Private Equity and Local Entrepreneurship - Spillover Effects of Institutional Ownership of Single Family Homes

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Abstract

I examine the spillover effects of institutional investment in single family housing on a community's economic development. Using mergers between institutional investors as quasi-exogenous shocks to ownership concentration, I find significant increases in housing and rental prices. While previous literature focuses on how rental prices increases harm renters, housing price increases benefit homeowners by reducing their credit constraints, leading to significant increases in mortgage refinancing activity and small business lending for homeowners. Entrepreneurship significantly increases in a neighborhood post-institutional consolidation, much of it concentrated in in tradable industries that are less dependent on local demand, due to reduced renter consumption out of rental price increases. Furthermore, job growth spurred by this surge in entrepreneurship predominantly focuses on low-skilled and low-wage positions.

Keywords: real estate, institutional investors, private equity, entrepreneurship

JEL Codes: G23, G50, H23, L26, R30

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1 Introduction

The purchasing of single-family homes by large investors, especially in the current housing market, serves only to make profits for the investors and provides no value to the communities where these homes are located. People should not have to compete with wealthy private equity and investor firms when they are trying to buy a home in their community.

US Representative Adam Smith, July 2022

The aftermath of the 2008 financial recession saw the emergence of a distinct group of private-equity backed investors seizing an opportunity presented by the foreclosure crisis. Since 2012, a convergence of economic and technological factors has paved the way for so-called institutional investors to transform potential owner-occupied homes into single-family rentals (SFR), a business model commonly referred to as "buy-to-rent." In 2022, the 20 largest U.S. institutional investors alone owned over 360,000 properties, and accounted for 13.2% of all single-family home purchases in 2021.[1] The growing influence of institutional ownership of residential housing has garnered significant attention from policymakers concerned about the potential harm these investors may bring to local communities, including legislative proposals to restrict future institutional ownership.[2] These policymakers argue that institutional investors predominantly harm local residents by driving up housing and rental prices while diminishing homeownership opportunities. However, recent literature indicates that communities can also benefit from the presence of institutional investors in terms of neighborhood safety and diversity. (Gurun, Wu, Xiao, and Xiao 2022; Austin, 2022) While previous literature focuses primarily on the effects on renters, I focus on a different

[1] 360,000 should be taken as a lower bound based on the paper's method of identifying institutionally-owned properties.

[2] In July 2022, Rep. Adam Smith introduced the "Saving Homes from Acquisition by Private Equity Act" bill proposing a 100% tax on private equity housing purchases. The following year saw the rise of similar bi-partisan initiatives in state legislatures such as Ohio and Texas aimed at discouraging institutional investment.

demographic that, aside from institutional investors, benefits from rising housing prices: incumbent homeowners.

This paper focuses on the spillover effects of rising house prices due to institutional investment in single-family housing on homeowner welfare and local economic activity. Institutional investors, by purchasing and converting homes into rental units, consolidate market power and reduce housing supply for potential homeowners. Limited home supply boosts purchase prices through increased competition amongst buyers, and a tighter rental market increases rental prices. However, while renters may be harmed by higher rental prices, higher housing prices benefit homeowners by increasing their housing wealth. This boost in housing wealth can promote real economic activity, as homeowners may increase their consumption or leverage their increased capital to create local businesses and foster entrepreneurship.[3] Conversely, elevated rental prices can reduce renters' local spending, influencing which industries are most likely to see entrepreneurial growth.

Beyond the implications of institutional investment in housing, this paper also weighs in on the discussion on the channel through which housing prices influence the broader economy. While the strong correlation between house prices and real economic activity is widely acknowledged, the precise mechanisms driving this relationship remain a subject of debate, involving either increased consumption out of housing wealth (Gete, 2010; Mian, Rao, and Sufi, 2013; Mian and Sufi, 2014) or the alleviation of homeowner credit constraints. (Kiyotaki and Moore, 1997; Bernanke and Gertler, 1989; Adelino, Schoar, and Severino, 2015; Corradin and Popov, 2015; Schmalz, Sraer, and Thesmar, 2017; Harding and Rosenthal, 2017) Investigating the sectors experiencing the most growth in entrepreneurial activity due to institutional investment allows this paper to contribute to this ongoing debate.

Analyzing institutional investors' local impact raises significant endogeneity concerns, as purchases on concentrated in areas expecting significant housing price and economic growth.

[3]However, increased rental prices may act as a countervailing force of suppressed consumer demand, which may balance out any potential benefits for the local community. As such, policymakers' concern on the potential negative impacts of institutional ownership may still be warranted.

Thus, concentrating on institutional market power alleviates endogeneity linked to entry choices, while permitting an analysis of their economic influence within pre-established markets. To address remaining endogeneity and reverse causality concerns, I utilize four mergers between institutional investors from 2016 to 2017 as quasi-exogenous shocks on single-family housing consolidation. I determine institutional ownership by matching ATTOM property data with institutional investor subsidiary records.[4] While the purchase decisions and neighborhood entries by these investors pose endogeneity challenges, mergers create discrete jumps in an acquiring investor's market share that are plausibly exogenous to local economic conditions. Employing a difference-in-difference methodology, this paper examines the impact of institutional power on local housing costs, credit provision, small business formation, and employment.

I find that the consolidation of institutional investor market share lead to significant local increases in housing prices by 4.9% and rental prices by 2.3%. Increases in rental prices are a result of ownership concentration under a single institutional investor, while increases in housing prices reflect not just a loss of competition but increased demand due to higher costs of rentals. Furthermore, the number of refinancing mortgages and small business loans originated in a neighborhood increase by 5.1% and 5.8% respectively post-consolidation. Refinancing mortgages are by definition driven by homeowners, and separating small business loans driven by homeowners versus non-homeowners show that the increase in small business lending is significant only for homeowners. These results support the credit channel hypothesis, in which credit supply and demand by homeowners increase substantially post-consolidation, as increases in housing collateral increase lender confidence in homeowner creditworthiness.

Following this reduction in credit constraints, many homeowners take advantage of their increased housing wealth as an opportunity to become more entrepreneurial. Using small business registrations as a proxy for entrepreneurial activity, I find that entrepreneurship sig-

[4]Subsidiary names are taken from SEC 10-K filings and OpenCorporates data for public and private investors respectively.

nificantly increases by 1.8% in a neighborhood post-institutional consolidation. This effect is strengthened to 4.0% when industries directly related to housing or finance, or dependent on local consumption are removed from the analysis, which suggests that the entrepreneurship that is arising from institutional investor spillovers is less reliant on local consumption for survival and growth. Additionally, industries that rely on local consumption, such as local amenities, experience a decrease in growth in neighborhoods where mergers have occurred. This result runs counter to the consumption channel mechanism, as increased rent prices lead to diminished consumption spending by renters. Collectively, these results imply that homeowners, benefiting from relaxed credit constraints, become more entrepreneurial, particularly in industries less dependent on local demand — a demand reduced by concurrent rent price increases.

Apart from the effects of declining local consumption, there are additional non-positive spillover effects from institutional investment in housing. I find that job creation arising out of the increase in entrepreneurial activity is concentrated amongst low paying, low skilled jobs with less than $3,333 monthly income, and requiring less than a bachelor's degree.[5] Job creation is also concentrated in relatively more blue-collar industries such as manufacturing, with high-tech industries such as finance and information actually experiencing negative effects on growth for both firm and job creation. Similarly, a modest reduction in median income and educational attainment are noted.

In conclusion, this study reveals that increasing institutional ownership of single-family homes impacts economic development in neighborhoods, benefiting homeowners while disadvantaging renters. Homeowners enjoy the perks of rising housing prices, decreased credit constraints, and more entrepreneurial opportunities, whereas renters face financial stress due to escalating rental costs, reducing their local consumption. Thus, entrepreneurial growth is limited to firms with the ability to expand their market beyond the local community. Although institutional investors can stimulate economic growth by injecting capital and

[5] Questions about the dislocation of renters due to being priced out of a community are unfortunately beyond the scope of this paper due to data limitations.

supporting small businesses, this uneven benefit distribution may simultaneously enhance inequality among households and firms. Future policies concerning institutional investors should aim to maintain the positive effects on homeowner wealth while mitigating the negative impacts on renter welfare.

The structure of this paper is organized as follows. Section 2 offers a comprehensive overview of the institutional details and provides a literature review that is relevant to the study. The key empirical predictions of the paper are presented in Section 3. Section 4 describes the data used in the analysis and provides information on the investors and mergers included in the sample period. The empirical results are presented in Section 5. Finally, Section 6 concludes the paper by summarizing the findings and offering suggestions for future research directions.

2 Institutional Details and Literature Review

Private equity's interest in single-family residential housing as an investment gained momentum with the launch of a pilot program by the Federal Housing Finance Agency (FHFA) in 2012. This initiative was a response to the aftermath of the 2008 financial crisis, during which a surge in foreclosures posed a threat to the overall economic recovery (Foroohar, 2016). In February 2012, the FHFA introduced a pilot program specifically designed for selected metropolitan areas. The program involved the auctioning of foreclosed properties to private investors, aiming to stabilize housing prices and meet housing demand that individual buyers were unable to fulfill. The targeted metropolitan areas included Atlanta, Chicago, Las Vegas, Los Angeles, Phoenix, and parts of Florida (Ganduri, S. C. Xiao, and S. W. Xiao, 2023).

Private equity's involvement in the single-family residential housing market gained traction following the launch of the FHFA's pilot program in 2012. Since the conclusion of the pilot program, private equity firms have rapidly expanded their presence in the national residential housing market, with estimates suggesting they purchased around \$16 billion (or potentially \$20 billion) worth of properties between 2012 and 2014 alone (Mills, Molloy, and Zarutskie, 2019; Mari, 2020). Notably, Blackstone's real estate holdings alone reached approximately \$60 billion by 2020 (Mari, 2020).[6]

Although the majority of single-family rentals are still owned by individual investors, institutional investors have concentrated their holdings, which allows them to wield a significant market power.[7] (Austin, 2022; Gurun et al., 2022; Christophers, 2023) The growth of institutional investor market power has been found to have contributed to at least half of the U.S. housing market recovery post-2008. (Lambie-Hanson, W. Li, and Slonkosky, 2019; Allen et al., 2018; Ganduri, S. C. Xiao, and S. W. Xiao, 2023) This market power has come at the expense of renters, as institutional investors utilize their power to extract higher greater rental surplus from renters and increase eviction rates. (Gurun et al., 2022; Raymond et al., 2016) In the post-COVID-19 landscape, institutional investors have shifted their focus to new markets such as Boise and North Carolina to capitalize on the trend towards remote working. Additionally, they have directed investments toward neighborhoods with predominantly minority residents, which has raised concerns about the impact that institutional investors may have on minority communities in particular.[8]

Institutional investors enjoy advantages over individual homebuyers, including substantial cash reserves and access to credit lines with lower interest rates than mortgages. This enables them to acquire properties at a significant discount (Smith and Liu, 2020; Allen et al., 2018). Technological advancements since 2008 have also facilitated institutional investors' ability to leverage economies of scale, enabling them to acquire and efficiently manage large portfolios of single-family properties across the United States. These advantages, coupled with a low-interest-rate environment and the availability of foreclosed housing being auc-

[6]The New York Times article cited misattributes the real estate purchases to Blackrock, rather than Blackstone. While the \$60 billion figure is roughly correct, the firm responsible for that figure is incorrect.

[7]Institutional investors are particularly present in key metropolitan areas, such as Sun Belt regions like Atlanta and Phoenix.

[8]Source: https://www.washingtonpost.com/business/interactive/2022/housing-market-investors/

tioned off by the government, have driven the entry of additional institutional investors into the single-family housing market. (Christophers, 2023)

Recent studies have also examined mergers between institutional investors and their spillover effects on communities. For example, Gurun et al. (2022) finds that institutional investor consolidation leads to higher rental prices but also enhances neighborhood quality by reducing crime rates. In the Atlanta region, Austin (2022) observes significant increases in housing prices and rents following investor consolidation, but also notes increases in socio-economic diversity due to minority residents being attracted by increased rental supply. While these papers primarily focus on how institutional investors extract wealth from renters, the present paper explores spillovers that can increase the wealth of resident homeowners, leading to increased real economic activity through entrepreneurship.

Furthermore, this paper relates to the broader literature on the linkages between housing wealth and the real economy. A significant debate exists in the literature on how housing wealth affects the real economy through homeowner behavior. One side of the debate emphasizes the consumer demand channel[9], where house prices shocks influence consumer demand. (Gete, 2010; Mian, Rao, and Sufi, 2013; Mian and Sufi, 2014; Gao, Sockin, and Xiong, 2020; Z. Li, Shen, and Zhang, 2021) Since household spending, and thus local consumer demand, is positively correlated with housing prices, shocks to housing values should have the greatest impact on firm growth and employment in non-tradable industries that are most reliant on local demand. The other side of the debate emphasizes on the credit constraints channel[10], where increases in housing collateral from price shocks increase lender willingness to extend credit to homeowners. With better access to credit, homeowners then become more entrepreneurial, leading to greater small business formation and employment. (Kiyotaki and Moore, 1997; Bernanke and Gertler, 1989; Adelino, Schoar, and Severino, 2015; Corradin and Popov, 2015; Schmalz, Sraer, and Thesmar, 2017; Fairlie and Krashinsky, 2012; Harding and Rosenthal, 2017; Guren et al., 2020) In this narrative, since smaller

[9] Emphasized more in the macreconomics literature

[10] Emphasized more in the entrepreneurial finance literature

firms and individual households tend to be more credit constrained, and housing wealth is often their greatest source of capital, increases in housing prices should lead to spillover effects being concentrated amongst smaller firms. Furthermore, firms that are concentrated in the tradable sector are more likely to benefit compared to the alternative hypothesis, as firm creation out of increased willingness for lenders to extend credit should be unrelated to local consumer demand. The results of this study strongly corroborate the credit constraint channel hypothesis, suggesting that house price increases due to institutional consolidation foster greater lending to homeowners and stimulate increased entrepreneurial activity within firms operating in the tradable sector.

Finally, this paper contributes to the literature on the impact of private equity investors on the real economy and societal welfare. Previous studies have highlighted benefits to consumers in areas such as food safety, workplace injuries, and product offerings (Bernstein and A. Sheen, 2016; Cohn, Nestoriak, and Wardlaw, 2021; Fracassi, Previtero, and A. W. Sheen, 2020), as well as drawbacks such as higher prices and lower product quality (Chevalier, 1995; Matsa, 2011; Eaton, Howell, and Yannelis, 2019). In terms of the housing market in particular, complaints by consumer advocacy groups regarding institutional investors shirking responsibility for property maintenance and repair have been supported by research (Fields, 2022). This paper aligns with both the positive and negative views of private equity's impact on household welfare, with the benefits and downsides bifurcated mainly between homeowners and renters, respectively.

3 Hypothesis Development

This subsection introduces the hypotheses that will be investigated in this paper, focusing on the impact of mergers between institutional investors on local communities. The aim is to analyze the differential spillover effects of these mergers on homeowners and renters within housing markets. The hypotheses revolve around the notion that homeowners are expected to experience advantages from increased house prices, resulting in reduced credit constraints. Conversely, renters are likely to face challenges due to rising rental prices, leading to decreased consumption. Consequently, the presence of institutional investor market power generates both positive and negative externalities for economic development.

Hypothesis 1 (H1): *Following institutional mergers, housing and rental prices increase in neighborhoods where both acquirers and targets owned properties.*

The increasing presence of institutional investors in the single-family housing market results in a higher level of market concentration controlled by a single landlord. This concentration enables landlords to exploit their market influence to extract more benefits from renters. (Gurun et al., 2022) Additionally, the consolidation of single-family properties under a sole owner diminishes the bargaining power of other buyers in the market, ultimately leading to an increase in house prices.

