Climate Regulation, Asset Stranding and Foreign Investors *

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Abstract

This paper shows that the effects of climate regulation depend critically on asset value. We study Boston's building climate regulation using transaction level data and a difference in differences design with multiple control groups. Following the policy, lower-priced properties become substantially less likely to trade: market inactivity rises by about 8 percentage points relative to control cities, prices grow 69 to 75 percent less among properties that still transact. These buildings are also more energy-intensive, suggesting that the assets most in need of investment are the most likely to become stranded. By contrast, transaction activity rises in higher-priced segments, where foreign purchases increase sharply and are disproportionately associated with investors from countries with stronger environmental performance, though this compositional pattern is less precisely estimated. The results reveal a previously overlooked channel of climate regulation: regulatory burdens can strand financially vulnerable assets while directing transactions and foreign capital toward more valuable properties.

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1 Introduction

Climate regulation can reshape real estate markets through both asset prices and the allocation of ownership. Existing research shows that transition risk is capitalized into expected returns and asset values (Ilhan et al., 2021, Bolton and Kacperczyk, 2021, Pástor et al., 2021). We examine a complementary adjustment margin: whether regulation changes market liquidity and the types of investors willing to own regulated assets. This margin is especially relevant in illiquid property markets, where some assets may reprice and continue trading while others may experience declining transaction activity. If these responses vary across the asset-value distribution, price changes alone may provide an incomplete measure of the regulation's incidence and its effects on capital allocation.

We study this question in the context of Boston's Building Energy Reporting and Disclosure Ordinance (BERDO), an energy reporting, disclosure, and action ordinance that initiated Boston's building-decarbonization framework, one of the earliest in the United States, affecting properties worth more than $180 billion.[1] We assemble a dataset linking three sources: annual emissions disclosures, transaction-level CoStar data covering more than 100,000 property sales, and a hand-constructed investor database linking buyers to ultimate parent entities, headquarters countries, and country-level environmental performance. Using a difference-in-differences design with multiple control groups, we show that BERDO generates a sharp divergence across the asset distribution.

At the bottom, regulatory burdens may be large relative to asset value. Market inactivity increases sharply. For bottom-decile properties, relative to control cities, market inactivity rises by about 8 percentage points; among the properties that still transact in the realized bottom tier, transaction volume stagnates and prices grow 69 to 75 percent less than the control-city counterfactual over the eight-year post-period. Value, not energy efficiency,

[1] Approximate assessed value of BERDO-regulated buildings, from the City of Boston Assessing Department (FY2025). Commercial and industrial parcels (Classes 3 and 4) account for $147.6 billion of Boston's $227.5 billion assessed real-estate tax base. Because BERDO also covers large multifamily, mixed-use, institutional, and exempt properties, the regulated asset base is roughly $180–200 billion.

predicts which lower-priced segments see reduced transaction activity. This inactivity margin is invisible in price data alone, meaning standard price-based measures systematically understate the true burden on financially vulnerable owners.

Higher in the distribution the pattern inverts. The price effect fades to zero and the volume gap narrows, market inactivity reverses as high-priced segments become more likely to trade, and the same shock attracts foreign capital. Boston's foreign purchase share in the top half more than tripled after BERDO, from about 13 percent in the three years before the shock to 49 percent in the first post-policy window, with inflows disproportionately from countries with stronger environmental track records. Capital from environmentally stronger countries flows toward the newly regulated market. Critically, this selective reallocation leaves the most financially vulnerable buildings, which are also the most energy-intensive, outside the ownership reallocation entirely.

These findings make four contributions. First, we identify a market inactivity margin through which climate regulation strands assets outright, a channel that operates independently of repricing. Second, we show this margin tracks financial vulnerability. Pre-policy price predicts stranding conditional on the earliest observed EUI, which becomes available after BERDO. Third, foreign purchase entry rises sharply after BERDO across control groups; the accompanying shift in buyer environmental performance is more tentative, concentrated in the higher-priced tiers and not significant in the pooled sample. Fourth, the excluded buildings are also the most energy-intensive. New inactive buildings use about 14 kBtu per square foot more than buildings that sold after BERDO.

The results extend beyond the Boston setting.[2] As building performance standards become increasingly widespread, our findings show that climate regulation operates through capital reallocation as well as asset repricing. By changing which assets continue to trade and which investors are willing to own them, regulation reshapes the allocation of capital

[2]Boston's BERDO is one of a growing number of building performance standards. Similar policies have been adopted in New York City (Local Law 97), Washington, D.C., Seattle, Denver, Cambridge (MA), Vancouver, and, through the recast Energy Performance of Buildings Directive, across the European Union.

across heterogeneous assets. Recognizing this channel is essential for understanding both the economic incidence and the environmental effectiveness of climate policy.

The remainder of the paper proceeds as follows. Section 2 situates our contribution within the climate finance, cross-border investment, and asset-stranding literatures. Section 3 lays out the mechanism and derives its empirical predictions. Section 4 describes our three-source database. Section 5 documents stranding at the bottom of the market and shows that the stranded segment has higher EUI and that market inactivity relates to pre-policy price rather than the earliest observed EUI. Section 6 documents the reallocation of ownership to foreign capital at the top. Section 7 concludes.

2 Related Literature

Our analysis contributes to three strands of the literature. First, we add to the rapidly growing climate finance literature, which studies how climate risks and climate policies affect asset prices, investor behavior, and capital allocation. Existing work shows that institutional investors price climate risk (Krueger et al., 2020), that investors respond to climate disclosures and sustainability information (Ilhan et al., 2023), and that transition risk is capitalized into expected returns and asset prices (Ilhan et al., 2021, Bolton and Kacperczyk, 2021, Pástor et al., 2021). Recent work also emphasizes how heterogeneous agents sort across locations in response to changing economic incentives (Couture et al., 2024). In commercial real estate, climate adaptation investments are capitalized into property values (Benetton et al., 2025). We identify a distinct adjustment margin. We show that climate regulation changes market participation itself, beyond its documented effects on prices or investor beliefs. Climate regulation eliminates buyers from financially vulnerable assets while simultaneously attracting foreign investors from environmentally stronger countries to higher-value properties.

Second, we relate to the literature on heterogeneous investors and cross-border capital allocation. Previous work shows that foreign investors respond to home-country conditions

and transmit information, preferences, and institutional experience across markets (Badarinza and Ramadorai, 2018, Cvijanović and Spaenjers, 2021, Dyck et al., 2019). We show that climate regulation reallocates ownership toward investors from countries with stronger environmental performance, particularly among higher-priced assets. The evidence points to a reallocation toward investors from environmentally stronger countries.

Third, we extend the literature on asset stranding and market liquidity (Ngai and Tenreyro, 2014). Most studies of transition risk focus on repricing (Bernstein et al., 2019, Murfin and Spiegel, 2020, Baldauf et al., 2020). We show that an equally important adjustment occurs through market inactivity. When regulatory burdens are large relative to asset value, price adjustment can give way to an extensive-margin response: buyer participation falls until trades stop occurring. Consistent with this market inactivity margin, the buildings that fail to receive new investment are also the most energy-intensive, suggesting that climate regulation can unintentionally exclude the buildings that most need new ownership and investment.

3 Mechanism and Empirical Mapping

Consider two otherwise similar office buildings in Boston, side by side, both subject to the new ordinance. Building A is in a secondary location, older, with thin cash flows. Compliance will cost $2 million over five years, roughly 40% of its annual income. The owner puts it up for sale. But buyers do the math: purchase price, the cost of bringing the building into compliance, and uncertain rent growth. The numbers do not work. No one bids. The building sits, unchanged, its energy performance frozen in time. It does not trade. It does not upgrade. It strands.

Building B is prime downtown real estate. The same requirements, but spread over a much larger income base. A foreign investor from Germany, accustomed to strict EU carbon rules, sees opportunity. She has managed retrofit programs before, knows the contractors,

understands the compliance timeline. She buys, retrofits, and repositions the asset, leasing at a premium. Value increases.

The same policy produces two outcomes. At the bottom, the burden is large relative to asset value, so buyers step back; the higher-EUI inactive segment, the least valuable properties, is locked out of the ownership-reallocation margin that may facilitate the investment regulation is meant to encourage. At the top, asset value absorbs the burden, and capital able to bear that cost steps in. Asset economics separates Building A from Building B. When the regulatory burden is large relative to asset value, the market simply stops functioning, and these buildings are left outside the ownership reallocation that regulation sets in motion elsewhere in the distribution. BERDO required reporting and periodic verification, and every five years an energy audit or a documented efficiency improvement, with retrofit costs falling only on buildings that chose the improvement pathway (Appendix A). Our results are consistent with this broad regulatory-burden mechanism.

This mechanism delivers two predictions that guide the empirical design. First, fixed components of regulatory burden can create a lower-tail market-inactivity region concentrated among the least valuable properties. We test this by sorting transactions into price tiers using predetermined year-specific price-per-square-foot cutoffs and examining whether market inactivity, price declines, and the resulting contraction in dollar volume are concentrated in the bottom of the distribution. Second, among assets that continue to trade, ownership shifts toward investors better able to manage regulatory exposure, specifically investors from countries with stronger environmental performance. We test this by examining whether foreign purchase share rises after BERDO and whether the composition of foreign buyers shifts toward investors from countries with stronger environmental performance, measured by the Environmental Performance Index (EPI).[3]

[3]A country-level measure of environmental outcomes published by Yale. We use the 2024 release (Wolf et al., 2024), standardized across countries. Both the cell-level EPI measure and foreign buyer status assignment are defined in Appendix A.

4 Data and Empirical Design

4.1 Data Sources and Matching

Our empirical strategy requires combining information on property transactions, energy performance, and investor characteristics. These come from three distinct sources.

The CoStar transaction database provides the primary source for all price, volume, and ownership analyses. It covers commercial property transactions in the Greater Boston area and in several comparison cities from 1995 through 2024. After removing under-contract records, transactions with missing price or date, and exact duplicates, the dataset contains more than 100,000 transactions. Each transaction record includes sale price, sale date, building size, property type, building class, construction year, submarket identifiers, and buyer and seller names. Table 1 reports summary statistics for the five variables underlying the paper's central results.[4]

The BERDO disclosure dataset contains annual building-level energy and emissions reports for all properties subject to BERDO. Reporting begins in 2015 and continues through the 2025 release, covering more than 20,000 building-year observations across roughly 5,800 properties. These data include square footage, address, sector classification, energy use intensity, and emissions intensity. For the energy performance analysis, we match BERDO buildings to CoStar transaction histories to create a separate matched sample with both energy disclosures and transaction records that are used exclusively for the analysis in Section 5.4.

To identify foreign buyers and characterize their home-country environmental performance, we construct a manually verified investor database. Starting from CoStar's buyer records, we identify each buyer's ultimate parent entity and confirm its country of domicile through corporate filings, company websites, and third-party data sources. The database covers more than 1,000 distinct buyers and sellers active in Boston and the control cities

[4]Appendix Table A1 reports additional descriptive statistics: log transaction volume, log foreign purchase volume, sale price, building size and age, ENERGY STAR score, and gross floor area.

over the sample period. A buyer is treated as foreign if its ultimate parent is headquartered outside the United States. We assign each foreign buyer an EPI score based on its home country, standardized to have mean zero and unit variance across all countries in the EPI dataset. This measure captures the regulatory environment to which each foreign investor is accustomed and serves as our proxy for the environmental performance of investors' home countries.

4.2 Outcomes and Units of Observation

A key feature of our empirical design is that each outcome variable is measured at a different level of aggregation, reflecting the nature of the question being asked.

The unit of observation in the price analysis is a transaction: one building, one sale price, one date. Prices are observed only when a trade occurs, so the transaction is the natural unit. For market inactivity, volume, foreign entry, and EPI, we aggregate transactions to the market-cell level (city, submarket, year, property type, building class).[5] Market inactivity requires this, since transaction-level data cannot show whether a segment records no transactions at all; we build a balanced panel that expands every cell to each year from 2009 to 2021 and includes zero-transaction cell-years. Volume, foreign entry, and EPI are estimated on cells that record at least one transaction, except the entry-conditional composition specification of Table 6, which further restricts to cells with at least one foreign purchase. The sample runs from January 2009 through September 2021 throughout, ending the month before a later regulatory amendment was signed so the post-period is free of a second shock.[6]

For energy use, the unit of observation is a building-year: each BERDO-regulated building reports its energy use once per year. We use this panel to characterize how energy performance differs across buildings by transaction history.

The key challenge, and contribution, of our database is linking these three sources. Trans-

[5]Foreign purchase volume is not one of the paper's headline variables; it uses the same construction and is reported in the appendix (Table F2).

[6]The 2021 amendment, discussed in Appendix A.

actions from CoStar are matched to BERDO disclosures by address and property characteristics. Investor information is merged to transactions by buyer and seller names. This integrated structure allows us to study prices, liquidity, ownership composition, and energy performance within a unified empirical framework, even though each variable is measured at a different unit. All variable definitions and construction details are in the Appendix.

4.3 Difference-in-Differences and Control Groups

We use a difference-in-differences framework that compares changes among Boston's regulated buildings with changes among comparable properties not subject to the mandate. Identification requires that Boston and the control groups would have followed similar trends absent BERDO. We study three external comparison groups and, for prices, an additional within-Boston comparison. Our strategy relies on the convergence of results across these designs: if the effect is real, it should appear regardless of which counterfactual we use. Control Group I, our baseline, uses five cities in states without building emissions mandates (Charleston, Nashville, Saint Augustine, San Antonio, and Savannah), comparable to Boston in property types, transaction frequency, and market structure, though they may differ from Boston in unobserved ways that affect real estate trends. Control Group III holds regional conditions fixed by comparing Boston to other Massachusetts cities not subject to BERDO (excluding Cambridge, which has its own mandate); these share Boston's state-level policies, labor markets, and weather, but may be affected by spillovers from Boston's regulation and are smaller markets with fewer transactions. Control Group IV, reported for price effects only, compares regulated large buildings to non-regulated smaller buildings within Boston, absorbing all Boston-specific shocks at the cost of comparing property types that differ systematically in volume and sale frequency.

Control Group II, a data-driven synthetic comparison, warrants more detail. We construct it from a weighted combination of donor cities, with weights inversely proportional to each donor's squared deviation from Boston's pre-policy path in log price per square foot (for

price, volume, and market inactivity) or log foreign-purchase volume (for the foreign-buyer outcomes), fit separately by price tier (Appendix A.2.7). Because the weights target one of these two measures rather than the aggregate fit across all outcomes, the pre-policy fit is not guaranteed and is assessed from the event studies below; the comparison may also be sensitive to the donor pool.

Our key identifying assumption is parallel trends, that Boston and the control groups would have followed similar paths absent BERDO, which we assess via event-study specifications reported alongside each result in Sections 5–6.

5 Stranding at the Bottom: Market Outcomes and Energy Use

This section examines how BERDO affected the lower end of Boston's property market. We first document three dimensions of stranding: an increase in market inactivity, a widening price gap, and a contraction in dollar transaction volume. We then characterize the energy performance of buildings that stopped trading after BERDO. Finally, we assess whether a building's transition into inactivity is more strongly associated with its pre-policy asset value or with its earliest available measure of energy use, which is observed after the policy took effect.

5.1 Market Inactivity

Market inactivity is our main stranding indicator. It measures whether a given market segment ceases trading altogether after BERDO. The difference-in-differences specification:

$$Inactivity_{ct} = \alpha + \beta \left(Boston_c \times Post\text{-}2013 \right) + X'_{ct}\gamma + \mu_c + \tau_t + \varepsilon_{ct}, \tag{1}$$

summarizes the average post-shock effect. The unit of observation is a market cell (c, s, t, p, b), a city-submarket-year-property type-building class group constructed from 2009 to 2021. $Inactivity_{ct}$ equals one if the cell records no transactions in year t. X_{ct} includes metro-level controls for changes in unemployment, log GDP, and log population. The specification also absorbs city, period, building-class, and property-type fixed effects.

