the first verified liquidation at each property, exclude a property with an earlier verified liquidation, and censor it at any later verified liquidation. The resulting primary sample contains 2,302 unique properties and all 57 qualifying liquidation events. Appendix A gives the source hierarchy, scope rules, identifier checks, matching procedure,

and full sample funnel.

3.6 Historical mall lifecycles

We assemble a source-bounded historical mall universe from Wikipedia and Wikidata, public dead-mall and redevelopment directories, and targeted searches by state and closure year. These sources generate candidates rather than determine inclusion. For every candidate, we verify the mall's name, location, historical address, operation as an ordinary multi-tenant mall in 2006, and the date at which ordinary mall operation ended. The event year is not a demolition date, redevelopment date, or OpenStreetMap edit.

Closed and redeveloped malls often lack a usable current OpenStreetMap polygon. We therefore recover each site from a reviewed historical address family, fix that family using its 2006 Infogroup roster, and follow establishments at the fixed site from 2000 through 2023. Historical footprints, archived OpenStreetMap objects, site plans, parcels, and aerial imagery validate or augment the address match where available.

The review contains 170 documented or retained cases, accepts 168 address rosters, and identifies 108 documentary-core malls with high-quality mappings for the primary analysis. Of these, 105 have at least ten establishments five years before death and 101 also have two current proper-mall comparisons within the production common-support rules. We match on Census region, baseline establishment count, employment, retail share, and 2006 e-commerce exposure. Because mall death is itself the outcome, the result is a descriptive matched lifecycle comparison rather than a causal event study.

3.7 Summary statistics

Table 1 reports summary statistics after sample construction. The broad exposure sample contains 33,701 Trepp retail properties with nonmissing 2006 tenant-mix exposure. The complete-liquidation sample contains 2,302 treated properties across 57 events. Baseline occupancy is high, and the treated properties are large on average because national retail chains often occupy shopping centers, anchor spaces, and other large-format locations.

4 E-Commerce and the Uneven Recovery of Retail Properties

4.1 Empirical design

We estimate how property outcomes changed with baseline exposure to the national rise of e-commerce. A property is more exposed when its 2006 tenant mix places more weight on goods categories in which online purchasing later became large. The design therefore compares properties that entered the e-commerce era with different tenant mixes, while holding the property-level exposure measure fixed throughout the outcome period.

For property i in year t, we estimate

$$Y_{it} = \alpha_i + \gamma_t + \sum_{s \neq 2013} \beta_s Exposure_i \times \mathbf{1}\{t = s\} + \varepsilon_{it}. \tag{1}$$

The specification includes property fixed effects and year fixed effects. Reported coefficients are scaled to a 10 percentage point increase in exposure. Standard errors are clustered by property. The main figure uses the preferred exposure measure, which combines 2006 property tenant mix with 2019 national category-level online-sales shares.

We omit 2013 as the reference year because property outcomes sit at roughly their pre-divergence peak there; it normalizes the paths and is not a claimed onset date, since online shopping rises throughout the sample. The solid vertical line in Figure 3 marks 2006, when tenant mix is fixed; the dashed line marks the 2013 reference year.

The identifying assumption is that, absent the differential shift toward online shopping across retail categories, properties with different baseline exposure would have followed similar outcome paths after accounting for the included fixed effects and controls. We assess this assumption with the displayed pre-period paths, specifications that compare properties within the same county-year, predetermined local controls, alternative tenant-mix baseline years, and alternative national e-commerce-share years. Appendix Figure C1 varies the tenant-mix year; Appendix Table C1 varies the national rate year.

4.2 Effects on occupancy, income, and business activity

Figure 3 reports the preferred event-study estimates. Averaged over 2014–2019, a 10 percentage point increase in baseline exposure is associated with 0.60 percentage points lower occupancy and 0.020 log points lower NOI per square foot. During 2020–2021, the corresponding gaps are 1.38 percentage points for occupancy and 0.031 log points for NOI. The total-establishment gap is 0.70 establishments over 2014–2019, 1.80 over 2020–2021, and 2.12 over 2022–2023. The retail-establishment gaps are 0.34, 0.83, and 0.89 over the same windows.

This timing fits a gradual technological shock, not a dated event: e-commerce is already growing before 2013, and the differential property response widens after it. Appendix Table C4 reports pre- and post-period averages and estimates that net out the linear pre-period slope; the signs are unchanged, and for occupancy the adjustment makes the estimate more negative, because the pre-period coefficients were drifting toward zero.

The within-county-year estimates are close to the benchmark. Over 2014–2019, a 10 percentage point increase in exposure is associated with 0.56 percentage points lower occupancy and 0.019 log points lower NOI when properties are compared within the same county-year. The similarity matters because the county-year specification absorbs changes in local demand, employment, construction, and other county-level conditions shared by exposed and less-exposed properties.

Appendix B audits the establishment-to-property link directly. Recomputing exposure from the final canonical roster and replacing the 24 deterministic address conflicts changes the preferred estimates only slightly. On a common property sample, an address-only roster and a strict construction that adds only directly contained Green Street establishments produce similar occupancy and NOI effects. Fractionally weighting establishments assigned to multiple properties also leaves the operating estimates essentially unchanged.

4.3 Where did expanding uses locate?

Figure 2 showed the sector diversifying and recovering fastest at smaller, already-diversified properties. Figure 4 asks whether the expanding uses located at the *most-exposed* properties—the ones that most needed them.

They did not, at least not in the numbers required to refill those buildings. Relative to

less-exposed properties, a 10 percentage point increase in exposure predicts 1.82 fewer retail businesses and 3.03 fewer total businesses per 100 baseline businesses over 2014–2019. Services and entertainment rise by 0.49, but non-retail businesses as a whole fall by 1.20. Over 2020–2023 the retail and total gaps widen to 5.68 and 6.52 while services and entertainment add 1.18 and the broader non-retail difference is near zero. Some growing uses do enter more-exposed properties; they do not come close to offsetting the loss.

This is the retail counterpart of the flight to quality documented in office markets after remote work (Gupta et al., 2026). The sector reallocated toward new in-person uses, but that activity chose smaller and already-diversified buildings over the ones most damaged by declining goods retail. Aggregate reallocation and in-place recovery are therefore different things—which is why the sector can look healthy while the most-exposed capital stays impaired.

4.4 Local conditions and property format

The main identification concern is that high-exposure properties may be located in places that were already on different economic paths. Table 2 addresses this concern with county-year fixed effects and predetermined local controls. The controls include baseline local business activity, income, retail intensity, and local outcome trends measured before the post-2013 divergence. We do not control for contemporaneous local income or business activity because those variables may themselves respond to e-commerce.

The county-year and predetermined-control estimates remain close to the benchmark. They show that the main exposure pattern is not explained by broad county-level shocks or by observed local conditions and trends measured before the decline. Appendix C reports the full set of ZIP and county controls, local balance and pre-period checks, and the related establishment-count estimates.

Property format is economically related to exposure. Malls, anchor-oriented centers, big-box properties, and retail-heavy centers were built around the goods categories most vulnerable to online substitution. An inline store can often be re-leased with limited work, while an empty anchor may require subdivision, new access, and substantial tenant improvements (Sills Cummis & Gross, 2025; CBRE, 2026).

Appendix Table C7 asks how much of the exposure effect remains after allowing baseline property-format groups to have different time paths. The coefficients attenuate. The attenuation is a decomposition, not a threat: part of e-commerce's total effect runs through the formats built around exposed goods retail—the property-format margin is asset specificity operating at the level of building type, which Section 5 measures directly. Appendix Figure C2 and Appendix Table C6 report size-weighted and Green Street estimates. The category contributions show that electronics, sporting and hobby stores, furniture and home goods, apparel, and books account for most of the variation in exposure rather than a single narrow industry (Goldsmith-Pinkham et al., 2020).

4.5 Aggregate income effects

The property-level coefficients measure effects for Trepp properties with observed NOI. To translate them to broader retail-property populations, we estimate how the NOI response varies with baseline property characteristics and reweight the observed Trepp properties to target populations defined in the mapped retail-place data. Appendix C.5 gives the complete specification, cell construction, and formulas.

Table 3 reports the resulting fitted percentage changes. For Trepp properties with observed NOI, the interacted fitted annual NOI decline is 4.1 percent in 2019. Reweighting the same source sample to all mapped OpenStreetMap retail places gives a 3.3 percent decline. In the non-interacted benchmark, the observed-NOI Trepp sample implies approximately $0.56 billion in annual NOI losses in 2019. Reweighting to all mapped OSM retail places gives a 2.9 percent decline, or approximately $1.27 billion in annual NOI.

The largest fitted losses are concentrated in large retail formats. In 2019, mapped mall and shopping-center places have a fitted NOI decline of 5.8 percent, OSM places with at least 100 establishments have a 5.4 percent decline, and Green Street large retail centers have a 4.0 percent decline. The corresponding 2020–2021 fitted changes are 0.7 percent for mapped mall and shopping-center places, −4.3 percent for OSM places with at least 100 establishments, and −3.2 percent for Green Street large retail centers, while the estimate for OSM places with one to five establishments is positive.

A useful benchmark is the disruption to office markets from remote and hybrid work. Realized

constant-quality office-rent declines range from 3.9 percent in Dallas–Fort Worth and 5.7 percent in Boston to 12.7 percent in Manhattan and 17.1 percent in San Francisco (Gupta et al., 2026; Abramson et al., 2025; CompStak, 2026). The retail estimates for all mapped places are comparable to the smaller office-market declines, while the large-format retail estimates lie within the broader range across major office markets.

4.6 Exposure and realized tenant loss

Table 4 connects the broad exposure measure to realized tenant departure. A 10 percentage point increase in e-commerce exposure raises the probability of a tracked chain exit by 3.2 percentage points, the probability of a large or chain-wide exit by 3.1 percentage points, and the probability of an exposed-category chain exit by 3.0 percentage points. The same exposure difference predicts more anchor or big-box exits and larger retail-establishment losses.

Exposure thus predicts both halves of the initial effect: more-exposed properties were likelier to lose major tenants, and they went on to lower occupancy, income, and activity. What exposure cannot show is the path *after* a tenant leaves. For that we turn to complete chain liquidations.

5 Adjustment after Complete Chain Liquidations

Exposure tells us which properties the shock hit hardest. It cannot tell us what happens inside a building once its tenant is gone—how long the space sits empty, who takes it, and whether income returns with occupancy. Complete chain liquidations let us watch that adjustment from a sharp, cleanly timed loss.

5.1 Sample and design

The main sample contains 57 complete physical-store liquidations in retail categories with large growth in online sales. Every event covers the full store network in its defined banner or corporate scope and passes the documentary, operating-chain identifier, and national-disappearance rules described in Appendix A. The restriction to high-e-commerce categories connects the events to the paper's setting, but the design does not require e-commerce to be the sole cause of a chain's liquidation.

Treatment membership is fixed before property outcomes are used. We assign every pre-event Infogroup store belonging to a qualifying operating-chain identifier, link those stores to the paper's fixed Trepp geography, consolidate Trepp records to physical properties, and keep the first verified liquidation shock at each property. We exclude properties with an earlier verified liquidation and same-year compound shocks from the primary sample, and censor a property when a later verified liquidation occurs. These rules produce 2,302 unique treated properties. The control universe excludes every property ever linked to a verified liquidation.

Let e_i be the liquidation year for treated property i, and let $\mathcal{C}_{ih}$ be the other retail properties in its county observed in both year $e_i - 2$ and year $e_i + h$. For outcome Y, we construct

$$\widetilde{\Delta}_{ih} Y = (Y_{i,e_i+h} - Y_{i,e_i-2}) - \frac{1}{|\mathcal{C}_{ih}|} \sum_{j \in \mathcal{C}_{ih}} (Y_{j,e_i+h} - Y_{j,e_i-2}) .$$

The event-study coefficient at horizon h is the property-weighted mean of $\widetilde{\Delta}_{ih} Y$, with year -2 normalized to zero. Requiring both the treated property and each comparison property to be observed at the baseline and target horizon makes the comparison pairwise balanced. Standard errors are clustered by physical property and liquidation event.

The identifying assumption is that, absent liquidation, treated properties would have moved like other observed retail properties in the same county. Complete liquidations strengthen the design because the chain cannot select only its weakest local stores: once an event qualifies, all identifiable pre-event stores enter treatment—so the design recovers the average property response to a full-network loss without asserting that the bankruptcy itself is random.

5.2 Physical and economic recovery

Figure 5 reports the event-study paths for occupancy and log NOI per square foot. Both outcomes are relatively flat through the pre-period and fall sharply with liquidation. The joint tests for years -4 and -3 have p-values of 0.144 for occupancy and 0.901 for log NOI.

One year after liquidation, occupancy is 6.18 percentage points lower, with a standard error of 1.34, and log NOI per square foot is 0.133 log points lower, with a standard error of 0.053, than at same-county comparison properties. At year three, the occupancy gap has narrowed to 2.52 percentage points, with a standard error of 0.85, while the log-NOI gap remains 0.067 log points,

with a standard error of 0.032. The average property therefore recovers some physical occupancy but remains economically impaired.

Appendix Figure D2 tracks properties that take a material early loss—occupancy down at least 10 points or NOI down at least 0.10 log point between years −1 and +2. Within three years of the trough, 47.6 percent recover at least 90 percent of the occupancy loss, but only 39.4 percent recover as much of the income loss. Occupancy keeps climbing; income rebounds for some in the first two years and then stalls.

This wedge between refilling and recovering is capital scarring, measured directly. A property returns to occupancy without returning to its former cash flow. The data cannot say how much of the gap is lower rent, concessions, tenant-improvement spending, subdivision cost, higher operating expense, or lost spillovers to neighboring tenants—but they establish that physical reuse and economic recovery are not the same event, which is the fact the frictionless benchmark denies.

5.3 Tenant replacement and change of use

Figure 6 asks whether other businesses replace the liquidated chain. We follow Infogroup establishment identifiers within each property, assign each business a fixed use category from its reported history, and scale outcomes by the number of businesses observed at the property two years before liquidation. The liquidated chain is excluded in every year. Each coefficient is the change from year −2, net of the change at other observed retail properties in the same county.

Other retailers do not replace the chain in the first three years, and non-retail uses do not fill the gap. The non-retail business stock is lower by 4.20 businesses per 100 baseline businesses at year +1, 6.45 at year +2, and 7.24 at year +3. Annual arrivals of new non-retail businesses fall by 1.67, 1.55, and 1.47 per 100 baseline businesses over the same horizons. The total number of businesses is 7.45 lower by year +3, although that estimate is less precise.

Appendix Figure D1 follows the former chain's exact postal unit when Infogroup reports an explicit suite, unit, floor, or building marker. By year +3, reuse is much more often another retailer than a non-retail business, and many former units have no matched business. The exact-unit result is not a direct vacancy measure, but it confirms that literal conversion of the old store to a non-retail use is uncommon during the first several years.

The picture is consistent across the two figures and the exact-unit follow-up (Appendix Figure D1): after a large tenant leaves, the space refills slowly if at all, and when it does the taker is more often another retailer than a new use. Diversification is happening somewhere in the sector, but it does not arrive automatically at the building that just lost its anchor.

5.4 Three channels: what makes a scar deep

The average path hides wide variation, and the theory in Section 2 says where to look for it. A scar should be deepest where the building sits furthest from frictionless redeployment: where it is most specialized, where the local pool of replacement users is thinnest, and where the owner is least able to finance conversion. Table 5 tests these three channels, one pre-liquidation measure each.

Specificity is a standardized index rising in rentable area per establishment and falling in tenant-category diversity—a big, single-purpose box scores high. A thin replacement market is a ZIP code at or below the sample-median count of non-retail establishments. Financial position is HPI-updated current LTV: scheduled balance over the securitization appraisal, rolled forward with county house-price growth. All three are fixed two years before liquidation. Each row is a separate event study that compares properties hit in the same year, absorbs the baseline outcome and size interacted with event time, and (for LTV) conditions on original LTV, baseline DSCR, loan age and term, and contract type. The coefficients are differential responses among liquidated properties, not new estimates of the average effect.

More-specific properties have larger immediate declines. A one-standard-deviation increase in specificity is associated with an additional 1.07 percentage point decline in occupancy and an additional 0.063 log point decline in NOI between years -2 and $+1$. Both estimates are marginally significant, and the specificity–NOI path also differs before liquidation, so we treat the NOI pattern as suggestive rather than as cleanly timed evidence.

Thin non-retail replacement markets have a 0.130 log point more negative NOI change by year $+1$, with no corresponding occupancy difference. The NOI gap is already visible at year -1, so this result is consistent with limited replacement options but does not establish that the difference begins at liquidation. Higher HPI-updated current LTV predicts larger occupancy declines: a 10

percentage point increase is associated with an additional 0.94 percentage point decline by year +1 and an additional 1.07 percentage point decline by year +2. The NOI estimates are small and imprecise.

Read together, the three channels point the same way. Adjustment is slowest and the scar deepest where space is tightly built around its old use, where few plausible new users operate nearby, and where leverage is high—exactly the properties the frictionless benchmark would treat as no harder to redeploy than any other. The same logic explains the aggregate flight to quality: the smaller, diversified properties that recovered fastest are low on all three frictions at once.

5.5 Validation and interpretation

The result is not driven by loose store-to-property matches. Restricting every component match to 250 meters produces year-one estimates of −7.06 occupancy points and −0.146 log NOI, and year-three estimates of −2.50 points and −0.087 log NOI. Excluding COVID-period liquidation events gives year-one estimates of −6.25 points and −0.138 log NOI. Aggregating same-year shocks, retaining observations after later shocks, and changing the control-exclusion rule also leave the conclusion intact. In leave-one-event-out estimates, the year-one occupancy effect ranges from −6.81 to −5.19 points and the year-one log-NOI effect ranges from −0.166 to −0.103.

Having seen the average path and the frictions that steepen it, we turn to the outcome when the frictions win outright and the retail cluster fails.

6 Mall Death and Failed Reallocation

Mall death is the frictionless benchmark's starkest failure: a property so far from redeployable that it stops operating altogether. It arrives only after years of departures, failed searches, and fraying tenant complementarities, and because death is an endogenous endpoint we treat this section as descriptive. It completes the sequence—from broad exposure, to the average post-loss path, to the case where reallocation never happens.

6.1 A common e-commerce headwind

Malls were unusually exposed to the shift toward online shopping, but exposure alone does not rank which malls died. Appendix Figure A1 shows that mean 2006 e-commerce exposure is 12.44 percent among current mall and shopping-center polygons and 5.70 percent among other mapped retail places under a common OpenStreetMap rule. Under the common dominant-address rule used for the historical comparison, mean exposure is 13.51 percent among 108 future-dead malls and 14.79 percent among 857 current proper malls.

The levels should be compared within each panel because the two panels use different site definitions. Within each comparison, the interpretation is clear. Malls carried much more exposure than retail places generally, but the malls that later died were not more exposed than other proper malls. E-commerce was a common format-level headwind rather than a sufficient statistic for mortality within the mall sector.

The same distinction appears when the mall sample is linked to the complete-liquidation sample. In 2006, tenants belonging to chains that later enter the liquidation sample account for 3.37 percent of establishments at future-dead malls and 3.79 percent at matched malls. Exact liquidation hits between event time -5 and mall death equal 0.75 and 1.35 percent of baseline establishments, respectively. The documented national liquidations therefore do not mechanically explain which malls die.

6.2 Tenant departures and failed replacement

Figure 7 follows 101 future-dead malls and their 202 matched current-mall assignments. Panel A shows the establishment stock, normalized to 100 five years before death. Activity at future-dead malls falls steadily to 61.5 at year -1 and 43.1 in the final year of ordinary operation, while the matched path is 100.5 and 100.1. By year $+2$, activity is 18.4 percent of baseline at future-dead malls and 94.0 percent among comparisons. Retail establishments fall most sharply, but non-retail establishments also contract. Mall death does not look like a rapid relabeling of the site into restaurants, services, offices, or other non-retail uses.