Hypothesis 2 (H2): *Due to homeowners experiencing a decrease in credit constraints from greater home equity, mortgage borrowing activity increases following institutional mergers in neighborhoods where both acquirers and targets owned properties.*

House price increases from institutional mergers raise the value of home equity for local homeowners. According to the credit constraints channel, this increase in home equity reduces credit constraints and enables homeowners to engage in more mortgage borrowing activity, leading to increased mortgage lending in neighborhoods where both acquirers and targets owned properties.[11]

Hypothesis 3 (H3): *Small business activity rises following institutional mergers in neighborhoods where both acquirers and targets owned properties. This effect is stronger for tradable industries, and is offset for non-tradable industries due to reductions in consumption.*

[11] Similar effects can be found in small business lending activity, which is also examined in the paper.

The consolidation of institutional investors has mixed effects on small businesses, which varies depending on industry. On one hand, increased rental prices reduces consumer purchasing power and negatively impact non-tradable industries that rely on local consumption, which goes against the consumption channel. On the other hand, in line with the credit constraint channel, reduced credit constraints for homeowners lead to increases in small business formation and lending activity, particularly among tradable industries that are less exposed to local demand. Overall, small business activity will increase in neighborhoods where both acquirers and targets owned properties prior to a merger, particularly amongst tradable industries.

Hypothesis 4 (H4): *Renters experience a reduction in disposable income due to higher rent prices, leading to lower consumption and income growth following institutional mergers in neighborhoods where both acquirers and targets owned properties.*

Higher rental prices resulting from institutional mergers create financial burdens for renters, leading to a decrease in disposable income. This reduction in income may, in turn, lead to lower levels of consumption and overall income growth in neighborhoods where both acquirers and targets owned properties.

4 Data

4.1 Data sources

Housing Property and Transaction Data: Data on property and transaction-level information is sourced from ATTOM, a provider that collects property and transaction-level information from county tax assessor records across the United States. ATTOM's database includes details on property location and characteristics, as well as transaction data. For each transaction, the data covers the transaction date, property address, names and mailing addresses of the counterparties involved, cash amounts transferred, and the nature of the transaction (e.g., arms-length purchase, cash payment, or financed with a mortgage).[12] The property-level characteristics provided by ATTOM encompass various details such as property type, address, lot and building size, year built, number of rooms, number of bedrooms and bathrooms, and additional features like porches, pools, basements, attics, and garages. To identify properties owned by institutional investors before mergers, the transaction data is utilized. This information is then merged with assessor-level data to determine the neighborhoods included in the sample. The definition of neighborhoods may vary depending on the empirical analysis, either based on zip codes or census tracts.

Mortgage Origination Data: Under the Home Mortgage Disclosure Act (HMDA), the Consumer Financial Protection Bureau publishes a public dataset for the vast majority of home mortgage applications and originations in the United States since 1990. This loan-level dataset contains information on the location of the mortgage to the census tract level, the size of the mortgage, loan purpose, loan type, owner-occupancy status, property type, applicant income, lien status, and the gender and race/ethnicity of the applicant. Following Duchin and Sosyura (2014) and Vojtech, Kay, and Driscoll (2020), I restrict my analysis to single-lien, conventional mortgages for owner-occupied, single family homes between 2012 and 2020.

Price indexes: Single family housing price and rental price zipcode-level indexes are used to assess the impact of institutional investor consolidation on housing prices. To construct the single-family housing price index, the study employs a hedonic regression model using arms-length transaction data obtained from ATTOM. The data covers the period between 2007 and 2022 and is merged with property-level assessor data to incorporate property characteristics. The hedonic regression model focuses on single-family houses, condos, coops, and townhouses. For a more detailed description of the index construction, reference can be made to Appendix C.2. The rental price index used in the analysis is sourced from Zillow, specifically the Zillow Observed Rent Index (ZORI), which provides monthly rent data at

[12] It's important to note that while buyer information is generally available, seller identities may be less frequently recorded by county tax assessor offices.

the zipcode level in nominal dollars. ZORI captures changes in rental prices over time by comparing price differences for the same rental units across different periods. This approach allows Zillow to account for variations in rental unit quality, ensuring that any observed effect of institutional investors on rental prices is not driven by changes in rental quality. Both indexes are deflated using the consumer price index (CPI) published by the U.S. Bureau of Labor Statistics, using 2007 as the benchmark.

US Census: The U.S. Census Bureau provides detailed annual economic and demographic datapoints for zipcodes[13] and census tracts throughout the United States via the American Census Survey (ACS). I collect data from the ACS 5-year survey on population, median income, home-ownership, number of housing units, poverty, employment, and ethnicity between 2012 and 2020. The Census Bureau also provides business registration data via the ZIP Codes Business Patterns (ZBP) dataset[14], providing data on the number of businesses registrations both in aggregate, and by firm size and NAICS code on a zipcode level. Lastly, the Census Bureau also provides detailed employment information via the LEHD (Longitudinal Employer-Household Dynamics) Origin-Destination Employment Statistics (LODES) dataset. LODES provides census block-level information on the number of jobs with unemployment insurance[15] for select states and years. Annually, I aggregate the number of private primary jobs[16] present in each census block up to the census tract and zipcode level by salary, education, and sector.[17]

[13] The ACS data technically provides data on ZIP Code Tabulation Areas (ZCTAs), rather than zipcodes directly. I match ZCTAs to zipcodes on an annual level.

[14] Zipcode-level data was moved to the County Business Patterns database after 2019.

[15] The unemployment insurance requirement excludes persons who are self-employed. As such, the effect of institutional investor mergers on employment numbers is driven mainly by additional hires from small business owners, rather than by an increase in the number of small business owners.

[16] By focusing on private primary jobs, I exclude Federal jobs, as well secondary jobs to focus on the impact of institutional investors on the overall number of people employed.

[17] LODES classifies sectors by two-digit NAICS codes. Detailed information on classification can be found on the LODES documentation.

4.2 Identifying Institutional Investors

The identification of institutional investors in assessor and transaction-level data poses challenges due to the use of subsidiary names to obscure ultimate ownership. To address this, I employ a method that combines the approaches of Ganduri, S. C. Xiao, and S. W. Xiao (2023) and Austin (2022) to identify institutional investor ownership. The process begins by examining transaction data from 2007 to 2022 and excluding non-arms-length transactions and transactions conducted by individuals. Next, I identify company owner mailing addresses associated with at least 100 transactions in a single year, resulting in a set of 500,261 addresses.

To match these addresses to institutional investors, I employ a combination of techniques. First, I fuzzy match on subsidiary names and addresses between ATTOM transaction records and institutional investor subsidiary names. Subsidiary names are taken from 10K filings for public firms, and hand-collected data from OpenCorporates for private firms. By leveraging these methods, the study links company names listed in property transaction data to one of the 23 largest institutional investors in the United States. Manual filtering is then conducted to eliminate incorrectly matched names and mailing addresses. This process is repeated iteratively until no further institutional investor subsidiaries can be identified from the data.

Upon completion, I identify a total of 368,316 single-family homes owned by 23 institutional investors as of the end of 2022. This matching process also captures the timing of each single-family rental (SFR) investment based on the last recorded transaction appearing in the database, categorized by year. Notably, the list of institutional investors includes those who have undergone mergers with other investors prior to 2022, yet still have properties listed under former subsidiaries.

For a more detailed description of the identification algorithm, you can refer to Appendix 4.2, which provides comprehensive information on the steps taken to identify institutional investors from the transaction data.

4.3 Institutional Investor Mergers

I identify four mergers of large institutional investors based on a web search and M&A records from the Securities Data Company (SDC) database. These mergers took place between 2015 and 2017.[18] Table 1 provides details of each merger, including the acquirer and target, the number of properties identified for each party, and the number of states and counties involved in each merger.

It is worth noting that all but one of the institutional investors involved in the mergers were publicly traded at the time. This allowed for easy identification of subsidiaries from the firms' 10K filings. In the case of Colony American Homes, which was private at the time of the merger, the combined firm, Colony Starwood, provided a comprehensive portfolio overview pre- and post-merger. This information enabled the identification of subsidiaries belonging to Colony American Homes and Starwood Waypoint prior to the merger.

The impact of institutional investor mergers on housing markets was geographically dispersed, spanning between 16 and 40 states for each merger. Unlike previous studies that focused on a subset of markets due to data limitations, this study benefits from ATTOM's comprehensive nationwide coverage of single-family transactions. As a result, it enables an exhaustive examination of the impact of mergers on housing markets across the United States.

4.4 Summary Statistics

Table 2 present the market share of acquiring institutional investors on both a census tract and zipcode level before and after mergers.[19] In each neighborhood, I calculate market share based on the number of properties owned by acquirers, divided by the total number of

[18]The additional merger between Front Yard Residential and Progress Residential in January 2021 is not included in the analysis due to the lack of census data beyond 2021 at the time of writing.

[19]I interchangeably refer to both geographic levels as "neighborhoods" throughout the rest of the paper, unless the level is specified. My definition of "neighborhood" is closer to Austin (2022) than to Gurun et al. (2022), the latter of which define "neighborhoods" as census tracts and census blocks depending on data availability.

single-family houses as described by ATTOM in the geographic area pre-merger. I also divide neighborhoods into "treatment", "large treatment", "very large treatment", and "control" groups. This division refers to neighborhoods with at least some overlapping properties between acquirers and targets, neighborhoods where acquirers gained at least 1% market share, neighborhoods where acquirers gained at least 2% market share, and neighborhoods with no overlapping properties between acquirers and targets, respectively. The statistics in the first two rows show that for a treated zipcode, the average market share of acquirers is 0.79% pre-merger and 1.38% post-merger. Similarly, for a treated census tract, the average market share of acquirers is 1.41% pre-merger and 1.91% post-merger. The market share statistics for census tracts are roughly in line with those of Gurun et al. (2022), which report a market share for acquirers that is 1.1% pre-merger, and 2.7% post-merger.[20]

Tables 3a and 3b presents a similar set of summary statistics for housing and rental prices respectively for treated and control zipcodes pre- and post-merger. Both housing prices and rental prices have been increasing over time, but prices have been increasing faster for treated neighborhoods in comparison to control neighborhoods. House prices increased on average by 48.5% for control neighborhoods, 54.2% for treated neighborhoods, 61.5% for neighborhoods with large treatment, and 68.4% for neighborhoods with very large treatment. Similarly, rental prices increased by 17.7% for control neighborhoods, 20.9% for treated neighborhoods, 24.7% for neighborhoods with large treatment, and 29.3% for neighborhoods with very large treatment. The fact that price growth increases monotonically with treatment size provides evidence that increases in acquirer market share has an impact on both housing and rental prices.

[20]Note that Gurun et al. (2022) defines market share as the share of rental units, rather than the share of single-family houses.

5 Empirical Results

5.1 Methodology

I utilize the four largest institutional SFR mergers to ascertain the impact of institutional investor market share consolidation on housing prices and entrepreneurship. Mergers between institutional investors create distinct jumps in the acquiring institutional investors' market share within neighborhoods, contingent on the level of overlap between the acquiring and target entities' portfolios. My main identification assumption is the independence of the degree of overlap between acquiring and target firms in a neighborhood from local economic conditions. Essentially, changes in ownership concentration are endogenous at a county level[21], but at a neighborhood level are plausibly exogenous to the underlying economic conditions. Under this assumption, this empirical framework enables me to isolate the impacts of institutional landlords from other coinciding economic and demographic changes in a neighborhood.[22]

To control for the potential selection effect of acquirers picking target firm portfolios in neighborhoods with higher growth potential, my sample consists of neighborhoods that have at least one property owned by an acquirer or a target firm in the year before the merger's completion. Neighborhoods where both acquirer and target firms owned properties in the year prior to a merger's completion are identified as "*treated*" neighborhoods, while those where only acquirer or target firms owned properties before the merger are labeled as "*control*" neighborhoods.[23] For each neighborhood, I use data from four years before the mergers to three to four years after the mergers in my panel dataset, depending on

[21] As Invitation Homes states under their 2017 10-K filing: "More specifically, we believe that the [m]ergers created a diversified and high-quality portfolio of homes in high-growth markets."

[22] Similar works such as the seminal paper Hastings (2004) and Azar, Schmalz, and Tecu (2018) similarly utilize mergers of geographically dispersed portfolios as a means of isolating the differential local impact of changes in ownership concentration.

[23] In situations where two or more mergers took place in different calendar years within the same neighborhood, I tally the number of properties acquired by all firms involved, assign the treatment year to the year of the most recent merger(s), and remove the neighborhood-year observations associated with the earliest merger(s).

data availability.[24] Since all mergers were finalized between 2016-2017, this provides me with at least three years of data for each neighborhood to examine the impact of mergers on outcome variables. I then estimate the following difference-in-difference model for all outcome variables:

$$Y_{n,c,t} = \alpha + \beta_1 Treat_n \times Post_{n,t} + \beta_2 Post_{n,t} + \beta_3 Treat_n + X_{n,t} + \gamma_{c,t} + \theta_n + \epsilon_{n,c,t} \tag{1}$$

$Y_{n,c,t}$ is the outcome variable for neighborhood n in county c in year t. $Post_{n,t}$ is an indicator variable that equals one for neighborhood-year observations after the completion of a merger. $X_{n,t}$ are control variables for neighborhood n, which are log total population, log median income, poverty rate, employment rate, labor force participation, homeownership, minority percentage of the population, and population ratios by age and education. To control for unobservable time-varying characteristics at a county level, I include $\gamma_{c,t}$ county $\times$ year fixed effects. This excludes county-level fundamentals such as public policies, demographics, and economic conditions from confounding the estimated treatment effect of landlords mergers on my outcome variables. In addition, I also include neighborhood fixed effects to control for any unobservable neighborhood-level characteristics that may impact local prices (such as geographic characteristics and climate conditions).

$Treat_{n,c}$ is an indicator variable that takes a value of one if a neighborhood n is treated. I use four different levels of treatment based on the increase in market share for acquirer firms after institutional mergers are completed. First, $I(\Delta(MarketShare) > 0)$ is a binary variable that equals one if acquiring firms gain any properties from target firms in neighborhood n as a result of institutional mergers. Second, $I(\Delta(MarketShare) \geq 1)$ is a binary variable that equals one if acquiring firms increase their market share of single family homes by at least one percentage point after institutional mergers in a neighborhood n. I define this as a "large" treatment, and is used as the baseline of my analysis throughout my paper.

[24] Data on small business registrations is available up until 2020, whereas other datasets extend their coverage up to the years 2021-2022.

Third, "very large" treatment is defined as $I(\Delta(MarketShare) \geq 2)$, which is a binary variable that equals one if acquiring firms increase their market share of single family homes by at least two percentage points after institutional mergers in a neighborhood n. Lastly, $\Delta(MarketShare)$ is a continuous variable which equals the total market share gained by acquirers as a result of institutional mergers.

Figure 1 showcases the treatment effect for census tracts in the greater Atlanta region, a market known to have drawn significant attention from institutional investors. Census tracts colored in the lightest blue represent neighborhoods where none of the properties were owned by institutional investors involved in the four mergers, hence these are excluded from the sample. The slightly darker, sky-blue census tracts are the control neighborhoods, where properties were owned by either acquirer or target firms, but not both. Census tracts shaded in an even darker baby blue are neighborhoods where both acquirer and target firms held property before the mergers. In my empirical setting, these are classified as "treated" census tracts according to the initial definition of treatment, but are reclassified into the control sample under stricter definitions. Consequently, darker shades of blue correspond to census tracts that experience more extensive degrees of treatment.