The event study in Figure 1, estimated on monthly market cells grouped into 18-month bins, shows that the inactivity gap in the bottom decile is near zero in every pre-shock period and builds steadily thereafter, consistent with market inactivity accumulating gradually, as financially vulnerable owners exhaust their options to sell.[7] Panel (b) shows the gap is small and insignificant in the Full Sample, where the bottom-decile rise is offset by an increase in market activity in the top half of the market.[8]

Table 2, Panel A, reports a consistent pattern across all three control groups. Market inactivity rises by about 8 percentage points under Control Groups I and II. The Control Group III estimate is similar in magnitude but not significant.[9] Together, Panels A and B show the effect is large and significant from the Bottom 10% through the Bottom 50%, and small and insignificant only in the Full Sample, where the lower-tier rise is offset by an increase in market activity in the top half of the market. Appendix Table B1 shows this split directly. Estimating the effect on mutually exclusive rather than cumulative tiers, market inactivity falls by about 9 percentage points in the top half, where high-priced segments become more likely to trade after BERDO (an acceleration we return to in Section 6), but rises by roughly 8 percentage points in the two lowest tiers.

Three further checks vary the panel construction, the fixed effects, and the price-tier assignment, and the estimate is stable across all three. First, restricting the cell universe to

[7] Appendix Figure B1 plots market inactivity in levels for Boston and control cities, showing parallel pre-shock paths and a sharp post-BERDO divergence in the lower tail. Appendix Figure B2 reports the Bottom 25% and Bottom 50% tiers. We use 18-month bins to reduce sampling noise in thinly traded segments. While interior bins are exactly 18 months, the first and last bins extend to the sample boundaries, January 2009 and September 2021.

[8] The event-study path also holds across control groups (Appendix Figure B3).

[9] It remains insignificant when observations outside the 1st–99th percentile of log price per square foot are excluded (Appendix Table B4).

cells active before June 2013 and holding it fixed gives a flat pre-shock trend and a larger estimate (Appendix Table B2). Second, the Boston × Post coefficient is unchanged to three decimal places under city, submarket, or full cell fixed effects (not tabulated). Third, fixing each property's price tier ex ante, from historical pre-policy sales or a hedonic prediction where none exists, rather than classifying each transaction by its realized sale price, yields a similar bottom-decile inactivity estimate (Appendix Table B3).

Because BERDO is a single city-level policy, we also rank the market inactivity estimate against a placebo distribution built by reassigning the June 2013 shock to each other city, and separately to each other metropolitan area in our sample (Appendix Table G1). Boston's market inactivity estimate is the most extreme in the predicted direction.

The adjustment of initially low-value properties to BERDO is therefore identified most cleanly on the market inactivity margin, observed for every market cell whether or not it trades. We next examine prices, to document the discount at which trades that did occur were executed, and then volume, to see what the price gap and the trading freeze mean together in dollar terms. Both are measured only where transactions take place, so the bottom-tier estimates below describe the properties that continue to sell, not the full ex-ante cohort.

5.2 Price Effects

We now turn to prices, estimated at the transaction level. The difference-in-differences specification:

$$\log P_{ict} = \alpha + \beta\Big(Boston_c \times Post\text{-}2013\Big) + X_i'\gamma + \mu_c + \tau_t + \varepsilon_{ict}, \tag{2}$$

summarizes the average post-shock effect, where β measures the cumulative gap over the eight-year post-period rather than an annual rate of change, P_{ict} is price per square foot of transaction i in city c at time t, and all other notation is as in Equation 1. To trace the

evolution across the shock, we interact treatment with indicators for each 18-month bin:

$$\log P_{ict} = \alpha + \sum_{b \neq -3} \beta_b \Big(Boston_c \times \mathbf{1}\{Bin_t = b\} \Big) + X_i'\gamma + \mu_c + \tau_b + \varepsilon_{ict}, \tag{3}$$

where Bin_t indexes 18-month bins relative to the June 2013 shock and $b = -3$, the third pre-shock bin, is the base category used in estimation. To avoid results depending on the choice of a single reference period, reported and plotted coefficients recenter each estimated β_b against the average of the (up to three) pre-shock bin coefficients, with the omitted bin itself contributing zero. Here τ_b denotes a full set of 18-month-bin fixed effects, in place of the time fixed effects τ_t of the difference-in-differences specification. All other terms are as in Equation 2.

Appendix Figure C1 plots mean log price per square foot for Boston and Control Group I by tier, indexed to 100 in 2009, showing that Boston stagnated at the bottom of the market while control cities appreciated substantially. Figure 2 delivers two results, both estimated off flat pre-shock coefficients that support the parallel-trends assumption.[10] First, the bottom-decile price gap is large and does not fade over time, ranging from 33 to 84 percent below the control-city counterfactual across post-shock periods and widest at the end of the sample. Second, the gap is confined to the bottom decile. Panel (b) shows it is near zero in the Full Sample, as higher-priced properties, which have no gap of their own, enter the sample and dilute the bottom-decile effect.

Table 3, Panel A, confirms large, significant gaps across the three external control groups. Over the eight-year post-period, gaps range from 69 to 75 percent for the bottom decile.[11] The within-Boston comparison (Control Group IV) is a noisier benchmark given its smaller sample, but still shows a significant 62 percent gap, providing further confirmation of the

[10] Appendix Figure C2 reports the Bottom 25%, Bottom 50%, and Top 50% tiers. The event-study path is also robust across multiple control groups (Appendix Figure C3).

[11] Fixing each property's price tier ex ante, as in Appendix Table B3, does not reproduce the realized-tier price gap. This regression is estimated only on properties that continue to sell after the shock, about 40 Boston post-policy transactions in the frozen bottom decile, so it describes the composition of surviving sellers' resale prices rather than a value change for the full ex-ante cohort.

pattern.[12] Combining the Control Group I estimate in Panel A with the broader tiers in Panel B shows that the gap is largest in the Bottom 10% and shrinks toward zero in the Full Sample, as higher-priced properties with no gap dilute the bottom-decile effect.[13] Boston's nominal prices stagnated while Control Group I appreciated by roughly 91 percent over the sample period, leaving a difference-in-differences gap of about 73 percent (Appendix Figure C1).

5.3 Transaction Volume

Transaction volume combines the price and activity margins into a single measure of the dollars changing hands. We test parallel trends using an event-time specification, and flat pre-shock coefficients support the assumption.[14] The difference-in-differences specification:

$$\log(\text{USDVolume}_{ct}) = \alpha + \beta\Big(Boston_c \times Post\text{-}2013\Big) + X'_{ct}\gamma + \mu_c + \tau_t + \varepsilon_{ct}, \tag{4}$$

where USDVolume_{ct} is total dollar transaction volume in cell (c, s, t, p, b) in year t, and all other notation is as in Equation 1.

Appendix Table D1, Panel A, reports log-point gaps of 1.6 to 1.7 for the bottom decile across control groups, corresponding to 79 to 83 percent lower nominal volume relative to control cities.[15] Panel B shows the gap is strongest in the lower tail and weakens as higher-priced transactions enter the sample, indicating that the volume response is concentrated at the bottom of the distribution.[16] This volume gap reflects stagnation in dollar terms: while

[12]The bottom-decile price gap is also robust to excluding observations outside the 1st–99th percentile of log price per square foot (Appendix Table C3).

[13]Appendix Table C1 reports the same estimates on mutually exclusive tiers. The price decline is confined to the bottom decile, with estimates close to zero in every tier above and no offsetting movement of the kind seen for market activity.

[14]The event-study specification and results are in Appendix Figure D2.

[15]The log specification drops cell-years with no transaction. Re-estimating by Poisson on the balanced panel, with volume set to zero in inactive cell-years, gives a similar coefficient and the same tier gradient (Appendix Table D2). Fixing the tier ex ante is not reliable for volume: only about a dozen Boston post-shock transactions remain in the bottom decile, and the three largest account for half the cohort's dollar volume, so a coefficient here would largely reflect the concentrated activity of a few large properties.

[16]Total square footage transacted also falls significantly in Boston's bottom decile relative to Control

transaction volumes and prices both rose during this period in the control cities, Boston's bottom-decile dollar volume remained flat. In contrast, control cities' bottom-decile dollar volume grew roughly fivefold over the same period (Appendix Figure D1).

Given the lower market activity and lower prices, the dollar volume of properties traded falls sharply relative to control cities, showing that stranding at the bottom of the distribution produces a significant contraction in the value of trade, concentrated among low-value properties, where fixed regulatory burdens may be most consequential relative to asset value.

5.4 Energy Use of Stranded Buildings

Having established the contraction in market activity among lower-value properties, we next characterize the energy use of the buildings left outside the transaction market. This evidence is descriptive rather than causal: BERDO energy disclosures begin after the policy, and the matched sample contains only Boston buildings, with no comparison-city counterfactual.

We use the matched BERDO–CoStar sample described in Section 4 to measure energy use intensity (EUI), annual energy use per square foot in kBtu. We classify buildings as always inactive, new inactive, continued active, or new active, based on each building's CoStar transaction history before and after the shock. New inactive buildings, those that traded before BERDO but not afterward, are the stranded group. Appendix Table E1 shows that new inactive buildings have the highest mean EUI, while new inactive and always inactive buildings have lower mean Energy Star scores (a 1–100 efficiency rating, higher is better) than buildings that sold after BERDO. The EUI pattern also reflects property-type composition across groups, so we adjust for it directly by estimating

$$Y_{it} = \alpha + \beta\,\text{NewInactive}_i + \delta\,\text{AlwaysInactive}_i + X_i'\gamma + \tau_t + \varepsilon_{it}, \tag{5}$$

where Y_{it} is EUI or the Energy Star score for building i in year t, and X_i includes log building

Group I (Appendix Table D4).

square footage, property-type fixed effects, building class, building age, and submarket fixed effects. New inactive buildings are compared to buildings that sold after BERDO, the continued active and new active buildings that form the omitted category, with always inactive buildings retained via their own coefficient δ. Table 4 reports both coefficients for EUI and for Energy Star: buildings that traded before BERDO but not afterward have post-policy EUI about 14 kBtu per square foot higher and Energy Star scores about 5.5 points lower than buildings that sold after BERDO, conditional on observable building characteristics. Always inactive buildings, which never traded in either period, show no statistically detectable gap on either measure, while the significant gaps are concentrated among buildings that lost market access after BERDO.[17] Because energy performance is observed only from calendar year 2014 onward, this comparison cannot show that these buildings were already energy-intensive before BERDO or establish that BERDO caused them to stop trading.

5.5 Asset Value, Energy Use, and Subsequent Market Inactivity

We next examine whether subsequent market inactivity is associated with asset value or energy performance among buildings that transacted before June 2013. We compare pre-policy price with the earliest available energy use intensity in a joint predictive specification.[18] Pre-policy price predicts subsequent market inactivity, while the earliest observed EUI has no independently detectable association (Appendix Table E3). Because the two predictors are measured on opposite sides of the policy date, this result should not be interpreted as a causal horse race between initial asset value and initial energy inefficiency. It instead provides suggestive evidence that financial vulnerability is associated with stranding after conditioning on the earliest energy-use measure available.

[17] Appendix Table E1 reports the number of matched buildings and mean EUI by group. EUI is an absolute measure of energy use. Results are similar whether or not the reporting year covering calendar-2020 energy use is included (COVID-related energy consumption fell that year), and when the panel is extended through calendar-2021 energy use. Energy Star, which ranks buildings against their peers in the same period, is less sensitive to a common shock; the estimate is less precise, since Energy Star scores are unavailable for some buildings and property types. The alternative specification pooling all comparison buildings together is reported in Appendix Table E2.

[18] The earliest available EUI is from calendar-2014 energy use and is therefore measured after BERDO.

6 Foreign Investment

Section 5 showed that BERDO strands the bottom of the market while trading accelerates at the top. Section 6.1 shows that this additional trading is disproportionately foreign, the paper's main reduced-form result on ownership reallocation. Section 6.2 presents complementary, correlational evidence that these buyers come from countries with stronger environmental performance.

6.1 Foreign Entry

Consistent with the second prediction from Section 3, foreign buyers become more likely to purchase at the top of the distribution, even as BERDO depresses activity at the bottom. Figure 3 plots cumulative foreign purchase volume by tier: Boston's top-half inflows accelerate sharply after the shock while bottom-half volume grows more slowly than in Control Group I. By 2021, the top half had absorbed about 12.7 billion dollars in cumulative foreign purchases, compared with 1.5 billion in the bottom half. Foreign purchase share rises from 10 percent in the Full Sample to 13 percent in the Top 50% tier before the shock, to 45 to 50 percent across the same tiers in the first post-shock window, before moderating in the following two windows (Appendix Table F1).

We measure this in a regression framework. The outcome is Entry_{ct}, an indicator equal to one if cell (c, s, t, p, b) records at least one foreign purchase. The event-study specification:

$$\text{Entry}_{ct} = \alpha + \sum_{b \neq -3} \beta_b \Big(Boston_c \times \mathbf{1}\{Bin_t = b\} \Big) + X'_{ct}\gamma + \mu_c + \tau_b + \varepsilon_{ct}, \tag{6}$$

where Bin_t, the base category, and the pre-shock recentering are defined as in Equation 3, traces the pre- and post-policy path; flat coefficients in every pre-shock bin support the parallel-trends assumption, both by price tier (Appendix Figure F1) and across control

groups (Appendix Figure F2).[19] The difference-in-differences specification:

$$\text{Entry}_{ct} = \alpha + \beta\Big(Boston_c \times Post\text{-}2013\Big) + X'_{ct}\gamma + \mu_c + \tau_t + \varepsilon_{ct}, \tag{7}$$

summarizes the average post-shock effect on entry, where all notation is as in Equation 1.

Table 5 reports the results. The probability a cell records any foreign purchase rises by 15 to 18 percentage points across all three external control groups (Panel A), a robust reduced-form pattern supported by flat pre-trends and multiple control groups. Panel B shows the effect strengthens as the sample is restricted toward higher-priced assets, from 15.7 points in the Top 90% tier to 25.0 points in the Top 50% tier, consistent with foreign capital concentrating in the most valuable properties.[20]

6.2 Environmental Profile of Foreign Buyers

To test whether the foreign inflow reflects selective entry by investors from environmentally stronger countries, we examine whether the foreign buyers entering Boston come disproportionately from environmentally stronger countries. This section presents complementary, correlational evidence consistent with the mechanism in Section 6.1, conditional on entry in a smaller, selected sample, and carrying less causal weight than the entry result itself.

The main outcome is EPI_{ct}, the mean standardized EPI score of foreign buyers in cell (c, s, t, p, b), estimated on the subsample of cells with at least one foreign purchase. We assign each foreign buyer the EPI score of its headquarters country and aggregate to the cell level. The specification:

$$\text{EPI}_{ct} = \alpha + \beta\Big(Boston_c \times Post\text{-}2013\Big) + X'_{ct}\gamma + \mu_c + \tau_t + \varepsilon_{ct}, \tag{8}$$

[19] Control Group I is shown in Appendix Figure F1, panel (a).

[20] The same pattern holds in dollar terms across control groups (Appendix Table F2) and under price tiers fixed ex ante (Appendix Table F3). This specification is estimated on cell-periods with at least one transaction, so a cell-period with zero transactions is absent rather than coded as no entry. We also confirm the result on a fixed pre-policy cell universe and directly at the transaction level (Appendix Table F4).

isolates buyer composition conditional on entry, holding fixed that a foreign purchase occurred. A separate zero-filled version of this measure, which sets EPI_{ct} to zero for cells with no foreign purchase and so mechanically combines entry and composition, is used only for descriptive purposes (Table 1) and the placebo inference (Appendix Table G1).

