Are Large Investors a Source of Declining Rental Affordability: Evidence from Boston, MA, and Houston, TX

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Abstract

In this paper, I study whether investor decisions and property acquisitions by large landlords are a meaningful driver of rent prices. Using a novel data set of landlord links and property holdings built by Gomory and Fish 2024 covering the Boston and Houston rental markets, merged with rental listing and building permit data, I present five sets of results. First, I show that failing to take into account shell ownership leads to underestimates of market concentration of more than 10%. Second, I show that, while shell ownership matters for accurately measuring levels of concentration, I find no evidence that concentration has increased over time. Third, I show that over 90% of the variation in concentration within a market across time is explained by entry, and only 5% by mergers. Fourth, I find that, on average, landlords raise prices by 1.6 percentage points after acquiring properties (over 4 percentage points by the fourth year post-acquisition). Fifth, I show that these acquisition effects are largely explained by post-acquisition renovations. Specifically, I estimate precise near-null (0.6 percentage points) effects for properties that are acquired but not subsequently renovated and larger (2.9 percentage points) effects for those that are subsequently renovated.[1]

[1]

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1 Introduction

Large landlords and the possibility of market power in rental housing are increasingly cited as potential contributors to high rent prices and severe rent burdens (Harris 2024; Vogell 2022). To date, however, the vast majority of the empirical studies on large landlords and investor behavior have focused either on the rise of 'institutional landlords' in the single-family rental market or on the general rise in the share of properties owned by trusted, limited liability corporations (LLCs), limited partnerships (LPs), or other non-individual entities.

Single-family rental units, however, comprise around 31% of the rental stock (Joint Center for Housing Studies of Harvard University 2024) of which institutional investors own around 2% (United States Government Accountability Office 2024)[2], which neglects the large majority of renter households.

Further, the studies noting the rise in investor-owned properties do not measure consolidation, and are likely to reflect increases in landlord professionalization, declines in the costs of registering an LLC, and changes in social norms surrounding property ownership as much as they are changes in market power. There is comparatively very little work both studying the entire rental market as a whole and using sophisticated landlord linkages, leaving whether consolidation has increased, and whether the behavior of large landlords is a meaningful driver of prices largely unanswered.

Historically, studying both consolidation and its effect on rental prices have been complicated by severe data limitations. Starting with consolidation, the fact that landlords frequently own properties via numerous LLCs and shell companies, makes assigning ownership, and thus calculating consolidation, challenging. Previous studies, such as Coven 2024, Hanson 2024, and Garriga, Gete, and Tsouderou 2023 use industry reports and public filings to compile lists of subsidiaries for single-family rentals (and multifamily in Garriga, Gete, and Tsouderou 2023), which allows them to accurately measure holdings for properties owned by large investors. This approach, however, is limited to studying the holdings of large, national investors, which will miss large local or regional landlords. Additionally, with relatively rare ex-

[2]It should be noted, however, that this percentage varies substantially across space, with the largest institutional holdings being located in the Sun Belt, with 25% of Atlanta, GA's single-family rental housing market, 21% of Jacksonville, FL's, 18% of Charlotte, NC's, and 15% of Tampa, FL's single-family rental market being owned by institutional investors, per a review done by the Government Accountability Office

ceptions like Garriga, Gete, and Tsouderou 2023, these studies focus on the single-family rental market, which, as mentioned before, is an important but limited part of the rental market.

On the prices side, in comparison with owner-occupied housing, where transactions are recorded, rental data has not been available at the property level or at a high enough frequency to enable causal estimates of the effect of either consolidation or acquisition by a large landlord. Existing public use rental data, such as those from the American Community Survey (ACS), will not contain unit level rent prices. Even restricted use data, from the ACS or the Consumer Price Index microdata, will generally not have enough listings to be sufficiently powered for most analysis. It is only with the recent advent of granular rental data from private companies that this analysis has been made possible.[3]

This paper investigates this gap in the literature by providing novel data documenting changes in landlord concentration, acquisition patterns, and pricing behavior in Boston and Houston. I present five sets of results about these rental markets.

Beginning with measuring ownership and market concentration, building on the work done by Gomory and Fish 2024, I show that market concentration has not meaningfully increased over time and that this result holds at different geographic and property-type aggregations. Mechanically, smaller market definitions produce higher measures of concentration however, even at very small geographic definitions, most units were not in a concentrated market at the beginning of my sample, nor are they in a concentrated market at the end of my sample. I do, however, show that taking into account networks of shell companies is quantitatively important for understanding concentration at smaller market definitions. In particular, at a Census Tract level market definition, failing to take into account these linkages underestimates average market concentration by around 10%, and by more if the market is defined more narrowly. Additionally, the increase in the share of properties owned via LLCs, LPs, or other non-person entities is dramatic, but not related to market consolidation.

[3]As I will discuss in the data section, one limitation of private rental data is that they tend to select for the higher income rental market, as the lower income rental market remains relatively informal. The ideal data for this paper would come from a rental registry, such as what Berkeley, California has, however, few American cities have these kinds of registries.

Collectively, I make two broad points about measuring concentration and market definition. First, for markets to be reasonably concentrated, markets need to be fairly segmented (either by geography or by housing type, or, more realistically, by both). Second, if markets are believed to be relatively segmented, then taking into account networks of shell companies is quantitatively important for accurate measurement.

Next, I show that where there are changes in concentration, these changes in concentration are overwhelmingly due to entry, rather than mergers and acquisitions. Following Arnold 2019, I regress predicted changes in the Herfindahl–Hirschman Index (HHI) due to entry, exit, and mergers on actual changes in HHI; in univariate regressions, I find that mergers explain around 5% of the variation in concentration, whereas entry explains around 90. In the full regression, changes in HHI due to mergers do not significantly predict actual changes in HHI.

Turning to the price effects of property acquisition, I document that there are economically meaningful effects of property acquisition on rent prices. Using the Sun and Abraham 2021 staggered differences-in-differences design and unit-level asking rent from Altos, I show that a rental unit being acquired by a landlord leads to an average 1.6 percentage point increase in rent prices relative to non-acquired units, with the largest effects occurring four years post-acquisition. Given these price effects, I explore possible economic models that could rationalize them. I present evidence that the price effects are driven by large landlords acquiring and renovating properties, primarily in lower income areas.

I begin by showing that, in contrast to popular media, transactions from non-corporate sellers to corporate buyers do not seem to result in large price increases. Next, I show that within the set of transactions done by corporate buyers, effect sizes are monotonic with landlord size, with acquisitions by large landlords producing the largest effect sizes. I argue, however, that this is likely due to large landlords being more likely to take out renovations, and more likely to engage in larger renovations.

To demonstrate that these price effects are largely driven by renovations, I estimate precise null effects for properties that are acquired but which do not file for a building permit post acquisition and large price effects for the properties that do file for a building permit. Next, I show that properties acquired and renovated by large landlords see larger price increases than properties acquired and renovated by non-large landlords. While I do not

rule out that market power may matter for certain transactions, I show that the average acquisition, if there are no subsequent renovations, results in relatively modest price increases.

Finally, consistent with my argument that larger price effects for large landlords represent changes in the intensive and extensive margins of renovation, I show that large landlords are about 8% more likely to receive a building permit post-acquisition and are more likely to take out larger renovations, as measured by the number of building permits issued (close to three times as many) and the reported value of these permits (about 25% higher). Further, the permit intensity findings (number of permits filed) hold conditional on property size, meaning they are not a mechanical result of large landlords acquiring large apartment complexes, which necessitate more and costlier permits.

Taken together, I argue that the large landlord's primary market advantage likely lies in being better capitalized and able to take on larger renovations than smaller landlords. This is partially consistent with Lee and Wylie 2024 who show that for single-family homes, units acquired by institutional landlords are more likely to engage in renovations, however, I find that for my sample, large landlords do engage in more extensive renovations. Broadly, this paper contributes to a growing set of literature documenting that large landlords enjoy cost and scale advantages over small-time landlords and that these advantages can be meaningful determinants of individual unit and building prices.

The paper is structured as follows: In Section 2, I briefly discuss the related literature on measuring consolidation and investor activity in the US housing market, effects of mergers and acquisitions in general, and on aggregate determinants of housing affordability. In Section 3, I detail both my data and the institutional background. In Section 4, I present descriptive evidence about trends in market consolidation and landlord behavior. In Section 5, I lay out my empirical strategy and present my event study results on property acquisition, as well as test possible mechanisms behind the aggregate price effects. Finally, in Section 6 I recap my findings and briefly discuss policy implications.

2 Related Literature

As mentioned previously, the primary obstacle that prevents studying consolidation is the fact that landlords frequently own properties via numerous LLCs and shell companies, which makes assigning ownership challenging. For instance, three properties owned by 'Fish Properties', 'Joe Fish Housing', and 'Durham Apartments' can all be owned by the same landlord but be unable to be linked because common ways of fuzzy matching, such as Levenshtein string distance, will consider those names to be very dissimilar. Gomory and Fish 2024, Messamore 2023, and Hangen and O'Brien 2024 overcome this challenge through a series of sophisticated data linkages based on using the businesses' names in conjunction with information from their business filings and other administrative datasets to deduplicate and consolidate landlord networks. All of these papers have generally shown that, while taking into account shell ownership matters for levels of consolidation, there has been little to no changes in the levels over time.

Previously, studies on investor behavior in the US housing market have tended to focus on the owner-occupied market – likely due to the much higher quality transaction data that is available. Within these studies, much of the literature has focused on the investor decisions by large institutional investors, which emerged largely as a response to the Great Recession (Mills, Molloy, and Zarutskie 2017), purchased distressed homes at discounts (Allen et al. 2017), and provided liquidity to housing markets (Ganduri, S. C. Xiao, and S. W. Xiao 2022). Outside the literature on institutional investors, papers such as Bayer, Mangum, and Roberts 2021 highlight the role that speculation and purchases by novice investors play. Importantly, however, both of these literatures share key differences with how the multifamily housing market functions. First, as I'll show, there hasn't been the same degree of large-scale entry into the multifamily market. Second, multifamily investors are likely much more sophisticated than most 'mom-and-pop' landlords and investors, as there are very high fixed costs in acquiring apartment complexes.

Going beyond linkages and patterns of investor behavior, there are a small set of studies documenting the effects of market consolidation and investor activity, which have found mixed impacts for tenants. Within the smaller set of studies focusing on the multifamily and overall rental market, Gomory 2021 finds that the same property, when owned by a large landlord, is more likely to evict its tenants than that same property when owned by a small landlord; Calder-Wang and Kim 2023 find that algorithmic pricing, which enables de facto collusion and thus a larger effective market share, increases

rent prices by up to 5%; Watson and Ziv 2024 find that the median markup in New York City is about 30% – substantially higher than what would be implied by competitive markets. In the single family rental market, where the literature has been primarily focused on national, institutional investors, Gurun et al. 2022 finds that mergers by institutional investors increases rent prices, however, while the mechanism comes from both market power and improvements to neighborhood quality. Using a similar mergers strategy to identify exogenous changes in concentration, Austin 2022 finds a consistent result in Atlanta, with the additional finding that these mergers of institutional investors tend to increase neighborhood diversity. Some studies (Francke et al. 2023, in particular), however, have found that institutional investors decrease rent prices – mostly by turning owner-occupied housing into rental housing.