Utilizing mergers as a treatment setting allows for the identification of the impact of increased concentration of institutional ownership that is potentially independent of local market conditions, which could otherwise influence selection. Institutional investors are known to employ complex algorithms[25] that integrate information on a multitude of market factors such as population growth, economic growth, education quality, crime rates, and others to select local markets for investment. (Christophers, 2023) Therefore, concentrating on neighborhoods with pre-existing institutional presence helps to minimize the selection bias associated with investor market entry decisions, facilitating a cleaner, quasi-exogenous setting to investigate the impact of institutional concentration on housing prices and economic activity.

[25] Some institutional investors, such Amherst Capital, openly advertise their methodology in their annual reports. Source: *U.S. Single-Family Rental: An Emerging Institutional Asset Class* (2016)

Despite this, the treatment effects may still be susceptible to selection bias for two reasons. Firstly, an acquiring firm might target another based on potential market synergies between their portfolios, leading to endogeneity in the overlap between acquiring and target firms. Press releases surrounding institutional mergers frequently highlight benefits in terms of cost efficiencies, indicating that treatment effects could be confounded by endogeneity.[26] Secondly, investor mergers might occur due to a misalignment between the acquirer and target's valuation of the target firm's portfolio. If a merger is dependent on the acquirer having a more optimistic perception of the growth prospects of the markets in the target portfolio, then selection bias could arise in the neighborhoods that are treated. Higher valuations might lead to a pre-existing acquirer presence in the neighborhood pre-merger.

Despite these potential issues, my interpretation of the empirical estimates remains internally valid, as the diff-in-diff estimator still captures the causal effects of mergers on outcome variables. However, in this empirical setting, selection bias in the choice of targets implies that the estimated average treatment effect on the treated would be larger than the average treatment effect. This is the predicted effect of institutional concentration on housing and rental prices, entrepreneurial activity, and employment.

5.2 Housing Prices

To test Hypothesis 1, I first examine the impact of institutional mergers on neighborhood house and rental prices:

$$log(Price)_{n,c,t} = \alpha + \beta_1 Treat_n \times Post_{n,t} + \beta_2 Post_{n,t} + \beta_3 Treat_n + X_{n,t} + \gamma_{c,t} + \theta_n + \epsilon_{n,c,t} \quad (2)$$

[26]For example, with respect to the Starwood Waypoint-Colony American Homes merger, the 2015 proxy statement by Starwood Waypoint states that "SWAY and CAH have portfolios with substantial market overlap, and the Combined Company Home Portfolio is characterized by a significant number of homes in each of its markets. Management believes this market overlap and density will create operating efficiencies due to economies of scale." Similarly, the 2015 10-K filing for American Homes 4 Rent points to the expected benefits of merging with American Residential Properties in terms of "expected operating efficiencies, cost savings, revenue enhancements, synergies or other benefits".

$$log(Rent)_{n,c,t} = \alpha + \beta_1 Treat_n \times Post_{n,t} + \beta_2 Post_{n,t} + \beta_3 Treat_n + X_{n,t} + \gamma_{c,t} + \theta_n + \epsilon_{n,c,t} \quad (3)$$

$log(Price)_{n,c,t}$ equals the logarithm of the house price index for a neighborhood n in year t.[27] As detailed in Appendix 6, the house price index is calculated using a hedonic regression model with ATTOM transaction data. Similarly, $log(Rent)$ equals the logarithm of the Zillow rental price index for a neighborhood n in year t. Table 4 presents the results of the diff-in-diff estimation of Equations 2 and 3 in Panels A and B, respectively.

In line with Hypothesis 1, both house prices and rental prices increase significantly following mergers between institutional investors. House prices for neighborhoods that have experienced a large institutional consolidation increase by 4.9% post-merger, whereas rental prices increase by 2.5% post-merger. In line with both Gurun et al. (2022) and Austin (2022), neighborhoods that experience only a small consolidation in institutional ownership do not see significant increases in house prices, whereas rent prices still see an increase of 1.1%. However, when institutional consolidation is large, the impact of mergers on housing prices becomes larger in both significance and magnitude than on rental prices.

Figure 2 provides an event study on the impact of a merger on housing and rental prices in a neighborhood where acquirers gained at least 1% market share. Recent econometrics literature contends that with staggered treatment, the typical twoway fixed effect (TWFE) estimate is heavily biased. Papers such as Goodman-Bacon (2021), Sun and Abraham (2021) and Callaway and Sant'Anna (2021) argue that due to treatment effect heterogeneity, the TWFE estimate becomes a weighted average of all treatment effects across event dates, as well as arising inconsistency from including late-treated observations in the control group for early-treated observations. This inclusion introduces treatment effects from other events, leading to significant bias in the TWFE estimate.

In response to the variability in treatment effects, I draw insights from contemporary advancements in the econometric difference-in-differences field, influenced by the works of Sun and Abraham (2021) and Callaway and Sant'Anna (2021). To examine the effects over

[27] To prevent outliers from skewing my results, house prices are winsorized at the 1% level.

time, I adopt the interaction weighted (IW) estimator as introduced by Sun and Abraham (2021). This approach allows me to estimate dynamic treatment effects, with the year before treatment serving as the baseline period. Additionally, I conduct supplementary robustness assessments to gauge the average treatment on the treated (ATT) across the group and time. These checks are detailed in Subsection 5.9.[28]

The results in Figure 2 show that the impact of institutional consolidation on single-family house prices is immediate and persists for two years after the merger, whereas rental prices do not diverge until four years afterwards. I detect no violation in the parallel trends assumption, as both house prices and rental prices between treated and control neighborhoods do not diverge before the mergers.

Increases in housing and rental prices have differential impacts on residents based on homeownership status. While the first order effects of institutional consolidation on housing prices are negative for renters, homeowners directly benefit from a higher institutional market share. Greater housing prices raise not only the value of institutional properties, but also raise the value of owner-occupied homes, thus increasing the home equity of local homeowners. How homeowners internalize this benefit, either via a reduction in credit constraints or an increase in the propensity to consume, is explored in the remainder of this section.

5.3 Mortgage Demand and Supply

Having found evidence for Hypothesis 1 that housing and rental prices significantly increase post-merger, I next examine whether Hypothesis 2 follows in that house price increases lead to reduced constraints for homeowners. To test this hypothesis, I examine changes in local mortgage application and origination activity surrounding institutional mergers. Due to geographic availability in the HMDA data, I define neighborhood as a census tract rather than as a zipcode for mortgage activity. I estimate the following diff-in-diff models for both

[28]Analogous robustness checks are also executed for evaluating changes in entrepreneurial activity post-merger.

purchase and refinancing mortgages:

$$log(Apps+1)_{n,c,t} = \alpha + \beta_1 Treat_n \times Post_{n,t} + \beta_2 Post_{n,t} + \beta_3 Treat_n + X_{n,t} + \gamma_{c,t} + \theta_n + \epsilon_{n,c,t} \quad (4)$$

$$log(Orig+1)_{n,c,t} = \alpha + \beta_1 Treat_n \times Post_{n,t} + \beta_2 Post_{n,t} + \beta_3 Treat_n + X_{n,t} + \gamma_{c,t} + \theta_n + \epsilon_{n,c,t} \quad (5)$$

$log(Apps)_{n,c,t}$ is the logarithm of 1 plus the number of mortgage applications filed by owner occupiers in neighborhood n in year t. Similarly, $log(Apps)_{n,c,t}$ is the logarithm of 1 plus the number of mortgage originations filed by owner occupiers in neighborhood n in year t. The definitions of $Treat_n$ and $Post_{n,t}$, along with the control variables and fixed effects remain the same as in Section 5.2. Tables 5 and 6 summarises the results from these regressions, with Panels A and B displaying the results for purchase and refinancing mortgages respectively for each table.

The number of applications reflects on the demand for credit by homeowners, while the number of originations measures the credit supply that lenders are willing to extend. As house prices appreciate, the demand for purchase mortgages expands due to the need for larger loan amounts and the recognition of homes as valuable sources of capital. Simultaneously, lenders view the underlying collateral (homes) as more valuable, which increases their willingness to supply credit. (Gimeno and Martínez-Carrascal, 2010; Igan and Loungani, 0012; Basten and Koch, 2015) Similarly, the demand for refinancing mortgages increases as homeowners see the potential value in extracting home equity, while lenders are more willing to supply credit due to the perceived increased value of the collateral. (Cloyne et al., 2019)

The results show that both application and origination activity significantly increase following institutional mergers in a neighborhood. For home purchase mortgages, neighborhoods that experience a large institutional consolidation experience a 8.7% increase in applications and a 9.4% increase in originations relative to control neighborhoods. Similarly, refinancing applications and originations increase by 6.3% and 5.1% respectively for large treated neighborhoods. Table A1 in the Appendix indicates that denial rates remain unchanged following institutional mergers, implying that the expansion in credit supply is predominantly influenced by an improvement in household credit-worthiness rather than lenders taking on more risk. Such findings align with prior studies such as Basten and Koch (2015) and Cloyne et al. (2019) that establish a connection between rising housing prices, increased housing demand, and the easing of household borrowing constraints.

Overall, the results support Hypothesis 2, in that they suggest that the house price appreciation brought about by institutional investor mergers increase both the demand and supply of credit for local homeowners, via a reduction in credit constraints. The question of what homeowners do with the additional equity extracted is explored in the next section.

5.4 Entrepreneurship Activity

In this subsection, I show that the growth in home equity post-institutional consolidation has helped promote entrepreneurial activity, and that the effect is driven via a reduction in credit constraints for local homeowners, rather than an increase in consumption out of housing wealth. I find that small business[29] registration increases post-merger, and the treatment effect becomes stronger when I exclude industries that are dependent on local consumption, such as construction and non-tradable industries. In line with Hypothesis 3, this suggests that the increase in entrepreneurial activity is due to homeowners having better access to credit, rather than an increase in consumption out of housing wealth. The findings are more consistent with the literature that links housing prices and economic activity through the collateral channel, rather than through the consumption channel, which suggests that institutional investors create positive externalities on the local economy by improving credit access for local homeowners and promoting entrepreneurship.

Since the ZBP data only provides small business registration numbers, rather than entry or exit rates, I also examine small business loans in the form of Small Business Administration (SBA) 7(A) loans to determine whether the growth in small business registration is driven

[29] Business with less than 5 employees

by an increase in entrepreneurial activity, or increases in business longevity which can be driven by increased consumer spending. I find that small business lending increases in a neighborhood post-merger, providing evidence for the former interpretation. Furthermore, since the data provides the borrower addresses, it provides further evidence that the increase in home equity incentivizes homeowners to start more businesses, rather than attracting outside businesses to take advantage of increased consumption.

5.4.1 Small Business Formation

To examine whether the rise in home equity after institutional mergers are correlated with increased entrepreneurial activity, I examine the number of small businesses registered in a neighborhood after institutional mergers occur. In addition, I attempt to establish the mechanism through which home equity affects entrepreneurship, either through reducing homeowner credit constraints, or by increasing consumption out of housing wealth. Shifting the geographic aggregation back to the zipcode level, I estimate the following diff-in-diff model:

$$log(Businesses+1)_{n,c,t} = \alpha+\beta_1 Treat_n \times Post_{n,t}+\beta_2 Post_{n,t}+\beta_3 Treat_n+X_{n,t}+\gamma_{c,t}+\theta_n+\epsilon_{n,c,t} \tag{6}$$

$log(Businesses + 1)_{n,c,t}$ equals one plus the logarithm of the number of small businesses registered in a neighborhood n. All other variables remain the same as in previous regressions. Table 7 displays the results of these regressions for Equation 6.

In Panel A, I find that after a large institutional consolidation of market share, the number of small businesses registered in a neighborhood increases by 1.8% on average. This result suggests that the increase in homeowner home equity brought about by institutional consolidation has increased entrepreneurship in local neighborhoods. However, the channel through which rising house values encourage small business formation is not clear. If house prices primarily help households smooth consumption patterns rather than by reducing the credit constraints for borrowing, then small business growth should primarily be concentrated in industries that are the most reliant on local demand, namely construction and nontradable firms. Furthermore, if growth is concentrated amongst firms that directly benefit from servicing and financing housing cost, namely F.I.R.E.[30] industries, it would provide further evidence against the collateral channel narrative.

In Panels B, C, and D, following Adelino, Schoar, and Severino (2015), I gradually exclude construction, nontradable, and F.I.R.E. firms.[31] The results in Column 2 show that the direction and significance of the treatment remains unchanged. In fact, the magnitude monotonically increases as I gradually exclude additional industries, going from 1.8% in Panel A to 4.0% in Panel D. Furthermore, Column (3) shows that the impact of a very large treatment on neighborhood small business registration goes from statistically insignificant in Panel A to a statistically significant 5.2% in Panel D. The results suggest that the increase in entrepreneurship is driven via the collateral channel, rather than through the consumption channel.

Figure 3 presents an event study analysis of the influence of institutional investor consolidation on small business growth, where treatment is defined as institutional investor consolidation of at least 2%. In comparison to the previous analysis, the event study uses a Poisson model regressing small business firm counts, rather than taking the log of firm registrations. Panel A of the figure reveals no significant impact of the treatment on the formation of all small businesses. However, Panel B tells a different story, demonstrating significant effects of the treatment when non-tradable sectors, construction, and F.I.R.E. industries are excluded from the analysis. Small business registration increases by 8.1-9.6% one year after an very large institutional consolidation in a neighborhood. As a robustness check, Figure A2 extends the analysis to non-small[32] businesses, but finds no significant effects in either Panel A or B. The absence of any impact on non-small businesses strengthens

[30]"F.I.R.E." stands for finance, insurance, and real estate industries.
[31]Definitions of non-tradables, construction, and F.I.R.E. industries are taken from Mian and Sufi (2014).
[32]Businesses with at least 5 employees.

the credit constraints channel narrative, suggesting that institutional investor consolidation specifically eases credit constraints for homeowners, without exerting a similar influence on larger enterprises.

As a further check on the collateral channel story, Panel E examines the impact of institutional consolidation on manufacturing small businesses. As Adelino, Schoar, and Severino (2015) explains, manufacturing industries are the least likely to be affected by local demand, while also requiring significant amounts of start-up capital to begin operations. Panel E shows that manufacturing small businesses increase by 7.6% in a neighborhood post-merger. This results provides further evidence that the collateral channel story is driving my results.

Lastly, to examine whether institutional consolidation helps promote entrepreneurship amongst the most productive sectors of the economy, Panel F looks at the number of high-tech small business firms registered in a neighborhood post-institutional consolidation.[33] The number of high-tech small businesses registered after a large institutional consolidation in a neighborhood decreases by 6.9%, and by 16% after a very large institutional consolidation. This results shows that there are caveats on the positive impact of institutional investors on promoting entrepreneurship. Businesses that rely on higher skilled, high paying jobs do not seem to benefit from house price increases brought about by institutional investors. As the results in Subsection 5.8 will demonstrate, the pattern persists when I examine the types of jobs created post-merger.

5.4.2 Small Business Loans

Several questions that are raised by the previous analysis are (1) whether the effect on small business registration are driven by reduced firm exits rather than increased firm entry, and (2) whether the increase in small businesses registrations are driven by local homeowners rather than by outsiders. If both are true, it would provide evidence against the collateral

[33]Definition of high-tech industries by NAICS code are taken from National Science Foundation (2020).

channel story as described by Hypothesis 2, as increased firm survival rates and outside investment would likely be driven more by increasing local demand, rather than by reduced credit constraints for local residents. Although data restrictions prevent me from answering this question directly, I can examine the change in small business lending post-institutional mergers to provide some evidence against these explanations, and act as an test to Hypothesis 2 in addition with the results on mortgage lending.