Panels B and C separate annual departures from entry. Four years before mall death, exits equal 22.5 percent of the prior-year tenant stock at future-dead malls and 15.9 percent at comparisons,

while entry equals 11.8 and 19.5 percent. In the final year of ordinary operation, exits are 39.7 versus 16.8 percent and entries are 9.7 versus 16.4 percent. Departures accelerate as death approaches, while entry remains persistently weak throughout the pre-death window.

The cumulative imbalance is large. In the final year, 74.8 percent of future-dead malls' baseline tenants are absent, compared with 46.7 percent among comparisons, while the stock of entrants equals only 17.9 versus 46.8 percent of baseline. The defining feature of mall death is therefore not a single closure. It is a sustained failure to replace departing tenants.

The decline is concentrated at the focal property. At year +2, the establishment index in a fixed one-to-five-kilometer ring around future-dead malls is 105.4, compared with 105.3 around matched malls, even as the malls themselves collapse. Broader ZIP-level evidence is mixed, so we do not conclude that every dead mall was located in a healthy market. The narrower result is that mall death is not uniformly a manifestation of general business decline immediately surrounding the site.

Exposure set the headwind every mall faced; it did not decide which ones failed. What separated the dead from the survivors was the slow arithmetic of exit outrunning entry—the same slow reallocation seen at healthier properties, run to its endpoint.

7 Discussion and Conclusion

Both the disruption and the recovery are real. E-commerce cut demand for stores selling easily shipped goods and inflicted large losses on the properties built around them; over the following years the sector adapted, adding health care, services, food, entertainment, and other uses that still require a physical location, and establishment activity strengthened after COVID.

But the recovery was a flight to quality, not a repair of the damaged stock. Growth concentrated at smaller and already-diversified properties. The most-exposed buildings stayed behind on occupancy, income, and business counts, because the expanding uses mostly chose other locations. A liquidation makes the mechanism visible one building at a time: occupancy eventually returns, but income does not return with it, and non-retail users do not rush in to fill the gap. The reused property carries a scar. How deep the scar runs is governed by three frictions—how specialized the building is, how thick the local market for new users is, and how heavily the property is

levered—and mall death is what happens when all three bind at once and reallocation simply fails.

The lesson reaches past retail. Much of the capital stock is durable and fixed in place, and when technological change moves demand away from its use, the frictionless promise that the asset will be redeployed at full value is not what the data show. Activity can reallocate across the sector while the original capital recovers slowly, partially, or never, and while the assets that recover are not the ones that were hurt. Seeing this requires a long horizon and a view of the individual asset: the disruption, the apparent sectoral rebound, and the persistent scar on the capital most directly displaced are all part of one adjustment, and only the last of them is invisible in aggregate statistics.

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Figures

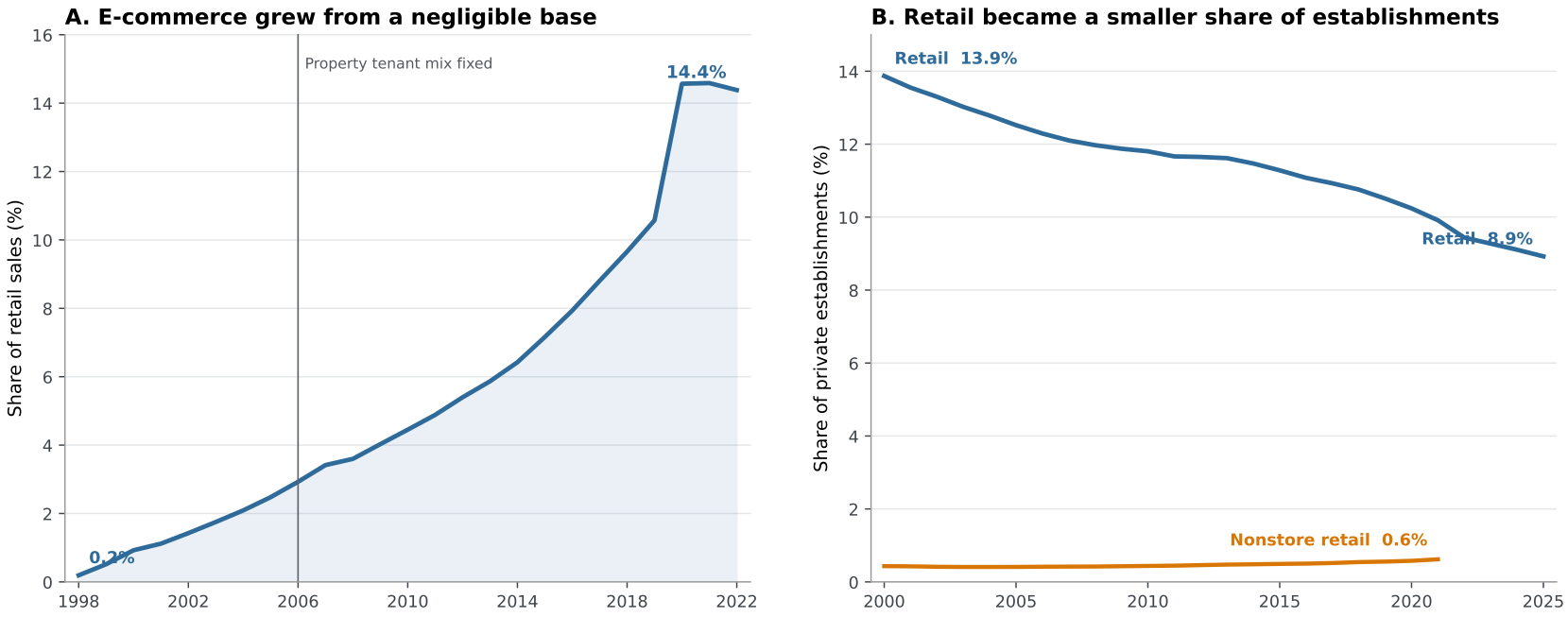


Figure 1: E-Commerce Growth and the Contraction of Retail Activity

Notes: Panel A plots the share of U.S. retail sales occurring online using Census retail e-commerce statistics. The vertical line marks 2006, when the tenant mix used in the preferred property-exposure measure is fixed. Panel B plots retail establishments as a share of all private establishments using the Quarterly Census of Employment and Wages. The nonstore comparison is shown only through 2021 because the 2022 NAICS revision eliminated nonstore retail as a separate subsector and reclassified establishments by product. Endpoint values are labeled. Appendix Figure C4 reports the corresponding employment shares.

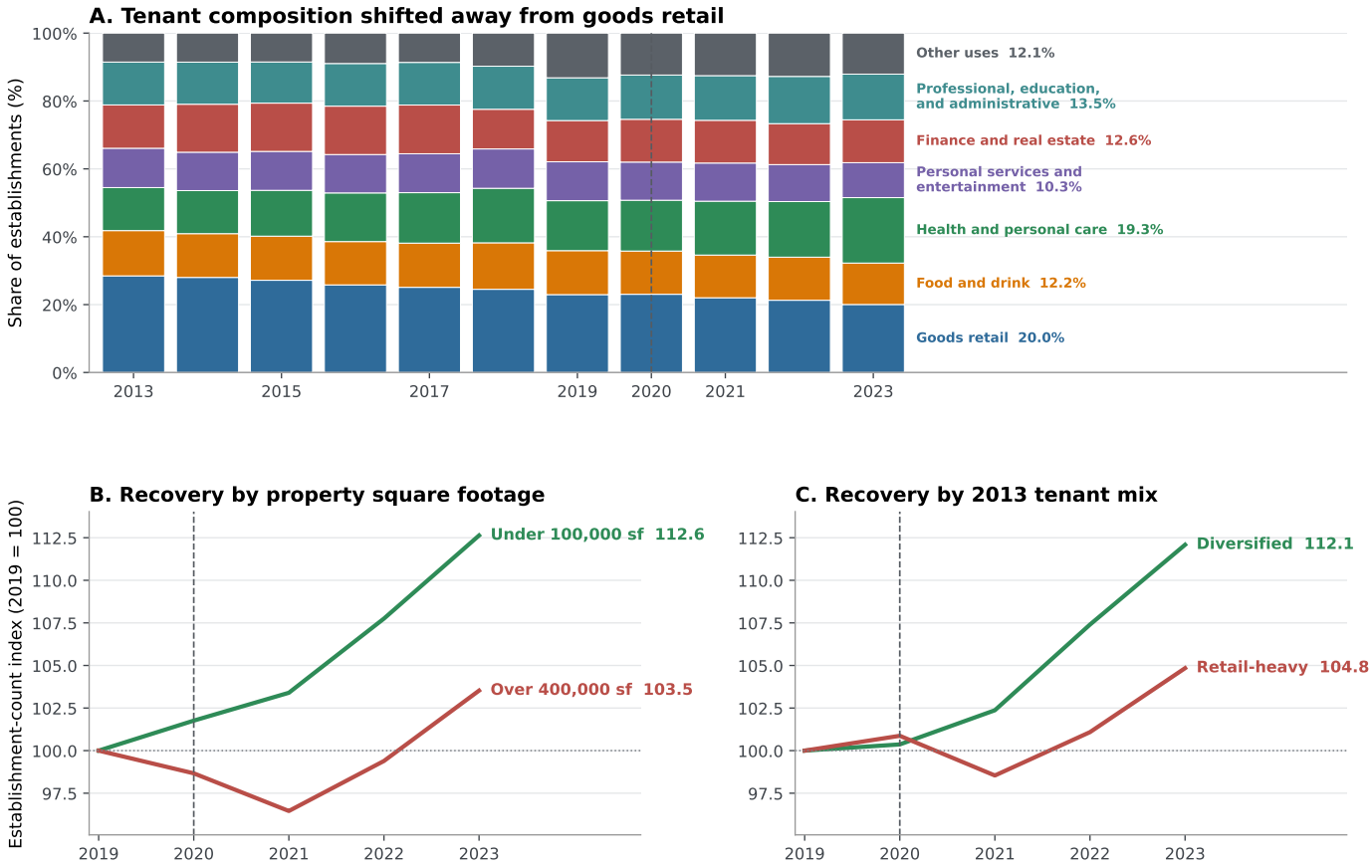


Figure 2: Retail Properties Diversified, but Recovery Was Uneven

Notes: Panel A reports establishment shares by tenant use at a fixed 2013 cohort of Trepp-linked retail properties. Each establishment receives one count, so the shares are not shares of occupied square footage. Panels B and C report establishment-count indices by fixed baseline rentable building area and tenant mix, respectively, with each series normalized to 100 in 2019. Size and tenant-mix groups are fixed using 2013 property characteristics, before the post-COVID period. The vertical line marks 2020. The panels are descriptive aggregate paths and do not condition on future property success.

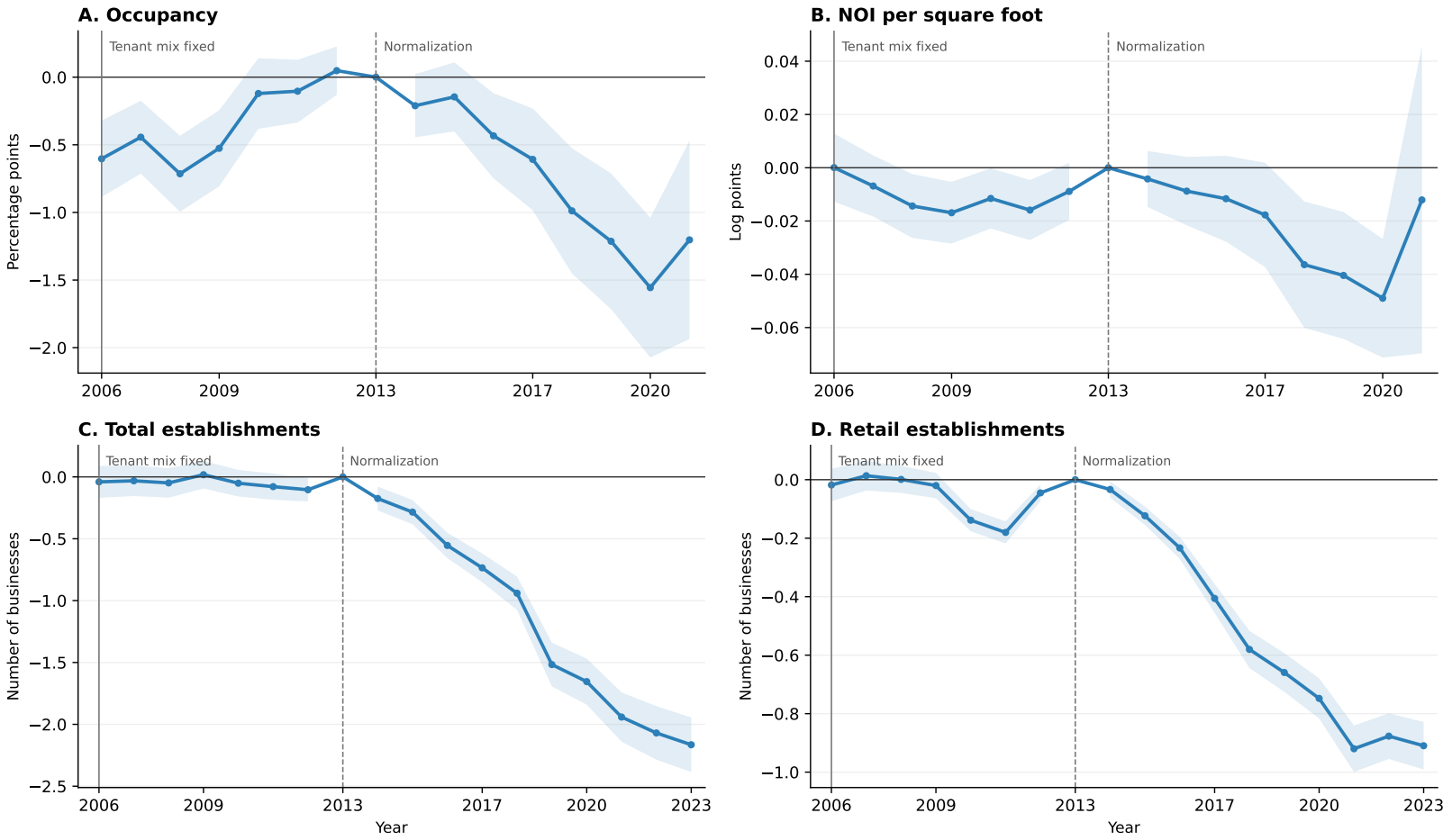


Figure 3: E-Commerce Exposure and Retail Property Outcomes

Notes: The figure reports event-study coefficients for a 10 percentage point increase in the preferred e-commerce exposure measure, which combines property tenant mix fixed in 2006 with national category-level online-sales shares from 2019. All panels include property and year fixed effects. The solid vertical line marks 2006, when tenant mix is fixed. The dashed vertical line marks 2013, the omitted outcome year. The choice of 2013 normalizes the paths at the approximate pre-divergence peak and does not treat 2013 as the start of e-commerce. Occupancy is measured in percentage points, NOI in log points, and establishment outcomes in counts. Shaded areas report 95 percent confidence intervals based on standard errors clustered by property.

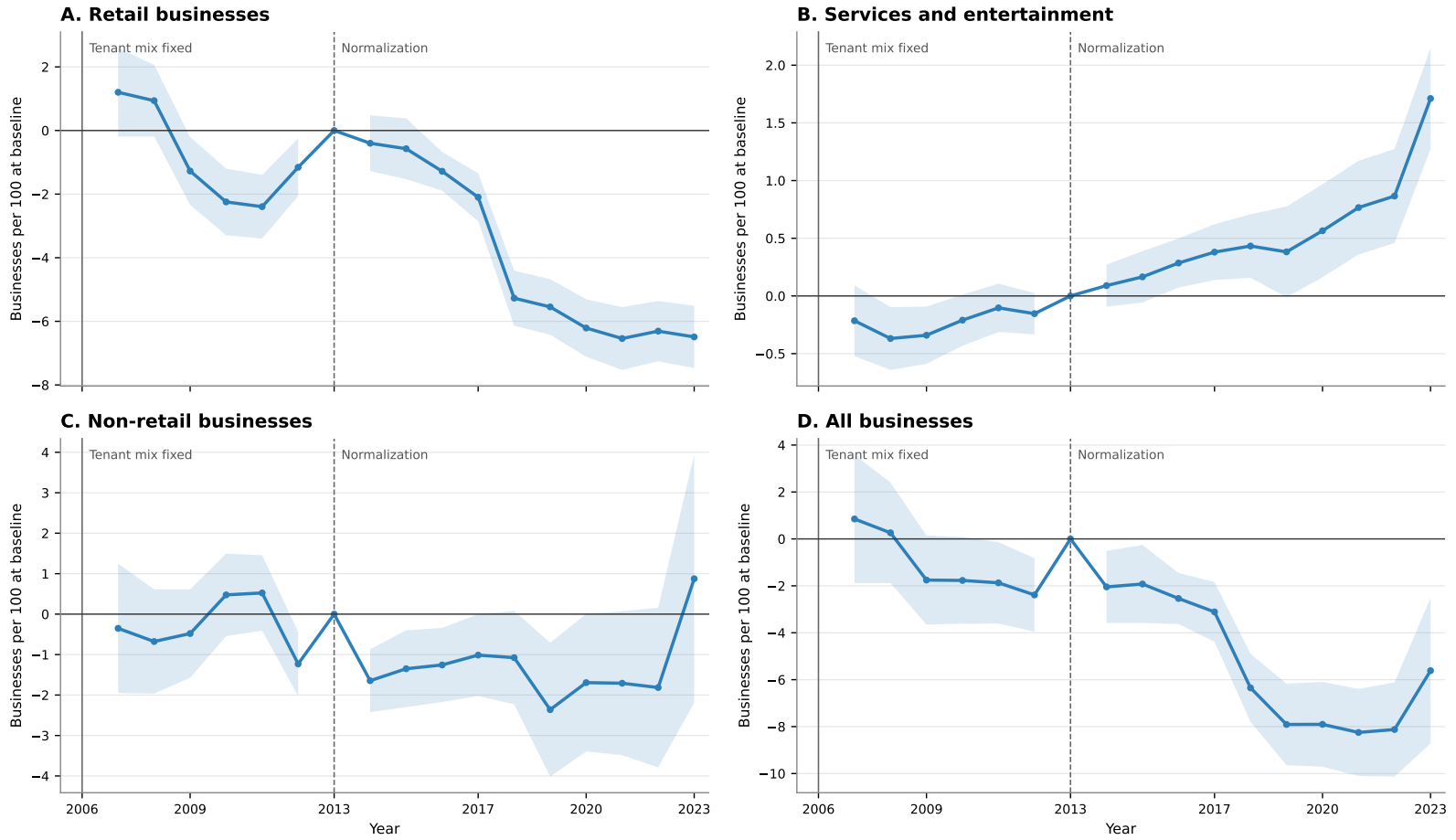


Figure 4: E-Commerce Exposure and the Reallocation of Property Use

Notes: The figure reports event-study coefficients for a 10 percentage point increase in the preferred 2006 e-commerce exposure measure. Each Infogroup establishment receives a fixed use classification based on the category reported most often over its observed history, with the latest category breaking ties. Outcomes are scaled per 100 businesses observed at the property in 2006. All specifications include property and year fixed effects. The solid vertical line marks 2006, when tenant mix is fixed. The dashed vertical line marks 2013, the omitted outcome year. Shaded areas report 95 percent confidence intervals based on standard errors clustered by property.