Table 6 reports the results for Control Group I with equal cell weights. Among cells receiving foreign investment, the post-BERDO shift toward buyers from higher-EPI countries is concentrated in the upper price tiers, rising from a small, insignificant effect in the Full Sample to significant effects in the Top 90% and Top 75% tiers.[21] Boston's post-BERDO foreign buyers do come from high-EPI countries such as Canada, Germany, and Norway (Appendix Table F6), consistent with the upper-tier composition result. These are conditional composition estimates, not an independently causal result: BERDO may affect which market cells receive foreign investment in the first place, so they do not isolate the composition change within a fixed set of cells.

7 Conclusions

This paper shows that building-decarbonization policies can be self-undermining. The same policy shock that strands assets at the bottom draws foreign capital to the top, concentrating capital from environmentally stronger countries in the higher-value tiers while leaving the higher-EUI inactive segment outside that reallocation. These findings carry four implications for policy design.

First, regulatory burdens must be scaled to asset value. A uniform requirement imposes similar fixed costs on every building, but those costs are far larger relative to income for low-value properties. The result is not limited to higher costs: some properties become substantially less likely to trade. Compliance pathways that tie required investments to

[21] Control Group II and transaction-weighted estimates show the same concentration in the upper tiers (Appendix Table F5). Composition conditional on entry also strengthens in the upper tiers under price tiers fixed ex ante, though at Top 75% and below the entry-conditional sample becomes too thin to estimate precisely in Control Group I (not tabulated).

income potential or phase in deadlines based on asset value would keep these buildings in the market and preserve the trading channel.

Second, penalties for noncompliance should avoid accelerating stranding. If fines or retrofit mandates make low-value buildings uneconomical to own, owners have no incentive to invest and no ability to exit. Extended compliance timelines, partial exemptions for financially distressed properties, or public-private retrofit financing could prevent the liquidity freeze we document.

Third, the trading channel itself is a policy instrument. In our data, the buildings frozen out of the market are also the most energy-intensive, and selection into that freeze tracks asset value, with no independently detectable role for efficiency. Policies that facilitate transactions could restore market access to the buildings currently excluded from it. Policies that freeze trading foreclose that channel entirely.

Fourth, the influx of foreign capital at the top is a signal of where capable capital is willing to operate under regulation. Investors from environmentally stronger countries may be better positioned to manage the transition, though our evidence identifies their country of origin rather than demonstrated expertise directly. The challenge is that they concentrate where asset values are highest. Policy should extend their reach downward through subsidy programs, risk sharing arrangements, or compliance credits that make lower value assets attractive to capable investors.

The broader lesson is that building-decarbonization policies are only as effective as the markets they operate through. Ignoring asset economics in regulatory design risks breaking markets outright, well beyond raising compliance costs. Effective decarbonization requires keeping markets active across the full asset distribution and designing compliance pathways that make lower value assets viable for investors capable of managing the transition.

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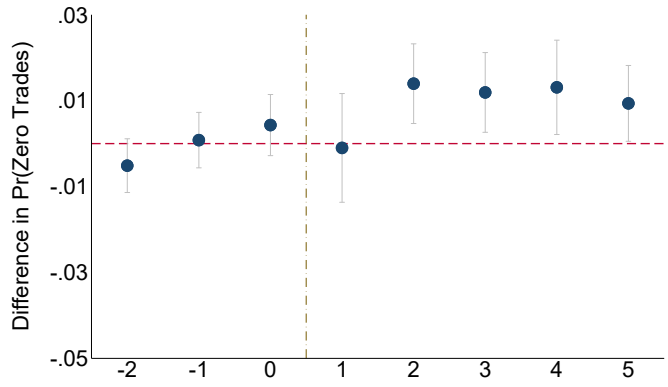
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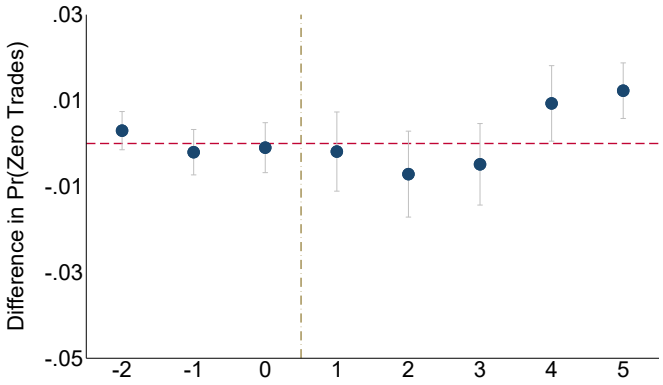
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Figures



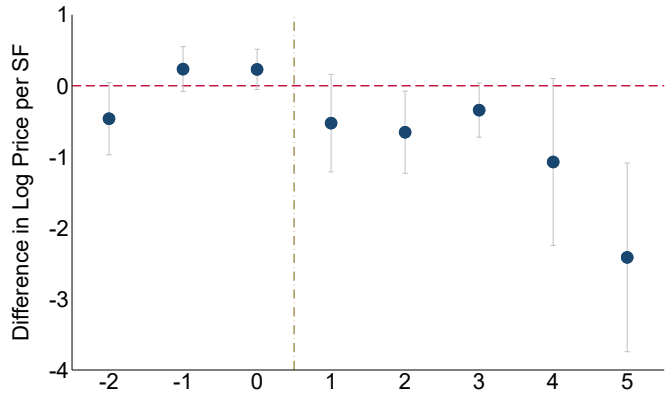
(a) Bottom 10%



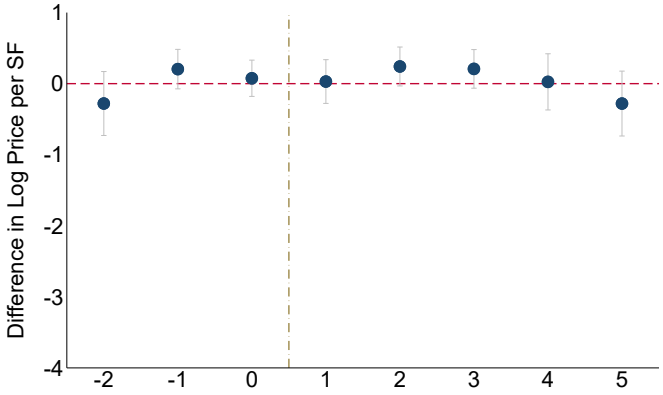
(b) Full Sample

Figure 1. Market Inactivity: Event Study by Price Tier

The shock is the adoption of Boston's BERDO in June 2013, and Boston is the treated market. The comparison is Control Group I. The dependent variable is an indicator equal to one when a cell defined by city, submarket, year, property type, and building class records no transactions, as defined in Equation 1. Event time is grouped into 18-month bins, estimated separately by price tier. The specification includes bin, city, property-type, and building-class fixed effects, as well as metro-year controls for changes in unemployment, log GDP, and log population. Bands show 95% confidence intervals based on heteroskedasticity-robust standard errors.



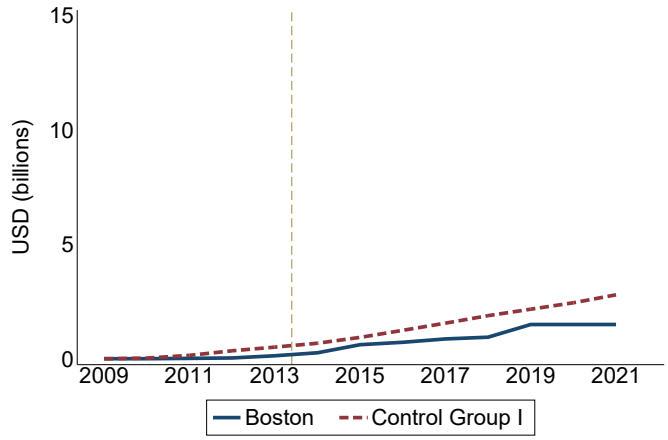
(a) Bottom 10%



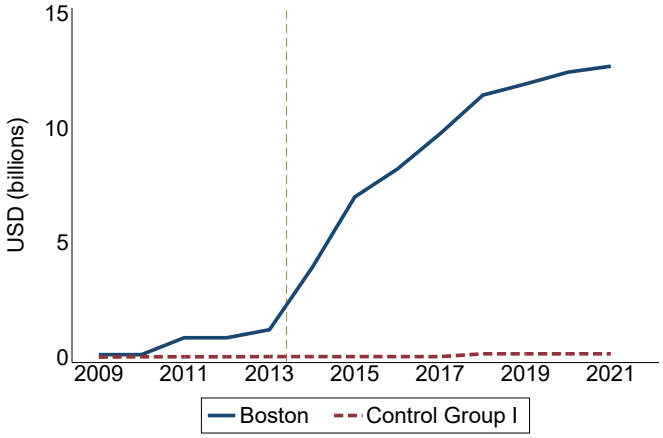
(b) Full Sample

Figure 2. Price: Event Study by Tier

Treatment effects on log price per square foot, from Equation 3 in event time with 18-month bins relative to the June 2013 shock, by price tier. Boston relative to Control Group I. Bin, city, property-type, and building-class fixed effects; building size and age, and metro-year controls for changes in unemployment, log GDP, and log population. Bands show 95% confidence intervals. Heteroskedasticity-robust standard errors.



(a) Bottom 50%



(b) Top 50%

Figure 3. Cumulative Foreign Purchase Volume by Price Tier

Cumulative foreign purchase volume (USD billions) from 2009, by price tier. Bottom 50% and Top 50% are mutually exclusive. Boston vs. Control Group I.

Tables

Table 1. Summary Statistics

Variable	Obs.	Mean	SD	Min	Max
Market Inactivity (=1 if no transaction)	1,568	0.800	0.400	0.0	1.0
Log Price per Square Foot	563	5.7	1.0	0.2	8.0
Foreign Entry (=1 if any foreign purchase)	1,596	0.039	0.195	0.0	1.0
Mean Standardized EPI Score of Foreign Buyers	1,596	0.06	0.33	-1.02	2.41
Energy Use Intensity (kBtu/sq. ft.)	1,972	77.8	60.0	0.0	437.9

Note: Boston statistics for January 2009 through September 2021, Full Sample tier, except Energy Use Intensity, which covers BERDO-regulated buildings matched to CoStar at the building-year level, 2014 through 2020, and is winsorized at the 1st and 99th percentiles. Market Inactivity, Foreign Entry, and Mean Standardized EPI Score of Foreign Buyers are each computed on their own full balanced market-cell panel, with the EPI measure additionally zero-filled for cells with no foreign purchase, so this statistic combines the entry and composition margins. Log Price per Square Foot is computed on the full transaction sample.

Table 2. Market Inactivity Effects

Panel A. Across Control Groups (Bottom 10%)

	Dependent variable: Market Cell with No Transactions		
	Control Group I	Control Group II	Control Group III
$Boston_c \times$ Post-2013	0.085***	0.078**	0.049
	(0.031)	(0.031)	(0.036)
Controls, Year and City FE	Yes	Yes	Yes
R-squared	0.051	0.049	0.096
Observations	7,518	15,120	2,408

Panel B. Across Tiers (Control Group I)

	Dependent variable: Market Cell with No Transactions		
	Bottom 25%	Bottom 50%	Full Sample
$Boston_c \times$ Post-2013	0.104***	0.070***	0.033
	(0.024)	(0.023)	(0.021)
Controls, Year and City FE	Yes	Yes	Yes
R-squared	0.055	0.057	0.059
Observations	9,072	9,660	10,066

Note: This table estimates Equation 1. The sample covers January 2009 through September 2021. The unit of observation is a market cell (city × submarket × property type × building class) on a balanced panel that keeps cell-periods with no transaction. Controls and fixed effects are metro-year changes in unemployment, log GDP, and log population, and property-type, building-class, city, and period fixed effects. Panel B uses Control Group I only. Heteroskedasticity-robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 3. Price Effects

Panel A. Across Control Groups (Bottom 10%)

	Dependent variable: Log Price per Square Foot			
	Control Group I	Control Group II	Control Group III	Control Group IV
$Boston_c \times$ Post-2013	-1.293***	-1.176***	-1.374***	-0.978***
	(0.305)	(0.268)	(0.331)	(0.311)
Controls, Year and City FE	Yes	Yes	Yes	Yes
R-squared	0.321	0.289	0.412	0.517
Observations	1,870	4,209	524	185

Panel B. Across Tiers (Control Group I)

	Dependent variable: Log Price per Square Foot		
	Bottom 25%	Bottom 50%	Full Sample
$Boston_c \times$ Post-2013	-0.839***	-0.192*	-0.043
	(0.219)	(0.114)	(0.090)
Controls, Year and City FE	Yes	Yes	Yes
R-squared	0.302	0.331	0.432
Observations	2,495	2,818	3,156

Note: This table estimates Equation 2. The unit of observation is a transaction. Controls and fixed effects are building square footage, building age, and property-type, building-class, year, and city fixed effects at the transaction level, and metro-year changes in unemployment, log GDP, and log population; city fixed effects are not applicable in Control Group IV, which also omits building square footage since treatment there is itself defined by a size threshold. Heteroskedasticity-robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 4. Energy Performance by Stranding Status

	Winsorized EUI (kBtu per Square Foot)	Energy Star Score
New Inactive	14.302**	−5.484*
	(5.992)	(2.968)
Always Inactive	0.129	0.767
	(5.239)	(3.258)
Controls, Year and Submarket FE	Yes	Yes
R-squared	0.632	0.262
Observations	1,907	1,519

Note: Estimates of Equation 5 for BERDO buildings matched to CoStar, calendar years 2014–2020; EUI is winsorized at the 1st and 99th percentiles. Controls are log building square footage, property-type fixed effects, building class, building age, year fixed effects, and submarket fixed effects. Regressions are weighted by building square footage. Buildings that sold after BERDO (continued active and new active buildings) are the omitted category. New Inactive is a transaction during 2005–May 2013 and none during June 2013–September 2021, Continued Active is transactions in both periods, New Active is none in the first period and at least one in the second, and Always Inactive is none in either period. Standard errors clustered by building in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 5. Foreign Entry at the Extensive Margin

Panel A. Across Control Groups (Full Sample)

	Dependent variable: Market Cell with at Least One Foreign Purchase		
	Control Group I	Control Group II	Control Group III
$Boston_c \times$ Post-2013	0.148***	0.180***	0.171***
	(0.043)	(0.045)	(0.048)
Controls, Year and City FE	Yes	Yes	Yes
R-squared	0.090	0.202	0.179
Observations	2,058	6,245	796

Panel B. Across Tiers (Control Group I)

	Dependent variable: Market Cell with at Least One Foreign Purchase		
	Top 90%	Top 75%	Top 50%
$Boston_c \times$ Post-2013	0.157***	0.173**	0.250**
	(0.054)	(0.077)	(0.112)
Controls, Year and City FE	Yes	Yes	Yes
R-squared	0.120	0.204	0.249
Observations	911	422	218

Note: This table estimates Equation 7. The unit of observation is a transacting market-cell period. Controls and fixed effects are as in Table 2, plus building age. Heteroskedasticity-robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 6. Foreign Buyer Environmental Profile: Composition Conditional on Entry

	Dependent variable: Mean Standardized EPI Score of Foreign Buyers		
	Full Sample	Top 90%	Top 75%
$Boston_c \times$ Post-2013	0.406	0.911*	1.631**
	(0.390)	(0.493)	(0.712)
Controls, Year and City FE	Yes	Yes	Yes
R-squared	0.267	0.303	0.446
Observations	170	107	70

Note: Each observation is a market-cell period with at least one foreign purchase. The dependent variable is the mean standardized EPI score of foreign buyers. Estimates compare Boston with Control Group I before and after BERDO. Regressions include city, period, property-type, and building-class fixed effects; cell-level mean building age; and metro-level economic controls. Robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Online Appendix

A Database and Variable Summary

A.1 BERDO Data

A.1.1 Data Overview

The BERDO disclosure dataset contains annual building-level energy and emissions reports for all properties subject to BERDO. Reporting begins in 2015 and continues through 2025. The regulated universe expands over time, reflecting both new construction and the lowering of the reporting threshold beginning with the 2021 cycle, when the ordinance was amended (BERDO 2.0) to cover more properties and to tighten the existing reporting and assessment obligations and introduce structured emissions standards and penalties for noncompliance by property type. As of the 2025 reporting cycle, the BERDO registry includes more than 5,800 properties, compared with roughly 2,500 in the initial 2015 cohort. Across all reporting years, the dataset comprises more than 20,000 building-year observations.