In addition to studies of market power and pricing behavior, there is a separate literature that focuses on cost advantages that large and institutional landlords may have. One advantage is that large landlords are more likely to successfully appeal their tax assessments, with Austin 2022 finding that institutional investors are able to use this strategy to reduce their collective tax bills by over 4 billion dollars. Another key advantage is that large-scale investors may have a comparative advantage in acquiring and renovating distressed properties. As Goodman and Golding 2021 discuss, institutional investors frequently purchase homes in cash and finance their purchases through securitization, this means that the rehab costs are not contingent on financing; a key difference compared to properties purchased via traditional mortgages. Better access to credit and internal financing may also be a comparative advantage even within the set of landlords that are considering purchasing larger apartment buildings. This likely interacts with landlords' acquisition and subsequent pricing decisions, as better capitalized landlords can undertake more substantial renovations.

Taken together, this paper contributes to three growing literatures: the literature on investor activity in the US housing market (An 2023; Allen et al. 2017; Bayer, Mangum, and Roberts 2021; Buchak et al. 2020; Hanson 2024; Mills, Molloy, and Zarutskie 2017; Garriga, Gete, and Tsouderou 2023), the broader literature on the documentation of "stealth mergers" and the effect of acquisitions on prices and product quality (Barrios and Wollmann 2024; Asil et al. 2024), and the literature on the broader determinants of housing affordability (Desmond and Wilmers 2019; Glaeser and Gyourko 2018; Mast 2023; Calder-Wang and Kim 2023; Molloy, Nathanson, and Paciorek 2022.

3 Data Description

3.1 Assessor Parcel Data and Landlord Linkages

I include here a brief description of the data used to create the panel of parcels, as well as information on how the landlord linkages were done. A full accounting of the data sources, imputation processes, and landlord linkages is provided in Gomory and Fish 2024.

Assessor parcel data for Gomory and Fish 2024 were collected individually by county for the Houston, TX, Baltimore, MD, Boston, MA, and Miami, FL metro areas. I subset the data to Houston and Boston and look at the principal cities/counties within each metro area (Harris County and the city of Boston, respectively). Collectively, these data contain a panel on the universe of buildings, as well as information on these buildings such as year built, square footage, land use, owner name, mailing address, and owner-occupancy tax-exemption status.

Information on the property owners contained in the parcel data is augmented by business listing data acquired from the Secretary of State in each state. These data contain the universe of business listings, the addresses, and the names of the officers and registered agents associated with these listings and are what allow me to link together layers of shell companies and LLCs.

Landlord linkages were done via network-based entity reconciliation. This process is fully laid out in Gomory and Fish 2024, however, the intuition is as follows: Suppose we see two Jeff Patels in the parcel ownership data, under what circumstances can we be more certain that these two entities represent the same individual? If both Jeffs are the owner of the same parcel in different years, we would be reasonably sure that they are the same owner. However, if two Jeffs are listed as the owner of two separate parcels, we might only conclude they represent the same owner if they both listed the same address as their contact address. In the case of LLC ownership, the process is similar, although obviously more complicated. If Patel Properties, Jeff Apartments, and Durham Realty, all own apartment complexes, we would conclude that they are jointly owned as a conglomerate if, for instance, Jeff Patel was listed as the president of each company, and they were all registered to the same mailing address. Finally, I merge the landlord linkages data with the universe of housing transactions from CoreLogic to identify arm's length transactions.

3.2 Determining Rental Status

An issue for measuring rental market consolidation is that most cities do not keep registries of the number of units available for rent within a given time period. While Census and American Community Survey (ACS) data can provide estimates for the renter populations at smaller geographies, and the rental vacancy rate at larger ones, neither can accurately measure the size of a given rental market, particularly if there are large changes in population, as is the case of Houston. Further, neither dataset is able to classify individual properties as having rental units or not, and as such, whether a given property should be counted within the broader rental market – a crucial step for calculating measures of concentration.[4]

To get around this, I classify units as rental properties based on whether they have more than three non-condominium units or if the property does not claim an owner occupancy exemption. One potential problem with this method is that a duplex that claims an owner occupancy exemption may still rent out the other unit; however, for measuring concentration, these properties are negligible. Importantly, this classification is not an issue for my event study regressions (beyond determining landlord size) as the sample is defined by having a matched listing in the rental data, not by claiming an owner occupancy exemption.

3.3 Rental Listing Data

Data on rental listings come from Altos Research. These data aggregate rental listings across a number of Multiple Listing Services (MLS) and contain a large fraction of unit level rental asking rents within a given rental market. A given listing contains the listed address, the property name or listing description, the number of bath and bedrooms, the date the listing was posted, the coordinates of the listing, and auxiliary information about the unit (e.g., whether the unit allows pets, has air conditioning, has parking, etc.). I convert these data into a panel of rental units and merge them with my parcel data (for a detailed description of this process, see appendix A.1)

One concern with the Altos data is that they skew towards multifamily units and towards higher income neighborhoods. In Table 2, I show that Altos listings that match parcels are, relative to the overall rental market, much more likely to be in large, multifamily complexes and within higher

[4]In Boston, I have to impute the number of units in each parcel; for details on how this was done, see appendix A.2.

income areas. I match about 60% of multifamily rental units to an Altos listing, but only about 12% of single family rental units.[5] As it pertains to my event study results, this limitation means that units that go from the informal to formal rental market will not be well represented.

[5]The true match rate for single-family rentals is likely higher as I classify single family units as rentals based on not claiming an owner occupancy exemption, which will include vacation units, second homes, vacant properties, and short term rentals, none of which would count as rental units for my study.

Summary Statistics of Likely Rental Properties

	Average Income Decile		Percent Matched	
Land Usage	Full Sample	In Altos	% Properties Matched	% Units Matched
Small-building Residential	3.500	3.900	0.410	0.440
Single-family Residential	5.500	5.800	0.120	0.120
Large-building Residential	4.900	5.300	0.620	0.740
Multi-use	4.500	5.000	0.650	0.790
Condominium	6.200	6.300	0.020	0.020
Residential Other	5.200	6.200	0.030	0.260
Commercial	5.100	6.200	0.140	0.450
Vacant	5.500	4.800	0.060	–[a]
Other	4.500	5.200	0.100	0.780

Table 2:

[a]Vacant Parcels do not match Altos rental listings

3.4 Permit Data

I merge my assessor parcel data with the universe of building permits obtained from Boston and Harris County.[6] Most of these permits reflect meaningful renovations (additions, demolitions, certificates of occupancy, etc.) and I drop the permits that were voided, subsequently denied, or that were appeals or minor amendments. As I can't see whether the permits were actually completed, I interpret analysis using the permit data as an intent-to-treat. Finally, in Harris County, many of these permits are responses to Hurricane Harvey – I keep these permits as many are substantive renovations. For Boston data, which contain the self-reported value[7] of the permit, I exclude permits that were greater than 10 million dollars in value.

3.5 Defining a Large Landlord

There is no consensus definition of what an institutional investor is. There is likewise no consensus definition on what a large landlord is. "Corporate investors", which are the only group that has a generally accepted definition, are typically identified by whether the listed owner on a deed or tax assessment contains "llc", "lp", "inc", or other corporate flags (see Lee and Wylie 2024 as an example of this approach). Corporate investors, however, comprise a broad and diverse group across scale, scope, business model, and maintenance, management, and pricing behavior. Investor definitions based on regular expression flags on names will lump together small "mom-and pop" investors with medium and large scale-local investors, nonprofits, iBuyers, private equity, and Real Estate Investment Trusts (REITs), amongst others.

Because of this definition, reports about the prevalence of investors (institutional or otherwise) run the risk of conflating changes in investor composition – whether more properties are owned by fewer landlords, for instance – with changes in ownership norms, tax code changes, and ease of transferring property ownership to shell companies. As an example of this conflation, a 2022 report by the National Association of Realtors, which has been subsequently cited by Housing and Urban Development (HUD) and in the popular press, classified owners as institutional investors with the aforementioned regular expressions definition. As a result, they found that:

[6]The Harris County data span 2005-2019, the Boston data span 2009 to 2019. In practice, this is not an issue, as the Altos data begins in 2011.

[7]Based on communications with the city of Boston, I interpret self-reported value as self-reported cost.

> Using deed records data, we found that institutional buyers purchased 13.2% of residential properties in 2021, up from 11.8% in 2020. ... Texas led all states with the highest share of institutional buyers at 28%, followed by Georgia (19%), Oklahoma (18%), Alabama (18%), and Mississippi (17%).

For another example of how failing to distinguish between different kinds of ostensibly large corporate investors can lead to strange inclusion criteria, consider three of the largest "corporate" landlords in my dataset: Boston Redevelopment Authority, who own and operate properties via an LLC, Alka Gupta, who, in conjunction with her foundation, runs a large network of subsidized housing units, and Harvard University.

I argue the investor definition used should be defined in conjunction with the goal of the study. For studying consolidation, it might be correct to include large non-profits, or the holdings by large colleges, especially in the denominator when calculating shares. However, for ascertaining the effect of acquisition of a large, corporate landlord, we likely would not want to include non-profits as they would not be presumed to either exploit any market power they have, nor pursue the same aggressive pricing strategies as a private equity owned property might.

As another example of how different kinds of investors manage properties differently, in a study of property transitions from owner occupied to rental housing, Billings and Soliman 2023 find that properties owned by REITs as opposed to private equity are more likely to generate negative property spillovers. They attribute this to the fact that REITs are legally incentivized to produce short term profits, which may lead to deferred maintenance, as opposed to private equity, which is afforded longer investment horizons.

Recent studies sidestep the common investor definition by looking at one specific class of investor (and usually within one property class type – single family rentals). The advantage of this is that by narrowing in on one set of investors it is easier to incorporate institutional knowledge of that investor class' typical business model, and it is easier to successfully classify firms[8]

[8]This is most common when studying REITs and other public institutional investors as the names of the parent companies and their subsidiaries are typically public. For private companies that own through layers of undisclosed shell companies, this is much more challenging.

Throughout this analysis, I refer to landlords by their scale, however, as discussed before, there is no agreed upon way to delineate landlords by size. Mechanically, any landlord who owns a large apartment complex will be a "large" landlord, however, we might want to distinguish between an apartment complex bought or owned by an investor who owns other apartment complexes versus an apartment complex bought or owned by an investor who owns just that building. To do this, my preferred measure of scale is the number of units owned by a landlord net of the parcel in question. As an example, if a landlord owns one apartment, each unit is owned by a landlord who owns zero units net of that parcel; for a landlord who owns two fifty unit complexes, each unit would be owned by a landlord who owns fifty units.