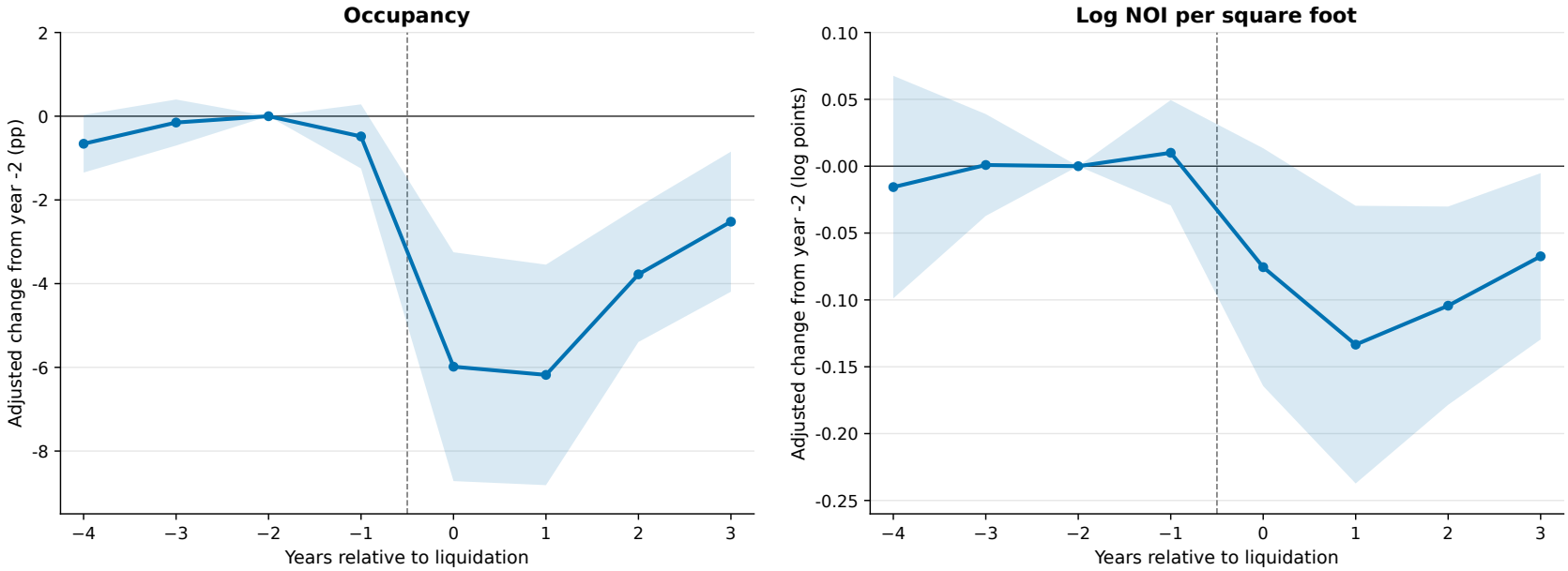


Figure 5: Property Adjustment after Complete Chain Liquidations

Notes: The figure reports property-weighted event-study effects in the primary complete-liquidation sample. Outcomes are changes from event year −2 net of the mean change among other retail properties in the same county that are observed in both the baseline and target years. The control universe excludes properties ever linked to a verified liquidation. A treated property contributes only its first qualifying liquidation and is censored at a later verified liquidation. Shaded areas report 95 percent confidence intervals based on standard errors clustered by physical property and liquidation event. The dashed line separates the last pre-event year from the documented liquidation year.

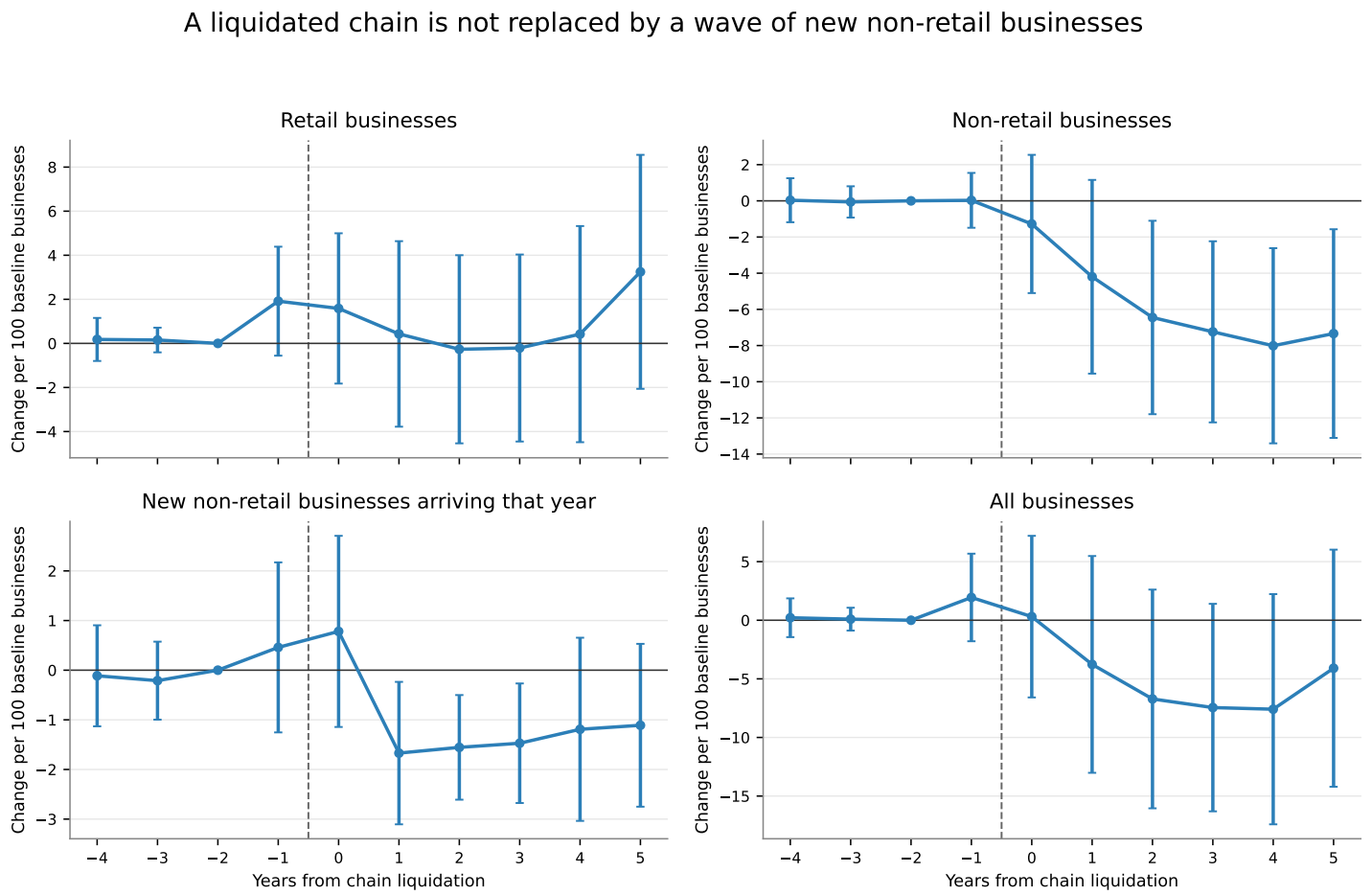
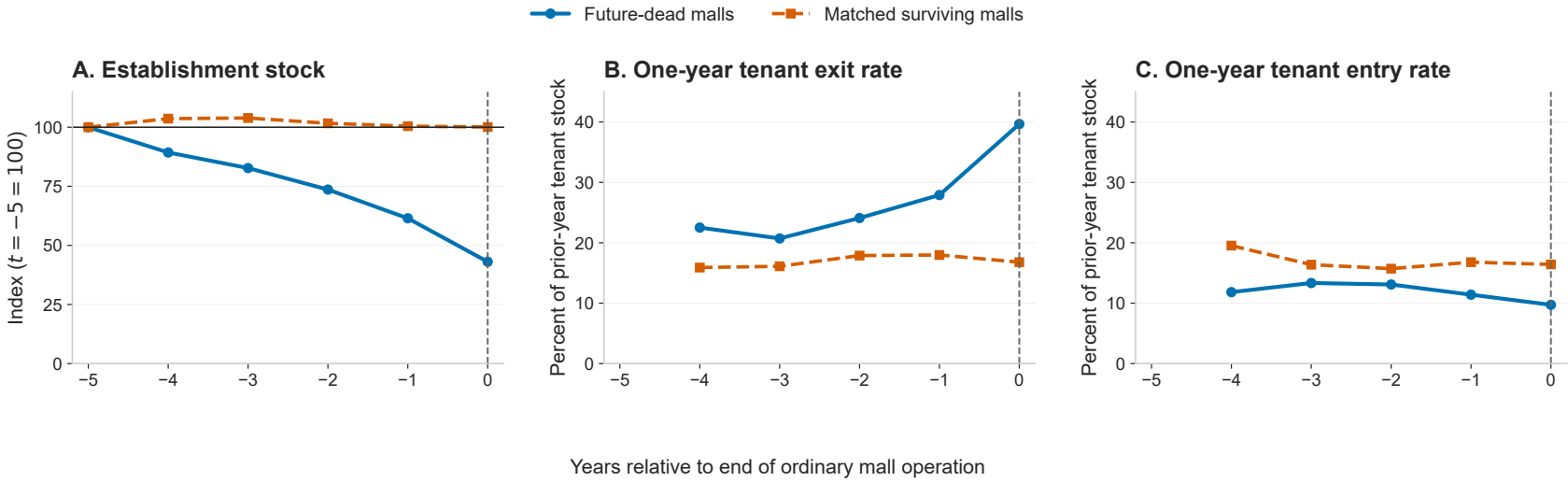


Figure 6: Tenant Replacement and Change of Use after Complete Chain Liquidations

Notes: The figure follows Infogroup establishment identifiers within treated properties. Each establishment receives a fixed retail or non-retail classification based on its reported industry history. Outcomes are scaled per 100 businesses observed at the property in event year -2. The liquidated chain is excluded in every event year. Coefficients are changes from year -2, net of changes at other observed retail properties in the same county; the comparison pool excludes all properties linked to a verified liquidation. Bars report 95 percent confidence intervals based on standard errors clustered by physical property and liquidation event.



Notes: 101 future-dead malls and 202 matched surviving-mall assignments. Panel A indexes establishment stock to 100 at $t = -5$; Panels B-C show one-year ABI transition rates relative to prior-year stock. $t = 0$ is the documented end of ordinary mall operation. These are descriptive lifecycle comparisons, not causal event studies.

Figure 7: Mall Death, Tenant Departure, and Failed Replacement

Tables

Table 1: Summary Statistics for Key Estimation Samples

Variable	N	Mean	SD	P25	Median	P75
Panel A. Exposure sample: one row per property, measured in 2013						
E-commerce exposure, pp	33,701	8.35	9.48	3.13	5.00	11.30
Rentable area, 000 square feet	26,743	82.12	139.48	14.84	42.47	93.52
Occupancy rate, 2013	13,113	93.64	11.50	91.12	100.00	100.00
Log NOI per square foot, 2013	11,842	2.464	0.836	1.998	2.482	3.006
Total establishments, 2013	32,286	21.41	55.99	2.00	6.00	19.00
Retail establishments, 2013	32,286	7.09	19.43	1.00	2.00	5.00
Retail share, 2013	32,286	0.36	0.31	0.11	0.30	0.50
Panel B. Verified-liquidation sample: one row per treated physical property						
Matched liquidating stores	2,302	1.03	0.18	1.00	1.00	1.00
Closing-store jobs	2,302	22.83	29.81	7.00	12.00	30.00
Rentable area, 000 square feet	2,302	209.97	252.04	59.89	128.85	255.90
Baseline occupancy	939	94.71	8.07	93.00	98.00	100.00
Baseline log NOI per square foot	835	2.390	0.705	2.043	2.449	2.781
All component matches within 250 meters	2,302	0.79	0.40	1.00	1.00	1.00
Panel C. E-commerce exposure by property category and target population						
Observed Trepp NOI sample	11,868	8.28	8.98	3.17	5.20	11.40
All Trepp retail properties	33,701	8.35	9.48	3.13	5.00	11.30
All OSM retail places	35,673	5.87	5.86	2.68	4.29	7.38
Top-30 metro OSM places	17,750	5.74	5.70	2.65	4.22	7.15
Major metro, within 10 miles	4,618	5.87	5.69	2.73	4.27	7.40
Major metro, 10-25 miles	8,871	5.81	5.76	2.68	4.28	7.22
Major metro, over 25 miles	4,261	5.45	5.58	2.49	4.03	6.65
Other metro OSM places	14,981	6.13	6.11	2.80	4.43	7.80
Outside CBSA OSM places	817	4.63	4.64	2.13	3.77	5.88
OSM mall/shopping-center places	943	12.44	7.50	7.34	12.20	16.90
OSM places with 100+ establishments	1,448	8.10	5.95	3.33	5.69	13.21
OSM places with 1-5 establishments	10,252	5.27	7.59	0.67	3.48	5.38
Green Street large retail centers	3,408	5.61	3.40	2.91	4.76	8.38
Panel D. Primary historical-mall sample: one row per future-dead mall						
E-commerce exposure, pp	108	13.92	3.72	11.29	13.99	15.83
Establishments, 2006	108	80.28	40.94	46.00	72.00	111.00
Establishment employment proxy, 2006	108	935.92	556.33	493.00	849.00	1,208.00
End of ordinary mall operation, year	108	2015.34	4.25	2012	2016	2019

Notes: Panel A contains one 2013 characteristics row per Trepp property with nonmissing 2006 tenant-mix exposure. Panel B is the primary first-liquidation risk set and contains 2,381 matched stores across 2,302 physical properties and 57 events. Baseline outcomes are measured two years before liquidation. Closing-store jobs use the pre-event Infogroup employment-size value and are a size proxy rather than payroll employment. Panel C reports the 2006 e-commerce exposure distribution, in percentage points, for property populations used in the reweighting exercises. Panel D contains the 108 documentary-core future-dead malls with accepted 2006 address mappings; 105 have sufficient event-time support and 101 also have two common-support comparison assignments. Infogroup employment-size values are size proxies rather than payroll employment. Indicator means are shares.

Table 2: E-Commerce Exposure Effects and Local-Time Controls

Outcome	Benchmark	County-year FE	Predetermined controls	County-year FE + controls
Panel A. Property outcomes				
Occupancy, 2014–2019	-0.600*** (0.151)	-0.559*** (0.169)	-0.570*** (0.152)	-0.558*** (0.170)
Log NOI per square foot, 2014–2019	-0.020*** (0.007)	-0.019** (0.008)	-0.018** (0.007)	-0.019** (0.008)
Occupancy, 2020–2021	-1.380*** (0.292)	-1.300*** (0.296)	-1.316*** (0.292)	-1.288*** (0.296)
Log NOI per square foot, 2020–2021	-0.031* (0.018)	-0.012 (0.023)	-0.028 (0.018)	-0.011 (0.023)

Notes: Coefficients report the effect of a 10 percentage point increase in e-commerce exposure measured using 2006 tenant mix. Every entry is an average of year-by-year event-study coefficients with 2013 omitted; window standard errors use the full coefficient covariance matrix. Predetermined local levels are measured in 2013 and local pretrends use the original pre-2013 periods. Standard errors are clustered by property. Infogroup count windows separate 2020–2021 from 2022–2023.

Table 3: Fitted Income Effects for Target Retail-Property Populations

Target population	Properties	Common support	Weighted exposure	2019 NOI change	2020–2021 NOI change
Observed Trepp NOI sample	11,868	100.0%	8.28	-4.1%	2.4%
All OSM retail places (interacted)	35,673	98.5%	5.93	-3.3%	1.2%
All OSM retail places (non-interacted)	35,673	98.5%	5.93	-2.9%	-0.9%
Top-30 metro areas	17,750	98.3%	5.80	-3.4%	0.9%
Major metro: within 10 miles of center	4,618	98.0%	5.93	-3.5%	0.7%
Major metro: 10-25 miles from center	8,871	98.5%	5.88	-3.3%	0.9%
Major metro: more than 25 miles from center	4,261	98.2%	5.49	-3.3%	1.0%
Other metro areas	14,981	98.7%	6.17	-3.5%	1.6%
Outside CBSAs	817	98.4%	4.82	-2.1%	1.4%
OSM mall and shopping-center features	943	95.3%	11.48	-5.8%	0.7%
OSM places with 100+ establishments	1,448	91.5%	7.75	-5.4%	-4.3%
OSM places with 1-5 establishments	10,252	98.7%	5.38	-1.9%	4.6%
Green Street large retail centers	3,408	94.0%	6.09	-4.0%	-3.2%

Notes: The table reports fitted percentage changes in annual NOI using e-commerce exposure measured from 2006 tenant mix; negative entries are income declines. Except for the explicitly labeled non-interacted row, the underlying regression allows the exposure effect to vary with baseline rentable area, baseline NOI per square foot, baseline establishment count, baseline retail share, Green Street coverage, mall or shopping-center format, anchor/big-box status, and geography. The non-interacted row applies the common county-year coefficient directly and implies annual all-OSM losses of $1.27 billion in 2019 and $0.38 billion over 2020–2021. Target rows reweight observed-NOI Trepp properties to match the target population over common cells defined by region, baseline exposure, establishment count, and NAICS-code coverage. Weighted exposure is the reweighted source-sample mean on common support, in percentage points; Table 1 reports raw target-population distributions. Effects are annual income-flow effects, not sale-price or welfare estimates.

Table 4: E-Commerce Exposure and Tenant-Loss Risk

	Any exit	Number exits	Large/chain-wide exit	Anchor/big-box exit	Exposed-category exit	Retail est. loss
E-commerce exposure	0.032***	0.036***	0.031***	0.009***	0.030***	0.078***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.010)
Observations	235,907	235,907	235,907	235,907	235,907	221,560
County-year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table reports post-2013 tenant-loss outcomes. Coefficients are scaled to a 10 percentage point increase in 2006 e-commerce exposure and compare properties within county-year cells. Large/chain-wide exits combine anchor or big-box exits, exits at properties with large average tenant space, and events whose source label identifies a chain-wide or mass-closure event. The tracked chain and anchor variables come from the closure-event system, so they measure the tenant-loss events studied below rather than a complete census of all national chains. Standard errors, clustered by property, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Pre-Liquidation Frictions and Post-Liquidation Adjustment

	Occupancy (pp)		Log NOI/sqft	
	Year +1	Year +2	Year +1	Year +2
	(1)	(2)	(3)	(4)
Panel A. Property specificity				
Property specificity (+1 SD)	-1.069*	-0.470	-0.063*	-0.030
	(0.588)	(0.800)	(0.038)	(0.034)
Number of properties	462	311	423	281
Panel B. Local replacement-market thickness				
Thin non-retail replacement market	1.590	1.472	-0.130*	-0.103
	(1.258)	(1.421)	(0.079)	(0.073)
Number of properties	478	324	434	292
Panel C. HPI-updated current LTV				
HPI-updated current LTV, +10 pp	-0.944*	-1.068***	-0.009	-0.033
	(0.556)	(0.384)	(0.025)	(0.025)
Number of properties	427	289	410	277
Property fixed effects	Yes	Yes	Yes	Yes
Closure-year × event-time fixed effects	Yes	Yes	Yes	Yes
Baseline outcome and area controls	Yes	Yes	Yes	Yes

Notes: Each row comes from a separate event-study model and reports the differential post-liquidation response relative to event year −2, not the average liquidation effect. Outcomes are pairwise-balanced changes net of changes among other observed retail properties in the same county. Property specificity is a standardized index that rises with log rentable area per establishment and falls with tenant-category entropy. A thin replacement market has no more than the production-sample median of 79 non-retail establishments in its ZIP. Current LTV is the scheduled loan balance divided by the securitization appraisal rolled forward with county FHFA residential house-price growth. All characteristics are measured at year −2. Models include property fixed effects, closure-year-by-event-time fixed effects, and baseline outcome and log-area controls interacted with event time. Panel C additionally includes original LTV, baseline DSCR, exact loan age and term, and original-contract categories, all interacted with event time. Standard errors are two-way clustered by property and liquidation event. The specificity–NOI and thin-market–NOI patterns are not cleanly timed; see the text.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A Data Construction and Measurement

This appendix describes the construction of the mapped retail-place panel, the Trepp–establishment match, the e-commerce exposure measure, the historical-mall universe and lifecycle comparison, and the verified-liquidation sample. It documents the linked data objects used in the empirical designs and gives human-readable accounts of how historical malls and liquidation events are discovered, selected, validated, and assigned.