I follow Barrios, Hochberg, and Yi (2022) and examine the change in SBA 7(A) small business loans following institutional mergers in a neighborhood. SBA 7(a) loans are given out to small businesses for working capital, equipment, inventory, real estate, or to refinance existing debt. The requirements for SBA 7(a) loans include being a for-profit business, having a maximum net worth of $15 million, and an average net income of $5 million or less over the previous two years. Data on SBA 7(A) loan origination provides information on borrower addresses to the zipcode level, allowing me to establish whether the increase in small business lending is driven by local residents. Furthermore, the SBA allows real estate to be used as collateral for loans[34], meaning that increases in SBA 7(A) lending activity post-mergers are likely to be driven by reductions in collateral constraints, rather than by increases in consumption or other alternative channels. Furthermore, I have excluded lending to the non-tradable, construction, and F.I.R.E industries, as well as businesses employing over 10 individuals, to ensure that the small business loans align with the sectors identified as benefiting from entrepreneurial growth in the prior subsection.

I estimate the following diff-in-diff models for SBA 7(A) loan origination activity in terms of both loan number and dollar volume:

$$log(LoanNbr+1)_{n,c,t} = \alpha + \beta_1 Treat_n \times Post_{n,t} + \beta_2 Post_{n,t} + \beta_3 Treat_n + X_{n,t} + \gamma_{c,t} + \theta_n + \epsilon_{n,c,t} \tag{7}$$

[34]For loans above $350,000, residential and/or investment real estate must be included as collateral. For loans between $25,000 and $350,000, real estate may be included as collateral if the business' available fixed or trading assets are not enough to fully secure the loan.

$$log(LoanVol+1)_{n,c,t} = \alpha + \beta_1 Treat_n \times Post_{n,t} + \beta_2 Post_{n,t} + \beta_3 Treat_n + X_{n,t} + \gamma_{c,t} + \theta_n + \epsilon_{n,c,t} \tag{8}$$

$log(LoanNbr + 1)_{n,c,t}$ is one plus the logarithm of the number of SBA 7(A) loans given out in neighborhood n in year t, while $log(LoanVol + 1)_{n,c,t}$ is one plus the logarithm of the dollar volume of SBA 7(A) loans given out in neighborhood n in year t. Table 8 presents the results of the regression models for SBA 7(A) lending around institutional mergers. Panel A presents the results for Equation 7, while Panel B presents the results for Equation 8. The results in Column (2) for both panels suggest that SBA 7(A) origination activity for small businesses in a neighborhood significantly increase post-merger, with loan numbers increasing by 5.8% and dollar volume increasing by 51.5%. This result provides further evidence that the collateral channel is driving the increase in entrepreneurial activity. Increases in the home equity for local homeowners reduces credit constraints for small business lending that increased small business formation.

5.4.3 Homeownership and Entrepreneurship

As a robustness check, I investigate whether the growth in entrepreneurial activity is actually related to homeownership. If areas with lower homeownership rates exhibit comparable or higher rates of small business growth compared to areas with higher homeownership rates, it would suggest that the increase in entrepreneurial activity is not primarily driven by homeowners. In such a case, the narrative that institutional consolidation reduces credit constraints for homeowners may be inaccurate. Following Barrios, Hochberg, and Yi (2022), I calculate the quartile of homeownership rates for each zipcode in my sample as of 2011. I then interact the variable $Treat_n \times Post_{n,t}$ in Equation 6 with the homeownership quartile for each zipcode. Table A2 reports the results. Column (1) demonstrates that the impact of institutional consolidation in treated neighborhoods increases monotonically with homeownership rates. Similarly, for Columns (2) through (4), the correlation between institutional consolidation and small business growth is statistically insignificant in the first quartile of homeownership rates but becomes positive and significant for all other homeownership quartiles. These findings lend support to the hypothesis that the influence of institutional investor mergers on entrepreneurial activity operates through homeownership and the accumulation of housing wealth.

As an additional robustness check, I investigate if the surge in small business lending predominantly stems from homeowners as opposed to non-homeowners. By matching the SBA 7(A) sample with historical tax assessor records from ATTOM, I ascertain which loans were awarded to businesses operated by homeowners. I then apply Equations 7 and 8 to both homeowner and non-homeowner business categories.[35] The findings are detailed in Tables A3 and A4. Notably, institutional mergers significantly impact small business lending to homeowner addresses, but not to non-homeowner addresses. This provides further evidence for the collateral channel, given that the uptick in lending is mainly attributed to homeowners witnessing a rise in their home equity.

5.4.4 Age Demographics and Entrepreneurial Activity

Entrepreneurial activity is likely primarily fueled by individuals who are below the retirement age of 65+, suggesting that institutional investors' influence might be particularly pronounced in areas with fewer retirees. To validate this premise, I run a similar robustness check as in the previous subsection, replacing quartiles based on homeownership with quartiles based on the percentage of the population aged 65 and above. The results, presented in Table A5, affirm this hypothesis. Notably, only for Q1, representing neighborhoods with the lowest retiree populations, exhibits statistically significant treatment effects of institutional investment on entrepreneurial activity, which is robust across all columns and treatment sizes. In fact, Column 1 displays negative treatment effects on entrepreneurship for Q3 and Q4, although subsequent columns show insignificant coefficients, suggesting that the nega-

[35] Given that homeowners might opt to establish businesses at non-homeowner addresses, particularly if they have considerable capital, the treatment effect for SBA loans to homeowners is best interpreted as a conservative estimate.

tive relation is less robust. This outcome supports the notion that higher housing prices is stimulating entrepreneurship, as I do not find similar impacts on populations with higher retiree populations – a group generally disinclined to pursue entrepreneurial ventures.

5.5 Local Amenities

The last section provided support to Hypothesis 3 in that reduced credit constraints increased entrepreneurial activity in industries less exposed to local demand. However, the conjecture that growth in non-tradable sectors has been adversely influenced remains to be substantiated. To affirm this aspect of Hypothesis 3, and to continue contesting the consumption channel narrative, I delve into the effects of institutional consolidation on the quantity of local amenities in a neighborhood, using the classification of amenities as delineated by Qian and Tan (2021). To test this part of Hypothesis 3 and further question the consumption channel theory, I look at how institutional consolidation affected the number of local amenities in a neighborhood, based on the definitions given by Qian and Tan (2021). Local amenities, encompassing retail stores, personal care services, restaurants, nightlife, and recreational facilities, are heavily reliant on local spending. Therefore, should there be an upsurge in consumption spending due to increased housing wealth, I anticipate a corresponding rise in the number of local amenities catering to this enhanced consumption. Given the lack of direct household consumption spending data, local amenities serve as a proxy for local consumption activity.

Table 9 examines the regression results from Equation 6, replacing small businesses with local amenities. Panels A through E present the regressions for retail, personal care, restaurants, nightlife, and recreational firms, respectively. Using Column (2) as my baseline, only restaurants exhibit an increase in number following institutional consolidation. The treatment effect for almost all other amenities is either insignificant or negative. Notably, retail, personal care and recreational amenities experience declines of 7.4%, 8.6% and 11.3% respectively, post a significant institutional consolidation in a neighborhood. Interestingly, contrary to other types of amenities, the number of restaurants increases following a large institutional consolidation.

Figure 4 offers an event study analysis, similar to the Poisson regression approach used in Figure 3, exploring the correlation between institutional mergers and amenities. The findings align with those from Table 9, where the correlation between institutional consolidation and the number of local amenities is either insignificant or negative. The significant positive effect for restaurants disappears, and the negative impact on the number of personal care, nightlife and recreational amenities persists one year post-merger.

Based on these results, I find scant evidence supporting the housing wealth consumption channel as the driver for entrepreneurial activity. If increases in housing wealth facilitated smoother consumption spending for homeowners, it would be logical to see local amenities as a whole benefiting from the surge in consumer spending. However, the insignificant to negative correlation between amenities and institutional consolidation suggests a negative, or at best, a non-positive relationship between institutional investor mergers and local consumption spending.

5.6 Income Growth

The preceding sections have underscored that institutional investors confer benefits on existing homeowners by elevating their home equity value. This, in turn, leads to eased credit constraints and a subsequent surge in entrepreneurial activity. However, the effects of institutional ownership on renters may not be as directly advantageous. Previous research, such as that conducted by Austin (2022) and Gurun et al. (2022), shows how institutional consolidation could negatively impact renters by driving up rental prices. Under Hypothesis 4,[36] a positive shock to rental prices would erode the disposable income available to them, compelling a reduction in consumption spending. Theoretically, the rental market impacts could act as a reversal of the consumption channel, where increases in rental prices lead

[36]Assuming a fixed income for renters

to lower economic activity, as declines in local consumption lead to lower income growth as existing businesses grapple with diminished consumer spending. Consequently, when it comes to business formation and economic activity, the advantage that institutional investors impart to local communities by relaxing homeowner credit constraints could be offset by the reduction in consumption spending induced by higher rental prices.

To test Hypothesis 4 and examine the impact of institutional mergers on the economic well-being of communities, I first examine the impact of institutional consolidation on the median income of residents. I employ the following diff-in-diff model on a census tract panel dataset:

$$log(Income)_{n,c,t} = \alpha + \beta_1 Treat_n \times Post_{n,t} + \beta_2 Post_{n,t} + \beta_3 Treat_n + X_{n,t} + \gamma_{c,t} + \theta_n + \epsilon_{n,c,t} \quad (9)$$

$log(Income)_{n,c,t}$ equals the logarithm of the median income of residents living in neighborhood n in year t. The control variables, Xn, t, remain consistent with those used in previous regressions, except for the exclusion of neighborhood median income, which is omitted for self-evident reasons. Table 10 presents the results of Equation 9. Panel A showcases the results for all residents in the area, while Panels B through F divide the impact by housing tenure, meaning whether residents were living in the same house as the previous year and, if not, the geographical distance between their previous and current residence.

Panel A reveals a decrease in median income for local residents by 1.3% following mergers by institutional investors. This outcome is further reinforced by 5, which upholds the parallel trends assumption before the treatment and exhibits a significant negative impact of institutional consolidation on median income one year post-treatment. This finding provides support to Hypothesis 4, and strengthens the narrative that institutional investors can exert negative economic effects on local neighborhoods, and that increases in rental prices can trigger declines in local consumption, leading to reduced economic output and slower income growth for residents.

However, Panels B through F suggest that the treatment effect is notably significant only for residents who have maintained residence in the same house as the previous year. This finding is mirrored in Figure A3, where the treatment leads to a significantly lower median income exclusively for residents who have stayed in the same house as the year prior. Institutional consolidation only significantly impairs income growth for long-term residents, but not for newcomers to the neighborhood. A potential explanation for this result could be the mobility restrictions faced by long-term residents, who may have deeper ties and commitments to their communities, making it more challenging for them to relocate to other neighborhoods in search of better job opportunities. Conversely, new residents moving into the neighborhood may enjoy more employment flexibility and could be better prepared to navigate job opportunities, resulting in a lesser impact on their income growth.

5.7 Demographic Outcomes

Lower median income for residents may also be driven by changing resident demographics and employment outcomes. Households without a college education and with a minority background tend to have lower incomes on average compared to other households. Furthermore, higher unemployment may also act as a drag on median income. If the positive economic effects of small business growth from reduced credit constraints is insufficient in balancing out the negative effects of reduced consumption, then the reduction in economic output may lead to higher unemployment.

To examine the impact of institutional consolidation on demographic outcomes, I run the following diff-in-diff models:

$$College_{n,c,t} = \alpha + \beta_1 Treat_n \times Post_{n,t} + \beta_2 Post_{n,t} + \beta_3 Treat_n + X_{n,t} + \gamma_{c,t} + \theta_n + \epsilon_{n,c,t} \quad (10)$$

$$Minority_{n,c,t} = \alpha + \beta_1 Treat_n \times Post_{n,t} + \beta_2 Post_{n,t} + \beta_3 Treat_n + X_{n,t} + \gamma_{c,t} + \theta_n + \epsilon_{n,c,t} \quad (11)$$

$$Unemployed_{n,c,t} = \alpha + \beta_1 Treat_n \times Post_{n,t} + \beta_2 Post_{n,t} + \beta_3 Treat_n + X_{n,t} + \gamma_{c,t} + \theta_n + \epsilon_{n,c,t} \quad (12)$$

The outcome variables $College_{n,c,t}$, $Minority_{n,c,t}$, and $Unemployed_{n,c,t}$ equal the fraction of the neighborhood population in percentage points that have a bachelor's degree, are of minority status, and are unemployed, respectively. Table 11 displays the results of Equations 10 through 12. For Panel A, I find that the percentage of residents that have college-level education drops by 0.43 percentage points in neighborhoods with a large institutional consolidation. For Panel B, in line with Austin (2022), I find that the minority population of a neighborhood significantly increases post-consolidation by 1.16 percentage points. For Panel C, I find no significant impact of institutional consolidation on unemployment rates.

The results outlining the change in demographics post-institutional consolidation show that the decrease in income growth is at least partially driven by increases in demographics with lower average incomes, such as non-college educated and minority households. However, the null result for unemployment shows that the decrease in median income post-institutional mergers is not driven by job loss or employment growth not keeping pace with population growth. One possibility is that the job creation associated with institutional mergers and the increase in entrepreneurial activity is concentrated amongst low-skilled, low-paying jobs, which I will examine in the next subsection.

5.8 Job Creation

Evaluating the impact of institutional consolidation on job growth allows me to shed light on several questions concerning the earlier findings related to homeowner entrepreneurship and resident income and demographics. If the observed job growth is predominantly concentrated among lower-skilled, low-paying jobs, it suggests that the benefits of the increased entrepreneurial activity for local residents are primarily accruing to homeowners. These homeowners are capitalizing on eased credit constraints to launch their own businesses, but these businesses only provide low-income jobs to other residents who, concurrently, are grappling with higher rental prices.

To scrutinize the nature of job growth associated with institutional investor consolidation, I employ the following difference-in-difference model using the LODES (Longitudinal Employer-Household Dynamics Origin-Destination Employment Statistics) private primary employment data:

$$log(Jobs+1)_{n,c,t} = \alpha+\beta_1 Treat_n \times Post_{n,t}+\beta_2 Post_{n,t}+\beta_3 Treat_n+X_{n,t}+\gamma_{c,t}+\theta_n+\epsilon_{n,c,t} \quad (13)$$

$log(Jobs + 1)_{n,c,t}$ is one plus the logarithm of the number of jobs in neighborhood n in year t. Table 12 displays the results for Equation 13. I run the regression model for various segments of the labor market. Panel A divides jobs by monthly wages, whereas Panel B divides jobs by educational attainment. For Table 12, I use census tracts as my definition of a neighborhood. Figure A4 shows that the relationship between ACS and LODES employment numbers are much closer with fewer outliers on a census-tract level than on a zipcode level.

Table 12 Panel A shows that large institutional consolidation in a neighborhood has a significant impact on job growth only for jobs paying below $3333 per month. The effect is strongest for jobs paying below $1250 per month, which is 2.6%, and is 2.4% for jobs paying between $1250 and $3333 per month. As a 2018 study Pew Research defines the national threshold for middle-class status as a $48,500 annual income,[37] it suggests that the job growth following increases in entrepreneurship have been concentrated amongst low-income labor. Panel B shows similar results, where the treatment is effect is positive and significant only for jobs that require less than a bachelor's degree. For jobs that require a bachelor's degree or more, institutional consolidation is correlated with a 0.9% decrease in job numbers.