The BERDO data play two roles in the paper. First, they identify the regulated building stock and provide the building-level energy-performance measures used in the analysis of energy performance and stranding. Second, they supply the information needed to characterize how energy performance differs across buildings and evolves within the matched BERDO-CoStar sample. In particular, the BERDO disclosures report annual energy use intensity (EUI), which we define in the paper as annual energy use per square foot, measured in kBtu per square foot. In the regression analysis of Table 4, EUI is winsorized at the 1st and 99th percentiles within each property-type group to limit the influence of extreme outliers driven by reporting errors.

A.1.2 Variable Summary

The BERDO data include variables related to emissions performance and potential compliance exposure:

- EUI: annual reported energy use intensity, measured in kBtu per square foot, which is the main energy-performance measure used in Section 5.4;
- Energy Star score: EPA's 1–100 percentile ranking of a building's energy performance relative to similar buildings of the same property type nationally, where higher scores indicate better performance. A score of 50 represents median performance, and a score of 75 or higher indicates a top performer eligible for ENERGY STAR certification; and
- ReportedGrossFloorAreaSqFt: total building square feet, used both as a size control and as an analytic weight in the EUI analysis.

A.1.3 BERDO-CoStar Matching for Energy Analysis

To study energy performance and stranding, we construct a matched BERDO-CoStar sample that links BERDO buildings to CoStar transaction histories at the property level. We use this matched sample only for the energy-performance analysis, covering BERDO reporting years 2015–2021 (calendar years 2014–2020).

Each matched building is assigned to one of four mutually exclusive transaction-status groups based on its CoStar transaction history:

- New Inactive: a transaction during 2005–May 2013 and none during June 2013–September 2021;
- Continued Active: transactions in both periods;
- New Active: none in the first period and at least one in the second; and
- Always Inactive: none in either period.

This classification allows us to compare energy performance across buildings with different market trajectories after the policy shock and to test whether stranding is concentrated among buildings with weaker underlying economics or worse energy performance.

These four groups are mutually exclusive and exhaustive within the matched sample. Appendix Table E1 reports the distribution of buildings across groups. The primary contrast is between new inactive buildings and buildings that sold after BERDO, comprising continued active and new active buildings. Always inactive buildings serve as a benchmark for buildings with no market exposure throughout the sample period. New active buildings identify buildings that first appear in the transaction market after BERDO.

The stranding horse race (Table E3) compares pre-policy price against the earliest observed EUI, from calendar-2014, as predictors of later market inactivity.

A.2 CoStar Transaction Data

A.2.1 Data Coverage and Structure

The CoStar transaction database provides the primary source for all price, volume, and ownership analyses in the paper. The dataset covers commercial property transactions in the Greater Boston area and in several comparison cities from 1995 through 2024. After removing under-contract records, transactions with missing price or date, and exact duplicates, the dataset contains more than 100,000 transactions. Each transaction record includes:

- sale price and sale date,
- building size (gross square feet),
- property type (office, retail, industrial, multifamily, specialty, etc.),
- building class (A/B/C),
- construction year, from which we derive building age as the difference between sale year and construction year; transactions with a missing construction year, or a construction

year later than the sale date, are excluded from the regression sample,

- CoStar-defined submarket identifiers,
- buyer and seller names, and
- investor type (private equity, developer, etc.).

A.2.2 Treatment Assignment and Regulatory Status

Our empirical design treats June 2013 as the start of the post-shock period. BERDO was approved by the Boston City Council in May 2013, and the first annual reporting deadline it created for regulated buildings was originally set for May 2014 and subsequently extended to September 2014, so June 2013 is immediately upon approval and precedes the first compliance deadline. We therefore align our identification with the adoption date rather than the first compliance deadline. The regulation's scope and requirements were public from approval, so any price or liquidity response would begin then, well before the obligations became binding.[22]

The 2013 ordinance went beyond annual energy and water reporting and public disclosure. It also required periodic third-party verification of the reported data and, every five years, an Energy Action or Energy Assessment for buildings not already highly efficient. Under the action path a building had to document a qualifying improvement, such as a 15% reduction in energy use or a 15-point gain in ENERGY STAR score; under the assessment path it had to obtain a professional energy audit (ASHRAE Level 2 for buildings above 50,000 square feet) identifying measures, costs, savings, and payback periods, and file the results with the City. These obligations created administrative, professional-services, due-diligence, and, on the action path, retrofit costs, and placed each covered building within Boston's longer-

[22]The public record indicates that disclosure was viewed at the time as an initial step rather than a final one. The 2014 draft update to Boston's Climate Action Plan proposed evaluating a potential retrofit ordinance after 2019 using the first five years of BERDO assessments and actions (City of Boston, *Greenovate Boston: 2014 Climate Action Plan Update*, draft for public comment, November 2014), and the ordinance was in fact strengthened along these lines in the 2021 amendment (BERDO 2.0).

run decarbonization framework. Binding annual emissions limits, declining to net zero by 2050 with financial penalties for exceedances, were introduced only later, through the 2021 amendment, after our sample period.

Because the shock falls mid-year, the annual market-cell panels split calendar 2013 into pre-shock (January–May) and post-shock (June–December) halves, with both halves assigned the 2013 values of the annual metro-level macro controls, which are measured only at annual frequency, while the period fixed effects distinguish the two halves. Transaction-level specifications instead use an indicator for sales on or after 1 June 2013, together with calendar-year fixed effects.

In the main sample construction, treated properties are those coded as BERDO-regulated under our rule, namely properties with at least 35 units or at least 35,000 square feet. We classify these properties as exposed from June 2013 onward, even though actual compliance obligations did not become binding until the following year and implementation was phased in over time.

In the cross-city difference-in-differences specifications, the treatment indicator is $Boston_c$, which equals one for Boston and zero for all comparison-group cities. The coefficient of interest is β on $Boston_c \times$ Post-2013, where Post-2013 equals one from June 2013 onward (for the annual market-cell panels, from the post-shock half of 2013 onward). In the event-time specifications, calendar time is grouped into 18-month bins anchored on June 2013. Interior bins are exactly 18 months, while the first and last bins extend to the sample boundaries (January 2009 and September 2021). $Boston_c$ is interacted with an indicator for each bin except the third bin immediately preceding the shock, which serves as the base category in estimation; reported and plotted coefficients are then recentered against the average of the (up to three) pre-shock bin coefficients. These event-time regressions absorb 18-month-bin fixed effects in place of the difference-in-differences panels' time fixed effects, and otherwise use the same controls and fixed effects. In the within-Boston specifications using Control Group IV, the treatment indicator is an indicator for regulated properties, with non-regulated

below-threshold buildings as the comparison group.

A.2.3 Sample Definition and Cleaning

The core difference-in-differences analysis restricts the regression window to 2009–2021 in order to define a pre-policy period running from 2009 through May 2013 and a post-policy period from June 2013 through September 2021, ending the sample the month before the BERDO 2.0 amendment was signed to keep the post-period free of a second regulatory shock. Transactions are excluded if:

1. sale price is non-positive or missing, or

2. the transaction is flagged as not completed.

Property-type and building-class fixed effects are included throughout. Price regressions additionally control for building square footage and building age. Transaction volume, foreign entry, foreign purchase volume, and conditional EPI regressions control for building age but not square footage. Market inactivity regressions include neither, since the balanced panel does not carry building-level characteristics into inactive cell-periods.

A.2.4 Price Per Square Foot and Log Transformations

The main price variable in the analysis is price per square foot, defined as:

$$\text{PricePSF}_i = \frac{\text{SalePrice}_i}{\text{GrossSqFt}_i},$$

where SalePrice_i is the transaction price and GrossSqFt_i is the gross building area in square feet for transaction i. All price regressions use the natural log of this variable, $\ln(\text{PricePSF}_i)$, to reduce the influence of outliers and to interpret coefficients as percentage differences. For total transaction volume, we use $\ln(\text{USDVolume}_{cstpb})$ in the volume regressions. For foreign purchase volume, we use $\ln(\text{ForVol}_{cstpb} + 1)$, where the additive constant retains cell-years

with zero foreign purchase volume, which occurs when a cell records transactions but none by foreign buyers.

A.2.5 Price Tier Construction

We define price tiers using predetermined price benchmarks anchored to the Boston market before the policy shock and applied uniformly to both Boston and the control cities. The baseline thresholds are fixed calibration values chosen to approximate Boston's pre-policy 10th, 25th, and 50th percentiles of price per square foot. We then construct a time path for each cutoff by projecting the baseline threshold forward at a fixed annual growth rate calibrated to Boston's pre-policy median price growth. This procedure generates annual cutoff values that evolve mechanically from pre-policy Boston benchmarks and are applied uniformly across Boston and the control cities.

For each year, we assign each transaction to a tier by comparing its price per square foot to the corresponding year-specific cutoff from this predetermined cutoff path. This construction yields consistent price segments of the market over time, anchored to Boston's pre-policy distribution rather than to contemporaneous within-year ranks. In the stranding analysis, we use cumulative lower-tail samples, Bottom 10%, Bottom 25%, Bottom 50%, and the Full Sample, to assess whether adverse effects are concentrated at the bottom of the market. In the foreign investment analysis, we use cumulative upper-tail samples, the Full Sample, Top 90%, Top 75%, and Top 50%, to assess whether reallocation is strongest at the top of the market. Each analysis samples cumulatively from the end of the distribution where its own effect concentrates: market inactivity and price declines at the bottom, foreign capital and buyer composition shifts at the top. The cutoff path is predetermined, but a transaction is assigned to a tier by its own realized price, which BERDO itself can move. We conduct this fixed ex-ante tier robustness check for both market inactivity and foreign entry.

Let $\bar{p}^{k}_{b,t_0}$ denote the calibrated baseline threshold approximating the k-th percentile of

the Boston price-per-square-foot distribution in the pre-shock period, for $k \in \{10, 25, 50\}$. We project these thresholds forward at a fixed annual growth rate g calibrated to Boston's pre-shock median price growth:

$$c_t^k = \bar{p}_{b,t_0}^k (1+g)^{t-t_0}.$$

The resulting cutoff schedule is applied in every year to all transactions in Boston and the relevant control-group sample, so tier assignment is based on common price-per-square-foot thresholds rather than market-specific contemporaneous price distributions. The labels "Bottom 10%", "Bottom 25%", and "Bottom 50%" are anchored to Boston's pre-shock percentile cutoffs and remain approximate over time because the thresholds are fixed and applied uniformly across markets rather than re-estimated within each market-year.

A.2.6 Submarkets and Geographic Matching

CoStar markets and submarkets are commercial real-estate delineations defined by CoStar to reflect economically coherent property clusters (CoStar Group, 2025). These geographic definitions form the basis of the hedonic controls and the volume aggregations used in the empirical analysis. Boston's 67 submarkets are used directly. For comparison cities, we use CoStar-defined submarket structures to ensure comparable geographic units. This harmonized geography supports the difference-in-differences and event-time analyses.

A.2.7 Control Group Construction

Three cross-city control groups are constructed, plus a within-Boston comparison used for price. Control Group I consists of five untreated comparison cities: Charleston, Nashville, Saint Augustine, San Antonio, and Savannah. Control Group II is a synthetic comparison benchmark constructed to match Boston's pre-policy trajectory using weighted donor cities, as described below. Control Group III consists of Massachusetts cities excluding Cambridge,

which is subject to its own building emissions regulations. Control Group IV uses below-threshold non-regulated properties within Boston as an internal comparison group.

For the synthetic benchmark we fit donor-city weights separately for two groups of outcomes. For the price, transaction-volume, and market-inactivity regressions the weights are fit to Boston's pre-policy path of log price per square foot; for the foreign-entry, foreign-purchase-volume, and foreign-buyer EPI regressions they are fit to the pre-policy path of log foreign-purchase volume. In each case we restrict the data to the relevant price-tier sample and collapse to city-by-year values over the pre-policy period 2009–2012, dropping calendar 2013 as a transition year because the shock occurs in June while donor cities are observed only annually.

Let Y_{mt} denote the pre-policy annual value of the weighting outcome for that group in city m and year t, and let B denote Boston. For each donor city m in the donor pool we compute its standardized distance from Boston,

$$D_m = \sum_{t=2009}^{2012} \left(\frac{Y_{mt} - Y_{Bt}}{s_t} \right)^2, \tag{9}$$

where s_t is the cross-donor standard deviation of Y in year t, floored at one. We then assign normalized inverse-distance weights

$$w_m = \frac{D_m^{-1}}{\sum_{k \in \mathcal{D}} D_k^{-1}}, \qquad w_m \geq 0, \quad \sum_{m \in \mathcal{D}} w_m = 1, \tag{10}$$

and merge them into the full-period transaction and market-cell datasets: the price-based weights are applied to the price, volume, and market-inactivity regressions, and the foreign-volume-based weights to the foreign-entry, foreign-purchase, and EPI regressions. Both schemes exclude the same donor cities with commercial-building energy ordinances during the sample period. For the price-based scheme, we retain donors with observed prices in all four pre-policy years. For the foreign-volume scheme, we retain all otherwise eligible donors and assign zero foreign purchase volume to years with no foreign purchases. This

distinction reflects the measurement of the outcomes: prices are observed when transactions occur, whereas the absence of a foreign purchase is itself an observed outcome. The weights give greater influence to donors whose designated pre-policy path is closer to Boston's. We assess pre-policy fit directly using the corresponding event-study estimates.

A.2.8 Market Inactivity

Market inactivity is measured at the market-cell level. A cell defined by city c, submarket s, year t, property type p, and building class b is classified as inactive in a given tier and period if it records no transaction satisfying that tier's price restriction; in the Full Sample this is equivalent to recording no transaction of any kind. This cell-level indicator is the outcome variable in Equation 1.

To construct the balanced panel used in the regression, we expand all observed cells to every year from 2009 to 2021, assigning a market inactivity indicator of one to cell-years with no recorded transactions and zero otherwise, following standard practice in the commercial real estate literature (Fisher et al., 2003, 2007). Let $\mathcal{I}_{cstpb}$ denote the set of property-level transactions i in cell (c, s, t, p, b). For building characteristics, we compute cell-level averages:

$$\text{AvgChar}_{cstpb} = \frac{1}{N_{cstpb}} \sum_{i \in \mathcal{I}_{cstpb}} X_i, \tag{11}$$

where X_i denotes the relevant property characteristic for transaction i, and $N_{cstpb} = |\mathcal{I}_{cstpb}|$ is the number of transactions in the cell. For the volume, foreign-entry, foreign-purchase, and environmental-profile outcomes, the control vector includes this average for building age together with property-type and building-class indicators and metro-level controls for changes in unemployment, log GDP, and log population matched by city and year. The market inactivity regression uses only the indicators and metro-level controls, since the average cannot be formed for inactive cell-years.

The level figure in Appendix Figure B1 and the difference-in-differences specification use

the same annual market-cell definition (city, submarket, property type, and building class). The level figure plots the share of cells with no transaction, indexed to 2009, while the difference-in-differences specification splits 2013 at the June shock and adds controls. The event study in Figure 1 instead uses monthly cells grouped into 18-month bins.