A.1 Retail-place boundaries

We begin with OpenStreetMap polygons tagged as retail land, malls, or shopping centers. The raw file contains 182,081 polygons. We apply area and tag filters to focus on retail places rather than individual storefronts or incidental retail points, leaving 60,757 polygons. We then require at least one Infogroup establishment in 2006 and nonmissing e-commerce exposure, leaving 35,673 retail-place polygons.

The OpenStreetMap layer provides a fixed geography of shopping centers, malls, and retail areas to which annual establishments can be assigned. When polygons overlap, establishments are assigned to the smallest containing polygon so that nested retail places, such as a shopping center inside a larger retail district, do not double-count the same establishment.

A.2 Establishments and tenant categories

Infogroup provides annual establishment records with business names, addresses, coordinates, industry codes, employment-size bins, and corporate-family identifiers. We clean establishment names and addresses, assign establishments to OpenStreetMap retail polygons using coordinates, and group establishments into broad use categories: goods retail, food and drink, health and personal care, services and entertainment, office and professional uses, and other uses. The establishment records are used to measure tenant mix, learn operating-chain identifiers, and enumerate pre-liquidation store locations.

The employment-size bins serve as size proxies rather than payroll or square-footage measures. For exposure construction, they give more weight to larger baseline establishments. For the liquidation sample, the pre-event bins provide a predetermined descriptive measure of the jobs

attached to the closing stores; they do not determine whether a store or property enters treatment.

A.3 Trepp retail properties

Trepp provides property-year financial and loan records for retail properties financed through CMBS. We match source Trepp records to establishment rosters and then consolidate records that refer to the same physical property before estimating outcomes. The raw Trepp retail file contains 52,488 property records. Consolidation produces 46,270 retail properties, of which 45,731 have usable coordinates. We match 39,665 consolidated Trepp properties to Infogroup in at least one year. A larger set of matched Trepp properties can be assigned baseline exposure; the main exposure outcome sample is smaller because it additionally requires the relevant financial outcomes.

The main operating outcomes are occupancy and log net operating income per square foot. A missing Trepp outcome can reflect loan maturity, exit from the reporting universe, or a missing field, so missing reports are never coded as either recovery or failure.

A.4 Trepp–establishment matching

The broad Trepp–Infogroup match uses two source-specific rosters. A source Trepp record linked to a Green Street retail center uses the center roster. Other records use the normalized Trepp street address, requiring an exact street number or a number inside a valid address range together with exact or fuzzy street-name agreement. We then map source Trepp records to canonical physical properties and deduplicate exact Infogroup establishment identifiers within canonical property-year. Appendix B gives the complete precedence rule, correction, and validation results.

The liquidation design uses a separate and narrower procedure. We link each fixed pre-event store by its exact Infogroup establishment identifier to the existing Trepp-linked geography. This requirement establishes that the liquidating chain occupied the treated property before the event.

The match is chosen entirely from pre-event identifiers and geography. Property outcomes do not determine whether a candidate match is accepted. In the 57-event core, 98.9 percent of matched stores link in the designated pre-event reference year and the remainder link in the adjacent pre-event year. Candidate and linked records agree on normalized company name in 99.9 percent of cases, street address in 99.0 percent, ZIP code in 99.9 percent, and state in every

case. All 57 events have at least one property match. A robustness sample requires all component matches to lie within 250 meters of the physical-property centroid.

A.5 E-commerce exposure

For each property or retail place, we construct baseline tenant weights from every Infogroup establishment assigned to that location. Let j index matched establishments, let m_{ij} be the Infogroup employment-size weight, and let $E_{c(j)}$ be the 2019 online sales share for the establishment's retail category, measured in the Census Annual Retail Trade Survey. Restaurants, services, professional uses, explicit nonretail industries, and unknown/default establishments receive $E_{c(j)} = 0$. The property-level exposure measure is

$$Exposure_i = \frac{\sum_{j \in J_i} m_{ij} E_{c(j)}}{\sum_{j \in J_i} m_{ij}}.$$

The denominator intentionally includes all matched activity. Exposure is therefore the share of a property's full baseline activity directly exposed to online goods sales, not average exposure conditional on being a retailer. The measure is predetermined with respect to later property outcomes because the tenant weights are fixed at baseline. The 2019 online sales shares capture the cross-category shift before the COVID shock. A property with more bookstores, electronics stores, furniture stores, sporting-goods stores, and apparel stores receives a higher exposure value than a property with more grocery, restaurant, local-service, or other mostly offline tenants. Robustness checks fix tenant mix in alternative baseline years and produce similar results.

A.6 Historical mall closures and lifecycle construction

Candidate discovery and eligibility. The candidate universe combines Wikipedia categories for defunct U.S. malls, Wikidata geography, the DeadMalls and MallsInAmerica directories, Dead Mall Explorer as an omission check, and targeted searches by state and closure year (Wikipedia contributors, 2026; Wikidata, 2026; DeadMalls.com, 2026; MallsInAmerica, 2026). These sources generate leads rather than determine inclusion. For each candidate, we retain the discovery source, duplicates, failed pages, exclusion reason, and subsequent documentary evidence. We verify the historical name, city and state, street address, operation as an ordinary multi-tenant mall in 2006,

the date ordinary operation ended, and the later disposition. The death date is not a demolition date, redevelopment date, map edit, anchor closing, rename, or ownership transfer.

The source-bounded review contains 170 malls that are documented cases or sufficiently plausible cases to require a mapping disposition. The primary tier contains 108 documentary-core malls with strong event and mapping evidence. Six additional cases are core mapping or event auxiliaries, 54 are documentary-conditional cases involving weaker dates, phased operation, surviving anchors, or external physical shocks, and two have event definitions held from production. The address audit preserves a nonempty 2006 roster for all 170 sites; the two holds do not enter the 168 accepted production rosters. This is an exhaustive review within the combined public-source universe, not a claim that the sources constitute a definitive national census of every mall death.

Fixed historical address families. For each mall, we begin with its documented 2006 street address, city, state, and ZIP code. We then review the Infogroup records at that address for street-number variants, directional prefixes, abbreviations, internal suite conventions, and separately addressed anchors that are demonstrably part of the same mall. An address enters the family only with mall-specific evidence; broad-radius searches do not define membership. We deduplicate exact establishment identifiers within the accepted family. The 168 accepted rosters contain 12,194 unique mall–establishment links in 2006.

Coordinates and historical footprints serve as validation. Each cohort is reviewed with address counts, tenant names, coordinate distributions, and a site atlas. Archived OpenStreetMap objects, historical polygons, site plans, parcel maps, and aerial imagery are used where available to confirm or augment the address construction. Only 86 of the 170 sites have defensible public historical geometry, so requiring a polygon would make mapping availability determine the sample. The complete address-led construction is therefore primary, while polygons and sourced footprint fallbacks are validation and sensitivity evidence.

Exposure and longitudinal validation. We freeze the accepted address family from the 2006 roster and read it back in every Infogroup vintage from 2000 through 2023. Every accepted mall has a nonmissing roster in every vintage. We visually review each lifecycle and record explicit dispositions for gradual closures, address aliases, successor uses, and unusual post-death paths.

The primary e-commerce exposure uses the same fixed 2019 category-level online-sales rates as the broad design and employment-size weights within the 2006 roster.

For the comparison with current proper malls, we place the historical and current samples on common footing by selecting each site's dominant 2006 address cluster. This construction yields 108 primary future-dead malls and 857 current proper malls after removing known dead-site overlaps. At seven independently constructed same-site links, the historical and current-mall exposure measures have a correlation of 0.999 and a mean absolute difference of 0.07 percentage point. Common historical-polygon and stricter address definitions provide additional sensitivity checks. The comparison is informative about exposure within the mall format, but it does not make the historical death date exogenous.

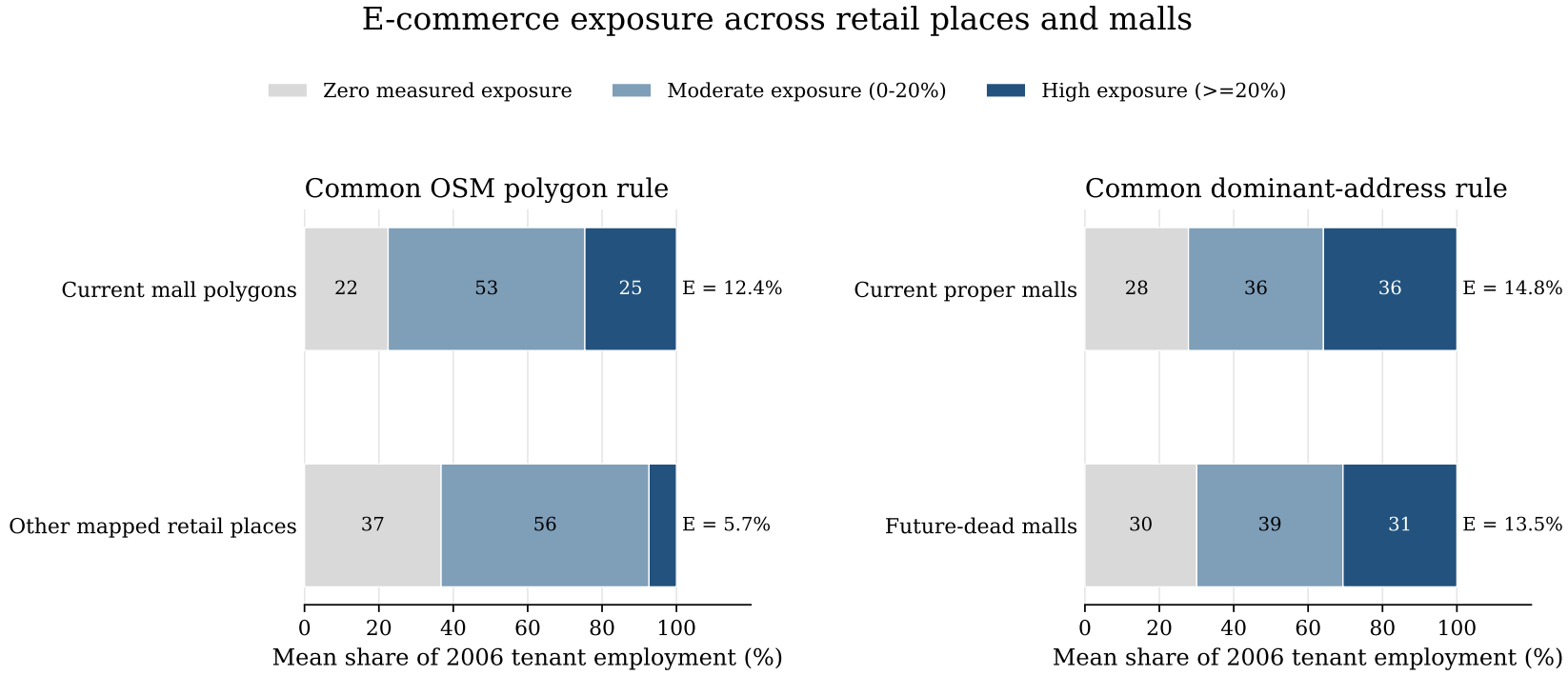
Lifecycle controls and tenant transitions. The lifecycle sample first requires at least ten establishments five years before death, which retains 105 of the 108 primary malls. A potential control passes the mapping-quality gate only if no more than 50 percent of its baseline establishments use the tier-5 address fallback and its mean establishment-to-site distance is no more than 1.5 kilometers. We require the control-to-dead-mall establishment-count ratio to lie in $[0.5, 2]$, the employment ratio to lie in $[1/3, 3]$, the absolute retail-share difference to be no more than 0.25, and the absolute 2006-exposure difference to be no more than 5 percentage points. Candidates are ranked using one set of pooled scales for log establishments, log employment, retail share, and exposure. We use the two closest same-region controls when two pass every gate; otherwise we use the two closest national controls that pass every gate. We never relax common support, and current properties overlapping any known dead-mall site are excluded.

This procedure retains 101 future-dead malls, 202 assignments, and 115 unique current-mall controls. Ninety-one matched sets use regional controls and ten use the national fallback. McFarland Mall, Totem Lake Mall, Capital Hill Mall, and Northwest Mall lack two controls on common support and are excluded only from the matched lifecycle exercise; they remain in the historical roster and unmatched exposure comparisons. All 202 selected assignments pass every gate.

At the matched-set level, future-dead and comparison malls average 68.39 and 71.33 establishments, 811.85 and 938.56 employment units, retail shares of 0.647 and 0.643, and 2006 exposures of

13.94 and 13.61 percentage points. The corresponding standardized differences are −0.091, −0.267, 0.033, and 0.094. Establishments, retail share, and exposure are therefore balanced to within 0.10 standard deviation; the moderate employment difference is the remaining observable-balance caveat.

The establishment-stock path is a ratio of pooled counts normalized to 100 at event time −5. An annual exit ending at event time k is an Infogroup establishment identifier present at $k-1$ and absent at k; an annual entry is absent at $k-1$ and present at k. Both rates divide pooled transition counts by the pooled establishment stock at $k-1$. Future-dead malls enter once and controls enter once per matched assignment. These definitions absorb common annual identifier churn through the same-calendar comparison, but the paths remain descriptive lifecycle evidence rather than causal event-study estimates.



Notes: Bars first compute employment shares within each property and then average equally across properties. Exposure categories use fixed 2019 industry online-sales rates. E is mean property exposure. Each panel applies one common geography rule; levels should be compared within panel.

Figure A1: Baseline E-Commerce Exposure across Retail Places and Malls

Notes: Panel A uses common OpenStreetMap polygons, while Panel B uses the common dominant-address rule for the historical mall comparison. Because the two panels use different site definitions, exposure levels should be compared within panel rather than across panels.

A.7 Verified-liquidation sample

Candidate discovery. We searched for U.S. goods-retail liquidations from 1997 through 2023. The search began with chains in the paper's earlier closing files and published research samples, and then expanded through bankruptcy trackers, lists of defunct retailers, trade publications, company and liquidator announcements, SEC and court materials, and contemporaneous national and local news. We repeated the search by year and retail category and logged prominent cases that were inspected but excluded. This produced 181 candidate events before applying any Infogroup or e-commerce screen. Candidate discovery never uses a property's subsequent occupancy, income, reporting status, or success in matching to Trepp.

Documentary scope. For every candidate, we record the legal or operating unit that closed, the closure year, the banners in scope, the source language, and a link to the supporting source. We prefer court or SEC filings, company or liquidator announcements, established trade publications, and contemporaneous news, in that order. Retrospective lists are used for discovery rather than final validation when stronger evidence is available.

The relevant question is whether every physical store in the defined retail network closed, not whether a legal debtor was formally liquidated. A single-banner chain qualifies when all of its stores close even if the trademark or website is sold. A parent-level event qualifies only if the evidence covers every included banner. A banner can qualify separately when its parent continues other businesses, provided every store in the focal banner closes. We exclude selected-store programs, conversions to another banner, going-concern sales that keep a material store fleet open, temporary closures, and ambiguous events without adequate documentary support. Restaurants, grocery, car rental, and local-service businesses are outside the paper's goods-retail mechanism.

The baseline requires a literal all-outlet closing within the documented scope. An ex ante identifiable company-owned, franchise, or dealer unit can be evaluated separately only if ownership status was fixed before the event, every store in that stratum closed, and the Infogroup identifier or an external store list isolates it from the surviving network. Near-liquidations that leave stores open remain a labeled sensitivity rather than entering the baseline. This rule is why a disappearing identifier alone is never sufficient evidence of liquidation.

Names and operating-chain identifiers. For each event, we build an alias dictionary from the public-facing banner, punctuation and spacing variants, historical spellings visible in Infogroup, and all banners explicitly included in a documented parent-level event. Automated matching permits exact normalized matches and controlled multiword-prefix matches, but not arbitrary substrings. We search the two years before the event, the event year, and the following two years to learn the stable chain-family identifier; name matching by itself never creates a treatment location.

Infogroup's establishment identifier refers to a single location, its subsidiary identifier often identifies an operating chain, and its ultimate-parent identifier can span several unrelated portfolio companies. We therefore prefer the dominant pre-event subsidiary identifier for a single-banner chain and use an ultimate-parent identifier only for a documented multi-banner parent liquidation. A candidate identifier must cover at least half of the alias matches to enter manual review; automatic acceptance requires at least 80 percent coverage, at least five pre-event name matches in at least two states, and at least 80 percent focal-chain purity among its pre-event retail records. We read back every distinct company name under the selected identifier and reject headquarters, non-retail operations, unrelated portfolio companies, and ambiguous names.

National disappearance and e-commerce gates. After selecting an operating-chain identifier, we retrieve its complete national retail footprint rather than only the stores found by name. The pre-event footprint is the larger of the $t-2$ and $t-1$ retail counts. A strict event must lose at least 90 percent of that footprint by $t+2$. The two-year window accommodates late-year liquidation sales and reporting lags. Once the event passes, every pre-event retail establishment carrying the validated identifier enters the candidate store universe; we do not retain only the locations that individually disappear.

We evaluated otherwise identical 75, 80, and 90 percent rules. The 80 percent rule adds no event to the 57-event e-commerce core. The 75 percent rule adds one apparent event whose ultimate-parent identifier also contains continuing banners. We therefore retain 90 percent as the primary rule and 80 percent as a sensitivity. Events too recent to be observed at $t+2$ are labeled as awaiting data rather than treated as failures.

The e-commerce restriction is separate from documentary validation. The primary sample keeps events beginning in or after 2005 whose modal four-digit retail category has a 2019 online-sales

share of at least 20 percent and an increase of at least 10 percentage points from 2006 to 2019. This rule uses the same Census category measures as the exposure design. Event-specific reporting that mentions Amazon, online competition, or a move to an online-only channel is preserved as corroborating evidence, but news wording does not decide inclusion and we do not claim that e-commerce was the sole cause of every liquidation.

Property assignment and final risk set. Candidate stores are the pre-event retail establishments carrying the validated chain identifier. We deduplicate establishment-year records and link exact establishment identifiers from $t-1$, or $t-2$ under a documented fallback, to the fixed Trepp property geography. The 57 core events contain 17,275 candidate stores. Of these, 5,884 exact Infogroup establishment identifiers, or 34.1 percent, link to 12,160 raw store–Trepp records; all events have property support. The raw-record count is larger because one physical shopping center can appear under multiple loans or securitizations, or as both a center and a subproperty. Before examining outcomes, a deterministic rule assigns each store to one Trepp record, preferring a retail classification and then the largest rentable area; direct polygon status, centroid distance, and Trepp identifier are used only as tie-breakers. This retains every matched store and yields 5,742 focal event–property links at 2,874 physical properties after consolidation. Multiple stores can occupy the same property, and a property can be linked to different liquidation events.

The production sample keeps the first focal liquidation at each physical property. It excludes 471 properties hit by an earlier verified liquidation and 101 same-year compound shocks, leaving 2,302 properties and all 57 events. A later verified liquidation censors the property in that year and thereafter; 702 primary properties are eventually censored under this rule. Controls are other same-county retail properties observed in both the baseline and target year. The control universe excludes all 3,956 properties ever linked to any matched literal-90 verified liquidation. Event time is the documented year in which the chain-wide closing process begins, and year -2 is the baseline.

A.8 Reporting, attrition, and interpretation

Trepp financial outcomes are strongest at short and medium horizons because CMBS reporting can end when loans mature, leave the reporting system, or stop reporting a field. The pairwise-balanced design requires the outcome at both year -2 and the target horizon for treated and comparison

properties. Thus the event-study coefficients never infer an outcome after reporting ends, and longer-horizon estimates apply to the properties still observed. Raw reporting-retention checks do not show worse post-event reporting for treated properties than for same-county comparisons after separating later-shock censoring, but changing sample composition remains a limitation of year-three estimates.