Breaking down job creation by industry, Figure 6 shows the treatment effect for different industries as defined in the LODES dataset. By industry, job growth is strongest amongst industries such as transportation, manufacturing, and accommodation, which tend to be lower-skilled industries. By contrast, industries which tend to require higher-skilled labor such as information, finance, and professional and scientific services see a negative impact of institutional consolidation on job growth.

[37]Source: https://www.pewresearch.org/short-reads/2020/07/23/are-you-in-the-american-middle-class/

Taken together, these results support the idea that amongst local residents, institutional investors primarily benefit local homeowners that use their increase in housing wealth to become entrepreneurs. Other residents, especially renters who suffer from higher rental prices, who are unable or unwilling to take advantage of rising home equity to become entrepreneurs, are less likely to see economic benefits from institutional investors, as long-term economic prosperity for local communities seems unlikely to be driven by growth in low-paying, low-skilled labor.

As a robustness check, Table A6 reruns Equation 13 using zipcodes instead of census tracts as the definition of a neighborhood. Figure A5 shows job creation by industry on a zipcode level rather than a census tract level. The results are qualitatively much the same as in Table 12 and Figure 6, where job growth is mainly concentrated in lower-skilled, less educated jobs. However, for Panel A, institutional consolidation is now also correlated with an increase in jobs paying above $3333, and for Panel B, the impact of institutional consolidation for jobs requiring a bachelor's degree becomes insignificant. Thus, treatment effects are somewhat lessened, but still significant when looking at job growth on the zipcode level, and overall conclusions remain unaffected.

5.9 Robustness Checks

In Subsection 5.2, I addressed the inherent bias of the Time-Work Fixed Effects (TWFE) estimate in the context of staggered treatment scenarios. The presence of heterogeneous treatment times and effects among different groups can significantly distort the estimate, concealing important cross-sectional and temporal variations (Goodman-Bacon (2021), Sun and Abraham (2021), Callaway and Sant'Anna (2021)). While earlier sections employed the approach suggested by Sun and Abraham (2021) for event studies, I have yet to address potential bias arising from varied treatment timing in my TWFE regressions.

As a robustness check for the estimated Average Treatment on the Treated (ATT), I adopt the methodology proposed by Callaway and Sant'Anna (2021) to estimate group-time ATTs for distinct cohorts, effectively addressing this heterogeneity.[38] The computation of individual group-time ATTs facilitates the assessment of treatment effects across different groups and their combination to yield an aggregated ATT estimate. Employing this methodology helps mitigate any bias associated with TWFE estimators in staggered treatment situations. Moreover, it allows me to deconstruct treatment effects by cohort, thereby exploring whether the observed effects in the paper are driven by earlier or later mergers.

To compute each group-time ATT, I utilize doubly robust estimators from Sant'Anna and Zhao (2020). These estimators offer notable gains in both consistency and semiparametric efficiency within difference-in-differences frameworks, particularly in the context of staggered treatment settings involving panel data. I calculate standard errors through a multiplier bootstrap procedure with 1000 iterations and cluster them at the neighborhood level. For each group-time ATT estimation, the comparison group comprises neighborhoods that have either not encountered treatment or will not undergo treatment, effectively accounting for the heterogeneity in treatment timing. All control variables remain the same as in previous models.

5.9.1 Housing Costs

The results from re-estimating Equation 2 for housing and rental prices using the methodology from Callaway and Sant'Anna (2021) are presented in Table A7. A comparative analysis of the aggregate Average Treatment Effect on the Treated (ATT) using Callaway and Sant'Anna (2021) with the findings from Subsection 5.2 reveals a persistent significance in the impact of institutional consolidation on housing and rental prices. The estimated impact translates to a 3.79% increase in housing prices and a 2.57% increase in rental prices, as shown in Panels A and B respectively.

The group-time ATT displays some interesting results on which cohorts are driving the observed effects. Notably, neighborhoods experiencing a merger in 2016 emerge as significant

[38] The authors themselves offer a convenient method for estimating group-time ATT within the R library package *did*.

contributors, presenting a group-time ATT that attains statistical significance at the 95% confidence level. In contrast, the 2017 cohort's ATT is statistically insignificant at the same confidence level.[39]

In Figures A6 and A7, I perform an additional event study analysis for housing and rental prices as an additional robustness check for the dynamic treatment effects estimated using Sun and Abraham (2021) in Subsection 5.2. Both figures unveil the absence of significant pre-treatment disparities in prices between treated and control neighborhoods. Housing prices exhibit elevation one year post-treatment, while rental prices reveal significant changes three years post-merger. Intriguingly, the upward trend persists in both housing and rental prices 4-5 years post-merger, underscoring the enduring and substantial impact of institutional mergers on price escalation in both markets.

5.9.2 Entrepreneurship

As a robustness check to Subsection 5.4, I apply Callaway and Sant'Anna (2021) to analyze the correlation between institutional consolidation and entrepreneurship activity. The findings, presented in Table A8, display a similarly noteworthy and statistically significant treatment effect concerning small business growth as in Subsection 5.4. Even after excluding construction, nontradable, and F.I.R.E. industries from consideration, the impact remains positive and significant. The aggregate ATT of an increase of 2.45% in small business registration within treated neighborhoods is evident post-merger. As observed in the housing market, the group-time ATT demonstrates significance exclusively for the 2016 cohort, with the 2017 cohort showing no such result.

Perusing Table A8 may give rise to a valid concern. In contrast to the TWFE outcomes, the aggregate ATT experiences a slight contraction upon the exclusion of construction, non-

[39] The insignificance of the 2017 cohort can be attributed, in part, to the geographic overlap of the larger 2017 merger (Invitation Homes acquiring Colony Starwood) with a 2016 merger (Starwood Waypoint acquiring Colony American Homes). Consequently, many neighborhoods affected by the 2017 merger are registered as undergoing treatment in 2016. This circumstance dampens the magnitude of the 2017 cohort and influences the estimation and weighting of the 2017 group-time ATT.

tradable, and F.I.R.E. industries from the model. This prompts an inquiry into whether growth is more pronounced in nontradable sectors compared to tradable ones. In pursuit of further insights, I conduct an additional robustness check—a focused event study analysis concerning small business growth in nontradable industries. This analysis, aligned with the framework of Callaway and Sant'Anna (2021), unfolds in Figure A8. The results indicate that growth among small businesses within nontradable industries remains statistically insignificant for a span of three years following treatment. Although the estimated coefficient turns positive and significant four years post-merger, this observation underscores that the impetus for small business growth is not predominantly concentrated within nontradable sectors. This pattern aligns with the credit constraints narrative, which posits that the reverberations of housing price shocks on entrepreneurial growth is concentrated in the tradable sector.

6 Conclusion

The burgeoning growth of institutional investors in the single-family housing market in the United States prompts significant inquiries into the welfare impacts of private equity ownership on local communities, as well as the implications of institutional consolidation for residents based on their homeownership status. By leveraging a series of national mergers involving institutional investors and utilizing comprehensive data on property ownership across the United States, I establish that house price increases resulting from the consolidation of institutional ownership lead to noteworthy upswings in entrepreneurial activity. This mechanism operates through housing price spillovers that enhance home equity and alleviate credit constraints for homeowners. Consequently, homeowners leverage their improved access to capital to engage in more entrepreneurial pursuits and foster small business formation. However, while homeowners experience advantages due to the rise of institutional investors in their neighborhoods, renters often encounter the adverse effects of institutional investor ownership. Escalating rental costs erode disposable income, contributing to a deceleration in income growth and a decline in educational attainment among local residents. Moreover, the job growth associated with institutional consolidation primarily features low-wage, low-skill positions that are unlikely to catalyze long-term prosperity for residents in neighborhoods where institutional investors are establishing their presence.

Aligning with the findings of Gurun et al. (2022) and Austin (2022), I determine that the interplay between greater housing wealth and entrepreneurial activity among homeowners, and reduced consumption capacity among renters underscores the complex and multifaceted overall impact of institutional ownership on the economic welfare of the neighborhoods they permeate. On one side of the spectrum, the assertions of policymakers, exemplified by Representative Smith, contending that private equity brings no economic benefit to local communities, do not encompass the complete picture. This is due to the surge in entrepreneurial activity and the establishment of small businesses, catalyzed by the appreciation in home equity, particularly in areas where private equity investors are significantly present. However, these concerns voiced by policymakers do carry validity; higher rental prices can indeed limit local consumption among renters, potentially jeopardizing the long-term economic prospects of the neighborhood. Furthermore, considering that job growth tends to be concentrated within low-paid, low-skilled employment sectors, the positive effects of heightened entrepreneurship spurred by institutional investors also come with certain reservations.

Future research aimed at quantifying the welfare implications of institutional investors on business growth, employment, and consumption within local communities holds the potential to provide valuable insights for both researchers and policymakers. Investigative efforts of this nature can play a pivotal role in facilitating an informed assessment of whether institutional investment in single-family housing is advantageous or detrimental to households across the United States. In tandem with these inquiries, it becomes increasingly important to grasp the potential ramifications of the ongoing decline in homeownership rates among

U.S. households.[40]" This understanding contributes to a more comprehensive understanding of the overarching impact of institutional investment.

A significant observation underscores this analysis: the benefits of institutional ownership are markedly tilted toward homeowners, while the downsides disproportionately impact renters. This phenomenon could potentially accentuate pre-existing issues of inequality that the United States currently contends with. As we contemplate these dynamics, it becomes clear that future researchers and policymakers hold the responsibility to deliberate on whether the positive outcomes attributed to institutional investors carry sufficient weight to counterbalance the negatives. Additionally, crafting forward-looking policies that retain the former benefits while mitigating the latter challenges emerges as a pressing imperative.

[40] According to Pew Research, as of 2021, 49% of adults identified the availability of affordable housing as a significant issue for their communities, reflecting a 10-percentage-point increase from early 2018. Source: https://www.pewresearch.org/short-reads/2022/03/23/key-facts-about-housing-affordability-in-the-u-s/

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Table 1: Institutional Investor Mergers

Acquirer			Target			Merger				
Name	Status	Properties	Name	Status	Properties	Announcement Date	Completion Date	Transaction Value ($US bil)	States	Counties
American Homes 4 Rent	Public	42499	American Residential Properties	Public	8166	12/3/2015	2/29/2016	1.429	16	113
Starwood Waypoint	Public	13735	Colony American Homes	Private	18900	9/23/2015	1/5/2016	7.700	20	114
Tricon American Homes	Public	5577	Silver Bay Realty Trust	Public	8218	2/27/2017	5/9/2017	1.503	38	297
Invitation Homes	Public	43818	Colony Starwood	Public	32573	8/10/2017	11/16/2017	8.604	40	409

This table documents the horizontal mergers between institutional investors used in the analysis for the paper. Details for the acquirer and target firms, as well as the date and size of the mergers are presented. The *Properties* column for both acquirers and targets lists the number of properties that were identified as belonging to the institutional investor in the year prior to the merger. For each merger, the *States* and *Counties* column equals the number of states and counties that where the acquirer and target firms collectively owned properties prior to the merger.

Table 2: Summary Statistics: Market Share

Statistic:	N	Mean	P1	P50	P99	SD
Zipcode-level:						
Treatment (pre-merger):	1741	0.79	0.00	0.36	5.92	1.16
Treatment (post-merger):	1741	1.38	0.02	0.67	9.04	1.96
Large Treatment (pre-merger):	291	2.38	0.39	1.88	9.52	1.82
Large Treatment (post-merger):	291	4.62	1.55	3.88	13.82	2.74
Very Large Treatment (pre-merger):	127	3.12	0.75	2.49	9.85	2.04
Very Large Treatment (post-merger):	127	6.45	2.87	5.64	16.01	2.92
No treatment:	2065	0.51	0.00	0.05	3.03	5.20
Census Tract-level:						
Treatment (pre-merger):	5785	1.41	0.00	0.78	9.26	2.69
Treatment (post-merger):	5785	1.91	0.05	1.14	11.14	3.07
Large Treatment (pre-merger):	757	4.25	0.49	2.90	34.56	6.12
Large Treatment (post-merger):	757	6.00	1.61	4.61	38.12	6.46
Very Large Treatment (pre-merger):	209	7.30	1.22	4.01	49.20	10.59
Very Large Treatment (post-merger):	209	10.02	3.46	6.82	52.76	10.84
No treatment:	9006	2.78	0.00	0.19	100.00	14.65

The table presents the summary statistics on the institutional market share of single family homes on both a zipcode and census-tract level, both pre- and post-merger. *Treatment* indicates that mergers in a zipcode/census tract led to a consolidation of institutional investor properties, leading to an increase in acquiring firm's market share. *Large Treatment* indicates that the increase in market share was at least 1%, and *Very Large Treatment* indicates that the increase in market share was at least 2%. *N* equals the number of zipcodes/census tracts that fit the treatment category.

Table 3: Summary Statistics: Prices

(a) House Prices

Period:	Pre-merger			Post-merger		
	N	Mean	SD	N	Mean	SD
All	15866	231808	144674	26638	344346	215114
$I(\Delta(MarketShare) > 0)$	5815	240006	132248	10135	370029	195704
$I(\Delta(MarketShare) \geq 1)$	656	195097	70283	1148	315014	105761
$I(\Delta(MarketShare) \geq 2)$	508	156386	53731	889	263314	85708

(b) Rent Prices

Period:	Pre-merger			Post-merger		
	N	Mean	SD	N	Mean	SD
All	15866	1341	839	26638	1579	741
$I(\Delta(MarketShare) > 0)$	5815	1320	344	10135	1596	449
$I(\Delta(MarketShare) \geq 1)$	656	1203	257	1148	1500	363
$I(\Delta(MarketShare) \geq 2)$	508	1116	189	889	1443	317

This table presents the summary statistics of housing and rental prices for zipcodes pre- and post-merger. N is equal to the number of zipcode-year observations for each treatment category and time period. *All* presents summary statistics for all zipcodes, while $I(\Delta(MarketShare) > 0)$, $I(\Delta(MarketShare) \geq 1)$, and $I(\Delta(MarketShare) \geq 2)$ present summary statistics for zipcodes that received treatment, large treatment, and very large treatment, respectively.

Table 4: Institutional Mergers and Housing Costs

Dependent Variable:	*Log(Price)*			
	$I(\Delta(MarketShare) > 0)$	$I(\Delta(MarketShare) \geq 1)$	$I(\Delta(MarketShare) \geq 2)$	$\Delta(MarketShare)$
	(1)	(2)	(3)	(4)
Panel A: Housing Price				
Treat × **Post**	0.055	0.049***	0.048**	0.021***
	(0.038)	(0.014)	(0.018)	(0.006)
Observations	37,817	37,817	37,817	37,817
Adjusted R^2	0.920	0.920	0.920	0.920
Panel B: Rental Price				
Treat × **Post**	0.011**	0.025*	0.038**	0.013***
	(0.005)	(0.013)	(0.014)	(0.003)
Observations	12,405	12,405	12,405	12,405
Adjusted R^2	0.960	0.960	0.960	0.960
ZIP Controls	X	X	X	X
County × Year FE	X	X	X	X
ZIP FE	X	X	X	X

Note: *p<0.1; **p<0.05; ***p<0.01

This table presents estimates of the diff-in-diff regression of housing and rental prices around institutional investor mergers in a zipcode. In Panel A, the dependent variable is the housing price index estimated via hedonic regression. In Panel B, the dependent variable is the rental index provided by Zillow (ZORI). The sample includes neighborhoods where acquiring and/or target firms owned properties prior to the merger. In Column 1, $Treat$ is a binary variable that equals 1 if both acquiring and target firms owned property in a zipcode prior to the merger. In Columns 2 and 3, $Treat$ is a binary variable that equals 1 if acquiring firms gained at least 1% or 2% market share post-merger, respectively. In Column 4, $Treat$ is a continuous variable that equals the single family market share percentage consolidated by acquiring firms in a zipcode. Controls for each zipcode include log total population, log median income, poverty rate, employment rate, labor force participation, homeownership, minority percentage, and population ratios by age and education. t-statistics are reported in parentheses. All columns include zipcode and county by year fixed effects. Standard errors are clustered by zipcode.