For the rest of this paper, the specific thresholds I use are as follows: a large landlord is one that owns at least 100 units, net of whatever parcel is being considered; a medium landlord owns [10-100) units; a small landlord owns [1,10); a single property landlord owns exactly one property. Additionally, a corporate landlord is any entity that owns through an LLC, LP, or other corporate vehicle and is not registered as a non-profit, educational institute, and is not flagged as a government organization. Using this definition excludes the aforementioned examples of the Boston Redevelopment Authority, Harvard University, and Houston non-profit. Taken together, a large corporate landlord is, from the perspective of a tenant, a unit owned by a corporate, non government/education/non-profit landlord who owns at least 100 units outside that parcel. As I show in my descriptive evidence and causal estimates, one justification of this classification is that it leads to clear delineations in landlord behavior. I include in Table 3 a reference guide for how these definitions are constructed

Table 3: Ownership and classification definitions

Construct	**Definition**
Conglomerate owner	All shell entities / legal names linked to the same underlying ownership group are aggregated into a single conglomerate owner. Ownership status, market shares, and classifications are defined at the conglomerate level unless specified otherwise.
Treatment timing	A parcel is treated at event time $T_i = t_0$ when its conglomerate owner changes at t_0 (ownership transfers to a different conglomerate). Event time is $\ell = t - T_i$ with $\ell = -1$ omitted as the reference period in event studies.
Owner size (net-of-parcel)	Let $U_{g,t}$ be total units owned by conglomerate g at time t across all holdings, and let $U_{p,t}$ be units on parcel p. Define net units $$U_{g,t}^{\text{net}}(p) = U_{g,t} - U_{p,t},$$ and classify size using $U_{g,t}^{\text{net}}(p)$. This avoids mechanically inflating size by counting the parcel under consideration; size can still vary across years due to other transactions.
Size bins	Large: $U_{g,t}^{\text{net}}(p) \geq 100$; Medium: $10 \leq U_{g,t}^{\text{net}}(p) < 100$; Small: $1 \leq U_{g,t}^{\text{net}}(p) < 10$; Single-property: owns exactly one property (property defined as *[parcel]*).
Corporate status	A conglomerate is corporate if *any* member entity indicates ownership through an LLC/LP/INC (regex-based), and the conglomerate is not classified as government, educational, or non-profit.
Large corporate	Indicator equal to 1 if a conglomerate is corporate and $U_{g,t}^{\text{net}}(p) \geq 100$.

One point of caution is that any null relative difference between landlord types could be driven by poor matching. For instance, a property owned by an unmatched LLC might be part of a broader network, which would attenuate comparisons between landlord types.

4 Descriptive Evidence

4.1 Trends in Rent Prices

I begin first by plotting Zillow's estimate of median rents for my two cities, Houston and Boston, along with Baltimore and Miami (the other cities from Gomory and Fish 2024) from 2011 to the end of 2018. As figure 1 shows, Houston saw some price appreciation from 2011 to 2016, followed by relatively stable rents thereafter.[9] In contrast, rents in Boston began higher and rose much more quickly as Boston experienced a large demand shock from high income workers trying to relocate there.

As I will show subsequently, it's hard to explain Boston's rapid price appreciation with a change in market concentration, as market concentration is relatively flat over the time period. Additionally, for acquisition effects (or any hypothesized determinant of prices) to be a large-scale driver of prices, the effects would need to be both large in magnitude and pervasive. As a benchmark for aggregate price effects, Calder-Wang and Kim 2023 find that the effect of RealPage on a rental market within the same approximate time frame as my sample tops out at around 3% when the market is fully penetrated.

4.2 Trends in Concentration and Corporatization

In figures 2 and 3, I plot the average Herfindahl–Hirschman Index (HHI) for rental properties by geographic market[10] and matching method[11]. Four patterns emerge. First, and mechanically, markets are more concentrated at smaller geographic levels. Second, there has been no meaningful change in concentration at any geography over time in either city (this result is robust to calculating HHI based on property values instead of units). Third, the matching method does matter for the level of concentration – as indicated by the "conglomerate" matching method being notably higher than the naive and entity methods –, and it matters more at smaller geographies. Failing to take account of these shell companies misses around 10% of average market concentration at the Census Tract level. Finally, at all geographic

[9] Interestingly, Hurricane Harvey does not show up in the time series

[10] Here, the geographies are weighted by the number of rental units in each geography-year.

[11] The matching methods are "naive" which means matching landlords on cleaned name and addresses, "entity" which means consolidating the same company but with alternate spellings, and "conglomerate", which refers to the full array of matching algorithms

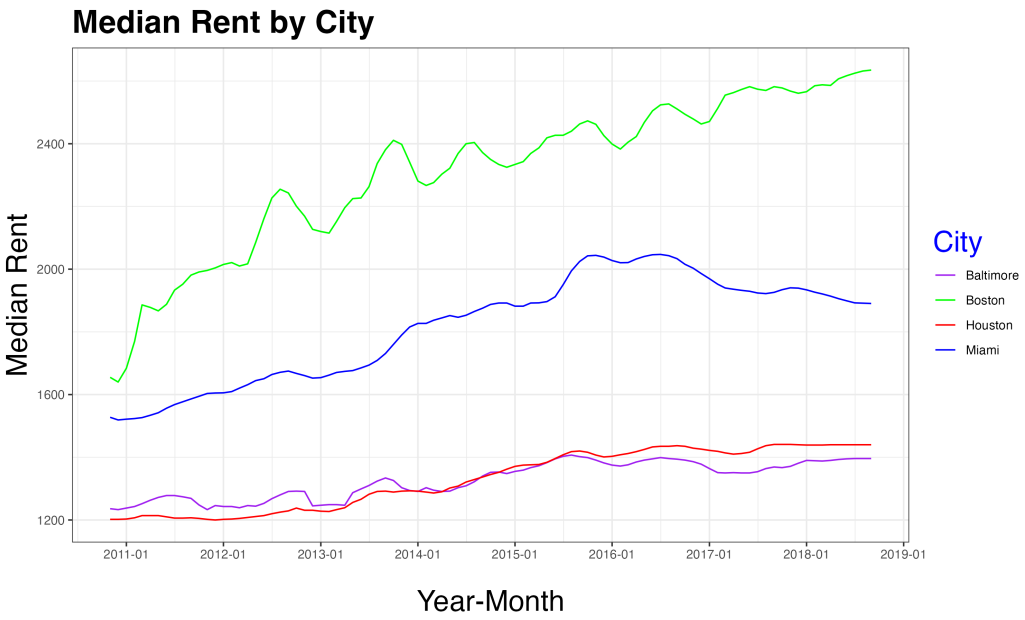


Figure 1: Median rents by city.
Notes: Data come from a discontinued version of Zillow's city-level median rent data series

levels, Houston is more consolidated than Boston. This, however, is a largely mechanical result of what is built rather than anything particular about ownership. Houston has a much larger share of its rental units from 50+ unit buildings, which mechanically results in a more consolidated rental market.

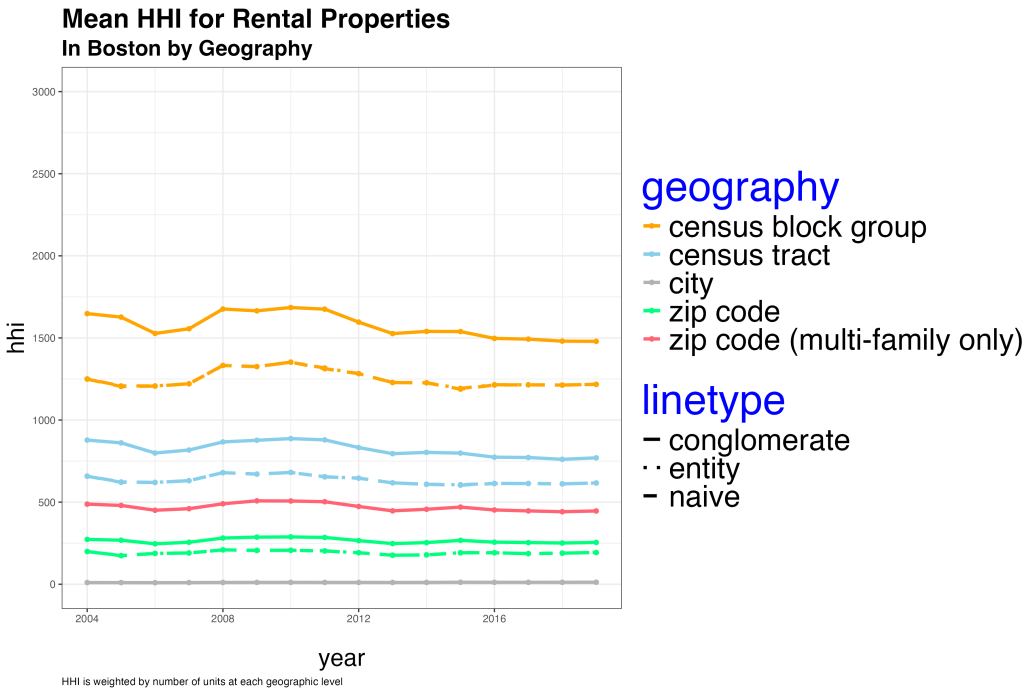


Figure 2: Market Consolidation: Boston

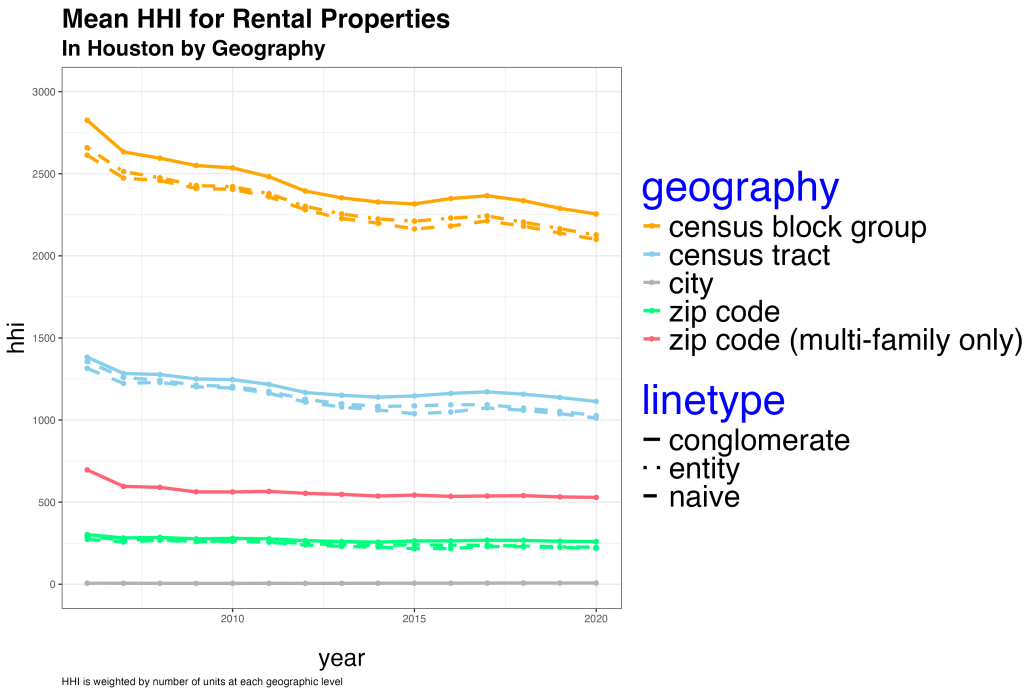


Figure 3: Market Consolidation: Houston

Next in figure 4, to reinforce the previous point that matching method does matter for understanding the scope of market concentration, I pool my data across cities and plot thin-slices of concentration by geography (Zip code, Census Tract, and Census Block Group) and city in 2019. On the x-axis is the HHI for each geographic observation if matching was done on name and address and on the y-axis the same but if matching was done using the full conglomerate matching method. A point being on the forty-five degree line indicates that, for that geographic observation, the matching method did not impact the estimated market concentration. Mechanically, the HHIs from the conglomerate method are at least as high as for the naive method, however, for certain observations, and in particular at smaller geographies, the changes in HHI can be dramatic once shell ownership is taken into account.