The Trepp sample is not the universe of U.S. retail property. It overrepresents larger properties with securitized debt. That selection is useful for observing property income and occupancy, but it matters for external validity. The broad exposure section therefore reports reweighted calculations that make the observed-NOI Trepp source sample resemble broader observed property populations, including mapped OpenStreetMap retail places.

Table A1: Data Sources and Main Uses

Source	Count or coverage	Main use
OpenStreetMap	60,757 retail, mall, or shopping-center polygons	Broad retail-location universe and Infogroup spatial assignment
OpenStreetMap + Infogroup	35,673 retail polygons with 2006 exposure	Baseline exposure and tenant-mix composition
Green Street	Curated mall and large-center universe	Large retail-center sample and validation
Infogroup	Annual establishment names, addresses, NAICS, employment bins, coordinates, and corporate identifiers	Tenant mix, operating-chain validation, and pre-event store universe
Trepp	46,270 consolidated CMBS retail properties	Occupancy, NOI, and fixed property geography
Liquidation documentary ledger	181 candidate events, 93 literal-90 verified events	Event scope, timing, sources, and visible exclusions
High-e-commerce liquidation sample	57 events; 2,302 first-shock properties	Main property-level event study
Historical-mall source ledger	170 fully dispositioned post-2006 candidate sites	Mall operation, death date, address, and subsequent-use evidence
Historical mall–Infogroup rosters	168 accepted sites; 12,194 unique 2006 mall–establishment links	Fixed-site exposure and establishment lifecycles
Matched mall-lifecycle sample	101 future-dead malls; 202 comparison assignments	Establishment stock, tenant exit, and tenant entry before mall death

Notes: Counts reflect the analysis samples used in the paper.

Table A2: Retail-Location and Trepp Sample Construction

Sample construction step	Count	Sample unit
Raw OpenStreetMap retail, mall, or shopping-center polygons	182,081	OpenStreetMap polygons
OpenStreetMap polygons after five-acre area threshold	60,757	OpenStreetMap polygons
OpenStreetMap polygons with at least one Infogroup establishment in 2006	35,673	OpenStreetMap polygons
OpenStreetMap polygons with nonmissing e-commerce exposure	35,673	OpenStreetMap polygons
Green Street malls and retail centers linked to Trepp	2,151	properties
Raw Trepp retail records before physical-property consolidation	52,488	Trepp records
Trepp retail properties after consolidation	46,270	properties
Trepp properties matched to Infogroup in any year	39,665	properties
Trepp properties in the baseline exposure sample	33,701	properties
Documentary liquidation candidates before Infogroup screens	181	candidate events
Events passing the literal-90 documentary and national-disappearance rules	93	verified events
Verified events passing the dated high-e-commerce-category rule	57	core events
Core event–property links after physical-property consolidation	5,742	2,874 physical properties
Primary first-liquidation risk set	2,302	physical properties; all 57 core events
Source-bounded historical-mall review queue	170	fully dispositioned mall sites
Accepted historical mall–Infogroup rosters	168	fixed 2006 address-family sites
Primary documentary and mapping tier	108	future-dead malls
Lifecycle support with at least ten establishments at $t = -5$	105	future-dead malls
Production matched lifecycle sample	101	future-dead malls; 202 comparison assignments

Notes: Counts come from the analysis sample. OpenStreetMap polygons are national features tagged retail, mall, or shopping center. Source Trepp records are matched to Infogroup rosters and then consolidated to physical properties before estimating property outcomes; counts and exposure are rebuilt from the canonical roster. A literal-90 event closes the complete physical-store network in its documented scope and loses at least 90 percent of its national Infogroup retail footprint by $t + 2$. The primary risk set keeps each property's first qualifying event, excludes earlier verified shocks and same-year compound shocks, and censors later verified liquidations. Historical malls must operate as ordinary multi-tenant malls in 2006 and later cease ordinary operation. All 170 source-bounded cases retain a documentary and mapping disposition; the two held event definitions do not enter the 168 accepted rosters.

Table A3: Coverage of the Retail-Location Panel Relative to National Retail Data

Category	CBP est., 2013	OpenStreetMap/Infogroup est., 2013	Coverage ratio	CBP growth 2006–2019	OpenStreetMap/IG growth 2006–2019	Corr. 2006–2019
All retail goods	1,063,616	267,926	0.25	-6.7%	7.0%	-0.34
Books	8,225	2,424	0.29	-60.4%	-39.0%	0.98
Clothing/accessories	146,350	70,109	0.48	-9.9%	-6.8%	0.67
Electronics/appliance	47,998	12,685	0.26	-47.7%	-57.5%	0.90
Furniture/home furnishings	51,103	12,842	0.25	-25.9%	-5.0%	0.68
Grocery/food stores	149,279	28,160	0.19	-1.5%	21.1%	0.51
Health/personal care	93,738	34,730	0.37	8.3%	50.2%	0.89
Sporting goods/hobby	21,559	6,240	0.29	-12.3%	-3.3%	0.49

Notes: CBP is County Business Patterns. OpenStreetMap/Infogroup counts are Infogroup establishments whose coordinates fall inside national OpenStreetMap retail, mall, or shopping-center polygons with area at least five acres. If an establishment falls inside multiple OpenStreetMap polygons, it is assigned to the smallest containing polygon. The coverage ratio is the OpenStreetMap/Infogroup count divided by the national CBP establishment count in 2013. The final column is the correlation between annual CBP and OpenStreetMap/Infogroup establishment counts over 2006–2019.

Table A4: Baseline Composition by E-Commerce Exposure Bin

Exposure bin	OpenStreetMap locations	Mean exposure pp	Mean est. per location	Retail goods share	Food/drink share	Health care share
Bottom 20%	7,135	0.8	18.0	14.5%	9.4%	14.9%
Middle 20%	7,159	4.3	23.9	19.1%	16.9%	11.7%
Top 20%	7,135	14.7	31.0	41.8%	13.5%	10.2%

Notes: Exposure bins are fixed using 2006 Infogroup employment-bin-weighted e-commerce exposure in the all-OpenStreetMap retail-boundary universe. The rows report the bottom, middle, and top quintile ranges shown in Appendix Figure C5; ties at cutoffs are retained. Mean establishments and composition shares use 2013 Infogroup establishments assigned to OpenStreetMap retail boundaries.

B Broad Trepp–Establishment Matching Audit and Robustness

This appendix describes and audits the establishment-to-property match used in the broad e-commerce exposure design. The relevant spatial units are not always identical. Trepp can report a loan, collateral component, or subproperty; Green Street generally describes an entire retail center; and Infogroup describes individual business locations. We therefore report both the production construction and alternatives that place greater weight on the Trepp street address or direct spatial containment. The verified-liquidation and historical-mall analyses use separate exact-identifier and reviewed-address procedures described in Appendix A.

B.1 Production construction and spatial units

The production match is built before source Trepp records are consolidated into canonical physical properties. For records linked to a Green Street center, the production combination uses the Green Street center roster in place of the Trepp-address roster. Direct polygon containment is Tier 1. A 25-meter boundary buffer supplies Tier 1b when its LinkTransformer score is at least 0.6 and Tier 1c otherwise. Lower tiers use address agreement within 500 meters, name and proximity within 300 meters, extended-address evidence within 200 meters, and a residual fallback. Records without Green Street coverage use the Trepp-address construction: an establishment must have the same normalized street number, or a number inside a valid address range, together with exact or fuzzy street-name agreement. The fuzzy street-name threshold is 0.80. A small anchor-recovery procedure searches for historical anchor names within 500 meters and contributes 111 canonical establishment–property rows in 2006.

After the source-specific rosters are constructed, raw Trepp records are mapped to canonical physical-property identifiers. An Infogroup establishment identifier is deduplicated within each canonical property-year. The same identifier can remain associated with more than one canonical property when collateral records overlap or when a center and one of its subproperties are both represented. The corrected production procedure recomputes establishment counts and exposure from this final canonical roster rather than inheriting exposure from one representative raw Trepp record.

B.2 Exposure denominator and deterministic corrections

The exposure numerator sums the fixed Infogroup employment-size weights of baseline establishments multiplied by their category-level 2019 e-commerce rates. The denominator sums the same weights across all matched activity. Nonretail and unknown/default establishments receive a zero e-commerce rate but remain in the denominator. In the corrected 2006 release, NAICS-mapped retail accounts for 63.60 percent of denominator weight, the SIC retail fallback for 0.28 percent, explicit nonretail uses for 33.12 percent, and unknown/default uses for 3.00 percent. This definition captures the share of a property's full matched baseline activity directly exposed to online goods sales. A retail-only denominator instead conditions on already operating in retail and therefore answers a different economic question.

The earlier clean handoff recomputed establishment counts after physical-property consolidation but retained exposure from a representative raw Trepp record. Recomputing exposure from the exact final roster changes 242 of the 33,667 properties with previously nonmissing exposure. The mean absolute change is 0.034 percentage point, 29 properties change by more than 10 points, and 42 properties with previously missing exposure become usable. Recomputed establishment counts agree exactly with all 820,285 property-years supported by the inherited roster.

We also apply a deterministic address-consistency screen to the Green Street–Trepp crosswalk. Twenty-four accepted links have incompatible leading street numbers. These links account for 18,368 establishment–property rows in 2013, or 2.53 percent of all rows in that year. For these cases, the corrected construction uses the Trepp-address roster unless property-specific evidence establishes a replacement. This correction requires no judgment based on exposure or outcomes. Combining the canonical exposure rebuild with the address quarantine produces 33,701 properties with nonmissing 2006 exposure.

B.3 Internal source comparisons

In 2006, the corrected production roster contains 615,918 establishment–property rows. The Green Street source contributes 268,646 rows at 2,084 canonical properties, while the Trepp-address source, including the conflict fallback, contributes 347,272 rows at 31,703 properties; 86 canonical properties receive rows from both sources. Although Green Street covers relatively few properties,

it supplies 43.6 percent of establishment–property rows; 60.9 percent of its rows are in tiers 2–5 rather than the Tier 1/1b/1c containment-and-buffer matches.

There are 1,705 source Trepp records for which both a Green Street and a Trepp-address roster can be constructed in 2006. Their median establishment-identifier Jaccard similarity is 0.096 and 21.3 percent have no identifier overlap. Agreement is substantially higher for enclosed centers than for open-air centers; records with missing format fall in between, consistent with a difference between center-level and address-level spatial units. Despite the roster difference, the two exposure measures have a Pearson correlation of 0.448, a rank correlation of 0.560, and a median absolute difference of 3.37 percentage points. Figure B1 displays these comparisons. Because the two rosters share the same underlying Infogroup lineage, this is a source-definition comparison rather than independent validation.

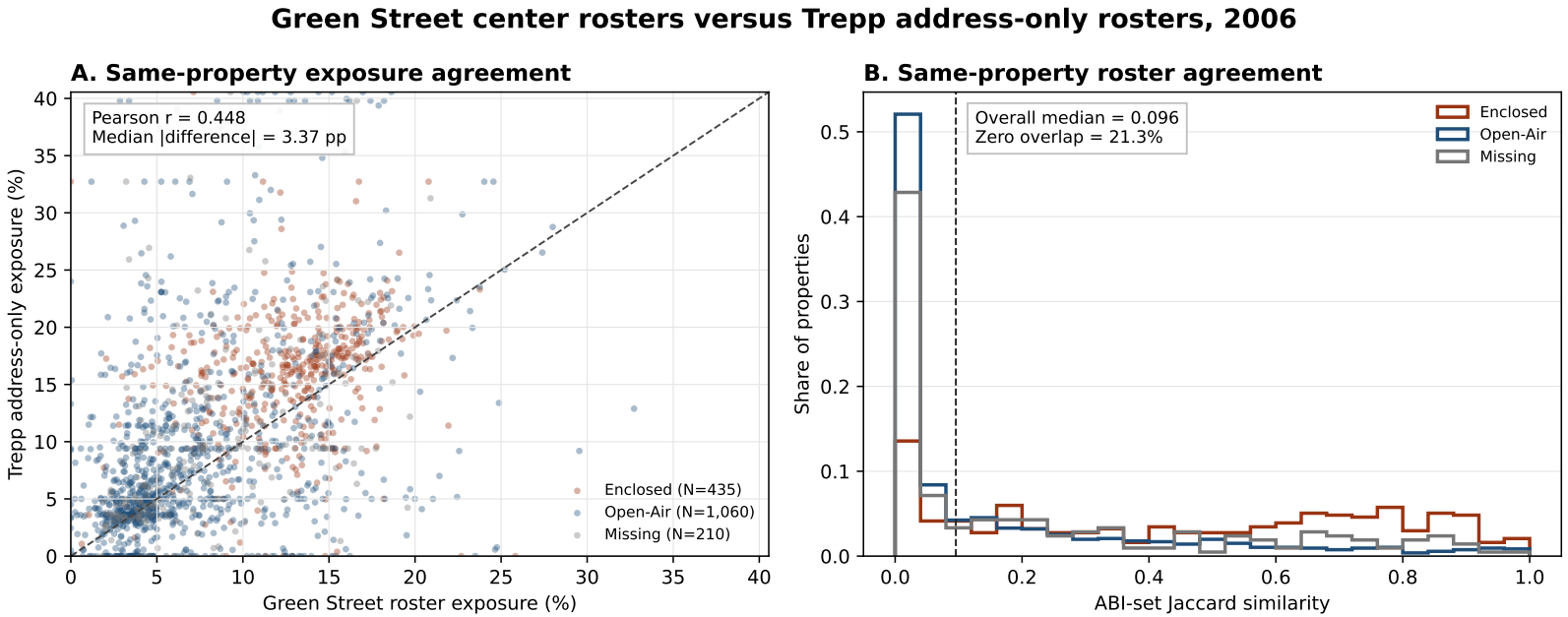


Figure B1: Green Street and Trepp-Address Roster Agreement

Notes: The figure compares the two separately constructed spatial rosters on source Trepp records for which both are available in 2006. Establishment overlap uses exact Infogroup establishment identifiers. Exposure uses a denominator over all matched activity, assigning a zero e-commerce rate to nonretail and unknown/default establishments, together with fixed 2019 category rates. The comparison diagnoses sensitivity to a center-level rather than address-level spatial unit; it is not a comparison of independent establishment data sources.

B.4 Outside-source checks and error taxonomy

Public owner directories and leasing plans help distinguish genuine mixed use from geographic spillover. Checks for Haynes Bridge Village and Aspen Hill Shopping Center corroborate banks, medical and dental offices, salons, fitness, tax preparation, shipping, cleaners, restaurants, and other services in the matched rosters (Westwood Financial, 2025; Aspen Hill Shopping Center, 2026). The Haynes Bridge leasing brochure, for example, lists more than twenty small tenants alongside a supermarket anchor. Because these public materials postdate the 2006 baseline, they validate the plausibility of the centers' spatial scope and mixed-use tenant rosters rather than individual historical 2006 establishments. This evidence supports retaining legitimate nonretail uses in the exposure denominator.

The same review also reveals the limits of center-level rosters. The Shops at North Bridge construction spans a mixed-use urban district: its 2013 roster contains 2,036 establishment–property rows across 940 street addresses and has a 90th-percentile establishment distance of 1,015 meters. One deterministic New York conflict links a Green Street roster labeled 27 East 61st Street to Trepp collateral labeled 25 West 51st Street. A separate, selected eight-mall directory audit flags 273 of 1,756 retail-SIC records, or 15.5 percent, as false positives, with roughly half attributed to neighboring-property spillover. Because those malls were selected and the directories are observed later than the 2006 baseline, that exercise identifies an error taxonomy rather than a national precision rate.

Cross-property reuse is another potential source of correlated measurement error. In the corrected 2006 release, 96,346 of 491,344 establishment identifiers, or 19.61 percent, appear at more than one canonical property. They generate 220,920 of 615,918 establishment–property rows, or 35.87 percent. These assignments can represent genuine overlapping collateral as well as error. We therefore report specifications that drop these identifiers and specifications that give each establishment–property assignment weight $1/n$, where n is the number of canonical properties receiving that establishment in that year.

B.5 Robustness across roster constructions

Table B1 reports average 2014–2019 effects per 10 percentage points of exposure. Recomputing exposure from the canonical roster and quarantining the 24 deterministic conflicts changes the occupancy estimate from −0.595 to −0.600, the log-NOI estimate from −0.020 to −0.020, the total-establishment estimate from −0.675 to −0.701, and the retail-establishment estimate from −0.334 to −0.339.

The financial estimates are also stable when the roster is replaced more broadly. The Trepp-address-only construction gives estimates of −0.571 for occupancy and −0.019 for log NOI. A strict hybrid that begins with the address roster and adds only directly contained Green Street establishments from crosswalks within 500 meters gives −0.583 and −0.020. On the 33,102-property intersection of the canonical, address-only, and strict constructions, the respective occupancy estimates are −0.599, −0.571, and −0.585, while the log-NOI estimates are −0.021, −0.019, and −0.019.

The establishment-count coefficients remain negative under every construction. Fractionally weighting identifiers that appear at multiple properties produces estimates of −0.505 for total establishments and −0.220 for retail establishments, compared with corrected preferred estimates of −0.701 and −0.339. Excluding all Green Street-source properties gives −0.407 and −0.188. Thus the exact count magnitude is sensitive to whether a center and its subproperties are treated as separate locations, but the conclusion that more-exposed properties lose establishments is unchanged. The duplicate-handling and retail-only rows are one-at-a-time alternatives to the rebuilt canonical roster rather than modifications layered on the 24-link quarantine.

The retail-only exposure calculation is included in Table B1 as an estimand comparison, not as a correction to the preferred variable. It excludes restaurants, services, professional offices, and other potential replacement uses from the denominator and therefore removes part of the cross-property composition margin that the paper seeks to measure. The preferred specifications retain all matched baseline activity in the denominator. The figure also reports the retail-only denominator applied to the Trepp-address-only roster. Figure B2 summarizes these construction checks graphically.