Table 5: Institutional Mergers and Mortgage Applications

Dependent Variable:	$Log(1 + Applications)$			
	$I(\Delta(MarketShare) > 0)$	$I(\Delta(MarketShare) \geq 1)$	$I(\Delta(MarketShare) \geq 2)$	$\Delta(MarketShare)$
	(1)	(2)	(3)	(4)
Panel A: Purchase				
Treat × Post	0.077***	0.087***	0.112***	0.048***
	(0.008)	(0.014)	(0.021)	(0.006)
Adj. R^2	0.931	0.931	0.931	0.931
Panel B: Refinancing				
Treat × Post	0.090***	0.063***	0.085***	0.047***
	(0.005)	(0.009)	(0.015)	(0.004)
Adj. R^2	0.960	0.960	0.960	0.960
Census Tract Controls	X	X	X	X
County × Year FE	X	X	X	X
Census Tract FE	X	X	X	X
Observations	131,401	131,401	131,401	131,401

Note: *p<0.1; **p<0.05; ***p<0.01

This table presents estimates of the diff-in-diff regression of conventional owner-occupied mortgage applications for single family homes around institutional investor mergers in a census tract. Panel A displays the results for home purchase mortgages, while Panel B displays the results for refinancing mortgages. The sample includes neighborhoods where acquiring and/or target firms owned properties prior to the merger. In Column 1, $Treat$ is a binary variable that equals 1 if both acquiring and target firms owned property in a census tract prior to the merger. In Columns 2 and 3, $Treat$ is a binary variable that equals 1 if acquiring firms gained at least 1% or 2% market share post-merger, respectively. In Column 4, $Treat$ is a continuous variable that equals the single family market share percentage consolidated by acquiring firms in a census tract. Controls for each census tract include log total population, log median income, poverty rate, employment rate, labor force participation, homeownership, minority percentage, and population ratios by age and education. t-statistics are reported in parentheses. All columns include census tract and county by year fixed effects. Standard errors are clustered by census tract.

Table 6: Institutional Mergers and Mortgage Originations

Dependent Variable:	*Log*$(1 + Originations)$			
	$I(\Delta(MarketShare) > 0)$	$I(\Delta(MarketShare) \geq 1)$	$I(\Delta(MarketShare) \geq 2)$	$\Delta(MarketShare)$
	(1)	(2)	(3)	(4)
Panel A: Purchase				
Treat × **Post**	0.083***	0.094***	0.124***	0.053***
	(0.008)	(0.015)	(0.022)	(0.007)
Adj. R^2	0.926	0.926	0.926	0.926
Panel B: Refinancing				
Treat × **Post**	0.093***	0.051***	0.069***	0.041***
	(0.005)	(0.010)	(0.015)	(0.005)
Adj. R^2	0.951	0.951	0.951	0.951
Census Tract Controls	X	X	X	X
County × Year FE	X	X	X	X
Census Tract FE	X	X	X	X
Observations	131,401	131,401	131,401	131,401

Note: *p<0.1; **p<0.05; ***p<0.01

This table presents estimates of the diff-in-diff regression of conventional owner-occupied mortgage originations for single family homes around institutional investor mergers in a census tract. Panel A displays the results for home purchase mortgages, while Panel B displays the results for refinancing mortgages. The sample includes neighborhoods where acquiring and/or target firms owned properties prior to the merger. In Column 1, $Treat$ is a binary variable that equals 1 if both acquiring and target firms owned property in a census tract prior to the merger. In Columns 2 and 3, $Treat$ is a binary variable that equals 1 if acquiring firms gained at least 1% or 2% market share post-merger, respectively. In Column 4, $Treat$ is a continuous variable that equals the single family market share percentage consolidated by acquiring firms in a census tract. Controls for each census tract include log total population, log median income, poverty rate, employment rate, labor force participation, homeownership, minority percentage, and population ratios by age and education. t-statistics are reported in parentheses. All columns include census tract and county by year fixed effects. Standard errors are clustered by census tract.

Table 7: Small Business Growth

Dependent Variable:	$Log(1 + \#$ of Businesses)			
	$I(\Delta(MarketShare) > 0)$	$I(\Delta(MarketShare) \geq 1)$	$I(\Delta(MarketShare) \geq 2)$	$\Delta(MarketShare)$
	(1)	(2)	(3)	(4)
Panel A: All Small Businesses				
Treat × **Post**	0.022***	0.018**	0.015	0.007**
	(0.004)	(0.007)	(0.010)	(0.003)
Adj. R^2	0.988	0.986	0.988	0.989
Panel B: Minus Construction				
Treat × **Post**	0.001	0.025***	0.031**	0.012***
	(0.006)	(0.008)	(0.012)	(0.003)
Adj. R^2	0.990	0.989	0.990	0.990
Panel C: Minus Construction and Non-Tradables				
Treat × **Post**	−0.008	0.033***	0.047***	0.017***
	(0.007)	(0.009)	(0.014)	(0.004)
Adj. R^2	0.983	0.983	0.983	0.983
Panel D: Minus Construction, Real Estate, Finance, and Non-Tradables				
Treat × **Post**	−0.006	0.040***	0.052***	0.019***
	(0.007)	(0.010)	(0.014)	(0.004)
Adj. R^2	0.954	0.950	0.954	0.953
Panel E: Manufacturing				
Treat × **Post**	−0.108***	0.076*	0.110*	0.008
	(0.027)	(0.041)	(0.058)	(0.017)
Adj. R^2	0.845	0.844	0.844	0.844
Panel F: High-Tech				
Treat × **Post**	0.010	−0.069**	−0.160***	−0.037***
	(0.021)	(0.032)	(0.048)	(0.013)
Adj. R^2	0.902	0.902	0.902	0.902
Observations	34,608	34,608	34,608	34,608
ZIP Controls	X	X	X	X
County × Year FE	X	X	X	X
ZIP FE	X	X	X	X

Note: *p<0.1; **p<0.05; ***p<0.01

This table presents estimates of the diff-in-diff regression of small business registrations around institutional investor mergers in a zipcode. Panel A displays the results for all industries, while Panels B through D cumulatively remove firms involved in construction industries, non-tradables, and finance industries, as defined by Mian and Sufi (2014). Panel E looks at small businesses defined as manufacturing firms as defined by Mian and Sufi (2014). Panel F looks at small business defined as high-tech firms as in National Science Foundation (2020). The sample includes neighborhoods where acquiring and/or target firms owned properties prior to the merger. In Column 1, $Treat$ is a binary variable that equals 1 if both acquiring and target firms owned property in a zipcode prior to the merger. In Columns 2 and 3, $Treat$ is a binary variable that equals 1 if acquiring firms gained at least 1% or 2% market share post-merger, respectively. In Column 4, $Treat$ is a continuous variable that equals the single family market share percentage consolidated by acquiring firms in a zipcode. Controls for each zipcode include log total population, log median income, poverty rate, employment rate, labor force participation, homeownership, minority percentage, and population ratios by age and education. t-statistics are reported in parentheses. All columns include zipcode and county by year fixed effects. Standard errors are clustered by zipcode.

Table 8: Institutional Mergers and New Business Loans

Dependent Variable:	$Log(1+\text{New Business Loans})$			
	$I(\Delta(MarketShare)>0)$	$I(\Delta(MarketShare)\geq 1)$	$I(\Delta(MarketShare)\geq 2)$	$\Delta(MarketShare)$
	(1)	(2)	(3)	(4)
Panel A: Loan Numbers				
Treat × Post	0.049***	0.058**	0.033	0.038***
	(0.018)	(0.026)	(0.038)	(0.010)
Adj. R^2	0.575	0.575	0.575	0.575
Panel B: Dollar Volume				
Treat × Post	0.326**	0.515**	0.519	0.329***
	(0.165)	(0.249)	(0.375)	(0.109)
Adj. R^2	0.426	0.426	0.426	0.426
ZIP Controls	X	X	X	X
County × Year FE	X	X	X	X
ZIP FE	X	X	X	X
Observations	30,578	30,578	30,578	30,578

Note: *p<0.1; **p<0.05; ***p<0.01

This table presents estimates of the diff-in-diff regression of SBA 7(A) loan origination activity around institutional investor mergers in a zipcode. Lending data excludes loans to industries in non-tradables, construction, and F.I.R.E industries, and for businesses employing more than 10 people. Panel A displays the results for the logarithm of one plus the number of loans originated, while Panel B displays the results for the logarithm of one plus the dollar volume. The sample includes neighborhoods where acquiring and/or target firms owned properties prior to the merger. In Column 1, $Treat$ is a binary variable that equals 1 if both acquiring and target firms owned property in a zipcode prior to the merger. In Columns 2 and 3, $Treat$ is a binary variable that equals 1 if acquiring firms gained at least 1% or 2% market share post-merger, respectively. In Column 4, $Treat$ is a continuous variable that equals the single family market share percentage consolidated by acquiring firms in a zipcode. Controls for each zipcode include log total population, log median income, poverty rate, employment rate, labor force participation, homeownership, minority percentage, and population ratios by age and education. t-statistics are reported in parentheses. All columns include zipcode and county by year fixed effects. Standard errors are clustered by zipcode.

Table 9: Amenities Post-Merger

Dependent Variable:	*Log*(1 + # of Businesses)			
	$I(\Delta(MarketShare) > 0)$	$I(\Delta(MarketShare) \geq 1)$	$I(\Delta(MarketShare) \geq 2)$	$\Delta(MarketShare)$
	(1)	(2)	(3)	(4)
Panel A: Retail				
Treat × Post	0.134***	−0.074**	−0.142***	−0.033**
	(0.022)	(0.036)	(0.051)	(0.016)
Adj. R^2	0.934	0.934	0.934	0.934
Panel B: Personal Care				
Treat × Post	0.008	−0.086***	−0.176***	−0.059***
	(0.019)	(0.031)	(0.047)	(0.013)
Adj. R^2	0.924	0.924	0.924	0.924
Panel C: Restaurants				
Treat × Post	0.065***	0.047**	−0.009	0.012*
	(0.013)	(0.019)	(0.028)	(0.007)
Adj. R^2	0.973	0.973	0.973	0.973
Panel D: Nightlife				
Treat × Post	−0.016	−0.040	−0.056	−0.031***
	(0.019)	(0.030)	(0.040)	(0.011)
Adj. R^2	0.848	0.848	0.848	0.849
Panel E: Recreation				
Treat × Post	−0.024	−0.113***	−0.218***	−0.055***
	(0.023)	(0.035)	(0.047)	(0.013)
Adj. R^2	0.847	0.847	0.847	0.847
Observations	34,313	34,313	34,313	34,313
ZIP Controls	X	X	X	X
County × Year FE	X	X	X	X
ZIP FE	X	X	X	X

Note: *p<0.1; **p<0.05; ***p<0.01

This table presents estimates of the diff-in-diff regression of the number of amenities available around institutional investor mergers in a zipcode. The dependent variable for Panels A through E is the logarithm of one plus the number of businesses registered in a zipcode that belong to the retail, personal care, restaurant, nightlife, and recreational industries, respectively. The definition for each industry by NAICS code are taken from Z. Li, Shen, and Zhang (2021). The sample includes neighborhoods where acquiring and/or target firms owned properties prior to the merger. In Column 1, $Treat$ is a binary variable that equals 1 if both acquiring and target firms owned property in a zipcode prior to the merger. In Columns 2 and 3, $Treat$ is a binary variable that equals 1 if acquiring firms gained at least 1% or 2% market share post-merger, respectively. In Column 4, $Treat$ is a continuous variable that equals the single family market share percentage consolidated by acquiring firms in a zipcode. Controls for each zipcode include log total population, log median income, poverty rate, employment rate, labor force participation, homeownership, minority percentage, and population ratios by age and education. t-statistics are reported in parentheses. All columns include zipcode and county by year fixed effects. Standard errors are clustered by zipcode.

Table 10: Institutional Mergers and Median Income

Dependent Variable:	$Log(1+\text{Median Income})$			
	$I(\Delta(MarketShare)>0)$	$I(\Delta(MarketShare)\geq 1)$	$I(\Delta(MarketShare)\geq 2)$	$\Delta(MarketShare)$
	(1)	(2)	(3)	(4)
Panel A: All Households				
Treat × Post	-0.007^{***}	-0.013^{***}	-0.010^{*}	-0.007^{***}
	(0.002)	(0.004)	(0.006)	(0.002)
Observations	100,273	100,273	100,273	100,273
Adj. R^2	0.967	0.967	0.967	0.967
Panel B: Same House				
Treat × Post	-0.009^{***}	-0.013^{***}	-0.003	-0.006^{***}
	(0.002)	(0.005)	(0.007)	(0.002)
Observations	93,720	93,720	93,720	93,720
Adj. R^2	0.940	0.940	0.940	0.940
Panel C: Different House, Same County				
Treat × Post	-0.006	0.004	-0.008	-0.003
	(0.009)	(0.015)	(0.024)	(0.007)
Observations	84,683	84,683	84,683	84,683
Adj. R^2	0.652	0.652	0.652	0.652
Panel D: Different House, Diff County, Same State				
Treat × Post	-0.019	-0.006	-0.019	-0.016
	(0.018)	(0.030)	(0.046)	(0.014)
Observations	52,774	52,774	52,774	52,774
Adj. R^2	0.595	0.595	0.595	0.595
Panel E: Different House, Different State				
Treat × Post	0.010	-0.007	-0.115^{**}	-0.018
	(0.021)	(0.035)	(0.055)	(0.016)
Observations	46,129	46,129	46,129	46,129
Adj. R^2	0.593	0.593	0.593	0.593
Panel F: Different House, Diff Country				
Treat × Post	0.057	0.045	0.163	0.018
	(0.077)	(0.151)	(0.208)	(0.068)
Observations	9,481	9,481	9,481	9,481
Adj. R^2	0.746	0.746	0.746	0.746
Census Tract Controls	X	X	X	X
County × Year FE	X	X	X	X
Census Tract FE	X	X	X	X

Note: *p<0.1; **p<0.05; ***p<0.01

This table presents estimates of the diff-in-diff regression of the number of amenities available around institutional investor mergers in a census tract. The dependent variable in each panel is the logarithm of one plus the median income of households in a neighborhood. Panel A looks at the median income of all households. Panel B looks at households who remained in the same house as one year before. Panel C looks at households that moved into a different house from the same county compared to one year before. Panel D looks at households that moved into a different house from a different county in the same state compared to one year before. Panel E looks compared to households that moved into a different house from a different as one year before. Panel F looks compared to households that moved into a different house from a different country as one year before. The sample includes neighborhoods where acquiring and/or target firms owned properties prior to the merger. In Column 1, $Treat$ is a binary variable that equals 1 if both acquiring and target firms owned property in a census tract prior to the merger. In Columns 2 and 3, $Treat$ is a binary variable that equals 1 if acquiring firms gained at least 1% or 2% market share post-merger, respectively. In Column 4, $Treat$ is a continuous variable that equals the single family market share percentage consolidated by acquiring firms in a census tract. Controls for each census tract include log total population, poverty rate, employment rate, labor force participation, homeownership, minority percentage, and population ratios by age and education. t-statistics are reported in parentheses. All columns include census tract and county by year fixed effects. Standard errors are clustered by census tract.