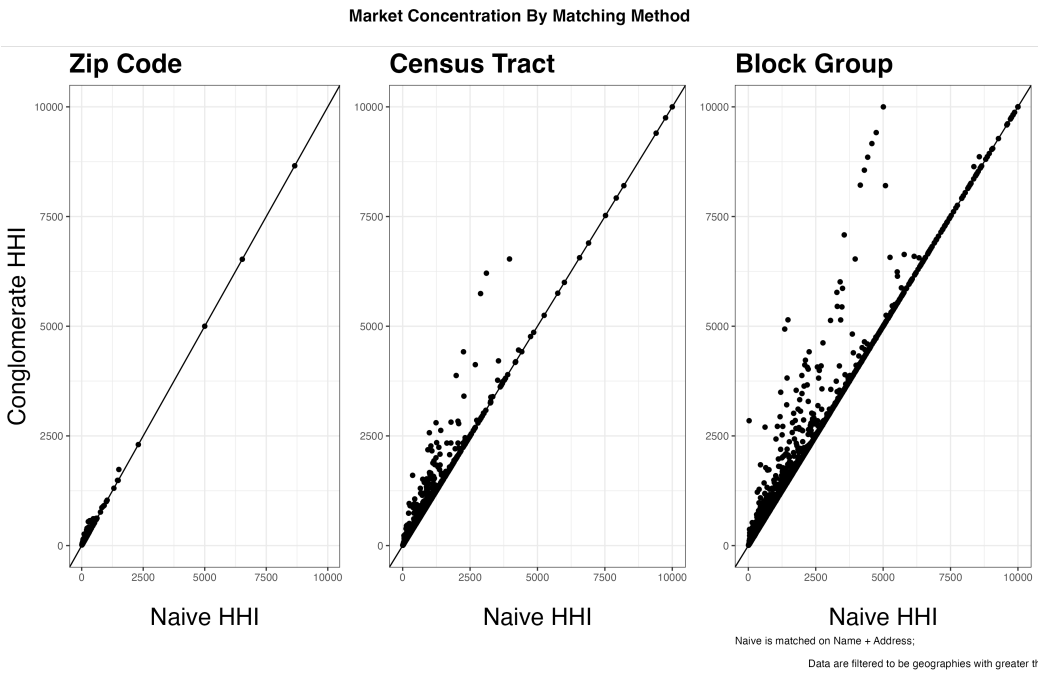


Figure 4: HHI by Matching Method

Finally, I recreate the finding from Joint Center for Housing Studies of Harvard University 2024 that there has been a notable rise of corporatization, as measured by the share of units owned by LLCs, LPs, and other corporate entities. In figures 5 and 6, I plot the share of rental units owned by a corporate landlord for different property types. Across all property types

and in both cities, corporatization has increased over time. Additionally, corporatization is highest for properties with more rental units, which is consistent with the common knowledge that larger apartments are more likely to be owned by professional landlords and not mom and pops. However, as my HHI descriptives show, the increase in non-individual owned properties is not mirrored by an accompanying rise in concentration.

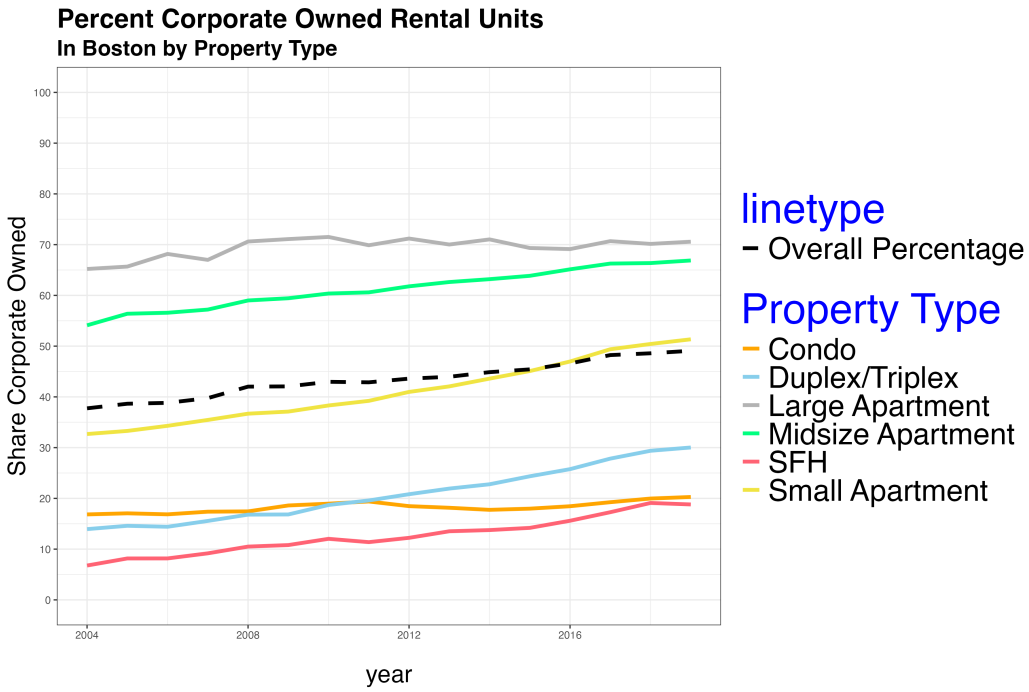


Figure 5: Corporate Ownership Trends: Boston

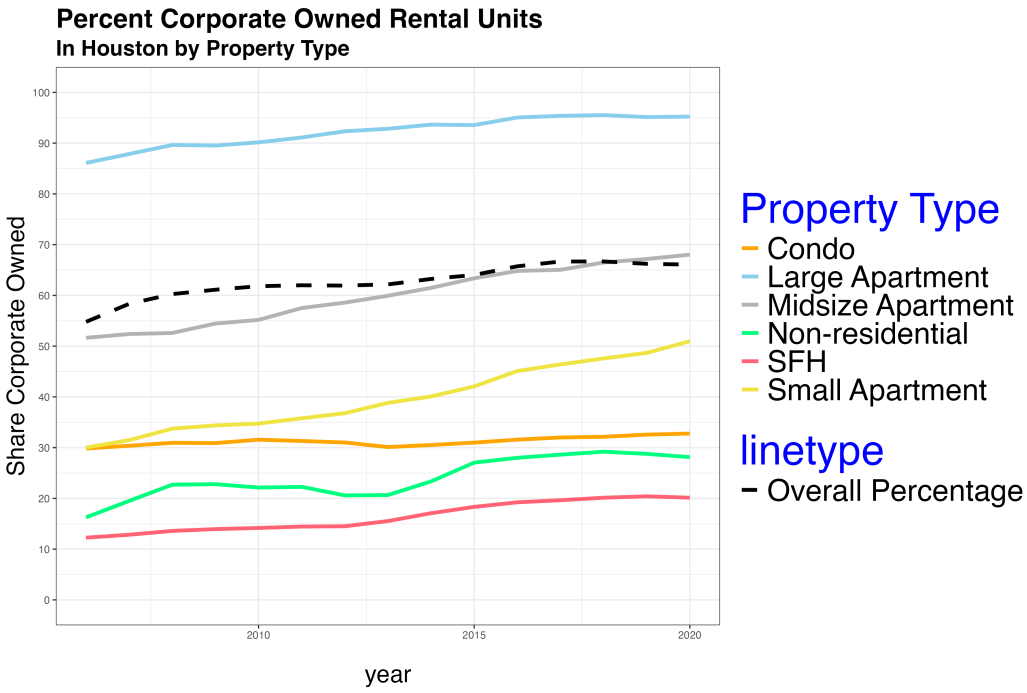


Figure 6: Corporate Ownership Trends: Houston

4.3 Patterns of Acquisition

Next, in table 4, I show the kinds of properties that are likely to be bought by different sized investors. In this table, I show the results of the following regression:

$$Log(Y_{it}) = \beta_0 + \beta_1 \text{Owner Size}_{it} + \beta_2 X_{it} + \delta_{nt} + \epsilon_{it}$$

where Y_{it} is the assessed value of the property per square foot in the prior year[12], Owner Size$_{it}$ are dummy variables for the size of the portfolio of the acquiring landlord in the prior year[13], relative to a "never purchased" reference category, X_{it} are property level controls for land usage and number of units, and δ_{nt} are geography (city or zip code) by year fixed effects.

Beginning with the first column, the owner size coefficients indicate that acquired properties tend to have lower assessed values, and properties acquired by large investors tend to have even lower assessed values relative to properties not purchased; for large landlords, they tend to invest in properties that have about 19% lower assessed values per square foot.

Moving to the second and third columns, I perform the same regression but control for zip code-year level average household income and then, in the third column, zip code by year fixed effects. Doing so substantially increases the coefficient on ownership for all investors, but increases them the most for large investors (19% lower to between 2.5 and 7 percentage points *higher*, depending on the controls used). I interpret this as saying that 1) patterns of property acquisition differ by landlord size and 2) larger landlords tend to invest in lower value properties, but this is driven mostly by investment into lower income neighborhoods.

[12]For value, I use the year prior to the acquisition to avoid reassessments and for square footage I use the total square footage of the parcel (land plus structure).