Table B1: Broad Trepp–Establishment Match Robustness

Construction	Occupancy	Log NOI/sqft	Total establishments	Retail establishments
Panel A. Alternative constructions				
Saved pre-correction construction	-0.595*** (0.150)	-0.020*** (0.007)	-0.675*** (0.055)	-0.334*** (0.021)
Canonical exposure rebuilt	-0.597*** (0.151)	-0.020*** (0.007)	-0.686*** (0.054)	-0.336*** (0.021)
Corrected preferred: 24 links quarantined	-0.600*** (0.151)	-0.020*** (0.007)	-0.701*** (0.054)	-0.339*** (0.021)
Trepp address-only	-0.571*** (0.143)	-0.019*** (0.007)	-0.602*** (0.029)	-0.315*** (0.018)
Strict address + contained Green Street	-0.583*** (0.147)	-0.020*** (0.007)	-0.609*** (0.041)	-0.317*** (0.022)
Drop multi-property identifiers	-0.614*** (0.189)	-0.021** (0.009)	-0.641*** (0.072)	-0.295*** (0.024)
Fractional multi-property identifiers	-0.596*** (0.151)	-0.020*** (0.007)	-0.505*** (0.049)	-0.220*** (0.015)
Exclude Green Street-source properties	-0.564*** (0.156)	-0.021*** (0.007)	-0.407*** (0.024)	-0.188*** (0.014)
Different estimand: retail-only denominator	-0.386*** (0.145)	-0.007 (0.007)	-0.617*** (0.040)	-0.335*** (0.018)
Panel B. Common support across roster definitions				
Canonical roster	-0.599*** (0.152)	-0.021*** (0.007)	-0.578*** (0.038)	-0.306*** (0.019)
Trepp address-only	-0.571*** (0.143)	-0.019*** (0.007)	-0.602*** (0.029)	-0.315*** (0.018)
Strict hybrid	-0.585*** (0.147)	-0.019*** (0.007)	-0.600*** (0.032)	-0.316*** (0.020)

Notes: Each cell reports the covariance-aware average 2014–2019 coefficient for a 10 percentage point increase in the indicated exposure, with a property-clustered standard error in parentheses. All specifications include property and year fixed effects and omit 2013. Panel A uses each construction's available support. Panel B restricts the three roster definitions to their common set of 33,102 baseline properties; outcome-specific property counts can be smaller because of outcome availability. The retail-only row changes the economic estimand rather than tightening the property match: it conditions on identifiable retail activity instead of measuring exposed goods-retail activity as a share of all matched activity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

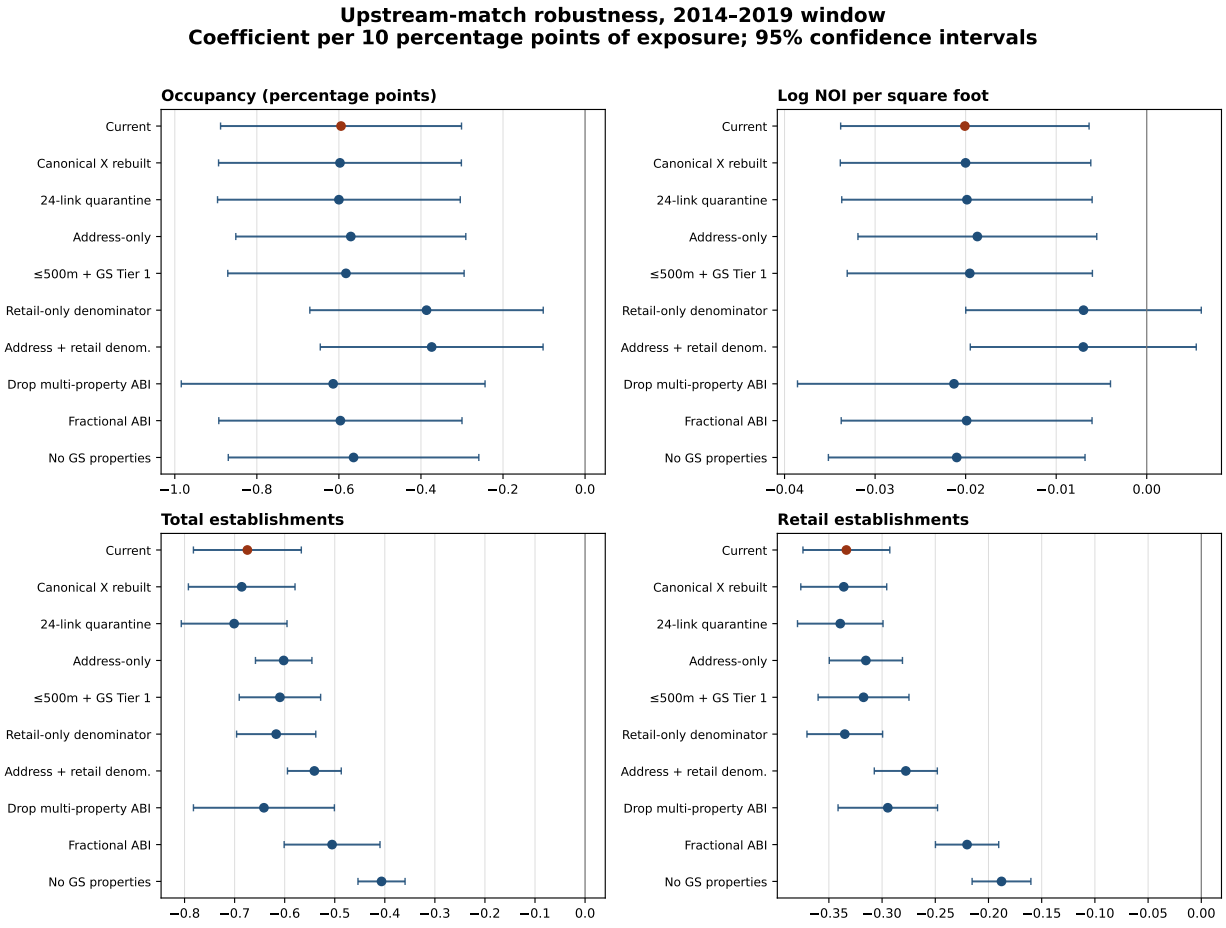


Figure B2: Broad Exposure Estimates Across Match Constructions

Notes: The figure reports covariance-aware 2014–2019 average event-study coefficients for a 10 percentage point increase in exposure. Specifications include property and year fixed effects, omit 2013, and cluster standard errors by property. Match-source, duplicate-handling, and denominator alternatives are defined in the text and Table B1.

B.6 Interpretation

Roster error need not create classical attenuation. Match source, property format, density, overlapping collateral, and exposure are related, and the same establishment roster constructs both exposure and the establishment-count outcomes. The financial outcomes are cleaner for this purpose because only their exposure variable depends on the establishment match. The stability of occupancy and NOI across the canonical, address-only, strict-containment, duplicate-handling, and Green Street-exclusion constructions indicates that the main property-performance results are not driven by one upstream matching rule. The establishment results are likewise stable in sign, although their unit magnitude depends on how shared centers and subproperties are counted.

C E-Commerce Exposure: Robustness and Aggregation

This appendix reports robustness checks for the broad exposure design, including the two components of the exposure measure, local conditions and pre-period paths, property format and size, retail-category contributions, reweighting, and detailed changes in property use.

C.1 The two components of exposure

The preferred measure combines a property-specific tenant distribution fixed in 2006 with national category-level online-sales shares from 2019. The two robustness exercises vary these components separately. Appendix Figure C1 changes the year in which property tenant mix is fixed while retaining the 2019 category rates. Appendix Table C1 holds the 2006 tenant mix fixed and substitutes later national category-rate vectors. In every specification, the outcome path is normalized to 2013.

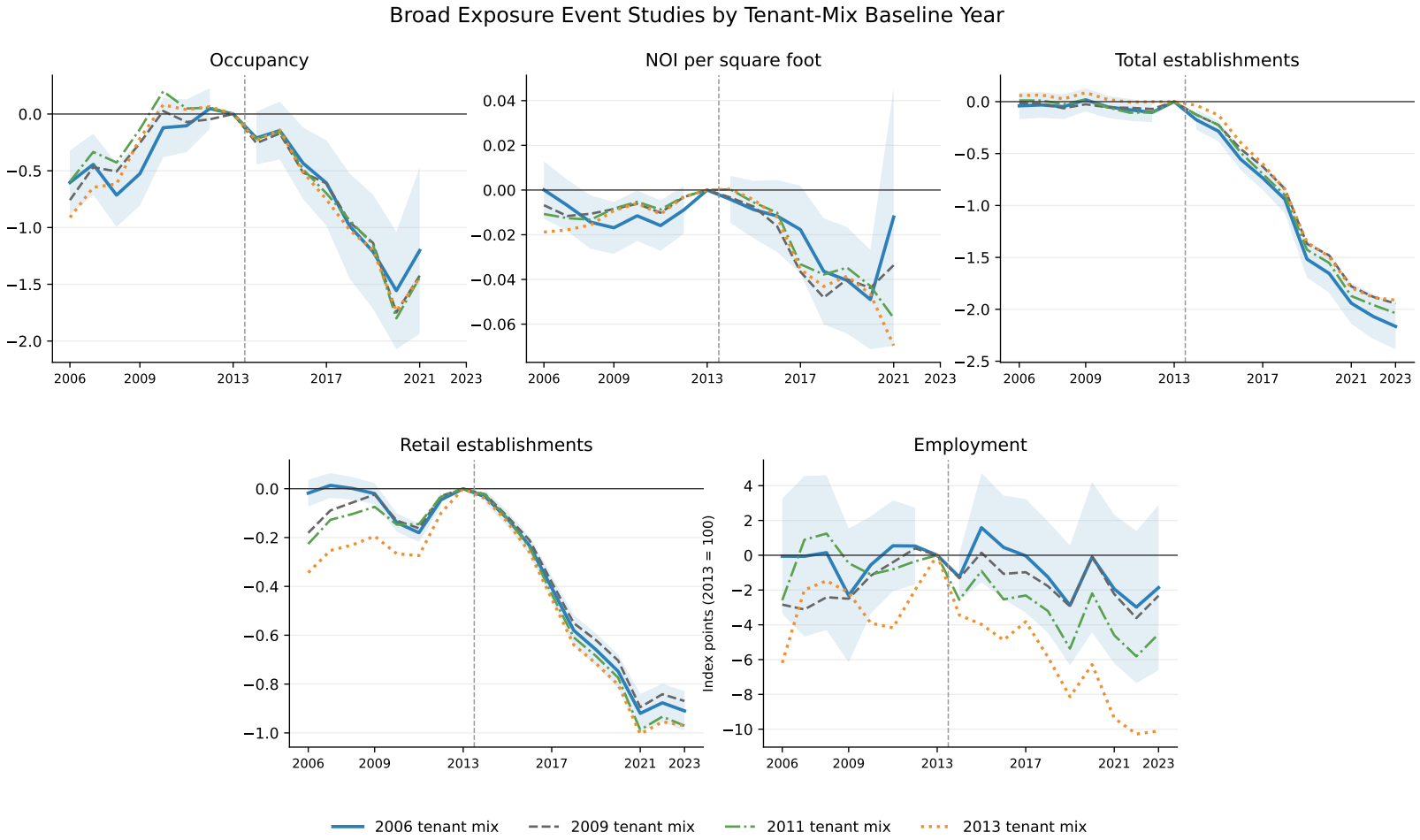


Figure C1: E-Commerce Exposure Results using Alternative Tenant-Mix Baseline Years

Notes: The figure reports event-study coefficients using alternative years to fix the property tenant mix while retaining the 2019 national category-level online-sales shares. All series are normalized to the same omitted outcome year, 2013. The purpose is to vary the property-composition component of exposure. The preferred measure fixes tenant mix in 2006, before the main property-outcome divergence.

Alternative national e-commerce-share years

The preferred exposure measure combines property tenant mix fixed in 2006 with national category-level online-sales shares from 2019. The property composition is the predetermined component of the design; the category shares summarize the relative extent to which different goods categories moved online. Table C1 holds the 2006 tenant mix fixed and replaces the 2019 category vector with each comparable later vector available in the national series. The table reports the resulting exposure distributions, correlations with the preferred measure, and outcome estimates. This exercise changes the snapshot used to summarize category-level online adoption without changing where exposed activity was located at baseline or the 2013 normalization of the outcome paths.

The later category vectors produce highly correlated exposure measures and similar estimates, so the results do not depend on using 2019 rather than a later snapshot of online adoption.

Table C1: Alternative National E-Commerce-Share Years

Panel A. Exposure distribution and property outcomes

Rate year	Properties	Mean exposure	SD	Corr. with 2019	Occupancy, 2014–19	Log NOI/sqft, 2014–19
					−0.600***	−0.020***
2019	33,701	8.35	9.48	1.000	(0.151)	(0.007)
					−0.451***	−0.015***
2020	33,701	11.74	11.93	0.992	(0.117)	(0.006)
					−0.479***	−0.017***
2021	33,701	11.76	11.33	0.990	(0.124)	(0.006)
					−0.462***	−0.016***
2022	33,701	11.88	11.33	0.986	(0.123)	(0.006)

Panel B. Business outcomes

Rate year	All, 2014–19	Retail, 2014–19	All, 2020–21	Retail, 2020–21	All, 2022–23	Retail, 2022–23
	−0.701***	−0.339***	−1.798***	−0.834***	−2.117***	−0.893***
2019	(0.054)	(0.021)	(0.098)	(0.038)	(0.109)	(0.040)
	−0.590***	−0.290***	−1.500***	−0.707***	−1.770***	−0.762***
2020	(0.046)	(0.017)	(0.084)	(0.031)	(0.094)	(0.033)
	−0.589***	−0.280***	−1.507***	−0.686***	−1.800***	−0.738***
2021	(0.050)	(0.017)	(0.090)	(0.031)	(0.101)	(0.033)
	−0.588***	−0.280***	−1.504***	−0.688***	−1.796***	−0.742***
2022	(0.050)	(0.017)	(0.090)	(0.031)	(0.101)	(0.033)

Notes: Tenant mix and establishment weights are fixed in 2006. Each row replaces the 2019 national category-level online-sales shares with the indicated year's shares. Effects correspond to a 10 percentage point increase in the resulting exposure measure. Standard errors clustered by property are in parentheses.

C.2 Local conditions and pre-period paths

Table C2: Broad Exposure Effects With Predetermined Local Controls

Outcome	Full-sample benchmark	County-by-year FE	County-controls sample benchmark	Predetermined county controls	County-by-year FE + county controls	ZIP-controls sample benchmark	Predetermined ZIP/county controls	County-by-year FE + ZIP/county controls
Panel A. Property outcomes								
Occupancy, 2014–2019	-0.600*** (0.151)	-0.559*** (0.169)	-0.598*** (0.151)	-0.570*** (0.152)	-0.558*** (0.170)	-0.578*** (0.160)	-0.545*** (0.162)	-0.530*** (0.193)
Occupancy, 2020–2021	-1.380*** (0.292)	-1.300*** (0.296)	-1.371*** (0.293)	-1.316*** (0.292)	-1.288*** (0.296)	-1.541*** (0.445)	-1.287*** (0.436)	-1.338** (0.526)
Log NOI per square foot, 2014–2019	-0.020*** (0.007)	-0.019** (0.008)	-0.020*** (0.007)	-0.018** (0.007)	-0.019** (0.008)	-0.022*** (0.007)	-0.022*** (0.007)	-0.022** (0.009)
Log NOI per square foot, 2020–2021	-0.031* (0.018)	-0.012 (0.023)	-0.030* (0.018)	-0.028 (0.018)	-0.011 (0.023)	-0.020 (0.030)	-0.018 (0.031)	-0.024 (0.038)
Log NOI per occupied square foot, 2014–2019	-0.015** (0.006)	-0.014** (0.007)	-0.015** (0.006)	-0.013** (0.006)	-0.015** (0.007)	-0.017*** (0.007)	-0.018*** (0.007)	-0.018** (0.008)
Log NOI per occupied square foot, 2020–2021	-0.017 (0.017)	-0.002 (0.022)	-0.016 (0.017)	-0.015 (0.017)	-0.001 (0.022)	0.001 (0.030)	0.000 (0.031)	-0.015 (0.040)
Panel B. Establishment outcomes								
Total establishments, 2014–2019	-0.701*** (0.054)	-0.650*** (0.053)	-0.703*** (0.054)	-0.683*** (0.052)	-0.653*** (0.053)	-0.779*** (0.074)	-0.685*** (0.074)	-0.621*** (0.085)
Total establishments, 2020–2021	-1.798*** (0.098)	-1.654*** (0.105)	-1.800*** (0.099)	-1.713*** (0.094)	-1.657*** (0.106)	-1.948*** (0.126)	-1.701*** (0.124)	-1.556*** (0.143)
Total establishments, 2022–2023	-2.117*** (0.109)	-1.953*** (0.119)	-2.117*** (0.110)	-1.993*** (0.105)	-1.954*** (0.120)	-2.271*** (0.138)	-2.013*** (0.132)	-1.861*** (0.153)
Retail establishments, 2014–2019	-0.339*** (0.021)	-0.330*** (0.022)	-0.341*** (0.020)	-0.344*** (0.021)	-0.332*** (0.022)	-0.390*** (0.034)	-0.351*** (0.033)	-0.327*** (0.038)
Retail establishments, 2020–2021	-0.834*** (0.038)	-0.810*** (0.040)	-0.836*** (0.038)	-0.837*** (0.038)	-0.812*** (0.041)	-0.939*** (0.064)	-0.834*** (0.061)	-0.794*** (0.069)
Retail establishments, 2022–2023	-0.893*** (0.040)	-0.867*** (0.043)	-0.895*** (0.040)	-0.896*** (0.041)	-0.870*** (0.043)	-1.033*** (0.070)	-0.917*** (0.067)	-0.869*** (0.075)
Non-retail establishments, 2014–2019	-0.362*** (0.043)	-0.320*** (0.041)	-0.362*** (0.043)	-0.339*** (0.041)	-0.321*** (0.041)	-0.389*** (0.055)	-0.334*** (0.056)	-0.294*** (0.064)
Non-retail establishments, 2020–2021	-0.964*** (0.079)	-0.844*** (0.085)	-0.965*** (0.080)	-0.876*** (0.076)	-0.845*** (0.086)	-1.009*** (0.088)	-0.867*** (0.089)	-0.762*** (0.105)
Non-retail establishments, 2022–2023	-1.224*** (0.092)	-1.086*** (0.101)	-1.222*** (0.092)	-1.097*** (0.088)	-1.084*** (0.102)	-1.238*** (0.102)	-1.096*** (0.099)	-0.993*** (0.118)

Notes: Coefficients report the effect of a 10 percentage point increase in e-commerce exposure measured using 2006 tenant mix. Every entry is an average of year-by-year event-study coefficients with 2013 omitted; window standard errors use the full coefficient covariance matrix. Predetermined local levels are measured in 2013 and local pretrends use the original pre-2013 periods. Standard errors are clustered by property. Infogroup count windows separate 2020–2021 from 2022–2023.

Table C3: Local Economic Conditions by Baseline E-Commerce Exposure

Local variable	Baseline difference	Pre-2013 trend difference	Coverage	Interpretation
ZIP log establishments	+5.8%	-0.06%/year	39.8%	Different baseline level; different pretrend
ZIP log employment	+9.7%	-0.04%/year	39.4%	Different baseline level; similar pretrend
ZIP log AGI per return	+0.5%	+0.03%/year	39.6%	Similar baseline level; similar pretrend
ZIP log wages per return	-0.1%	-0.01%/year	39.6%	Similar baseline level; similar pretrend
ZIP log returns/exemptions	-2.4%	-0.03%/year	39.6%	Different baseline level; similar pretrend
County log establishments	-6.6%	-0.02%/year	99.3%	Different baseline level; different pretrend
County retail share	+0.15 pp	-0.002 pp/year	99.3%	Different baseline level; different pretrend
County log payroll per worker	-0.6%	+0.00%/year	99.3%	Different baseline level; similar pretrend

Notes: Local levels are measured in 2013. Pretrend variables are annualized changes before 2013, using 2006–2013 where available and 2010–2013 for SOI variables. Log-variable coefficients are converted to percent differences associated with a 10 percentage point increase in 2006 tenant-mix exposure; share coefficients are percentage points. Standard errors are clustered by county.

Table C4: Bartik Event-Study Pretrend Quantification

Outcome	Mean pre-period coefficient	Average post-2013 coefficient	Ratio of pre to post	Post effect after pretrend adjustment	Percent change	N
Occupancy	-0.176	-0.600	0.293	-1.209	-101.5	28,072
Log NOI/sqft	-0.013	-0.020	0.670	-0.027	-34.592	25,120
Total establishments	-0.054	-0.701	0.077	-0.563	19.628	33,701
Retail establishments	-0.096	-0.339	0.283	-0.298	12.100	33,701

Notes: The table summarizes the Figure 3 event-study specification for the preferred 2006 exposure measure. Pre-period coefficients use 2009–2012; post coefficients average 2014–2019. The pretrend adjustment subtracts the linear pre-period slope projected into the post period.