A.2.9 Volume Measures

Transaction volume corroborates the market inactivity results by capturing the intensive margin of trading activity. We define USDVolume_{cstpb} as the total dollar value of all transactions in cell (c, s, t, p, b):

$$\text{USDVolume}_{cstpb} = \sum_{i \in \mathcal{I}_{cstpb}} \text{SalePrice}_i, \tag{12}$$

where $\mathcal{I}_{cstpb}$ denotes the set of property-level transactions in the cell. The main-text equations use USDVolume_{ct} because identification varies at the city-year level; p and b are absorbed into fixed effects, while s enters through the market-cell definition.

A.2.10 Foreign Purchase Volume

We define ForVol_{cstpb} as the total dollar value of purchases made by foreign buyers in cell (c, s, t, p, b):

$$\text{ForVol}_{cstpb} = \sum_{j \in \mathcal{F}_{cstpb}} \text{SalePrice}_j, \tag{13}$$

where $\mathcal{F}_{cstpb}$ denotes the set of foreign-buyer transactions in the cell. Building characteristics are averaged within the cell using Equation (11). The main-text equations use ForVol_{ct} because identification varies at the city-year level; p and b are absorbed into fixed effects, while s enters through the market-cell definition.

A.2.11 Foreign Entry

We define Entry_{cstpb} as an indicator equal to one if cell (c, s, t, p, b) records at least one foreign purchase:

$$\text{Entry}_{cstpb} = \mathbf{1}\{\text{ForVol}_{cstpb} > 0\}, \tag{14}$$

where ForVol_{cstpb} is defined in Equation 13. The main foreign-entry regression is estimated on market cell-periods with at least one transaction of any kind, so a zero denotes an active cell with no foreign purchase; cell-periods with no transaction at all are absent from the sample. The main-text equations use Entry_{ct} for the same reason as the other cell-level outcomes: identification varies at the city-year level, with p and b absorbed into fixed effects and s entering through the market-cell definition.

For Appendix Table F4, Panel A addresses this conditioning directly: it restricts to cells (city, submarket, property type, building class) active in at least one pre-June-2013 period, expands each surviving cell to every period from 2009 to 2021, and sets the entry indicator to zero for any period, including one with no transaction of any kind, that records no foreign purchase; this holds the universe of market segments fixed using only pre-shock information. Panel B estimates the same specification directly on transactions, asking whether a completed sale was more likely to have a foreign buyer, which avoids cell aggregation but conditions on a trade occurring.

A.2.12 Environmental Performance Index

For each aggregation cell, we compute the mean standardized EPI score of foreign buyers:

$$\text{EPI}_{cstpb} = \frac{1}{N^F_{cstpb}} \sum_{j \in \mathcal{F}_{cstpb}} \text{EPI}^{\text{std}}_j, \tag{15}$$

where N^F_{cstpb} is the number of foreign-buyer transactions in the cell. For descriptive purposes (Table 1) and the placebo inference (Appendix Table G1), we assign the outcome a value of zero for cells with no foreign transactions on a full balanced market-cell panel, so every

cell is retained in every period regardless of whether it transacted. This zero-filled measure mechanically combines whether foreign buyers are active in the cell with, conditional on activity, their environmental profile. The main regression specification (Table 6) instead restricts to cells with at least one foreign purchase, isolating buyer composition conditional on entry, as in Equation 8. The main-text equations use EPI_{ct} because identification varies at the city-year level; p and b are absorbed into fixed effects, while s enters through the market-cell definition.

A.2.13 Macro Controls

We include metro-level macro controls for unemployment, GDP, and population, matched by city and year. These controls are constructed in a separate metro-year file merged into the transaction and market-cell samples using data retrieved from FRED. In the empirical analysis, they enter in first differences rather than levels, with the differenced series created during the data-construction step prior to merging into the regression samples. The appendix and replication files document the construction procedure.

A.2.14 Construction of Pre-period City Trends

For the composition-and-trend robustness checks in Tables C2 and D3, we allow each city to have its own linear pre-policy drift. We construct these trends using only pre-policy observations from 2009 to 2012 and then extrapolate them to the full estimation window.

For each outcome Y_{ct}, where Y_{ct} is either log price per square foot or log transaction volume, we estimate the following pre-policy regression separately by city:

$$Y_{ct} = \alpha_c + \eta_c t + \upsilon_{ct}, \qquad t \in \{2009, 2010, 2011, 2012\}, \tag{16}$$

where t is a linear time index and η_c is the city-specific pre-policy slope. We then carry the estimated slope $\hat{\eta}_c$ forward to the full 2009–2021 sample and include it as an additional

control in the augmented robustness specification:

$$Y_{ct} = \alpha + \beta\,(Boston_c \times Post\text{-}2013) + X'_{ct}\gamma + \delta_{pt} + \kappa_{qt} + \lambda\,(\hat{\eta}_c \cdot t) + \mu_c + \tau_t + \varepsilon_{ct}, \qquad (17)$$

where δ_{pt} denotes property-type-by-year fixed effects and κ_{qt} denotes quality-by-year fixed effects, with quality defined using price-per-square-foot tiers defined by predetermined cut-offs.

This procedure allows each city to follow its own linear pre-policy trend before the BERDO shock and provides a direct check that the estimated Boston post-shock effect is not driven by differential local drift already present before treatment. In the price robustness table, these city-specific trends are added after the composition fixed effects in Column (3). In the volume robustness table, the same trend adjustment is applied after the composition fixed effects in Column (3).

A.2.15 Treatment-Level Placebo Inference

BERDO is assigned once, at the city level, so treatment varies across a single unit. Standard errors that treat within-city transactions or market cells as independent understate the sampling uncertainty of the treatment effect. We compute placebo permutation probabilities across cities for the four headline outcomes (market inactivity, price, transaction volume, and foreign entry) plus foreign purchase volume and the foreign-buyer EPI outcome, using the Bottom 10% price tier for market inactivity, price per square foot, and transaction volume, and the full sample for foreign entry, foreign purchase volume, and EPI. Holding the price-tier rule fixed, we reassign the June 2013 treatment date to each comparison city in turn, re-estimate the difference-in-differences coefficient, and rank Boston's estimate against the placebo estimates. We also run the test with the metropolitan area as the unit and with a placebo treatment date of June 2011. The exercise assumes the comparison cities are exchangeable with Boston absent BERDO, and because it keeps the realized-price tier

definitions it does not address selection into price tiers.

The comparison pool is fixed using pre-policy information only. A city enters if metro-level macro controls are available for it, if it records at least ten Bottom 10% transactions before June 2013 and Bottom 10% activity in at least two of the pre-policy years, and if it adopted no commercial-building benchmarking, audit, disclosure, or performance-standard ordinance during the sample period (this drops Austin, Pittsburgh, and Miami). This leaves Boston and eighteen comparison cities. For each placebo estimate we take the ratio of the coefficient to its standard error. The reported probability is the number of comparison units whose absolute ratio is at least as large as Boston's, plus one, over the number of units. The directional probability counts placebo t-statistics less than or equal to Boston's for price and volume, and greater than or equal to Boston's for market inactivity, foreign entry, foreign purchases, and EPI, using the same plus-one correction. In the directional ranking Boston is the most extreme unit for all six outcomes, by city and by metro, so the directional probability sits at the lowest value the pool sizes allow. On the two-sided ranking of $|t|$ Boston is again first for price, volume, foreign entry, foreign purchases, and EPI; for market inactivity it is second, as one comparison unit records a larger-magnitude effect of the opposite sign. The June 2013 pool is nineteen cities and twelve metropolitan areas. The June 2011 falsification truncates the sample at May 2013, so placebo units with no bottom-decile transaction in the resulting two-year window yield no estimate and are dropped, leaving seventeen to nineteen cities and eleven to twelve metropolitan areas depending on the outcome. Appendix Table G1 reports the results.

A.3 Investor Database

A.3.1 Construction and Coverage

To refine CoStar's buyer-country classifications and to assign environmental performance scores to foreign investors, we construct a manually verified investor database. Starting from the universe of foreign-flagged buyers in CoStar, we identify each buyer's ultimate parent

entity and confirm its country of domicile through corporate filings, company websites, and third-party data sources. The database covers all foreign buyers active in Boston and the control cities over the sample period. Appendix Table A2 summarizes the resulting buyer composition by origin and investor type.

A.3.2 Foreign Status

A buyer is treated as foreign if its ultimate parent is headquartered outside the United States. A transaction is classified as a foreign purchase if any participating buyer meets this criterion, including minority joint-venture partners, and the full sale price is attributed to foreign volume. This reflects our interest in the presence of foreign capital rather than its precise ownership share.

A.3.3 Environmental Performance Index

We assign each foreign buyer a standardized Environmental Performance Index score based on the EPI of its country of domicile. The EPI is a country-level index of environmental performance (Wolf et al., 2024). We standardize scores to have mean zero and unit variance across all countries in the EPI dataset. This measure captures the regulatory environment to which each foreign investor is accustomed and serves as the primary proxy for the environmental performance of investors' home countries in the analysis.

Table A1. Additional Descriptive Statistics

Variable	Obs.	Mean	SD	Min	Max
Panel A. Transaction and market-cell level, Boston					
Log Transaction Volume	314	17.6	1.7	11.7	21.8
Log Foreign Purchase Volume (USD)	1,596	0.728	3.599	0.0	21.594
Sale Price (USD Millions)	563	89.7	139.3	0.1	915.5
Building Square Footage (000s)	563	201.3	247.6	14.0	1,764.8
Building Age (Years)	544	71.4	42.9	0.0	200.0
Panel B. Building-year level, BERDO-regulated buildings					
ENERGY STAR Score	1,573	66.9	26.2	1.0	100.0
Reported Gross Floor Area (000s)	1,976	279.7	360.6	35.0	3,254.4

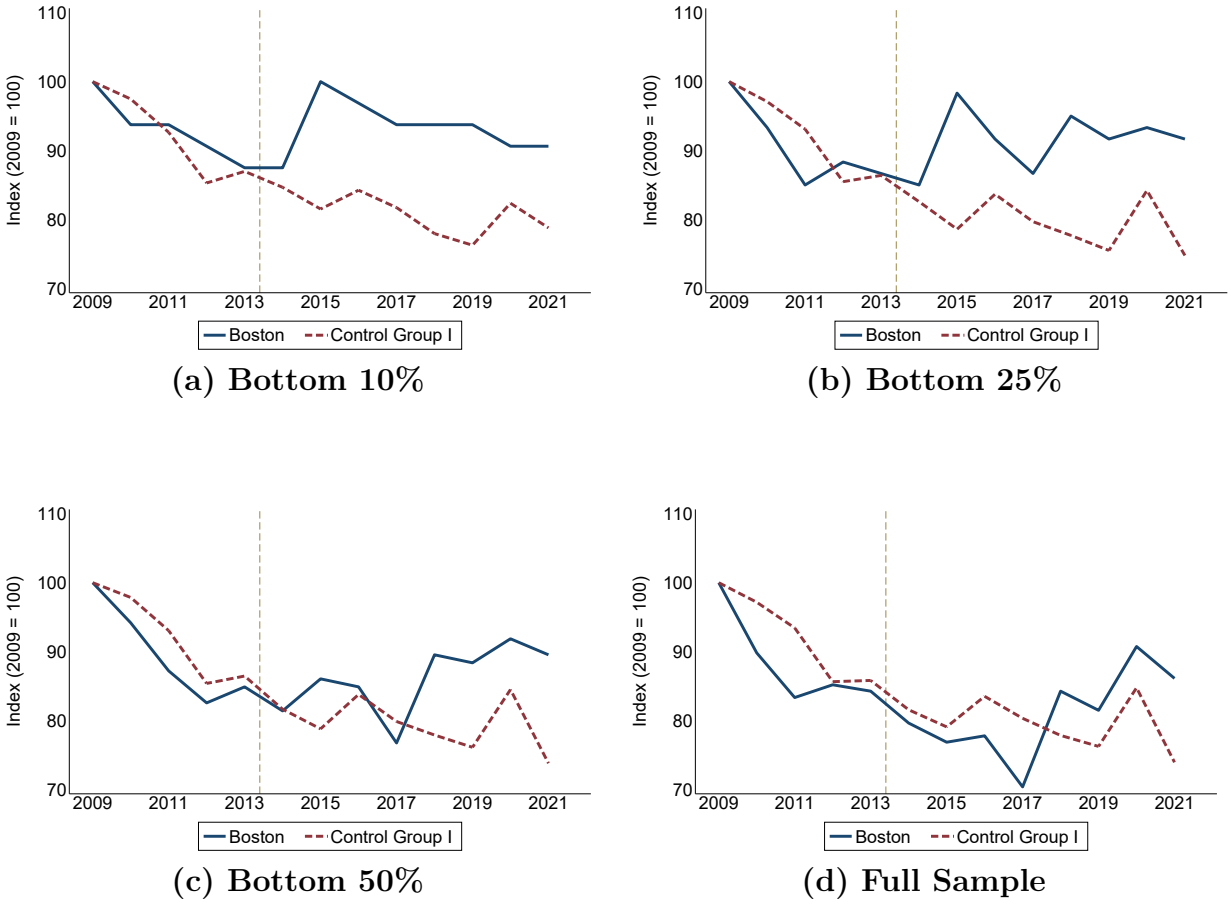
Note: Panel A reports Boston transaction and market-cell characteristics, January 2009 through September 2021; Log Transaction Volume is computed on cells with at least one transaction, and Log Foreign Purchase Volume on the same zero-filled balanced market-cell panel as the Foreign Entry statistic in Table 1. Panel B reports characteristics of BERDO-regulated buildings matched to CoStar, at the building-year level over 2014 through 2020.

Table A2. Investor Profiles

	Domestic	Foreign	Total
Number of unique buyers	250	48	298
Mean deal size (USD millions)	78.5	141.5	89.7
Median deal size (USD millions)	28.9	69.8	39.5
Investor type share of volume (%)			
Developer	33.0	47.7	
Investment manager and fund	32.0	32.3	
Public and listed REIT	25.1	3.0	
Pension and sovereign wealth fund	2.0	12.8	
Bank and insurance	2.0	0.8	
User and individual	4.8	1.9	

Note: This table reports investor profiles for treated Boston properties over January 2009 through September 2021. The hand-constructed investor database classifies buyers by origin and investor type. Investor type shares are computed using transaction volume in USD. The displayed categories exclude other and unclassified buyers and so do not sum to 100 percent.

B Online Appendix: Market Inactivity



(a) Bottom 10%

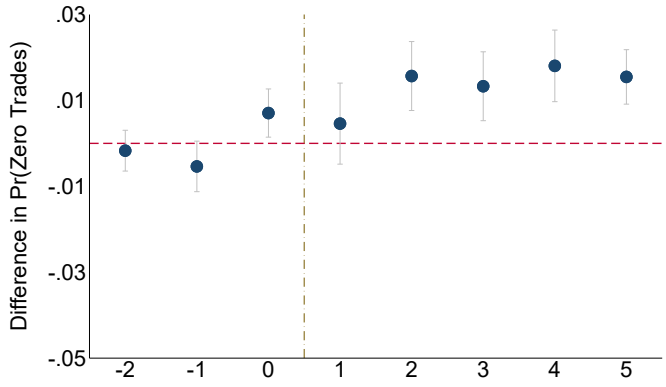
(b) Bottom 25%

(c) Bottom 50%

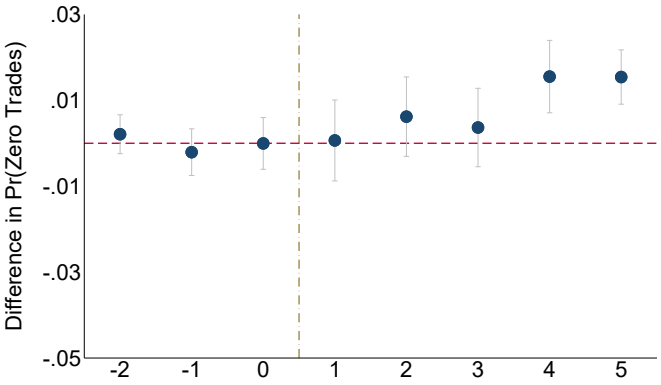
(d) Full Sample

Figure B1. Market Inactivity Index by Price Tier

Share of market cells recording zero transactions, indexed to 100 in 2009, by price tier. Boston vs. Control Group I.