[13]Bins are greater than 100 units, between 10 and 100 units, between 1 and 10 units, and 0 units, meaning the landlord had no other units prior to this acquisition. See table 3 for a full mapping of definitions

Table 4: All Cities Acquisition Regressions

Dependent Variable:	Log(Assessed Value per Sq Foot in Prior Year)		
Model:	(1)	(2)	(3)
Variables			
Bought by Large Corp Landlord	-0.192***	0.025***	0.070***
	(0.007)	(0.006)	(0.005)
Bought by Medium Corp Landlord	-0.192***	-0.088***	-0.056***
	(0.007)	(0.005)	(0.005)
Bought by non-Corp Landlord	0.020***	0.058***	0.086***
	(0.005)	(0.004)	(0.003)
Bought by Single Property Corp Landlord	0.195***	0.072***	0.069***
	(0.007)	(0.006)	(0.005)
Log(Average Income) of Zip Code- Year		0.989***	
		(0.002)	
Fixed-effects			
Year-City	Yes	Yes	
Land Usage	Yes	Yes	Yes
Zip Code- Year			Yes
Fit statistics			
Observations	572,914	572,914	572,937

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

4.4 Roll-ups and Changes in Concentration

In the spirit of Asil et al. 2024, I look descriptively at the role that roll-ups play in the rental market. In this section, I document two facts: First, conglomerates use shell companies to conceal holdings, and, while roll-ups and shell companies are important for understanding investor behavior, actual market share is driven much more by entry and exit than by acquisitions.

Beginning with the roll-ups, I show that large landlords frequently own their units through a network of shell companies. Looking at the five hundred largest property owners in Boston and Houston, the median conglomerate is composed of ten entities. Interestingly, major builders (Lennar, DR Horton, Taylor Morrison, etc.), own large numbers of properties under very few LLCs, whereas shell ownership is much more common for owners of apartment complexes. Further, not all large landlords use not-obviously related LLC names. AvalonBay, the national REIT, tends to own properties through LLCs that contain the name "Avalon". One possible explanation for this is that, as is commonly reported[14], shell companies are more common when the landlord wants to purchase a large number of properties in geographically small areas (for instance, infill development or redeveloping a commercial area) and wants to avoid property holdouts.

Next in table 5, I follow Arnold 2019 and decompose changes in HHI at the zip code level into variation from mergers, any transactions, entry, exit, and reallocation.[15] I find that only about 5% of the variation in concentration over time is due to mergers. Importantly, as my data are able to link together shell companies, this finding is robust to a roll-ups strategy, in which individual transactions do not meaningfully increase HHI, although aggregate transactions might.

[14] `https://www.sfchronicle.com/sf/article/vc-hostile-takeover-fillmore-neighborhood-19719260.php` and `https://www.sfchronicle.com/bayarea/article/solano-new-city-investors-silicon-valley-18331296.php`, as examples

[15] Reallocation should be thought of as an intensive margin including properties that are renovated and expanded or which a share of units are removed from the market (Airbnb in a large apartment complex, for example). For practical purposes, reallocation often looks like entry if a vacant parcel is intensely developed, but isn't assigned a new parcel identifier.

Table 5: Herfindahl-Hirschman Index Change Regressions

Dependent Variable:	ΔHHI					
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Δ HHI from Mergers	0.0546					
	(0.0615)					
Δ HHI from Transactions		0.4296*				0.0405
		(0.2276)				(0.0290)
Δ HHI from Entry			1.081***			
			(0.0356)			
Δ HHI from Exit				0.4160*		
				(0.2216)		
Δ HHI from Reallocation					0.9467***	0.9297***
					(0.0281)	(0.0369)
Fit statistics						
R^2	0.051	0.196	0.920	0.215	0.951	0.952
Observations	1,644	1,644	1,644	1,644	1,644	1,644

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

5 Empirical Strategy

5.1 Acquisition Regressions

I now ask whether landlord acquisition has any effect on unit-level asking rents. To analyze the effect of an acquisition on rent prices, I follow the event study specification outlined by Sun and Abraham 2021. This regression estimates the average treatment on the treated (ATT) by comparing changes in rent prices for units acquired by landlords to changes in rent prices of units that were not acquired during the sample period. Additionally, I argue that the ATT is the exact parameter of interest as media reports and popular sentiment commonly report rent increases as evidence of predatory landlord behavior, however, these naive numbers lack any notion of a control group to ground the estimates.

In my baseline specification, I define a "treatment" as any parcel that was bought by any landlord. In each regression, the comparison group is defined as any unit that is never acquired by any landlord during my sample. In subsequent regressions, I will redefine treatment to be, for instance, "purchase by a large landlord", however, the control group of units that were never acquired will remain the same.

To estimate this regression, following the Sun and Abraham 2021 notation, I regress the following:

$$Y_{it} = \alpha_i + \delta_{t,c} + \sum_{s,e} \sum_{l \notin -1} \beta_{s,e}(1_{t=s,E_i=e}) + \epsilon_{i,t}$$

where Y_{it} represents log rent prices of unit i in time t, α_i represents the unit fixed effect, $\delta_{t,c}$ represents a city-specific common time trend. Here, the β vector is the parameter of interest, as it captures many people's first thoughts when they hear their building will be acquired: "By how much more will my rent go up?"

Additionally, I bin my event study to be within four pre/post -treatment periods. This is done primarily because even in my pooled sample, I have very few units that were observed five years before or after treatment (and because those periods would only be estimated using results from the very

early acquired or very late acquired cohorts). Finally, I cluster the standard errors from my regression analysis at the parcel level.

5.2 Identifying Assumptions

For this to be a valid causal estimand I need the following assumptions:

Assumption 1: Parallel Trends in Baseline Outcomes

Following the Sun and Abraham 2021 notation, I need that for all $s \neq t$, the $E[Y_{i,t}^{\text{inf}} - Y_{i,s}^{\text{inf}} | E_i = e]$ is the same for all $e \in supp(E_i)$. In my case, the parallel trends assumption is defined within cities and would be violated if acquiring landlords were picking properties based on differential trajectories within markets (e.g., buying in hot submarkets or if there were property type specific pre-trends).[16]

Assumption 2: No Anticipation

I need there to be no anticipatory behavior prior to acquisition – for instance, this would be violated if incumbent landlords raised rents prior to acquisition. Anecdotally, this is sometimes done by sellers prior to acquisition to try and negotiate better selling prices.

Assumption 3: No spillovers

Finally, I need that there are no spillovers from acquisition onto the rents of other properties. This would be violated if acquiring landlords made changes to the neighborhood, as documented in Gurun et al. 2022 or if acquiring landlords increased prices by withholding supply.[17] To the extent that there are spillovers, the way in which they bias my results will depend on the nature of the spillovers. If there are positive spillovers, as would be the case with landlords improving the neighborhood or withholding supply,

[16] As a robustness check, in Appendix B.1, I allow for the parallel trends assumption to hold within-Census Tract and estimate treatment configurations jointly. My results are qualitatively unchanged

[17] Spillovers could also occur if landlords change the pricing policy on all of their units once acquisition occurs, but given the market is so diffuse, this is not a meaningful concern.

these would bias my results downwards. In the case of negative spillovers, my results would be biased upwards.

5.3 Acquisition Results

I begin my analysis in figure 7 by showing the effects of property acquisition on rent prices for any rental property acquisition done by any kind of landlord. I find that there are positive price effects (ATT of 1.6 percentage points) which steadily rise, peaking at around 4% in the fourth year post-acquisition. Visual inspection of the pre period coefficients show no sign of significant pretrends.

Next, I explore whether transactions by certain kinds of buyers explain the price effects. First, I check whether acquisition effects are different depending on whether the buyer/seller is corporate or non-corporate (here, I follow the general literature and define corporate as owning via an LLC, LP, or other investment vehicle). In figure 8, I plot the same event study regressions for four separate kinds of transactions: any transaction, as shown previously, properties bought by a corporate buyer, properties bought by a non-corporate buyer, and properties bought by a corporate buyer from a non-corporate seller.

I find that acquisition effects are effectively zero for non-corporate buyers and positive for corporate buyers; the 95% confidence interval on the ATT for non-corporate buyers is (-0.005,0.007), whereas the analogous confidence intervals for corporate buyers is (0.013,0.028). However, I also find that they are centered at zero for transactions between non-corporate sellers and corporate buyers (ATT of 0.006 with a standard error of 0.006). Instead, price effects are coming from corporate buyers buying from corporate sellers, which complicates some of the intuition people have of savvy buyers acquiring properties from uninformed sellers and increasing prices.

I extend my analysis of transactions by corporate buyers by looking at whether landlord size explains my price effects. In figure 9, I keep the same landlord size bins from my descriptive analysis (single property owners, small landlords, medium-sized landlords, and large landlords) and perform the same event study regressions with transactions by each kind of landlord. In Table 6, I show the aggregated results of my event study plots.