Table C5: E-Commerce Exposure, Establishment Counts, and Employment

	2014–2019		2020–2021		2022–2023	
Outcome	Benchmark	County-by-year FE	Benchmark	County-by-year FE	Benchmark	County-by-year FE
Total establishments	-0.701*** (0.054)	-0.650*** (0.053)	-1.798*** (0.098)	-1.654*** (0.105)	-2.117*** (0.109)	-1.953*** (0.119)
Retail establishments	-0.339*** (0.021)	-0.330*** (0.022)	-0.834*** (0.038)	-0.810*** (0.040)	-0.893*** (0.040)	-0.867*** (0.043)
Non-retail establishments	-0.362*** (0.043)	-0.320*** (0.041)	-0.964*** (0.079)	-0.844*** (0.085)	-1.224*** (0.092)	-1.086*** (0.101)
Employment	-0.564 (1.315)	-0.426 (1.344)	-1.025 (2.176)	-0.752 (2.144)	-2.423 (2.274)	-1.993 (2.277)
Apparel	-0.209*** (0.013)	-0.205*** (0.014)	-0.493*** (0.024)	-0.484*** (0.026)	-0.553*** (0.026)	-0.539*** (0.028)
Food/drink	-0.011** (0.005)	-0.006 (0.006)	-0.058*** (0.008)	-0.045*** (0.009)	-0.078*** (0.009)	-0.060*** (0.010)
Health	-0.070*** (0.008)	-0.062*** (0.008)	-0.136*** (0.014)	-0.120*** (0.015)	-0.245*** (0.025)	-0.216*** (0.026)
Home furnishing	-0.009*** (0.002)	-0.009*** (0.003)	-0.029*** (0.004)	-0.029*** (0.004)	-0.037*** (0.004)	-0.037*** (0.005)
General merchandise	-0.005** (0.002)	-0.002 (0.002)	-0.023*** (0.004)	-0.016*** (0.004)	-0.012*** (0.004)	-0.006* (0.004)

Notes: Outcomes are establishment counts from Infogroup, plus the clean employment index built from fixed Infogroup employment-size values based on each establishment's latest positive 2007–2023 value. Coefficients report changes associated with a 10 percentage point increase in 2006 e-commerce exposure. Standard errors are in parentheses and clustered by property; *, **, and *** denote $p < 0.10$, $p < 0.05$, and $p < 0.01$. The benchmark specification includes property and year fixed effects. The county-by-year specification compares properties within the same county-year. Windows separately report the pre-COVID period, COVID years, and post-COVID years; establishment and employment data extend through 2023.

C.3 Property format and size



Figure C2: Broad Exposure Effects in Large-Property and Size-Weighted Samples

Notes: The figure reports average event-study coefficients for a 10 percentage point increase in 2006 e-commerce exposure. All panels use property and county-year fixed effects. The Green Street series restricts the sample to properties covered by Green Street. The square-footage series weights observations by rentable area. Vertical lines report 95 percent confidence intervals with standard errors clustered by property. Appendix Table C6 reports the corresponding numbers.

Table C6: Broad Exposure Effects in Large-Property and Size-Weighted Samples

Outcome/window	Full sample	Green Street-covered	Square-footage weighted
Occupancy, 2014-2019	-0.559*** (0.169)	-1.537* (0.926)	-0.412** (0.189)
Occupancy, 2020-2021	-1.300*** (0.296)	-5.680*** (1.886)	-1.360*** (0.334)
Log NOI/sqft, 2014-2019	-0.019** (0.008)	-0.019 (0.058)	0.003 (0.011)
Log NOI/sqft, 2020-2021	-0.012 (0.023)	-0.210* (0.124)	-0.020 (0.029)

Notes: Coefficients report the average effect of a 10 percentage point increase in 2006 e-commerce exposure. All columns use property and county-year fixed effects. The square-footage column weights observations by rentable area. Standard errors are clustered by property.

Table C7: Broad Exposure Estimates with Baseline Property-Type Controls

Outcome	Benchmark	County-by-year FE	+ Size/year	+ Mall/year	+ Tenant mix/year	+ Baseline quality/year	N
Occupancy, 2014-2019	-0.600*** (0.151)	-0.559*** (0.169)	-0.408** (0.179)	-0.531*** (0.169)	-0.584*** (0.201)	-0.591*** (0.166)	28,072
Occupancy, 2020-2021	-1.380*** (0.292)	-1.300*** (0.296)	-0.838*** (0.312)	-1.087*** (0.296)	-1.414*** (0.340)	-1.322*** (0.294)	28,072
Log NOI/sqft, 2014-2019	-0.020*** (0.007)	-0.019** (0.008)	-0.018** (0.008)	-0.018** (0.008)	-0.008 (0.009)	-0.018** (0.008)	25,120
Log NOI/sqft, 2020-2021	-0.031* (0.018)	-0.012 (0.023)	-0.006 (0.023)	-0.010 (0.023)	0.004 (0.026)	-0.010 (0.023)	25,120
Total establishments, 2014-2019	-0.701*** (0.054)	-0.650*** (0.053)	-0.590*** (0.044)	-0.656*** (0.057)	-0.524*** (0.048)	-0.666*** (0.051)	33,701
Total establishments, 2020-2021	-1.798*** (0.098)	-1.654*** (0.105)	-1.622*** (0.084)	-1.662*** (0.110)	-1.323*** (0.083)	-1.686*** (0.101)	33,701
Total establishments, 2022-2023	-2.117*** (0.109)	-1.953*** (0.119)	-1.998*** (0.103)	-1.973*** (0.125)	-1.371*** (0.094)	-1.986*** (0.115)	33,701
Retail establishments, 2014-2019	-0.339*** (0.021)	-0.330*** (0.022)	-0.248*** (0.018)	-0.325*** (0.021)	-0.372*** (0.023)	-0.337*** (0.022)	33,701
Retail establishments, 2020-2021	-0.834*** (0.038)	-0.810*** (0.040)	-0.653*** (0.030)	-0.790*** (0.037)	-0.912*** (0.042)	-0.820*** (0.041)	33,701
Retail establishments, 2022-2023	-0.893*** (0.040)	-0.867*** (0.043)	-0.716*** (0.032)	-0.849*** (0.040)	-0.982*** (0.045)	-0.877*** (0.043)	33,701

Notes: All estimates use 2006 tenant-mix exposure. Cells report event-study average effects of a 10 percentage point exposure increase. Financial outcomes are averaged over 2014–2019 and 2020–2021; establishment outcomes are averaged separately over 2014–2019, 2020–2021, and 2022–2023. Controls are predetermined baseline property characteristics, standardized and interacted with year indicators; missing baseline controls are retained with missing-value indicators.

C.4 Category contributions

Table C8: Category Contribution Weights in Broad Exposure

Category	Mean property share	Mean contribution to exposure	Approx. e-commerce share	Contribution to exposure variance
Sporting/hobby	0.026	1.020	39.795	0.242
Electronics	0.020	0.910	45.499	0.205
Furniture/home	0.020	0.825	40.552	0.192
Books	0.005	0.398	72.533	0.160
Apparel	0.041	1.284	30.690	0.160
Office/gift/misc.	0.036	0.487	12.853	0.041
Health/personal	0.098	1.117	11.400	0.029
General merchandise	0.085	0.804	9.402	0.015
Auto/building/garden	0.031	0.062	2.152	-0.003
Grocery/food stores	0.112	0.403	3.603	-0.018
Other retail	0.205	1.039	5.259	-0.024

Notes: The contribution weight is Cov(category exposure contribution, full exposure)/Var(full exposure), computed from the establishment-level 2006 Trepp-Infogroup panel using employee-size weights and 2019 Census ARTS e-commerce rates. The denominator includes all matched activity; nonretail and unclassified establishments remain in the denominator with zero e-commerce rates. Exact property-by-category contributions are not saved in the canonical exposure file, so these weights are used to describe category drivers.

C.5 Reweighting the NOI estimates

The source sample consists of Trepp retail properties with positive 2013 NOI and nonmissing 2006 e-commerce exposure. We report a non-interacted aggregate benchmark and an interacted fit. The non-interacted calculation applies the county-year exposure coefficient directly to source-property exposure and baseline NOI before reweighting.

For the interacted fit, we estimate the following event-study regression for log NOI per square foot:

$$Y_{it} = \alpha_i + \gamma_{c(i)t} + \sum_{s \neq 2013} \left[\beta_s E_i + \theta_s'(E_i X_i) + \pi_s' X_i\right] \mathbf{1}\{t = s\} + \varepsilon_{it}.$$

The variable E_i is 2006 e-commerce exposure divided by ten, so coefficients correspond to a 10 percentage point increase in exposure. The fixed effects are property fixed effects α_i and county-year fixed effects $\gamma_{c(i)t}$. The vector X_i contains the baseline characteristics used to let the exposure effect vary across properties: standardized log rentable area, standardized baseline log NOI per square foot, standardized log baseline establishment count, standardized baseline retail share, indicators for Green Street coverage, mall or shopping-center format, and anchor or big-box tenant presence, an indicator for top-30 metro areas, indicators for major-metro distance bands of zero to ten miles, ten to twenty-five miles, and more than twenty-five miles from the metro center, and an indicator for being outside a CBSA. The regression also includes missing-value indicators for the four standardized continuous variables. Standard errors are clustered by property. Appendix Table C9 reports the coefficients.

For target population g, we form weights from cells defined by Census region, 2006 exposure bin, 2006 establishment-count bin, and 2006 NAICS-code-coverage bin. A target cell is retained if it contains at least three observed-NOI Trepp properties. If cell k contains N_k^g target properties and N_k^S source properties, each source property in the cell receives weight $w_{ig} = N_k^g / N_k^S$.

For window W, the fitted log-NOI effect for source property i is

$$\widehat{\ell}_{iW} = E_i \left(\widehat{\beta}_W + \widehat{\theta}_W' X_i\right),$$

where $\widehat{\beta}_W$ and $\widehat{\theta}_W$ average the relevant event-year coefficients over the window. The target-

population percentage effect is the baseline-NOI-weighted fitted change:

$$\widehat{\Delta}_{gW} = 100 \times \frac{\sum_{i \in S_g} w_{ig} NOI_{i,2013} \left[\exp(\widehat{\ell}_{iW}) - 1\right]}{\sum_{i \in S_g} w_{ig} NOI_{i,2013}}.$$

Table 3 reports the fitted percentage changes for the target populations discussed in the main text. Appendix Table C9 reports the underlying interaction coefficients.

Table C9: Interacted Broad Exposure Income Regression

Exposure coefficient	2014–2019	2019	2020–2021
Base exposure effect	-0.155** (0.078)	-0.279* (0.155)	0.211 (0.186)
Log rentable area	0.040*** (0.013)	0.063*** (0.022)	0.043 (0.027)
Baseline log NOI/sqft	0.049** (0.020)	0.112* (0.060)	-0.249*** (0.087)
Baseline establishments	-0.021 (0.014)	-0.035 (0.022)	-0.047 (0.038)
Baseline retail share	-0.022 (0.016)	-0.028 (0.026)	-0.054* (0.031)
Green Street-covered	0.036 (0.046)	0.000 (0.064)	-0.133 (0.126)
Mall or shopping-center format	-0.022 (0.020)	-0.025 (0.035)	-0.051 (0.056)
Anchor/big-box tenant	0.166** (0.077)	0.244 (0.150)	-0.053 (0.172)
Top-30 metro	-0.037 (0.036)	-0.062 (0.074)	-0.232*** (0.089)
Major metro, within 10 miles	0.049 (0.041)	0.088 (0.080)	0.180* (0.094)
Major metro, 10-25 miles	-0.002 (0.039)	0.010 (0.081)	0.073 (0.082)
Major metro, over 25 miles			
Outside CBSA	0.080 (0.065)	0.288** (0.143)	0.084 (0.172)
Observations		160,410	
Properties		25,120	
Property fixed effects		Yes	
County-year fixed effects		Yes	
Characteristic-by-year controls		Yes	

Notes: Coefficients are window averages from a year-by-year event-study regression for log NOI per square foot. The base row is the exposure coefficient when listed characteristics equal zero. Interaction rows report how the exposure coefficient changes with each characteristic. Continuous characteristics are standardized before interaction. The regression also includes listed characteristics interacted with year indicators, property fixed effects, and county-year fixed effects. Standard errors are clustered by property.

C.6 Detailed change-of-use categories

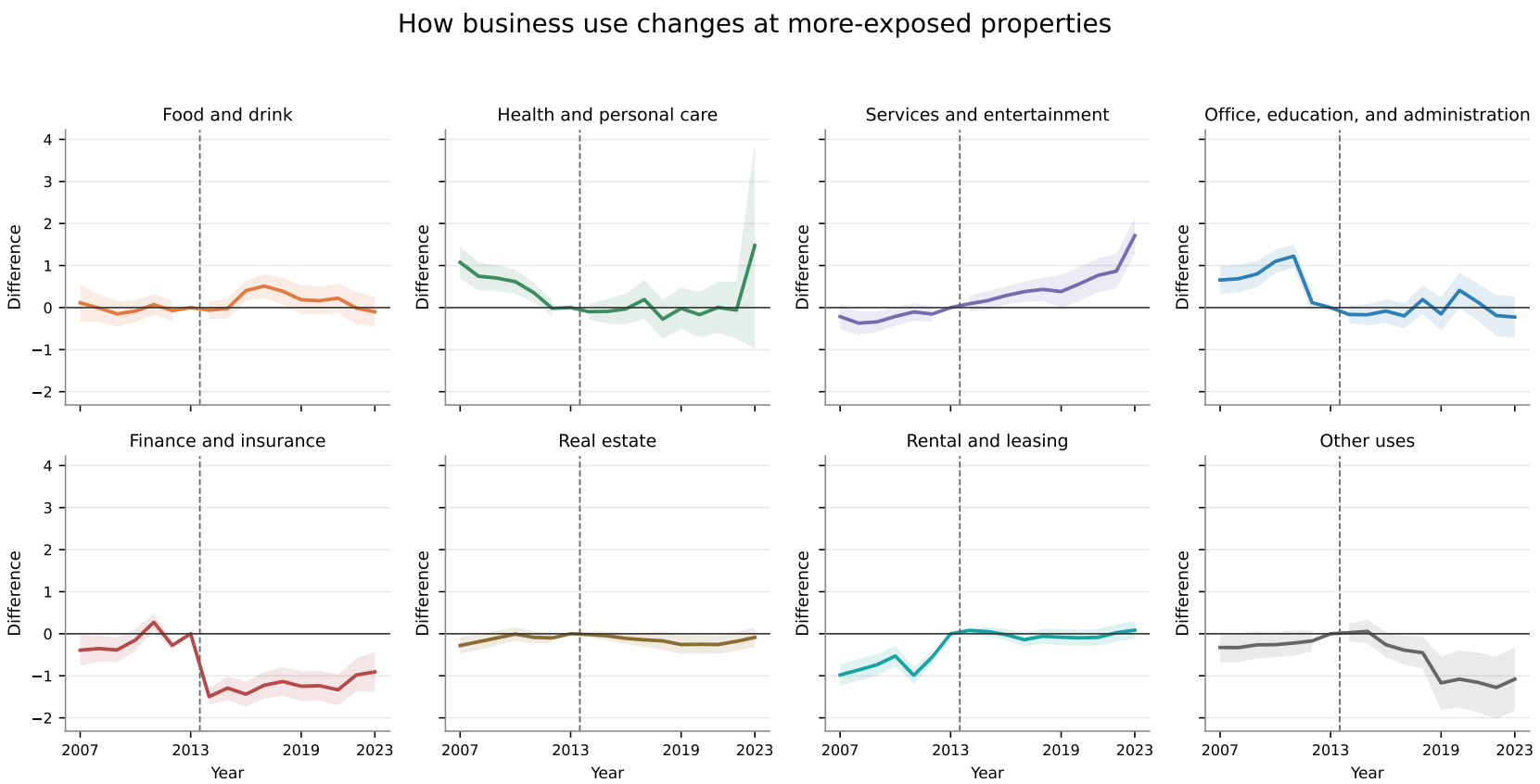


Figure C3: E-Commerce Exposure and Detailed Changes in Property Use

Notes: The figure reports event-study coefficients for a 10 percentage point increase in 2006 e-commerce exposure, relative to 2013. Each Infogroup establishment receives a fixed use classification based on the category reported most often over its observed history, with the latest category breaking ties. Outcomes are scaled per 100 businesses observed at the property in 2006. All specifications include property and year fixed effects. Shaded areas report 95 percent confidence intervals based on standard errors clustered by property.

C.7 Additional diagnostics

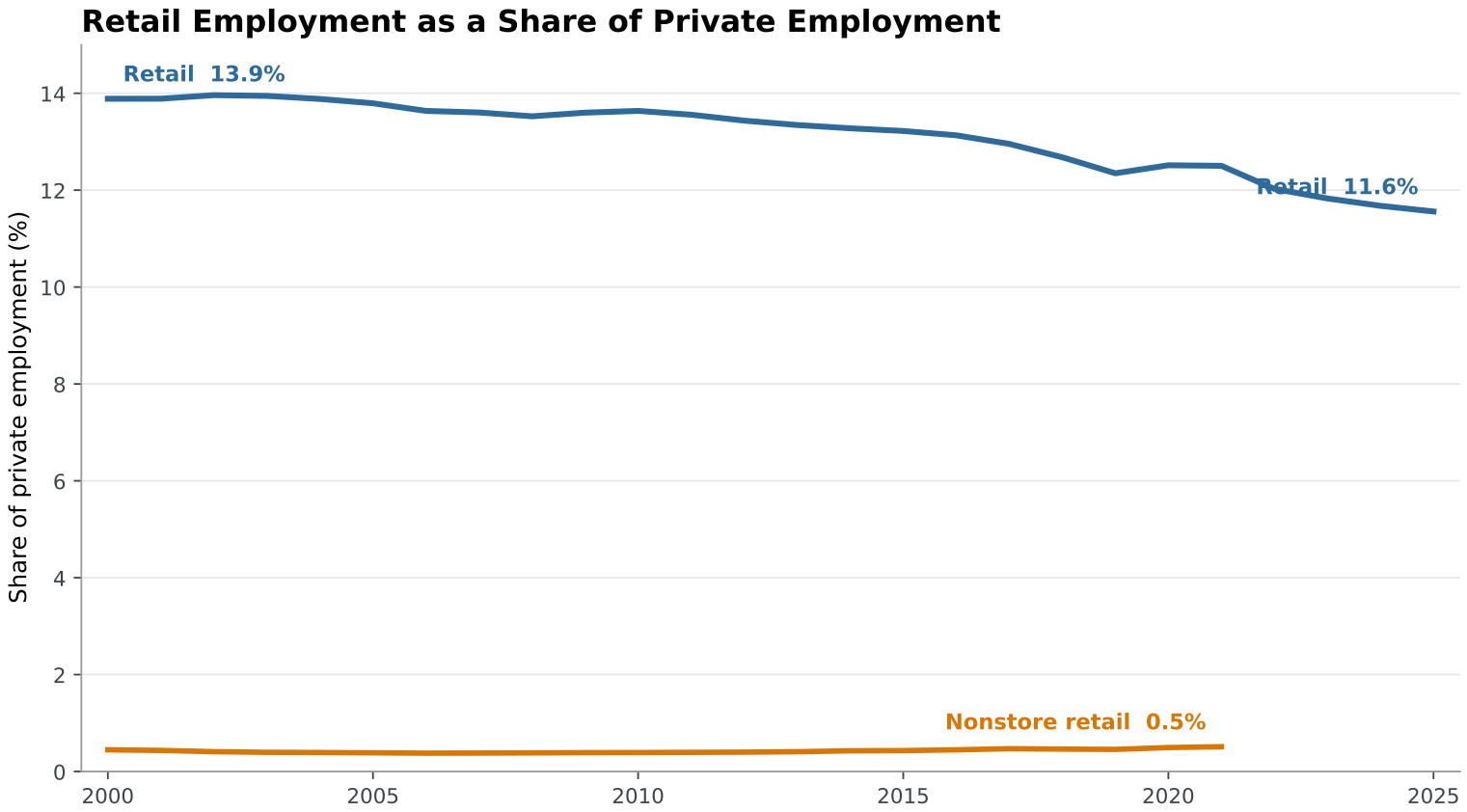


Figure C4: Retail Employment as a Share of Private Employment

Notes: The figure plots retail employment as a share of all private employment using the Quarterly Census of Employment and Wages. The nonstore comparison is shown only through 2021 because the 2022 NAICS revision eliminated nonstore retail as a separate subsector and reclassified establishments by product. Endpoint values are labeled.