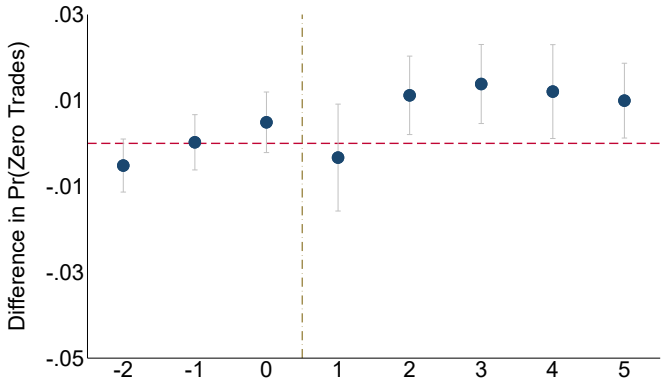
(a) Bottom 25%



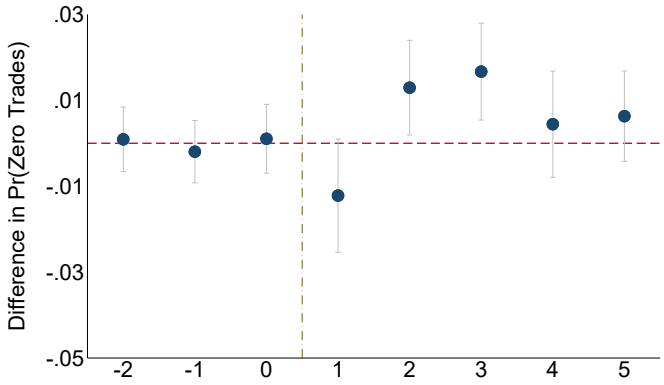
(b) Bottom 50%

Figure B2. Market Inactivity: Event Study by Price Tier, Additional Tiers

Treatment effects on market inactivity from Equation 1 in event time with 18-month bins relative to the June 2013 shock, Bottom 25% and Bottom 50% price tiers. Boston relative to Control Group I. Controls and fixed effects as in Figure 1. Bands show 95% confidence intervals. Heteroskedasticity-robust standard errors.



(a) Control Group II



(b) Control Group III

Figure B3. Market Inactivity in the Bottom 10%: Control Group Robustness

Treatment effects on market inactivity from Equation 1 in event time with 18-month bins relative to the June 2013 shock, Bottom 10% price tier. Boston relative to Control Groups II–III. Bands show 95% confidence intervals. Controls and fixed effects as in Figure 1. Heteroskedasticity-robust standard errors.

Table B1. Market Inactivity Effects Across Mutually Exclusive Price Tiers

	Dependent variable: Market Cell with No Transactions			
	Bottom 10%	10–25%	25–50%	Top 50%
$Boston_c \times$ Post-2013	0.085***	0.088***	-0.034	-0.085**
	(0.031)	(0.030)	(0.032)	(0.037)
Controls, Year and City FE	Yes	Yes	Yes	Yes
R-squared	0.051	0.041	0.047	0.094
Observations	7,518	4,004	1,960	1,400

Note: This table estimates Equation 1 for Boston relative to Control Group I on mutually exclusive price tiers rather than the cumulative tiers of the main text. Controls and fixed effects are as in Table 2. Heteroskedasticity-robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table B2. Market Inactivity: Alternative Cell Universes

	Dependent variable: Market Cell with No Transactions		
	(1)	(2)	(3)
$Boston_c \times$ Post-2013	0.138**	0.364**	0.111*
	(0.058)	(0.180)	(0.063)
Controls, Year and City FE	Yes	Yes	Yes
R-squared	0.072	0.140	0.077
Observations	2,982	896	4,144

Note: This table re-estimates the Bottom 10% market-inactivity specification of Table 2 for Boston relative to Control Group I under alternative definitions of the cell universe, holding the dependent variable, controls, and fixed effects fixed. Column (1) restricts the panel to cells with at least one within-tier transaction before June 2013. Column (2) additionally requires at least two such pre-June-2013 periods, which leaves two Boston cells. Column (3) uses the full window but excludes cells observed transacting within the tier in only one period. Heteroskedasticity-robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table B3. Market Inactivity: Fixed Ex-Ante Price Tiers

	Dependent variable: Market Cell with No Transactions			
	Bottom 10%	Bottom 25%	Bottom 50%	Full Sample
$Boston_c \times$ Post-2013	0.088***	0.017	0.019	0.036
	(0.032)	(0.022)	(0.023)	(0.023)
Controls, Year and City FE	Yes	Yes	Yes	Yes
R-squared	0.052	0.054	0.053	0.051
Observations	6,832	8,274	8,582	8,820

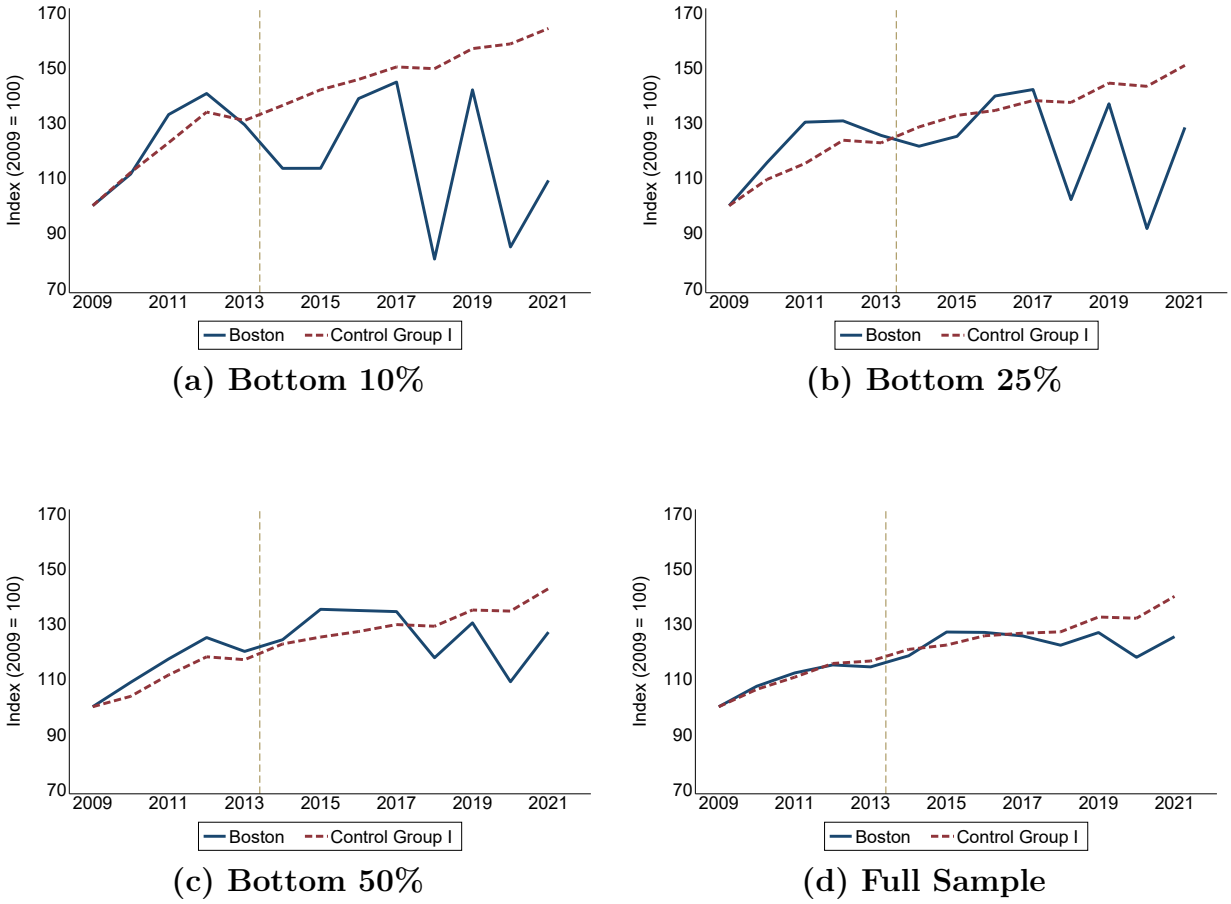
Note: This table re-estimates the market-inactivity specification of Table 2 for Boston relative to Control Group I with each property's price tier fixed ex ante rather than each transaction classified by its realized sale price. The tier is taken from the property's last sale before June 2013, using the full transaction history, or where none exists from a hedonic prediction fitted on pre-June-2013 sales and evaluated at 2012, restricted to buildings that existed before 2013. About half of Control Group I properties are classified from an actual sale. The fallback treats each building's structural characteristics as predetermined. Controls and fixed effects are as in Table 2. Heteroskedasticity-robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table B4. Market Inactivity: Excluding Outliers

	Dependent variable: Market Cell with No Transactions		
	Control Group I	Control Group II	Control Group III
$Boston_c \times$ Post-2013	0.094***	0.087***	0.053
	(0.031)	(0.030)	(0.035)
Controls, Year and City FE	Yes	Yes	Yes
R-squared	0.052	0.053	0.095
Observations	7,448	14,896	2,408

Note: This table estimates Equation 1 for the Bottom 10% segment across Control Groups I–III, excluding observations below the 1st percentile and above the 99th percentile of log price per square foot in the underlying transaction sample before reconstructing the balanced market-cell panel. Controls and fixed effects are as in Table 2. Heteroskedasticity-robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

C Online Appendix: Prices



(a) Bottom 10%

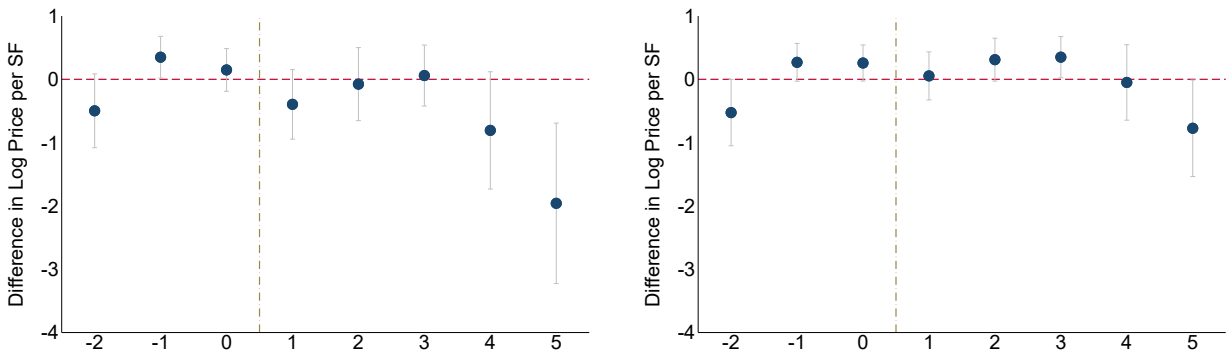
(b) Bottom 25%

(c) Bottom 50%

(d) Full Sample

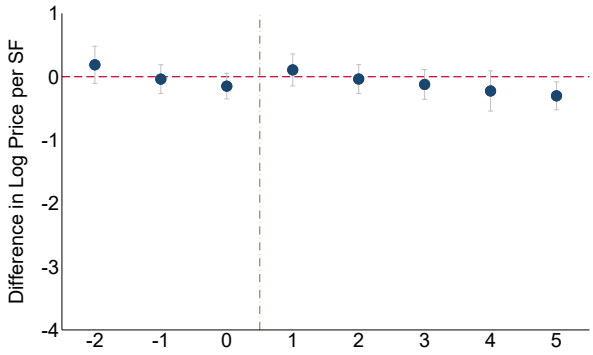
Figure C1. Price Index by Tier

Mean log price per square foot, indexed to 100 in 2009, by price tier. Boston vs. Control Group I.



(a) Bottom 25%

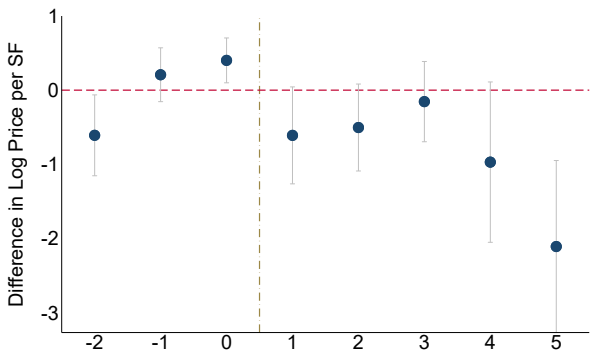
(b) Bottom 50%



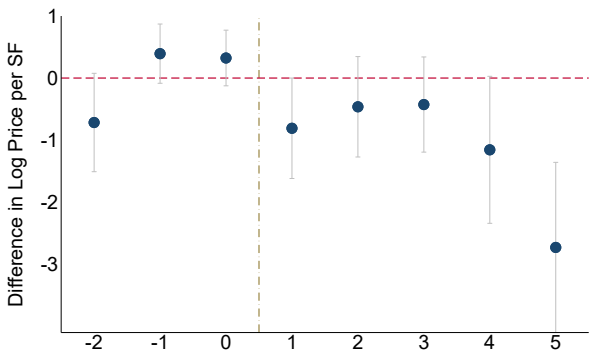
(c) Top 50%

Figure C2. Price: Event Study by Tier, Additional Tiers

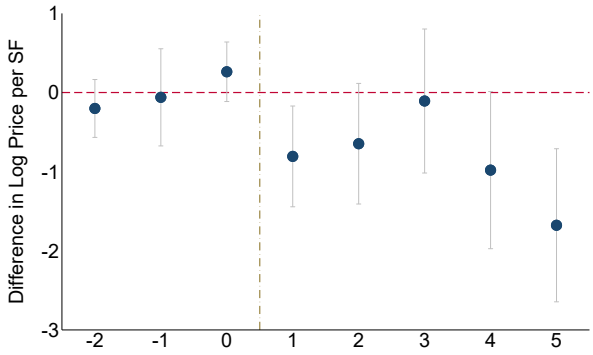
Treatment effects on log price per square foot from Equation 3 in event time with 18-month bins relative to the June 2013 shock, Bottom 25%, Bottom 50%, and Top 50% price tiers. Boston relative to Control Group I. Controls and fixed effects as in Figure 2. Bands show 95% confidence intervals. Heteroskedasticity-robust standard errors.



(a) Boston vs Control Group II



(b) Boston vs Control Group III



(c) Boston vs Control Group IV

Figure C3. Price: Event Study Across Control Groups

Treatment effects on log price per square foot from Equation 3 in event time with 18-month bins relative to the June 2013 shock, Bottom 10% price tier. Boston relative to Control Groups I–IV. Controls and fixed effects as in Figure 2; city fixed effects not applicable in Control Group IV. Bands show 95% confidence intervals. Heteroskedasticity-robust standard errors.