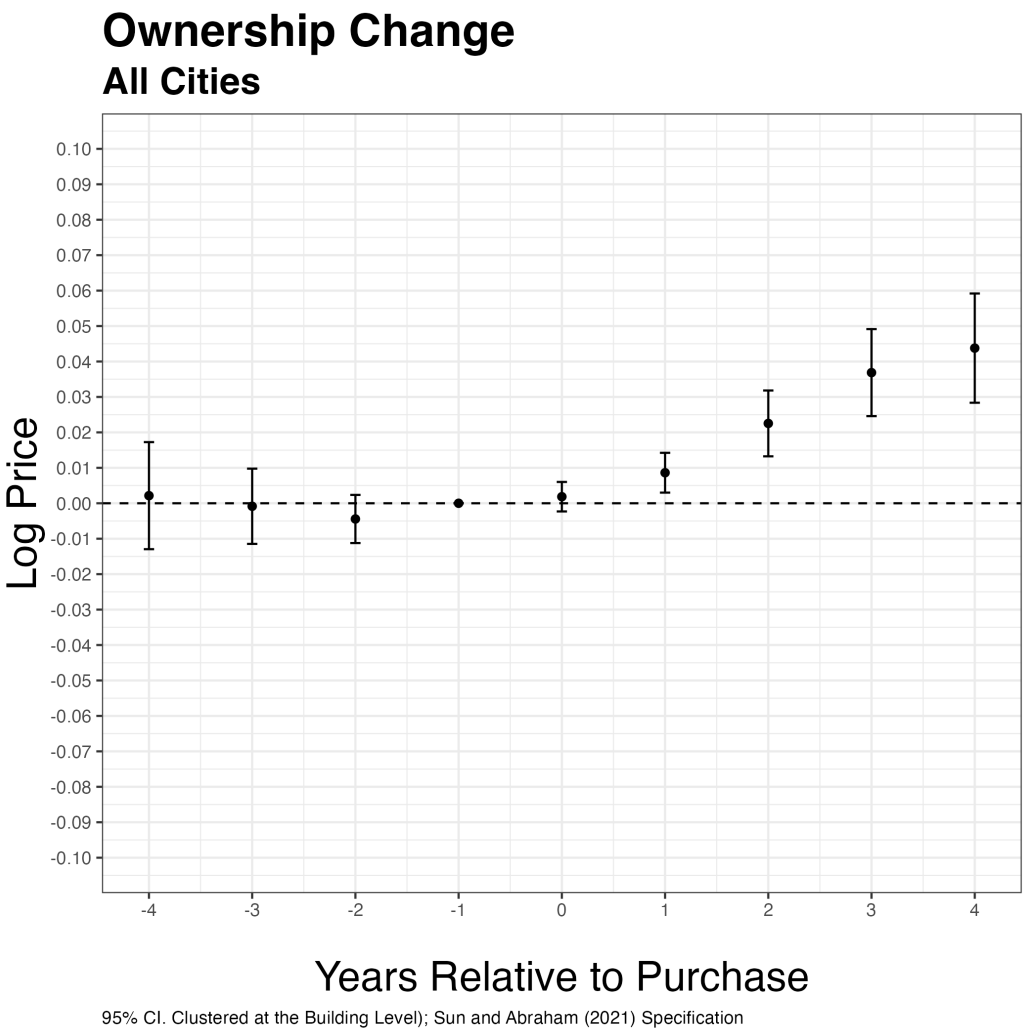


Figure 7: Event Study Results: Any Change in Owner

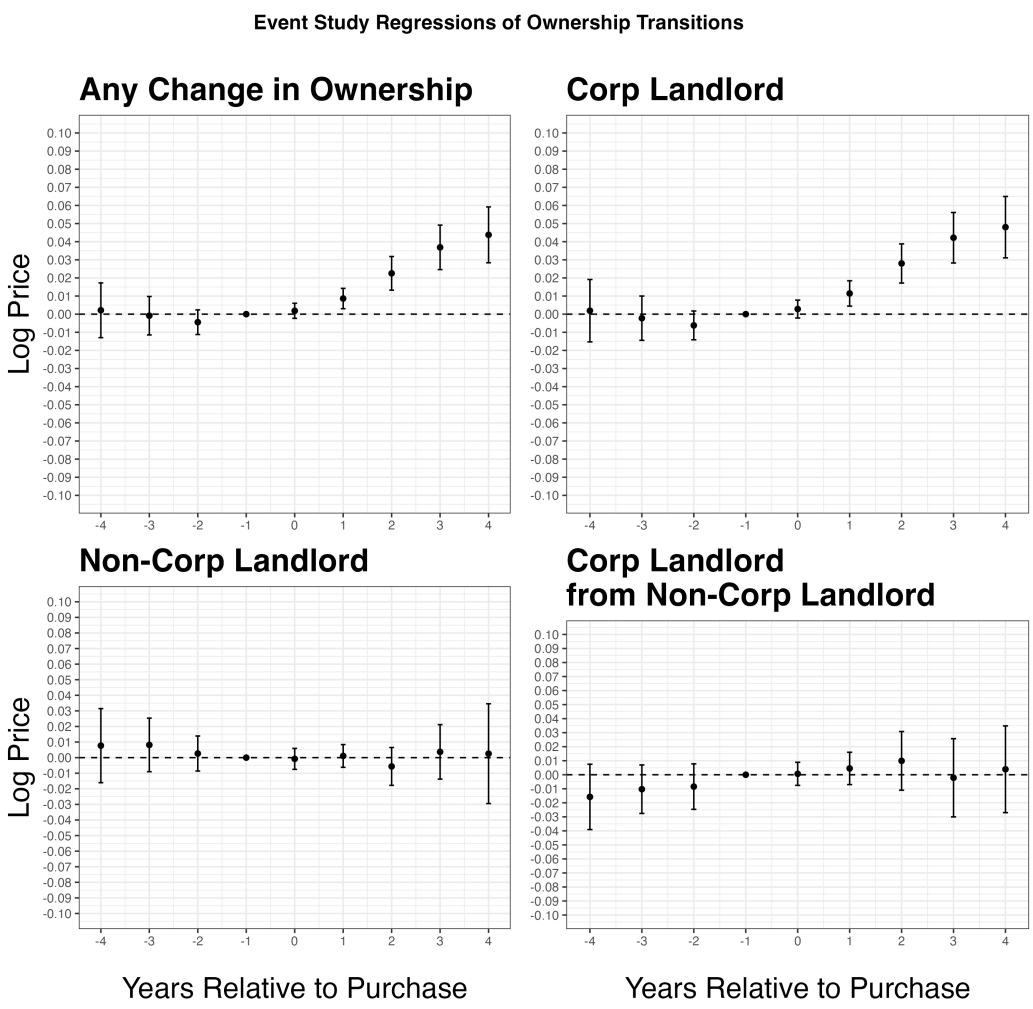


Figure 8: Event Study Results: Owner Type

I find that the effects are monotonic with size, with acquisitions by large landlords causing higher prices than those from medium-sized ones, and price effects from properties being acquired by small and single property landlords being indistinguishable from zero. Except for transactions from single property landlords, the p-values on the pre trend coefficients are statistically insignificant (although some caution is warranted, as these tests can be underpowered – see Roth 2022). The relative noisiness of the small and single property acquisitions is also indicative of the fact that they are less common, which is, in and of itself, evidence that the effects are mostly coming from properties acquired by medium and large landlords.[18]

[18]Small investors tend to build their portfolios by acquiring previously owner occupied units and renting them out, which mechanically would not enter into these regressions.

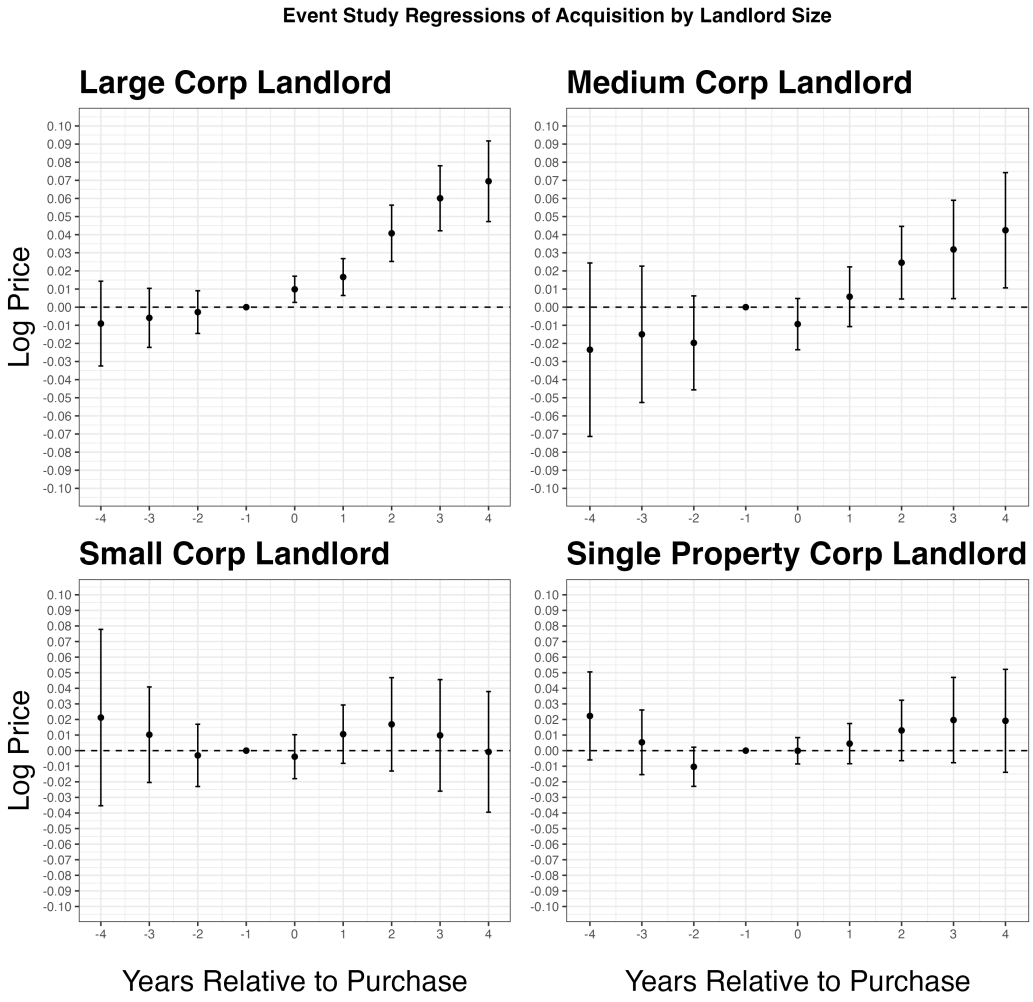


Figure 9: Event Study Results: Landlord Size

Table 6: Acquisition Regressions by Size

Dependent Variable:	Log(Price)			
	Small	Medium	Large	Single
Model:	(1)	(2)	(3)	(4)
Variables				
ATT	0.008	0.013*	0.030***	0.009
	(0.010)	(0.007)	(0.006)	(0.007)
Wald P-Value for Pre-Trends	0.162	0.483	0.127	0.035
Fixed-effects				
Unit	Yes	Yes	Yes	Yes
Year x Metro	Yes	Yes	Yes	Yes
Fit statistics				
Observations	253,706	254,515	262,833	259,468

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

As I showed in my descriptive analysis, on average, acquisitions do not meaningfully change market concentration. Given that there's very little variation in market concentration, it's hard for that to explain why transactions for large landlords increase prices by more than those from single, small, or medium-sized landlords. Instead, what I show in figure 10 and Table 7 is that the positive price effects are likely driven by post-acquisition renovations.

To show this, I define two sets of treatments for these regressions: acquisitions by landlords who do not receive a building permit on the property they acquired and acquisitions by landlords who do. I plot these regressions for both any acquisition and acquisitions by large landlords. I show that for the all acquisitions sample, the price effects are almost entirely explained by renovations, and that for acquisitions by large landlords, the bulk of the effect is explained by renovations (the ATT for large landlords is 3 percentage points, 4.4 for those who permit, 1.1 for those who do not; for any ownership change, the overall effect is 1.6 percentage points, 2.9 for those who permit, and 0.6 for those who do not). As I will show in the subsequent section, large landlords are also more likely to take out building permits following acquisitions.