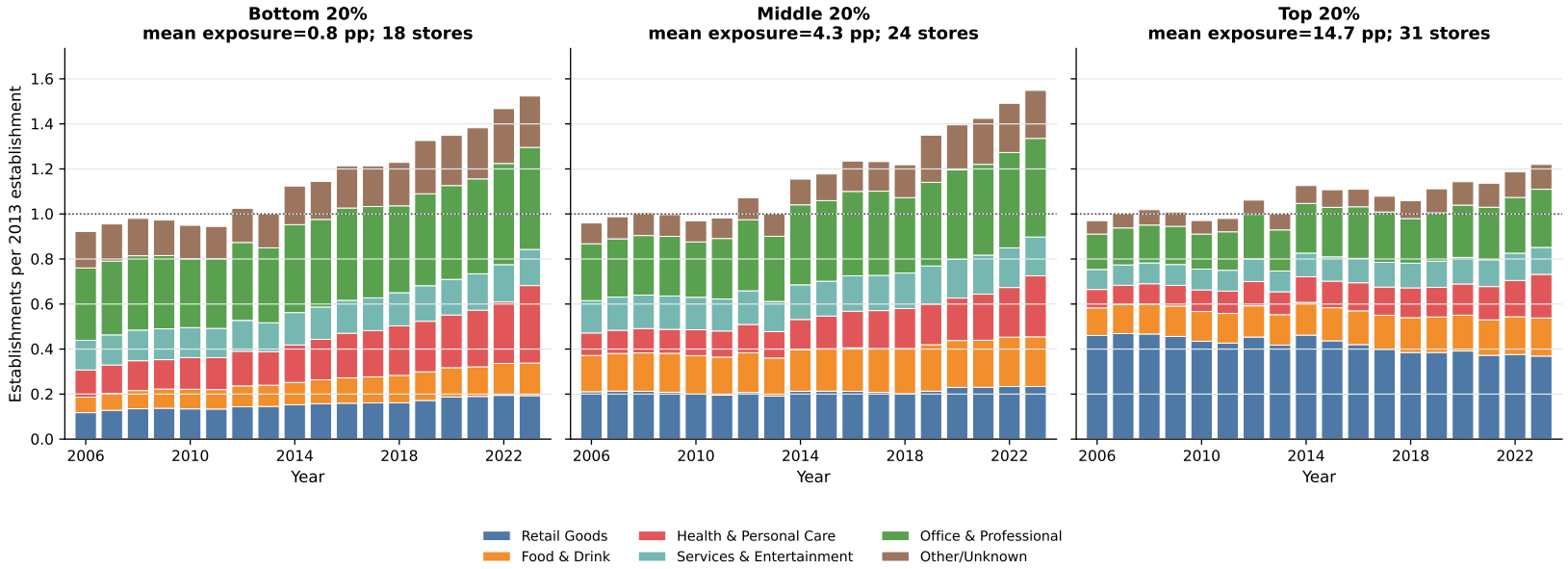


Figure C5: Tenant-Mix Reallocation by Baseline E-Commerce Exposure

Notes: Retail places are grouped by baseline e-commerce exposure measured from 2006 tenant mix. Each bar reports the number of establishments in a broad use category divided by that exposure group's total 2013 establishment count. The bars therefore show how each group's tenant mix evolves relative to its own pre-period size, not shares within a single year.

Table C10: Broad Exposure Effects on CMBS Loan-Status Outcomes

Outcome	Specification	2014–2019	2020–2021	Properties	Observations
Special servicing	Property and year FE	-0.399** (0.161)	0.028 (0.223)	33,667	395,242
Delinquency	Property and year FE	-0.281 (0.191)	0.254 (0.289)	33,667	395,242
Foreclosure / REO	Property and year FE	0.083 (0.115)	0.048 (0.194)	33,667	395,242
Delinquency / foreclosure / REO	Property and year FE	-0.285 (0.192)	0.447 (0.298)	33,667	395,242
Loan-status row observed	Property and year FE	-1.644*** (0.238)	-4.787*** (0.383)	33,667	538,672

Notes: Each cell reports the average event-study coefficient for a 10 percentage point increase in 2006 e-commerce exposure, with standard errors in parentheses. The omitted year is 2013. Loan-status outcomes are reported in percentage points and are measured in years with an observed Trepp loan-status row. The final row reports whether a loan-status row is observed in the full property-year panel. Standard errors are clustered by property.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D Verified Liquidations: Link, Validation, and Robustness

Relation to the broad exposure design

The two empirical designs answer different parts of the same question. The exposure design uses predetermined 2006 tenant mix and shows that locations more exposed to categories that move online subsequently lose establishments, occupancy, and NOI. The liquidation design begins only after a chain passes a complete-network documentary rule, an operating-identifier audit, a national-disappearance threshold, and the same high-e-commerce-category screen. It then assigns all of the chain's pre-event stores. Table 4 reports the bridge from baseline exposure to later tenant loss; the documented procedure in Appendix A explains why treatment in the liquidation design does not condition on which local stores the chain selected.

Event-time and robustness checks

The main figure displays the full pre-event path. The joint far-pre p-values are 0.144 for occupancy and 0.901 for log NOI, and neither path shows an economically comparable decline before the documented liquidation year. Because all stores in a qualifying network are assigned to treatment, this pre-trend check does not condition on the chain's local store-selection choice.

The match and sample checks point in the same direction as the main result. Requiring every store-to-property component to lie within 250 meters strengthens the year-one estimates and leaves the year-three occupancy estimate essentially unchanged. Restricting events to the pre-COVID period produces year-one estimates of -6.25 occupancy points and -0.138 log NOI, nearly identical to the full sample. Aggregating same-year shocks, not censoring at later liquidations, and excluding both the earlier local-exit and verified-liquidation universes from controls also leave the signs and magnitudes stable. No single liquidation event drives the result.

The primary sample gives each physical property one first verified shock. A robustness path that retains every core event–property link is smaller, as expected when later shocks and repeated properties are allowed to contribute, but still shows a clear post-liquidation decline. Adding RadioShack as a separately labeled near-liquidation event also leaves the primary path stable.

Financial position: current-LTV balance and pre-event paths

Appendix Table D1 assesses whether properties with different HPI-updated current LTVs were already observably different before liquidation. Conditional on original LTV, baseline DSCR, loan age and term, and original-contract categories within the same closure year, none of the continuously measured baseline characteristics differs at the 10 percent level. Joint tests for operating, finance-and-contract, and liquidation-shock characteristics yield p-values of 0.520, 0.135, and 0.991. Pre-liquidation occupancy and NOI paths are also jointly flat, with p-values of 0.497 and 0.771. These checks support comparability on observed levels and operating trends, but not quasi-random assignment: the county residential-HPI denominator may also capture local collateral or demand conditions.

Table D1: Predetermined Characteristics and Pre-Liquidation Paths by HPI-Updated Current LTV

	LTV coefficient (+10 pp) (1)	Standardized association (2)	p-value (3)	Observations (4)
Panel A. Predetermined levels				
Occupancy at $t = -2$	0.063	0.015	0.812	758
	(0.266)			
Log NOI/sqft at $t = -2$	−0.024	−0.071	0.195	764
	(0.018)			
Establishments at $t = -2$	1.788	0.059	0.214	799
	(1.437)			
Employment at $t = -2$	26.536	0.058	0.224	799
	(21.835)			
Log rentable area	−0.003	−0.007	0.895	799
	(0.025)			
Retail-space specificity	−0.033	−0.070	0.137	741
	(0.022)			
Amortization term, months	−0.031	−0.001	0.980	799
	(1.266)			
Securitized DSCR	0.010	0.028	0.539	357
	(0.016)			
Log original balance	0.003	0.005	0.905	786
	(0.022)			
Securitized occupancy	0.087	0.031	0.565	789
	(0.151)			
Liquidating-store jobs	−0.021	−0.002	0.963	799
	(0.459)			
Store-to-property distance, meters	−0.801	−0.006	0.896	799
	(6.098)			
Panel B. Joint tests for predetermined levels				
	Number of variables		Joint p-value	Number of properties
Core operating characteristics	6		0.520	695
Finance and contract characteristics	4		0.135	343
Liquidation shock and match	2		0.991	799
Panel C. Pre-liquidation operating paths				
	All-pre p	Far-pre p	Linear-trend p	Properties
Occupancy	0.497	0.809	0.573	745
Log NOI/sqft	0.771	0.593	0.941	749

Notes: HPI-updated current LTV is measured two years before liquidation as current scheduled loan balance divided by the securitization appraisal rolled forward with county FHFA residential house-price growth. Panel A reports separate same-closure-year regressions that condition on original LTV, baseline DSCR, exact loan age, exact original term, and original-contract categories. Column (2) expresses the association from column (1) as the change associated with a one-standard-deviation difference in current LTV, measured in standard deviations of the row characteristic. Panel B reports joint tests within the displayed domains. Panel C reports tests from the dynamic occupancy and NOI specifications, which additionally include property fixed effects, closure-year-by-event-time fixed effects, contract-category-by-event-time fixed effects, and baseline outcome and log-area controls interacted with event time. "All pre" covers years −4, −3, and −1; "far pre" covers years −4 and −3; year −2 is omitted. Standard errors are clustered by liquidation event in Panels A and B and by property and liquidation event in Panel C.

Exact-unit reuse after liquidation

The property-level establishment results allow a new business to enter anywhere within the matched property. We also construct a narrower exact-unit sample to see what happens at the former chain's own postal unit. We recover the raw Infogroup address for each matched establishment and parse explicit suite, unit, floor, and building markers. Each establishment identifier receives the unit reported most often over its observed history. We keep a baseline property–unit only when exactly one retail establishment occupies it two years before liquidation. This produces 762 focal units at 755 of the 2,302 liquidation properties.

Appendix Figure D1 compares changes at those focal units with changes at identified retail units in other properties in the same county. The original chain disappears rapidly after the liquidation begins. By year +3, another retail business is 11.87 percentage points more likely to occupy the unit, while a non-retail replacement is only 1.61 percentage points more likely. The probability that Infogroup places no matched business in the unit rises by 53.43 percentage points. The last outcome is not a direct vacancy measure: it records the absence of a matched Infogroup establishment at that exact unit. The result nevertheless confirms that literal conversion of the former store to a non-retail use is uncommon in the first several years.

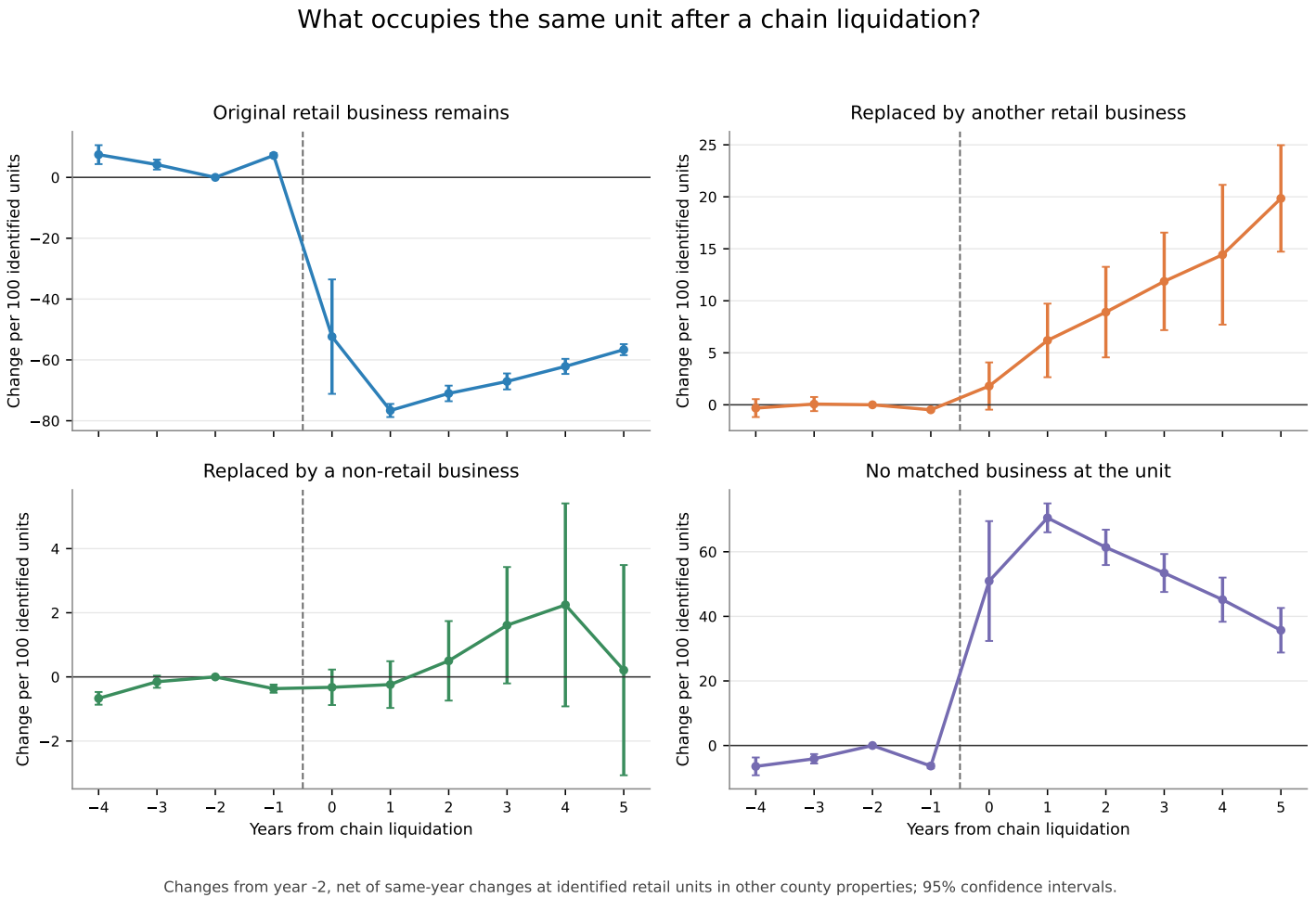


Figure D1: Reuse of the Former Chain's Exact Unit

Notes: The figure restricts the liquidation sample to properties where the focal chain has an explicit suite, unit, floor, or building marker and is the unique retail occupant of that normalized unit in event year −2. Each establishment receives its modal normalized unit over its observed history. Outcomes are changes from year −2, net of same-year changes at identified retail units in other properties in the same county. "No matched business" means that Infogroup does not place an establishment in the exact unit; it is not coded as physical vacancy. Bars report 95 percent confidence intervals.

Recovery after a material early loss

The recovery exercise begins with the same verified-liquidation sample and year-−2 baseline as the main event study. For each outcome, we identify properties whose largest loss between years −1 and +2 is economically material: at least 10 percentage points for occupancy or 0.10 log point for NOI per square foot. We then start the recovery clock in the year of that largest loss. A property has recovered once it has reversed at least 90 percent of the loss. The recovery estimate treats the end of a property's Trepp reports as the end of observation rather than as either recovery or failure.

Appendix Figure D2 and Appendix Table D2 report the results. Income rebounds for a subset of properties in the first two years and then changes little, while occupancy recovery continues. Three years after the trough, the estimated recovery rates are 47.6 percent for occupancy and 39.4 percent for NOI per square foot. The ordering is the same when the material-loss window ends in year +1 and when the material-loss thresholds are varied.

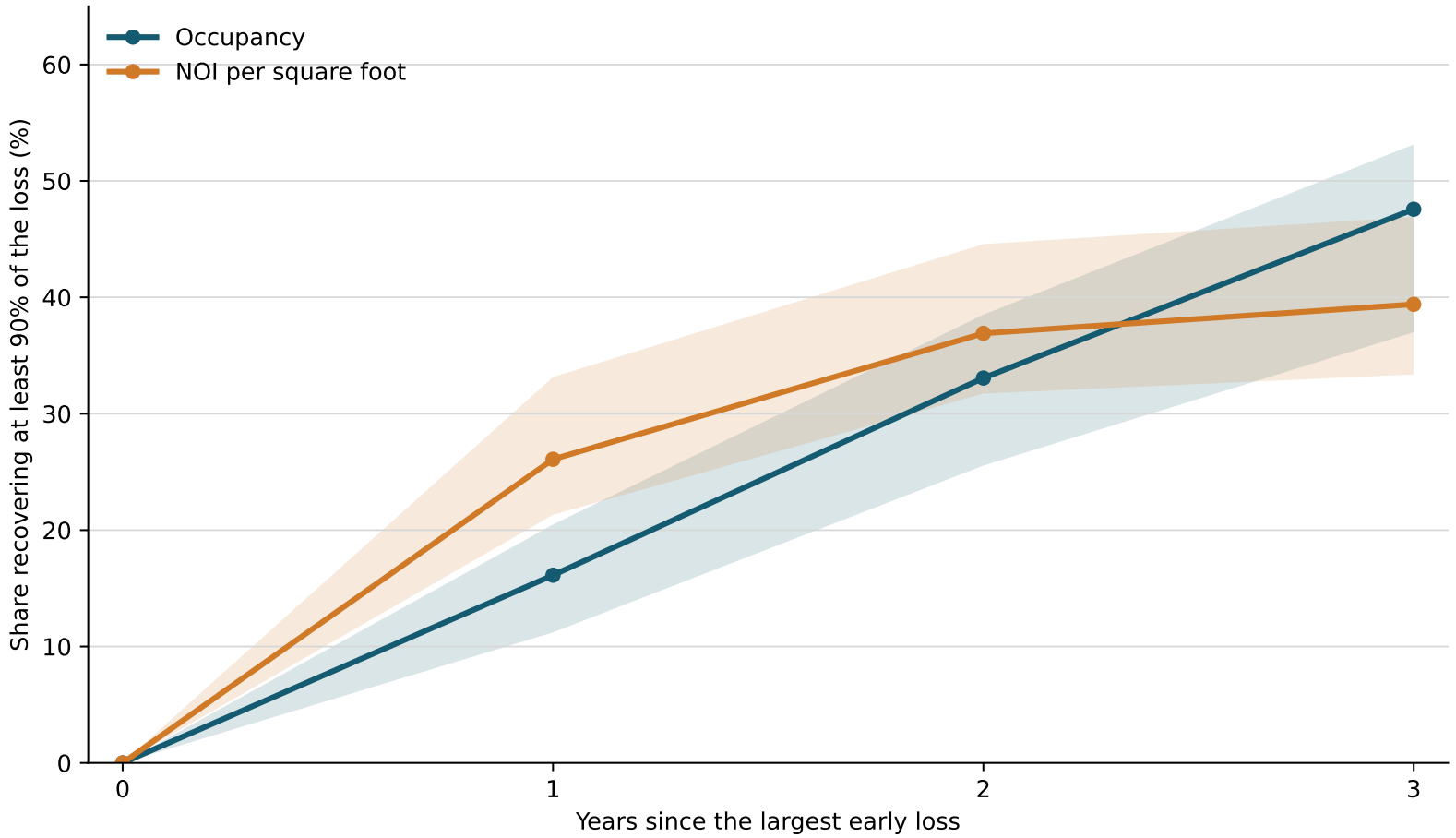


Figure D2: Recovery after a Material Early Loss

Notes: The figure reports cumulative recovery after the largest early decline in the verified-liquidation sample. A material early loss is an occupancy decline of at least 10 percentage points or a log-NOI-per-square-foot decline of at least 0.10 between event years -1 and $+2$, relative to year -2. Recovery occurs when at least 90 percent of the loss has been reversed. Shaded regions are 95 percent confidence intervals from resampling liquidation events.

Table D2: Recovery after a Material Early Loss

	Occupancy	Log NOI/sqft
Recovered by year 1 after trough	16.1%	26.1%
Risk set at year 1	186	230
Recovered by year 2 after trough	33.1%	36.9%
Risk set at year 2	104	123
Recovered by year 3 after trough	47.6%	39.4%
Risk set at year 3	60	76
Properties with a material early loss	263	463

Notes: The sample is the verified complete-liquidation sample. A material early loss is an occupancy decline of at least 10 percentage points or a log-NOI-per-square-foot decline of at least 0.10 between event years -1 and $+2$, relative to year -2. The trough is the largest loss in that window. Recovery occurs when at least 90 percent of the loss has been reversed. Entries are Kaplan–Meier cumulative recovery estimates; reporting ends after the last observed Trepp report.