Table C1. Price Effects Across Mutually Exclusive Price Tiers

	Dependent variable: Log Price per Square Foot			
	Bottom 10%	10–25%	25–50%	Top 50%
$Boston_c \times$ Post-2013	-1.293***	0.016	0.031	0.010
	(0.305)	(0.058)	(0.035)	(0.088)
Controls, Year and City FE	Yes	Yes	Yes	Yes
R-squared	0.321	0.732	0.836	0.577
Observations	1,870	625	323	338

Note: This table estimates Equation 2 for Boston relative to Control Group I on mutually exclusive price tiers rather than the cumulative tiers of the main text. Controls and fixed effects are as in Table 3. Heteroskedasticity-robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table C2. Price Effects with Composition Controls and City Trends (Bottom 10%)

	Dependent variable: Log Price per Square Foot		
	(1)	(2)	(3)
$Boston_c \times$ Post-2013	-1.107***	-1.100***	-1.185***
	(0.276)	(0.286)	(0.289)
Controls, Year and City FE	Yes	Yes	Yes
Property Type × Year FE	Yes	Yes	Yes
Quality × Year FE	No	Yes	Yes
Pre-period City Trends	No	No	Yes
R-squared	0.379	0.392	0.394
Observations	1,870	1,870	1,870

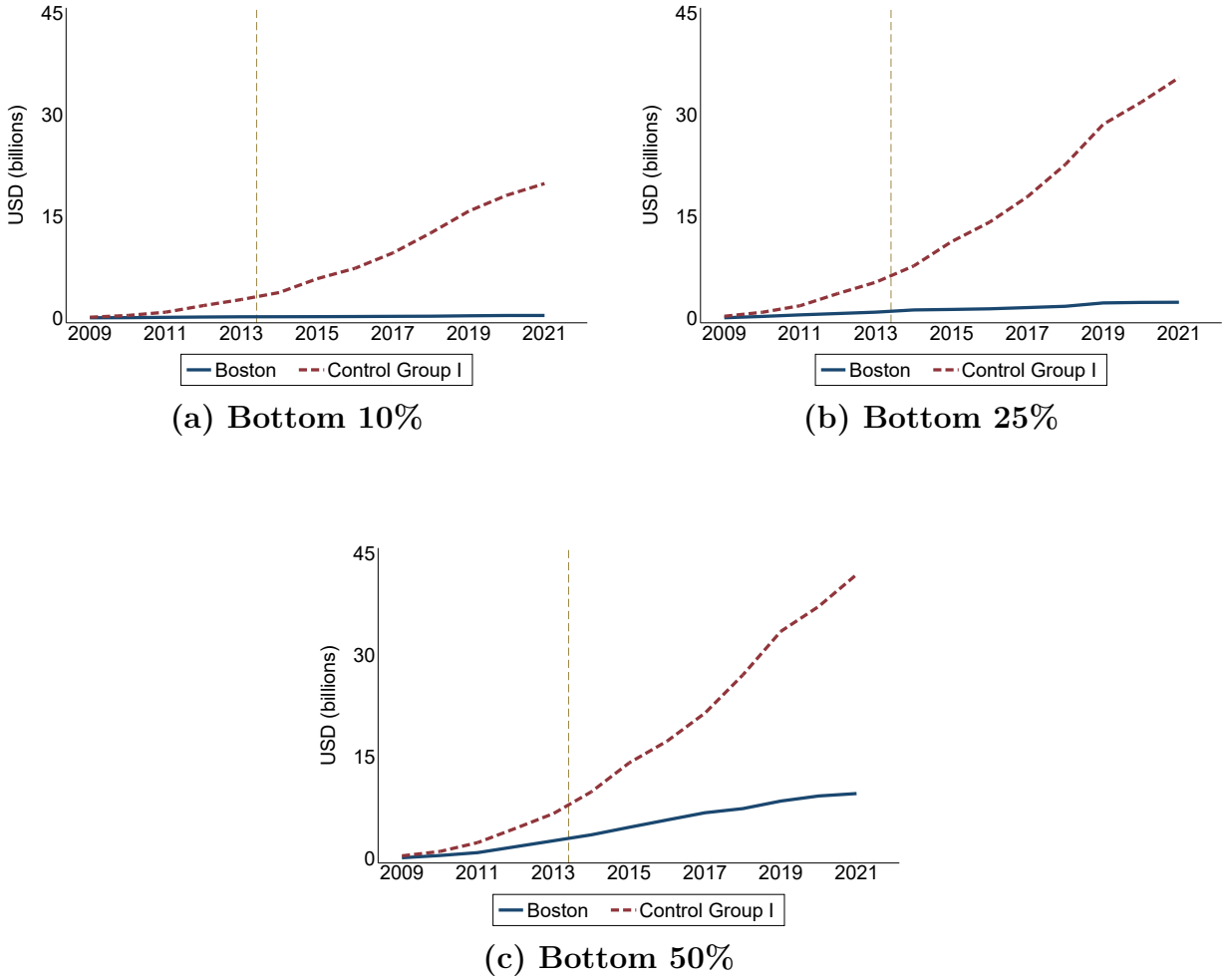
Note: This table estimates Equation 17 for Bottom 10% properties in Boston relative to Control Group I. The base controls and fixed effects are as in Table 3; the rows below the coefficient show the additional fixed effects and city-specific linear pre-policy trends added by column. Heteroskedasticity-robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table C3. Price Effects: Excluding Outliers

	Dependent variable: Log Price per Square Foot			
	Control Group I	Control Group II	Control Group III	Control Group IV
$Boston_c \times$ Post-2013	-0.602**	-0.610***	-1.162***	-0.821***
	(0.236)	(0.229)	(0.309)	(0.297)
Controls, Year and City FE	Yes	Yes	Yes	Yes
R-squared	0.364	0.334	0.418	0.470
Observations	1,833	4,138	513	181

Note: This table estimates Equation 2 for properties in the Bottom 10% across Control Groups I–IV, excluding observations below the 1st percentile and above the 99th percentile of log price per square foot within each estimation sample. Controls and fixed effects are as in Table 3. Heteroskedasticity-robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

D Online Appendix: Transaction Volume



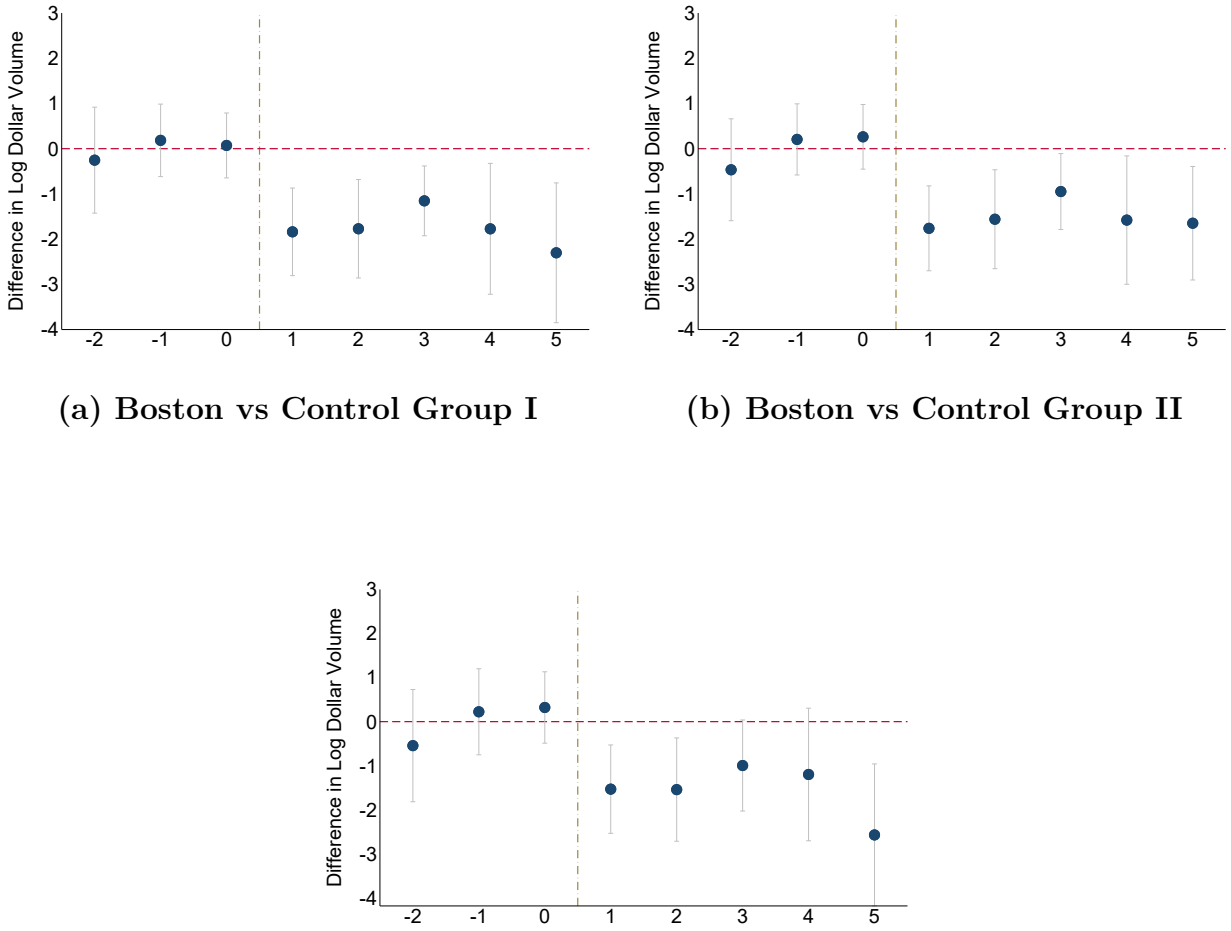
(a) Bottom 10%

(b) Bottom 25%

(c) Bottom 50%

Figure D1. Cumulative Transaction Volume by Price Tier

Cumulative transaction volume (USD billions) from 2009, by price tier. Boston vs. Control Group I. The vertical dashed line marks June 2013.



(a) Boston vs Control Group I

(b) Boston vs Control Group II

(c) Boston vs Control Group III

Figure D2. Transaction Volume: Event Study Across Control Groups

Treatment effects on log transaction volume from Equation 4 in event time with 18-month bins relative to the June 2013 shock, Bottom 10% price tier. Boston relative to Control Groups I–III. Controls and fixed effects as in Figure 1, plus building age. Bands show 95% confidence intervals. Heteroskedasticity-robust standard errors.

Table D1. Transaction Volume Effects

Panel A. Across Control Groups (Bottom 10%)

	Dependent variable: Log Transaction Volume (USD)		
	Control Group I	Control Group II	Control Group III
$Boston_c \times$ Post-2013	-1.700***	-1.606***	-1.586***
	(0.451)	(0.425)	(0.449)
Controls, Year and City FE	Yes	Yes	Yes
R-squared	0.350	0.373	0.402
Observations	1,301	1,908	387

Panel B. Across Tiers (Control Group I)

	Dependent variable: Log Transaction Volume (USD)		
	Bottom 25%	Bottom 50%	Full Sample
$Boston_c \times$ Post-2013	-1.054***	-0.413**	-0.308*
	(0.305)	(0.203)	(0.183)
Controls, Year and City FE	Yes	Yes	Yes
R-squared	0.353	0.347	0.411
Observations	1,703	1,904	2,058

Note: This table estimates Equation 4. The unit of observation is a transacting market cell. Controls and fixed effects are as in Table 2, plus building age.
Heteroskedasticity-robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table D2. Transaction Volume: Including Zero-Transaction Periods

Panel A. Across Control Groups (Bottom 10%)

	Dependent variable: Transaction Volume (USD)		
	Control Group I	Control Group II	Control Group III
$Boston_c \times$ Post-2013	-1.580***	-1.588***	-1.236***
	(0.439)	(0.430)	(0.455)
Controls, Year and City FE	Yes	Yes	Yes
Pseudo R-squared	0.182	0.194	0.275
Observations	7,518	15,120	2,408

Panel B. Across Tiers (Control Group I)

	Dependent variable: Transaction Volume (USD)		
	Bottom 25%	Bottom 50%	Full Sample
$Boston_c \times$ Post-2013	-1.083***	-0.657**	-0.351
	(0.413)	(0.263)	(0.263)
Controls, Year and City FE	Yes	Yes	Yes
Pseudo R-squared	0.192	0.210	0.392
Observations	9,072	9,660	10,066

Note: This table re-estimates the volume effect of Table D1 on the balanced cell-year panel, with within-tier transaction volume set to zero for cell-years that record no transaction. Estimation is by Poisson pseudo-maximum-likelihood on the level, so the coefficients are semi-elasticities. Controls and fixed effects are as in Table 2, plus building age. Heteroskedasticity-robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table D3. Transaction Volume Effects with Composition Controls and City Trends (Bottom 10%)

	Dependent variable: Log Transaction Volume (USD)		
	(1)	(2)	(3)
$Boston_c \times$ Post-2013	-1.569***	-1.600***	-1.624***
	(0.424)	(0.426)	(0.430)
Controls, Year and City FE	Yes	Yes	Yes
Property Type × Year FE	Yes	Yes	Yes
Quality × Year FE	No	Yes	Yes
Pre-period City Trends	No	No	Yes
R-squared	0.393	0.404	0.404
Observations	1,301	1,301	1,301

Note: This table estimates Equation 17 for Bottom 10% properties in Boston relative to Control Group I. The base controls and fixed effects are as in Table 2, plus building age; the rows below the coefficient show the additional fixed effects and city-specific linear pre-policy trends added by column. Heteroskedasticity-robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table D4. Alternative Measure of Transaction Volume

	Dependent variable: Transacted Square Feet	
	(1)	(2)
$Boston_c \times$ Post-2013	-149,182**	-147,253**
	(63,744)	(62,348)
Controls, Year and City FE	Yes	Yes
Year × Property Type	No	Yes
Year × Building Class	No	Yes
R-squared	0.152	0.200
Observations	1,301	1,301

Note: This table estimates Equation 4 using alternative volume measures for properties in the Bottom 10% against Control Group I. Controls and fixed effects are as in Table 2, plus building age. Heteroskedasticity-robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

E Online Appendix: Energy Performance

Table E1. Energy Performance and Size by Stranding Status

	New Inactive	Continued Active	New Active	Always Inactive	Total
Mean building size (Sq. Ft.)	327,145	263,265	216,793	208,385	246,639
Share office (%)	42.5	69.1	48.6	28.2	48.0
Share housing (%)	39.7	21.6	21.1	41.2	28.9
Share other property types (%)	17.8	9.3	30.3	30.6	22.9
Mean EUI, 2014–2020	82.4	74.4	76.9	79.8	77.9
Mean Energy Star, 2014–2020	61.8	68.6	69.6	61.7	66.2
Mean post-shock transactions	0.0	1.5	1.2	0.0	0.8
Matched buildings	73	97	142	85	397

Note: This table reports descriptive characteristics for BERDO buildings matched to CoStar records, grouped by transaction-status group as defined in Appendix A. EUI and Energy Star rows are building averages over 2014–2020. Mean post-shock transactions counts CoStar-recorded sales from June 2013 to September 2021.