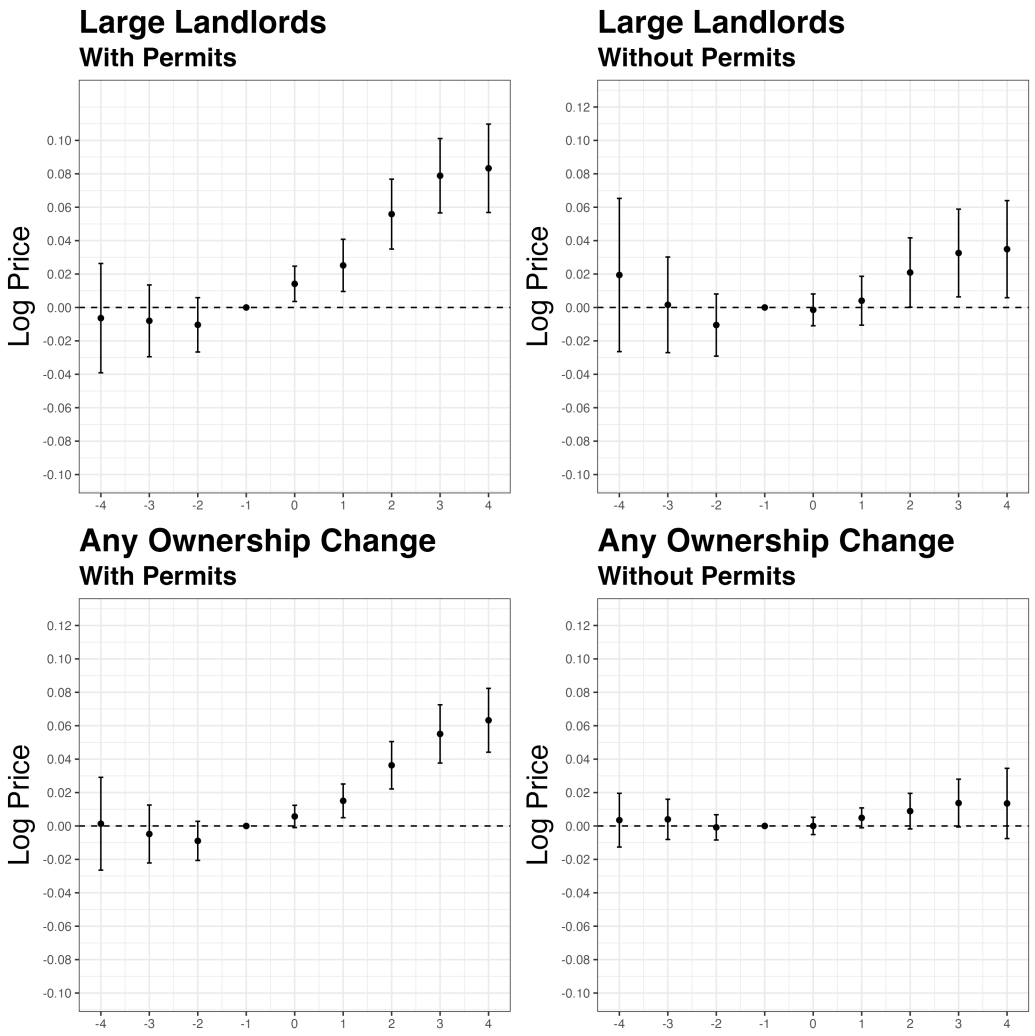


Figure 10: Event Study Results: Permitting Activity

Dependent Variable:	Log(Price)					
	Owner Change	Owner Change (permit)	Owner Change (no permit)	Large	Large (permit)	Large (no permit)
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
ATT	0.016***	0.029***	0.006*	0.030***	0.044***	0.011**
	(0.003)	(0.005)	(0.003)	(0.006)	(0.008)	(0.005)
Wald P-Value for Pre-Trends	0.261	0.303	0.525	0.127	0.696	0.003
Fixed-effects						
Unit	Yes	Yes	Yes	Yes	Yes	Yes
Year x Metro	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	280,261	264,905	267,661	262,833	258,433	256,705

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

Table 7: ATTs by post-acquisition permitting behavior

5.4 Permitting Patterns

In this section, I turn to building permit data to ascertain whether the event study results in the previous section are consistent with large landlord acquired properties being more likely to see renovations and with large landlords taking out larger renovations, conditional on taking out any. To answer this, I regress probability of taking out a permit in the five years post-acquisition as well as post-acquisition permitting intensity on the landlord size of the owner (with the same reference category of "small"), plus city and year fixed effects.

In the first column in table 8, I present the results of a linear probability model regression on whether a parcel received a permit regressed on the landlord size of the owner, plus city and year fixed effects. At baseline, about 13% of acquired units file for a building permit within the subsequent five years (17% for small landlords), however, this varies substantially by property type – about 41% of large multifamily properties file for a building permit within five years of acquisition. A large landlord is around 8% (1.4 percentage points) more likely to file a permit after acquiring a parcel, relative to a small landlord. In the next column, I repeat the previous regression, but control for the number of units of the acquired property. Here, large landlords are about 2 percent less likely to file for a permit than a small landlord, indicating that the higher probability of renovation is primarily due to the kinds of properties large landlords acquire.

Turning to whether large landlords take out more extensive renovations, I further subset my data to parcels that were acquired and which filed at least one building permit following their acquisition. I repeat the previous regression specification, but I use the number of building permits filed in the five years post acquisition as the dependent variable and employ a Poisson specification. The intuition in the regression is that the number of permits provides a crude approximation of how intense renovations are for acquired properties. As I show in table 8, conditional on filing for a building permit, large landlords file for about 2.9 times as many permits (this corresponds to about 19 more permits than a small landlord acquiring a property). Conditional on the number of units in the building, the coefficient on large landlord shrinks by about 50 percent from 1.06 to 0.47, however, the coefficient remains large and statistically significant.

Finally, I leverage the fact that Boston requires permit applicants to disclose the value of their permits.[19] Again, I repeat the same specification

[19]From conversations with the city of Boston, I understand value to be cost

as before, but with the log of the permit value as the dependent variable.[20] Unconditionally, I find that large and medium-sized landlords take on renovations that are around 24 and 44% more costly than those by small landlords. Conditional on the number of units, however, the coefficient on large landlords shrinks to around 18%, although again it remains positive and significant.

Taken together, I interpret my regressions as evidence that large landlords are more likely to acquire properties to renovate them and increase rents, and that the landlords that do renovate are involved in heavier renovations, which can help explain why price effects for properties purchased by large landlords are larger.

[20]Here, I exclude renovations over 10 million dollars.

Table 8: Permit Value and Number of Permits

Dependent Variables:	Permit Filed		Number of Permits		Log Sum Permit Value	
Model:	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	Poisson	Poisson	OLS	OLS
Variables						
Large Owner	0.014***	-0.011***	1.06***	0.466***	0.243***	0.183*
	(0.003)	(0.003)	(0.054)	(0.058)	(0.093)	(0.096)
Medium Owner	-0.002	-0.004	0.171***	0.140***	0.448***	0.427***
	(0.003)	(0.003)	(0.035)	(0.034)	(0.078)	(0.078)
Single Property Owner	-0.042***	-0.043***	0.039***	0.015	-0.319***	-0.315***
	(0.002)	(0.002)	(0.015)	(0.014)	(0.056)	(0.056)
Number of Units		0.002***		0.005***		0.007***
		(5×10^{-5})		(0.0002)		(0.003)
mean values: small	0.173	0.173	10.3	10.3	605,669	605,669
Fixed-effects						
Year	Yes	Yes	Yes	Yes	Yes	Yes
City	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	440,554	440,554	57,737	57,737	13,609	13,609

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

6 Conclusion

In this paper, building on the work done by Gomory and Fish 2024, I provided novel evidence to the scale of corporate landlordship and rental market consolidation and to the business models pursued by different landlord typologies. I documented that while market consolidation has not risen, being able to link shell companies together matters for accurately measuring the levels of market concentration, and that traditional measures of corporatization and consolidation can create inaccurate and incomplete portraits of rental markets.

Further, I show that large landlords have meaningfully different business models as compared to small and medium-sized landlords. In particular, I show that large landlords acquire properties differently than smaller scale ones, and, the properties they do acquire tend to be either underpriced, or, more likely, distressed. On the pricing side, I find landlords raise prices when acquiring properties, that the effect size of these acquisitions is driven mostly by purchases by large and medium-sized landlords, and that, within these medium and large-sized landlords, the effects are driven primarily by properties that are bought and subsequently renovated. While I do not rule out that, where there are large changes in market concentration, there can be large price effects, my results show that for the typical renter, what matters most is whether the landlord undergoes a post-acquisition renovation, and for a particular market, what matters most for concentration is entry, rather than acquisition.

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A Appendix: Data

A.1 Altos Cleaning

To convert these data into a panel that tracks the price history of a single unit over time, I first merge the altos rental listings onto the assessor parcel data described previously. For these merges, I keep only those where there was a unique match between the listing and the parcel. Non-unique matches are most frequent with condominiums, where one address may have fifty distinct parcels (and owners), and, as such, it's unclear to whom the rental listing should be assigned to. In table A.2, I show the results of this merge. Of my 220,000 unique addresses in the Altos data, I match 79% of them to a parcel in the parcel data and 75% of these matches are unique, meaning a listing matched exactly one parcel.

Match Statistics of Altos Listings

Variable	Value
Number of Listings	220,892
Matched Altos	174,356
Matched Unique Altos	164,726
Percent Matched	79
Percent Matched Unique	75

Table A.2: Match Statistics

Following the merge, I filter the listings to be only those that have been on the market for less than ten weeks at the same price. This is done to filter out listings that are left up in perpetuity, and which are not likely to be available to rent.

Next, I group these listings by number of bedrooms and bathrooms within each parcel and aggregate up to a "unit" level by taking the median price across grouped listings, within a year. To make this concrete, consider an apartment complex that offers one bedroom- one bath units, two bedroom- one bath units, and two bedroom - two bath units. Within these unit types, some units will be more expensive than others for unobservable reasons (views, floor size, etc.). Because these unobservables make assembling a true unit panel impossible, I instead aggregate by number of beds and baths to create "representative" units within each building. At the end of this process, I have a sample of about 659,000 bed-bath-unit-years.

One concern with the Altos data is that they skew towards multifamily units and towards higher income neighborhoods. I find this to be partially true. In table 2, I show that Altos listings that match parcels are, relative to the overall rental market, much more likely to be in large, multifamily complexes and within higher income areas. I match about 60% of multifamily rental units to an altos listing, but only about 12% of single family rental units.

A.2 Parcel Cleaning

To get around this, I classify units as rental properties based on whether they have more than three non-condominium units or if the property does not claim an owner occupancy exemption. One potential problem with this method is that a duplex that claims an owner occupancy exemption may still rent out the other unit; however, for measuring concentration, these properties are negligible. Importantly, this classification is not an issue for my event study regressions (beyond determining landlord size) as the sample is defined by having a matched listing in Altos, not by claiming an owner occupancy exemption.

Unit Imputation For Boston, unit data was not available. However, two sources of information allow me to directly infer the correct number of units. First, the detailed land usage and building code information contain the number of units for small properties (1,2,3, and 4 unit buildings) and for larger multifamily properties the Boston address file typically lists one address per unit, which allows me to aggregate up to calculate the total number of units in a property. For the small number of properties where address data was not available and there was not a corresponding land or building code designation, units were imputed based on the square footage of the building. For more information on unit imputation, see Gomory and Fish 2024.

B Appendix: Robustness Checks

In this appendix, I repeat the analysis specified in 5.1, however, instead of following Sun and Abraham 2021, I perform a traditional (two-way fixed effects) event study specification with multiple treatment types estimated simultaneously and with tract-by-year fixed effects. Specifically, I estimate regressions of the form:

$$Y_{it} = \sum_{n=1}^{N} \sum_{\ell \in \mathcal{L} \setminus \{-1\}} \beta_{n,\ell} \, D_i^{(n)} \, \mathbf{1}\{t - T_i = \ell\} \; + \; \delta_{g(i),t} \; + \; \varepsilon_{it}, \tag{1}$$

where $D_i^{(n)} = \mathbf{1}\{\text{type}_i = n\}$ is an indicator for transaction type n. In each regression, transaction types are mutually exclusive, for instance, all corporate acquisitions could be partitioned into single, small, medium, and large corporate acquisitions, each of which would represent a separate treatment.