Table 11: Neighborhood Demographic Outcomes

	$I(\Delta(MarketShare) > 0)$	$I(\Delta(MarketShare) \geq 1)$	$I(\Delta(MarketShare) \geq 2)$	$\Delta(MarketShare)$
	(1)	(2)	(3)	(4)
Panel A: College Education (%)				
Treat × Post	-0.354^{***}	-0.434^{**}	-0.354^{***}	-0.434^{**}
	(0.092)	(0.168)	(0.260)	(0.079)
Adj. R^2	0.965	0.965	0.965	0.965
Panel B: Minority Population (%)				
Treat × Post	0.529^{***}	1.161^{***}	0.884^{**}	0.455^{***}
	(0.146)	(0.290)	(0.412)	(0.129)
Adj. R^2	0.958	0.958	0.958	0.958
Panel C: Unemployment Rate (%)				
Treat × Post	0.122	0.038	0.329	0.002
	(0.076)	(0.143)	(0.203)	(0.062)
Adj. R^2	0.768	0.768	0.768	0.768
Census Tract Controls	X	X	X	X
County × Year FE	X	X	X	X
Census Tract FE	X	X	X	X
Observations	100,273	100,273	100,273	100,273

Note: *p<0.1; **p<0.05; ***p<0.01

This table presents estimates of the diff-in-diff regression of the demographic characteristics of neighborhoods around institutional investor mergers in a census tract. Panel A focuses on the correlation between institutional mergers and the percentage of households with a bachelor's level degree or higher. Panel B focuses on the minority population of a census tract around institutional mergers. Panel C looks at the unemployment rate of a census tract around institutional mergers. The sample includes neighborhoods where acquiring and/or target firms owned properties prior to the merger. In Column 1, $Treat$ is a binary variable that equals 1 if both acquiring and target firms owned property in a census tract prior to the merger. In Columns 2 and 3, $Treat$ is a binary variable that equals 1 if acquiring firms gained at least 1% or 2% market share post-merger, respectively. In Column 4, $Treat$ is a continuous variable that equals the single family market share percentage consolidated by acquiring firms in a census tract. Controls for each census tract include log total population, log median income, poverty rate, labor force participation, homeownership, and population ratios by age. t-statistics are reported in parentheses. All columns include census tract and county by year fixed effects. Standard errors are clustered by census tract.

Table 12: Job Creation Post-Merger

Dependent Variable:	*Log*(1 + *Jobs*)			
	$I(\Delta(MarketShare) > 0)$	$I(\Delta(MarketShare) \geq 1)$	$I(\Delta(MarketShare) \geq 2)$	$\Delta(MarketShare)$
	(1)	(2)	(3)	(4)
Panel A: Income				
$0 − 1250	0.010***	0.026***	0.049***	0.016***
	(0.003)	(0.005)	(0.008)	(0.002)
$1251 − 3333	0.012***	0.024***	0.036***	0.016***
	(0.003)	(0.005)	(0.006)	(0.002)
$3333+	−0.004	0.009	0.024***	0.008***
	(0.003)	(0.006)	(0.008)	(0.002)
Panel B: Education				
Less Than Highschool	0.018***	0.017***	0.026***	0.036***
	(0.003)	(0.005)	(0.009)	(0.005)
Highschool	0.014***	0.019***	0.029***	0.031***
	(0.002)	(0.004)	(0.008)	(0.005)
Some College	0.008***	0.009**	0.028***	0.009***
	(0.002)	(0.005)	(0.006)	(0.002)
Bachelor's and higher	−0.001	−0.009*	0.009	−0.001
	(0.003)	(0.005)	(0.007)	(0.002)
Tract Controls	X	X	X	X
County × Year FE	X	X	X	X
Tract FE	X	X	X	X
Observations	110,476	110,476	110,476	110,476

Note: *p<0.1; **p<0.05; ***p<0.01

This table presents estimates of the diff-in-diff regression of the number of primary private jobs around institutional investor mergers in a census tract. Panel A divides jobs by income, while Panel B divides jobs by educational requirements. The sample includes neighborhoods where acquiring and/or target firms owned properties prior to the merger. In Column 1, $Treat$ is a binary variable that equals 1 if both acquiring and target firms owned property in a census tract prior to the merger. In Columns 2 and 3, $Treat$ is a binary variable that equals 1 if acquiring firms gained at least 1% or 2% market share post-merger, respectively. In Column 4, $Treat$ is a continuous variable that equals the single family market share percentage consolidated by acquiring firms in a census tract. Controls for each census tract include log total population, log median income, poverty rate, labor force participation, homeownership, minority percentage, and population ratios by age and education. t-statistics are reported in parentheses. All columns include census tract and county by year fixed effects. Standard errors are clustered by census tract.

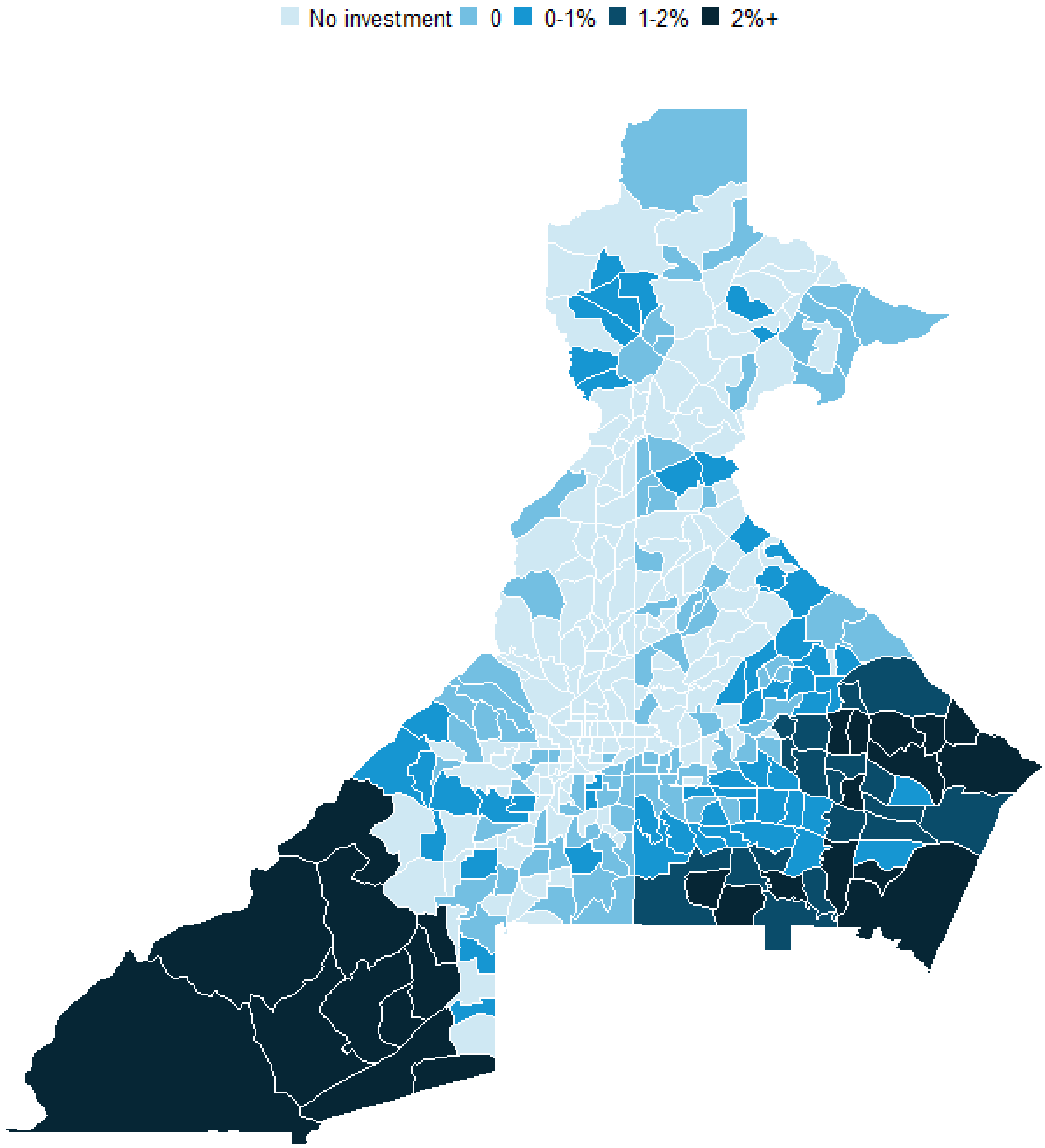


Figure 1: Institutional Investor Mergers in Atlanta

This figure presents a geographic representation of treated and control neighborhoods in the paper's analysis, focused on the greater Atlanta region. Census tracts in the lightest blue color had no institutional investors, and are not included in the sample. Census tracts in slightly darker sky blue are control neighborhoods where no institutional consolidation occured. Subsequent darker shades of blue correspond to census tracts that experienced larger treatments in terms of larger institutional investor consolidation of single family homes.

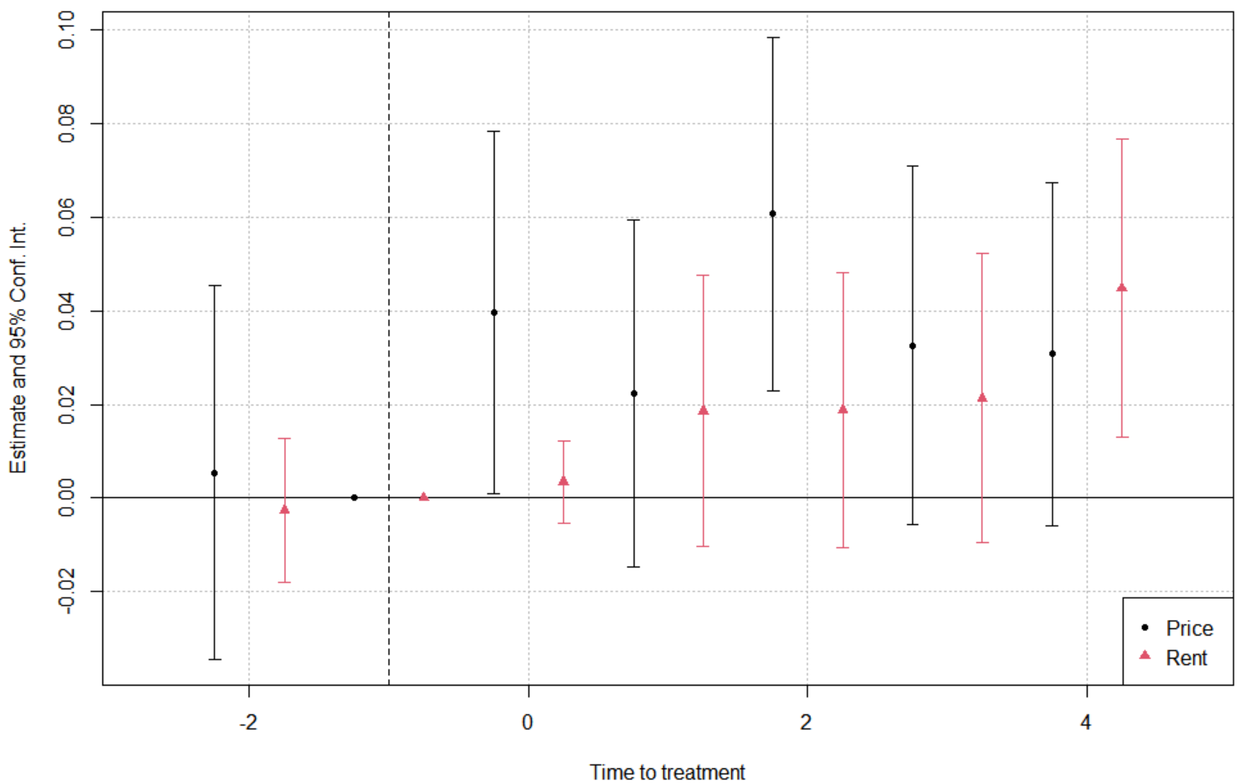
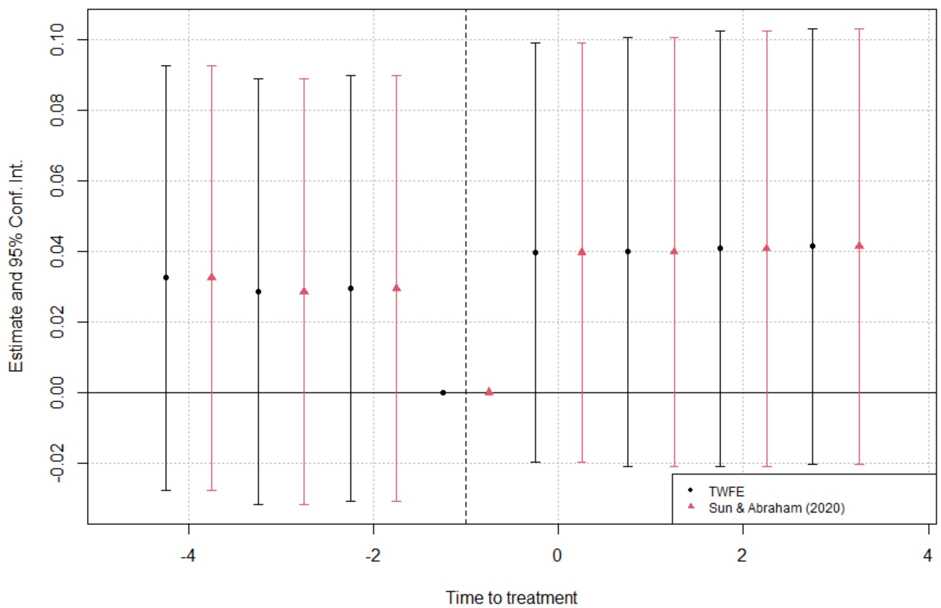
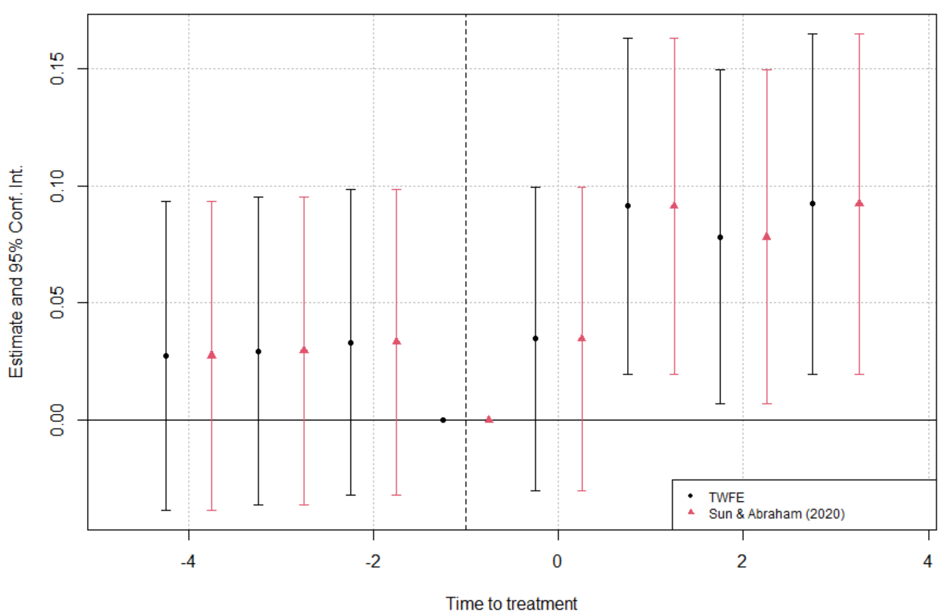


Figure 2: Differences in house and rent prices between treated and control neighborhoods around mergers

This figure presents the coefficients of a regression of house price and rental price indexes between treatment and and the year in relation to institutional mergers. House prices in black are measured via hedonic regression as described in Appendix C.2. Rental prices in red are taken from Zillow (ZORI). A zip is treated if acquirers gained at least 1% market share in the zip n as a result of mergers. Estimates are measured via Sun and Abraham (2021) to account for biases due to differential timing. The horizontal axis shows two years prior to the merger to four years after the merger. The vertical axis represents the difference between treated and control zips in terms of the logarithm of the respective house and rental price indexes. Each estimate is presented with 95% confidence intervals.