Table E2. Energy Performance by Stranding Status: Pooled Comparison Group

	Winsorized EUI (kBtu per Square Foot)	Energy Star Score
New Inactive	14.265**	−5.696**
	(5.599)	(2.874)
Controls, Year and Submarket FE	Yes	Yes
R-squared	0.632	0.262
Observations	1,907	1,519

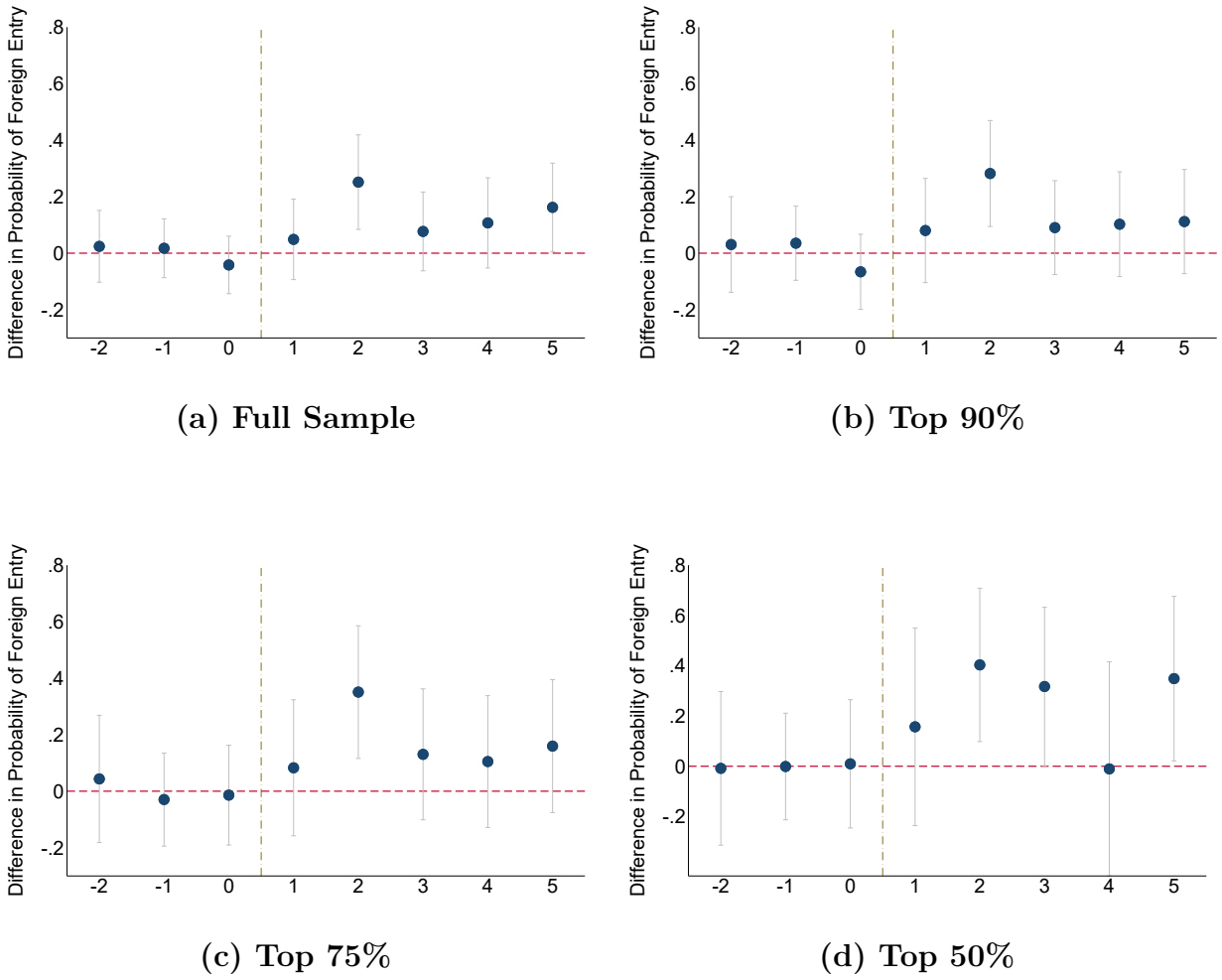
Note: Alternative to Table 4, comparing new inactive buildings to all other buildings pooled together rather than to buildings that sold after BERDO specifically. EUI is winsorized at the 1st and 99th percentiles. Controls are log building square footage, property-type fixed effects, building class, building age, year fixed effects, and submarket fixed effects. Regressions are weighted by building square footage. Standard errors clustered by building in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table E3. Stranding Predictors

	Dependent variable: New Inactive (Indicator)		
	(1) Price only	(2) EUI only	(3) Joint
Pre-shock price ratio (std.)	−0.111***		−0.110***
	(0.041)		(0.041)
Earliest available EUI (std.)		−0.038	−0.035
		(0.035)	(0.034)
Controls, Property-Type and Class FE	Yes	Yes	Yes
Observations	86	86	86
R-squared	0.302	0.270	0.307

Note: New inactive and continued active are defined as in Appendix A; the comparison group here is continued active buildings. The sample is the Bottom 50% price tier. The pre-shock price ratio is the log ratio of a building's last pre-June-2013 price per square foot to the applicable bottom-decile cutoff. All specifications include log building square footage, property-type fixed effects, building-class fixed effects, and building age. Heteroskedasticity-robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

F Online Appendix: Foreign Investment

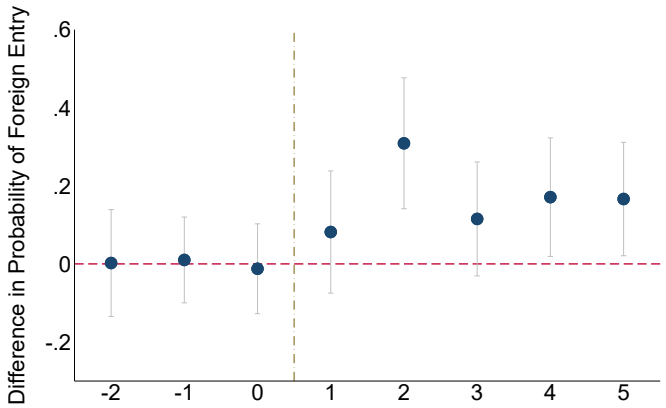


(a) Full Sample **(b) Top 90%**

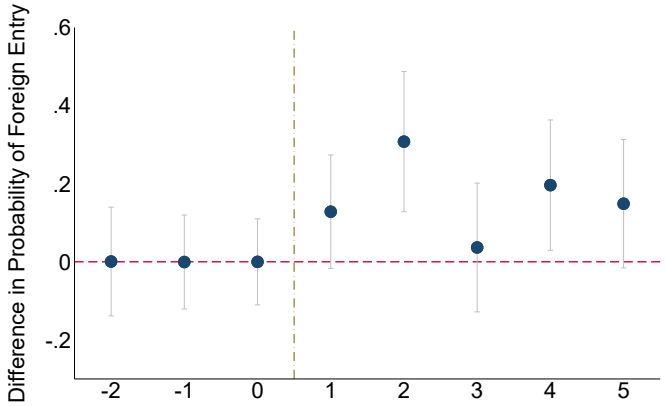
(c) Top 75% **(d) Top 50%**

Figure F1. Foreign Entry: Event Study by Price Tier

Treatment effects on the probability a cell records at least one foreign purchase, in event time with 18-month bins relative to the June 2013 shock, by price tier. Estimates of Equation 6. Boston relative to Control Group I. Controls and fixed effects as in Figure 1, plus building age. Bands show 95% confidence intervals. Heteroskedasticity-robust standard errors.



(a) Control Group II



(b) Control Group III

Figure F2. Foreign Entry: Event Study Across Control Groups

Treatment effects on the probability a cell records at least one foreign purchase, in event time with 18-month bins relative to the June 2013 shock, Full Sample. Estimates of Equation 6. Boston relative to Control Groups II–III. Controls and fixed effects as in Figure 1, plus building age. Bands show 95% confidence intervals. Heteroskedasticity-robust standard errors.

Table F1. Foreign Purchase Share by Tier

Panel A. Boston Share

Period	Full Sample	Top 90%	Top 75%	Top 50%
Jun 2010–May 2013	10.2	10.3	11.1	13.2
Jun 2013–May 2016	45.1	45.2	46.4	49.8
Jun 2016–May 2019	31.4	31.7	31.5	33.6
Jun 2019–Sep 2021	10.6	10.7	10.9	11.1

Panel B. Gap, Boston minus Control Group I

Period	Full Sample	Top 90%	Top 75%	Top 50%
Jun 2010–May 2013	1.6	1.9	1.8	11.1
Jun 2013–May 2016	38.8	39.0	41.0	49.0
Jun 2016–May 2019	24.7	24.1	23.5	19.6
Jun 2019–Sep 2021	4.7	3.9	9.7	11.1

Note: This table reports foreign purchase share by price tier in successive windows aligned to the June 1, 2013 shock: a pre-shock window of June 2010–May 2013, and post-shock windows of June 2013–May 2016, June 2016–May 2019, and June 2019–September 2021. Foreign share is the percentage of transaction volume purchased by foreign buyers within each tier. Tiers are constructed exactly as in Table 6. Panel A reports Boston's raw share; Panel B reports Boston minus Control Group I, in percentage points.

Table F2. Foreign Purchase Volume

Panel A. Across Control Groups (Full Sample)

	Dependent variable: Log Foreign Purchase Volume (USD)		
	Control Group I	Control Group II	Control Group III
$Boston_c \times$ Post-2013	2.869***	3.375***	3.319***
	(0.777)	(0.815)	(0.859)
Controls, Year and City FE	Yes	Yes	Yes
R-squared	0.099	0.212	0.189
Observations	2,058	6,245	796

Panel B. Across Tiers (Control Group I)

	Dependent variable: Log Foreign Purchase Volume (USD)		
	Top 90%	Top 75%	Top 50%
$Boston_c \times$ Post-2013	3.055***	3.271**	4.519**
	(0.965)	(1.392)	(2.050)
Controls, Year and City FE	Yes	Yes	Yes
R-squared	0.132	0.213	0.254
Observations	911	422	218

Note: This table estimates the same specification as Table 5 applied to log dollar foreign purchase volume rather than the extensive-margin indicator. Many cells record zero foreign purchase activity; we add one before taking logs to retain these observations, and results are similar using the inverse hyperbolic sine transformation. Controls and fixed effects are as in Table 2, plus building age. Heteroskedasticity-robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table F3. Foreign Entry: Fixed Ex-Ante Price Tiers

	Dependent variable: Cell Records at Least One Foreign Purchase			
	Full Sample	Top 90%	Top 75%	Top 50%
$Boston_c \times$ Post-2013	0.149***	0.150**	0.281***	0.209
	(0.045)	(0.059)	(0.085)	(0.152)
Controls, Year and City FE	Yes	Yes	Yes	Yes
R-squared	0.092	0.148	0.282	0.322
Observations	1,833	650	267	137

Note: This table re-estimates Table 5, Control Group I, with each property's price tier fixed ex ante rather than each transaction classified by its realized sale price. The tier is constructed exactly as in Appendix Table B3. The four columns hold the ex-ante tier fixed: the first keeps every cell with a classified property (from a pre-2013 sale, or a hedonic prediction where none exists); the others additionally drop the frozen Bottom 10/25/50% tiers. Controls and fixed effects are as in Table 2, plus building age. Heteroskedasticity-robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table F4. Foreign Entry: Robustness to Active-Cell Conditioning

Panel A. Fixed Pre-Policy Cell Universe

	Dependent variable: Cell Records at Least One Foreign Purchase		
	Control Group I	Control Group II	Control Group III
$Boston_c \times$ Post-2013	0.067***	0.073***	0.071***
	(0.017)	(0.021)	(0.017)
Controls, Year and City FE	Yes	Yes	Yes
R-squared	0.080	0.210	0.143
Observations	4,368	4,764	2,142

Panel B. Transaction-Level Foreign-Buyer Probability

	Dependent variable: Transaction Has a Foreign Buyer		
	Control Group I	Control Group II	Control Group III
$Boston_c \times$ Post-2013	0.128***	0.148***	0.141***
	(0.032)	(0.031)	(0.037)
Controls, Year and City FE	Yes	Yes	Yes
R-squared	0.083	0.103	0.144
Observations	3,156	9,146	1,196

Note: Panel A uses a fixed pre-policy cell universe and Panel B a transaction-level specification, both addressing the active-cell conditioning of the main-text entry result (Table 5); construction details are in Appendix A. Requiring two active pre-policy periods (Control Group I only) gives a larger coefficient of 0.192 (0.043), $N = 1{,}624$. Controls and fixed effects are as in Table 5. Heteroskedasticity-robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

G Online Appendix: Treatment-Level Placebo Inference

Table F5. Foreign Buyer EPI: Transaction-Weighting and Control Group II

Panel A. Control Group I, Transaction-Weighted

	Dependent variable: Mean Standardized EPI Score of Foreign Buyers		
	Full Sample	Top 90%	Top 75%
$Boston_c \times$ Post-2013	0.437	1.108**	1.893***
	(0.424)	(0.519)	(0.680)
Controls, Year and City FE	Yes	Yes	Yes
R-squared	0.271	0.281	0.443
Observations	170	107	70

Panel B. Control Group II

	Dependent variable: Mean Standardized EPI Score of Foreign Buyers		
	Full Sample	Top 90%	Top 75%
Equal cell weights	0.387	1.066**	1.084
	(0.333)	(0.503)	(0.667)
R-squared	0.336	0.338	0.409
Transaction-weighted	0.548*	1.319**	1.406**
	(0.328)	(0.516)	(0.620)
R-squared	0.338	0.342	0.422
Controls, Year and City FE	Yes	Yes	Yes
Observations	360	182	92

Note: This table reports the robustness of Table 6 to transaction-weighting, which weights each cell by its number of foreign purchases (Panel A, Control Group I), and to Control Group II under both weighting schemes (Panel B), which additionally applies the synthetic-control weights. Each observation is a market-cell period with at least one foreign purchase. Controls and fixed effects are as in Table 2, plus building age. Heteroskedasticity-robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table F6. Foreign Buyer Home Countries

	EPI score	Boston		Control Group I	
		Pre	Post	Pre	Post
Germany	74.5	1	16	1	2
United Kingdom	72.6	0	2	2	3
Sweden	70.3	1	5	0	2
Norway	69.9	0	4	0	1
Switzerland	67.8	2	3	0	3
Australia	63.1	2	2	0	3
Japan	61.4	0	10	2	5
Canada	61.1	0	32	11	68
Singapore	53.0	0	1	15	14
Israel	48.0	2	4	0	1
Other	52.9	1	13	1	14
Total foreign purchases		9	92	32	116
Mean standardized EPI		1.23	1.44	1.02	1.11
Share from EPI > U.S.		0.67	0.88	0.53	0.80

Note: Full sample, all price tiers. Counts of foreign purchases by buyer home country in Boston and Control Group I, before and after the June 2013 shock. EPI and its standardization are defined in Appendix A. "Other" pools the remaining countries; the value shown for it is their mean EPI score.

Table G1. City- and Metro-Level Permutation Tests

	City			*Metro*		
Outcome	Units	$p_{\|t\|}$	p_{dir}	Units	$p_{\|t\|}$	p_{dir}
Panel A. Treatment date June 2013						
Market Cell with No Transactions	19	0.105	0.053	12	0.167	0.083
Log Price per Square Foot	19	0.053	0.053	12	0.083	0.083
Log Transaction Volume (USD)	19	0.053	0.053	12	0.083	0.083
Log Foreign Purchase Volume (USD)	19	0.053	0.053	12	0.083	0.083
Market Cell with at Least One Foreign Purchase	19	0.053	0.053	12	0.083	0.083
Mean Standardized EPI of Foreign Buyers (Zero-Filled)	19	0.053	0.053	12	0.083	0.083
Panel B. Placebo treatment date June 2011 (falsification)						
Market Cell with No Transactions	19	0.632	0.316	12	0.667	0.333
Log Price per Square Foot	17	0.235	0.941	11	0.273	0.909
Log Transaction Volume (USD)	17	0.118	0.941	11	0.182	0.909
Log Foreign Purchase Volume (USD)	18	0.500	0.722	12	0.417	0.833
Market Cell with at Least One Foreign Purchase	18	0.444	0.722	12	0.333	0.833
Mean Standardized EPI of Foreign Buyers (Zero-Filled)	18	0.500	0.833	12	0.583	0.833

Note: Placebo permutation test: each comparison unit is reassigned the treatment date, the difference-in-differences coefficient re-estimated, and Boston ranked against the placebo distribution by coefficient over standard error. p_{dir} ranks in the predicted direction, $p_{|t|}$ in absolute value, each with a plus-one correction. Panel B uses a placebo date of June 2011. The EPI row uses the zero-filled full-sample outcome of Table 1, which combines the entry and composition margins; this differs from the conditional-on-entry composition estimate of Table 6. Donor pool, unit counts, and construction: Appendix A.2.15.