B.1 Event Study Results

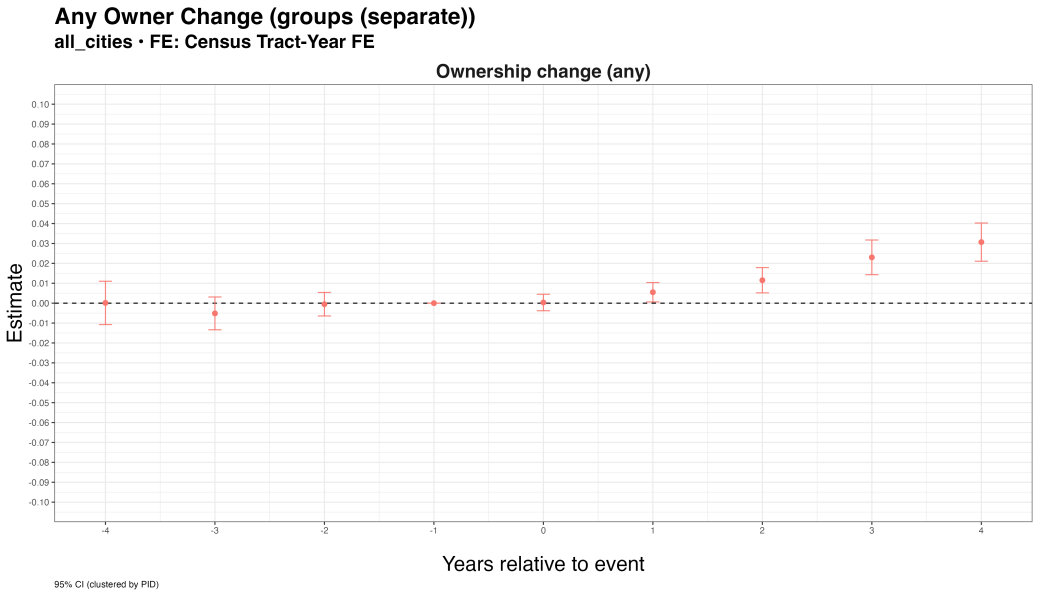


Figure B.1: Event Study Results: Any Change in Owner; Census Tract by Year Fixed Effects

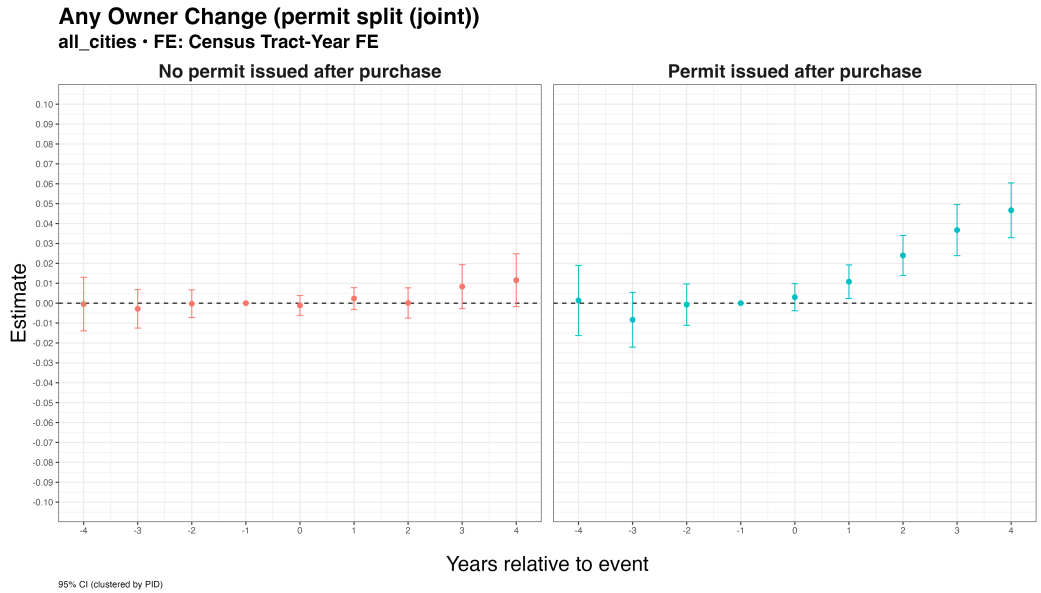


Figure B.2: Event Study Results: Any Change in Owner; Census Tract by Year Fixed Effects; By Permit Behavior

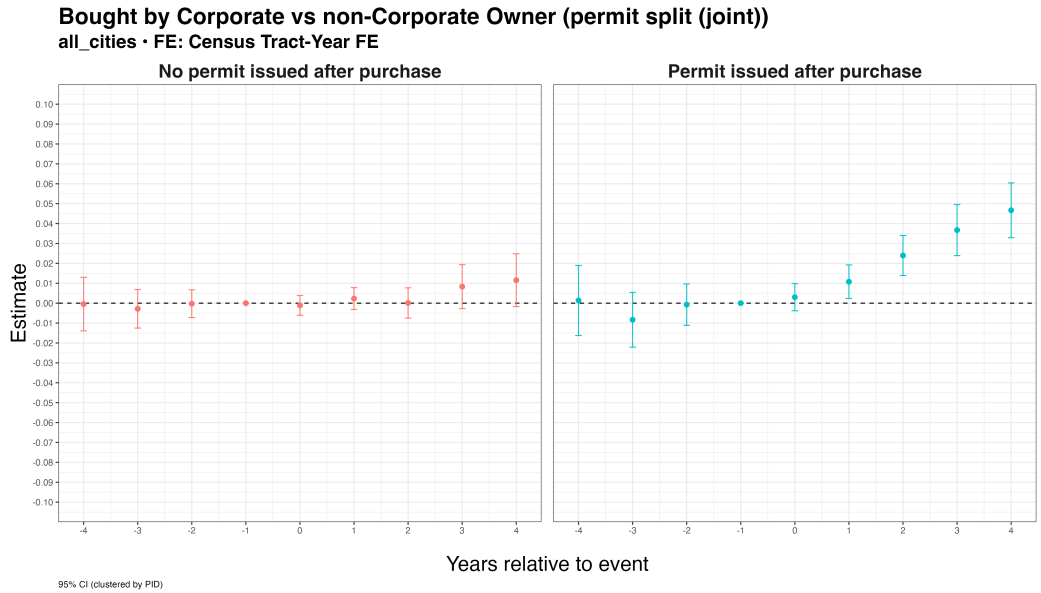


Figure B.3: Event Study Results: Corporate vs non-Corporate; Census Tract by Year Fixed Effects

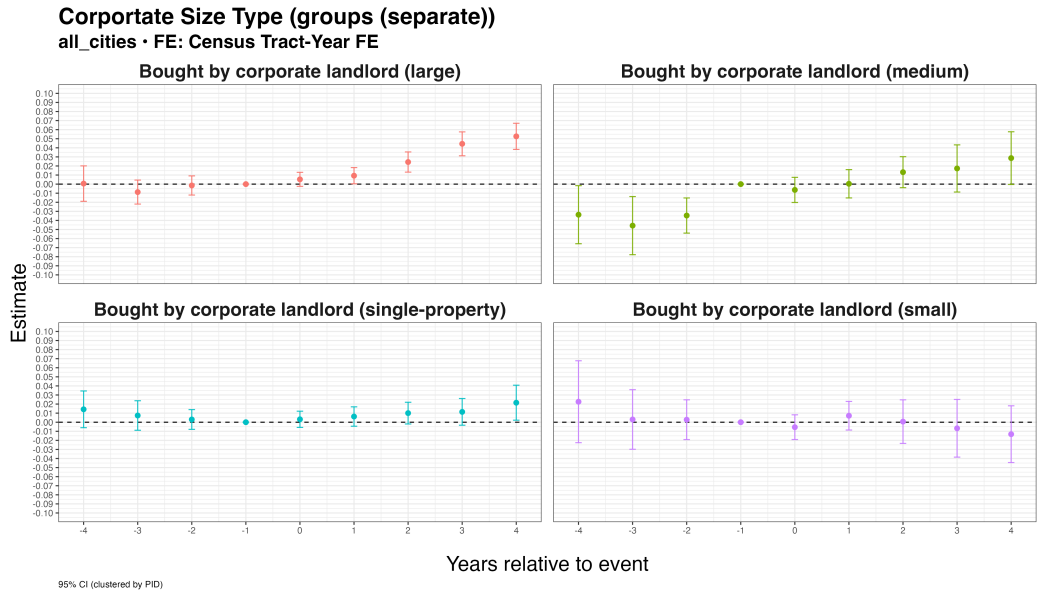


Figure B.4: Event Study Results: Corporate Acquisitions; Census Tract by Year Fixed Effects; By Ownership Type

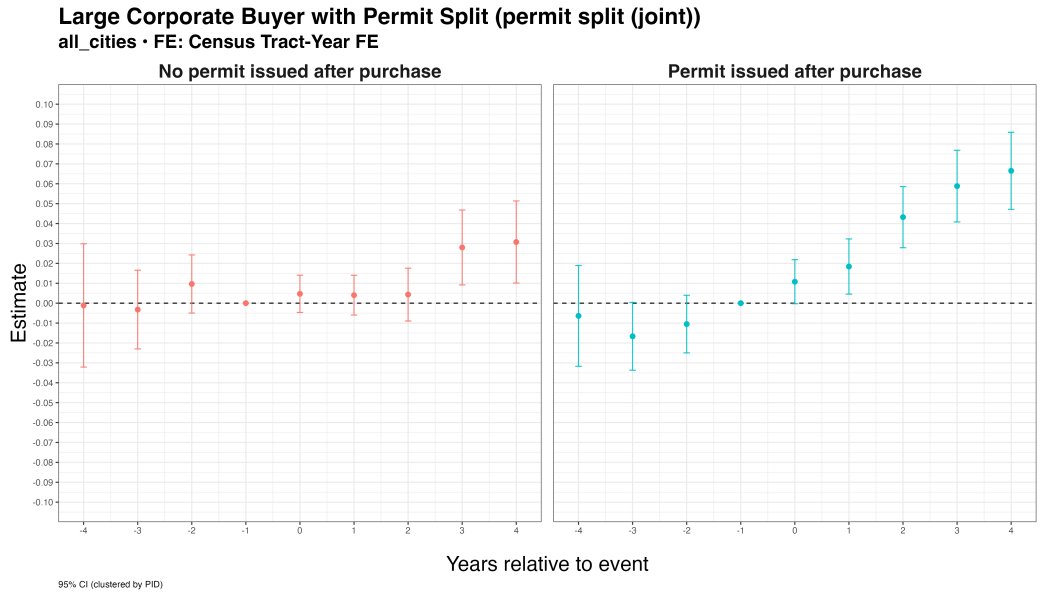


Figure B.5: Event Study Results: Large Corporate Bought; Census Tract by Year Fixed Effects; By Permit Status