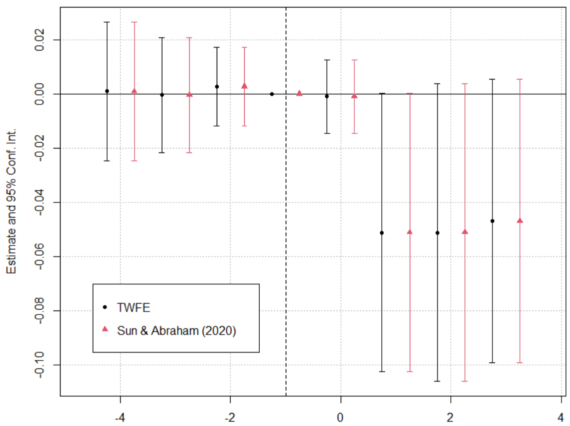
(a) All Industries



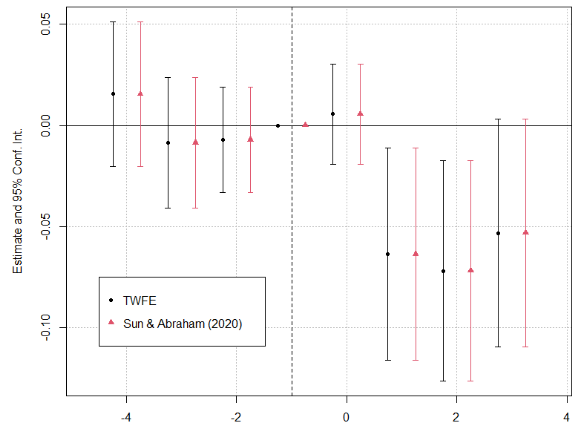
(b) Minus Non-Tradables, Construction, and F.I.R.E.

Figure 3: Differences in small business registrations between treated and control neighborhoods around mergers

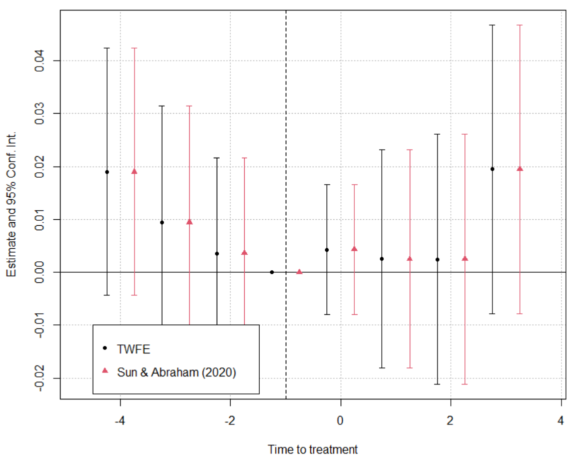
This figure presents the coefficients of a poisson regression of small business registration numbers between treatment and and the year in relation to institutional mergers. A business is defined as small if it has less than four employees. TWFE estimates are presented in black, while Sun and Abraham (2021) adjustments are presented in red. Panel A displays the estimates for all industries. Panel B displays the estimates for industries that are not non-tradable, construction, or F.I.R.E. A zip is treated if acquirers gained at least 2% market share in the zip n as a result of mergers. The horizontal axis shows four years prior to the merger to three years after the merger. The vertical axis represents the difference between treated and control zips in terms of the number of small businesses registered between treated and control zips. Each estimate is presented with 95% confidence intervals.



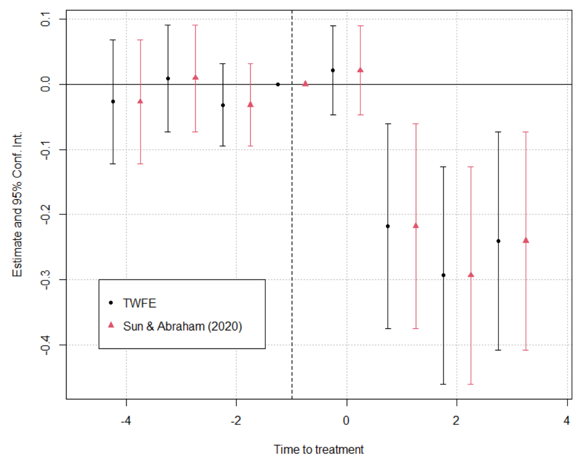
(a) Retail



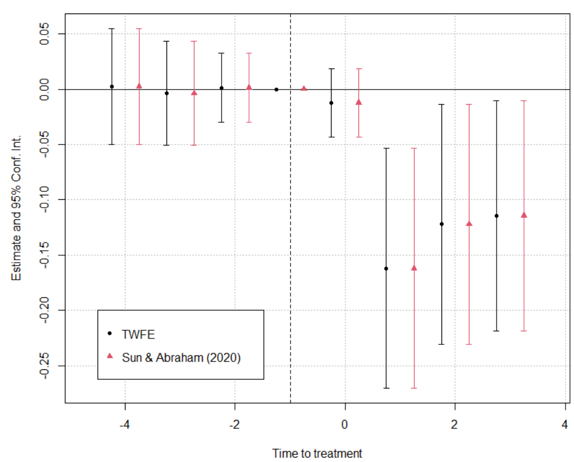
(b) Personal Care



(c) Restaurants



(d) Nightlife



(e) Recreation

Figure 4: Differences in amenities between treated and control neighborhoods around mergers

This figure presents the coefficients of a poisson regression of the number of amenities between treatment and and the year in relation to institutional mergers. Definitions of amenities are taken from Qian and Tan (2021). TWFE estimates are presented in black, while Sun and Abraham (2021) adjustments are presented in red. A zip is treated if acquirers gained at least 2% market share in the zip n as a result of mergers. The horizontal axis shows three years prior to the merger to three years after the merger. The vertical axis represents the difference between treated and control zips in terms of the number of small businesses registered between treated and control zips. Each estimate is presented with 95% confidence intervals.

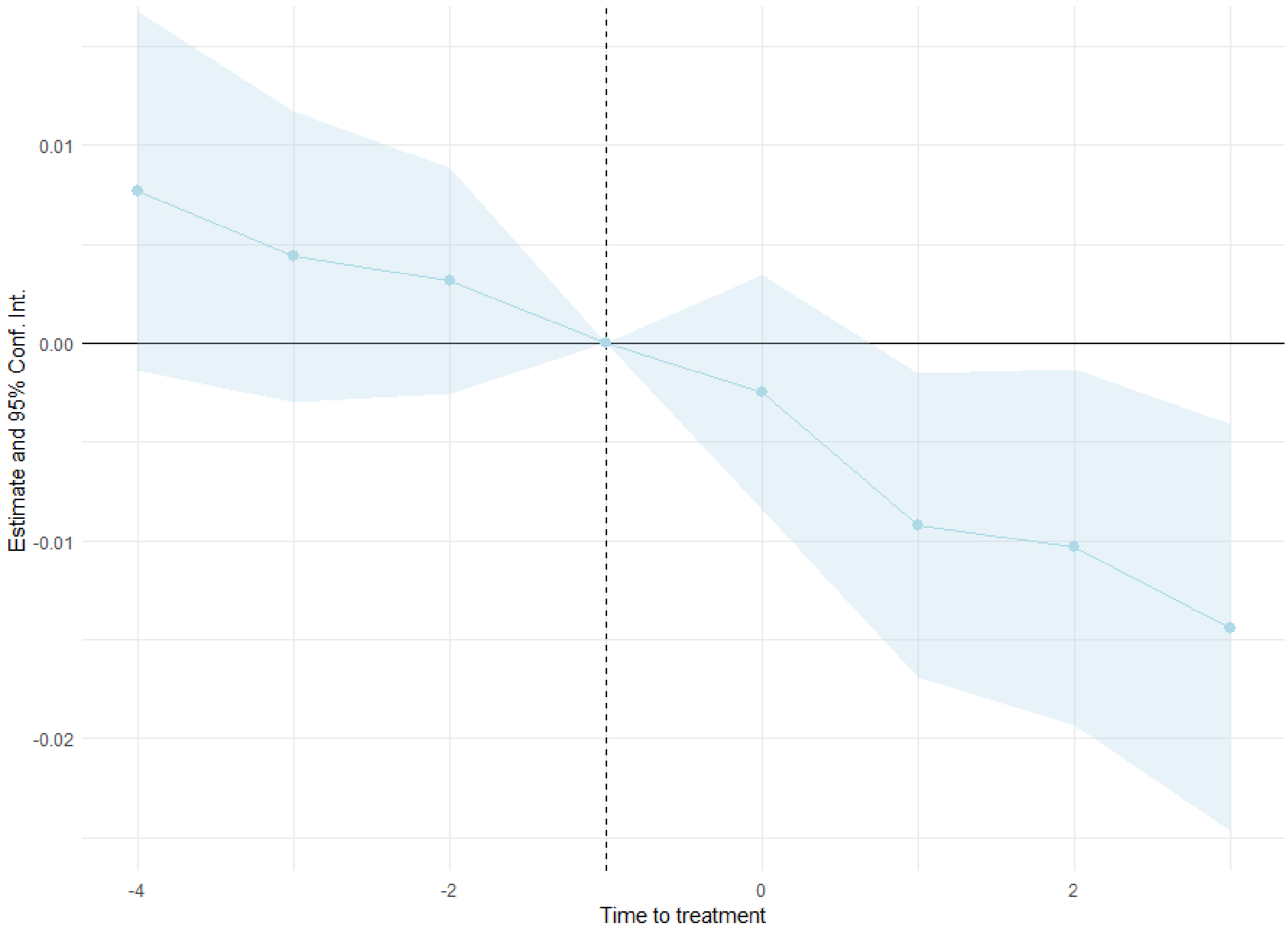


Figure 5: Differences in household median income between treated and control neighborhoods around mergers

This figure presents the coefficients of a regression of log median income between treatment and and the year in relation to institutional mergers. Estimates are measured via Sun and Abraham (2021) to account for biases due to differential timing. A census tract is treated if acquirers gained at least 1% market share in the census tract n as a result of mergers. The horizontal axis shows four years prior to the merger to three years after the merger. The vertical axis represents the difference between treated and control census tracts in terms of the number of small businesses registered between treated and control census tracts. Each estimate is presented with 95% confidence intervals.

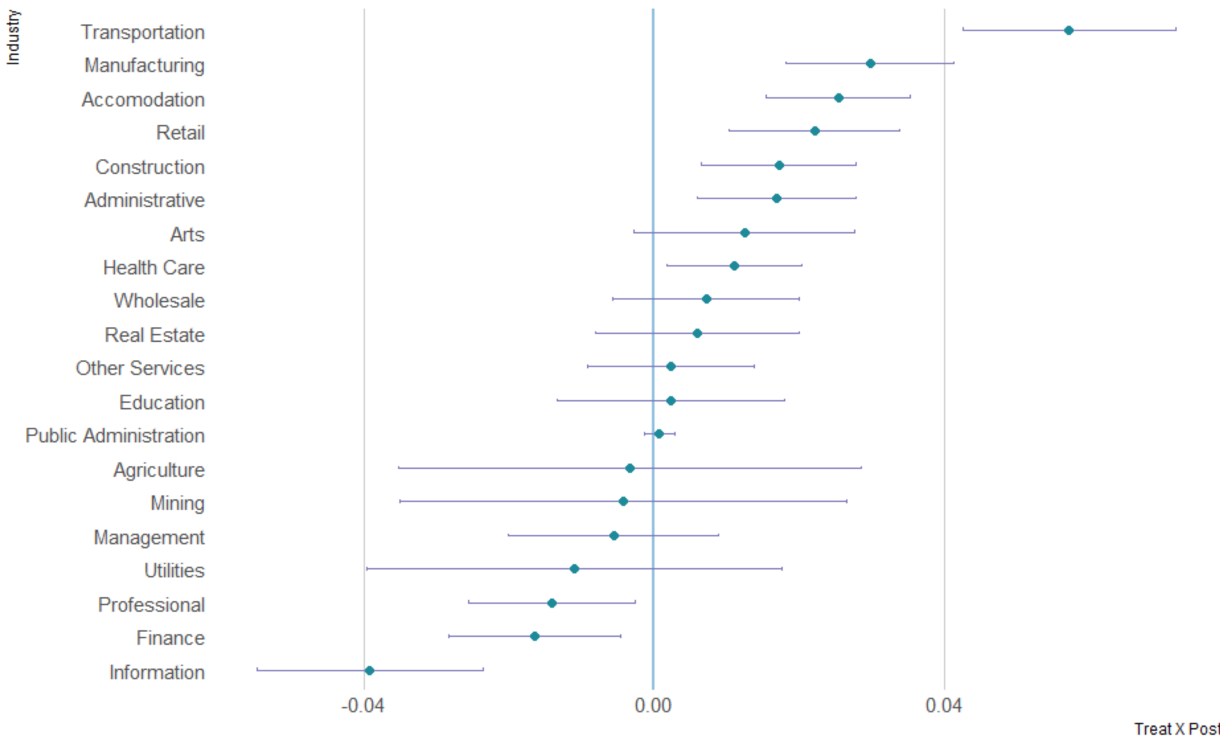


Figure 6: Differences in job numbers by industry between treated and control census tracts around mergers

This figure presents the coefficients of a diff-in-diff regression of institutional investor mergers on the number of private primary jobs registered in a census tract, separated by industry. A census tract is treated if acquirers gained at least 1% market share in the census tract n as a result of mergers. The horizontal axis shows the estimate for β_3 in Equation 13 with 95% confidence intervals. The vertical axis separates the estimates for industries as defined in LODES.

Appendix A. Additional Tables

Table A1: Institutional Mergers and Mortgage Denial Rates

Dependent Variable:	Denial Rate (%)			
	$I(\Delta(MarketShare) > 0)$	$I(\Delta(MarketShare) \geq 1)$	$I(\Delta(MarketShare) \geq 2)$	$\Delta(MarketShare)$
	(1)	(2)	(3)	(4)
Panel A: Purchase				
Treat × Post	0.005**	−0.003	−0.004	−0.001
	(0.002)	(0.004)	(0.005)	(0.002)
Adjusted R^2	0.462	0.462	0.462	0.462
Panel B: Refinancing				
Treat × Post	−0.001	0.004*	0.005	0.001
	(0.001)	(0.002)	(0.003)	(0.001)
Adjusted R^2	0.630	0.630	0.630	0.630
Census Tract Controls	X	X	X	X
County × Year FE	X	X	X	X
Census Tract FE	X	X	X	X
Observations	131,401	131,401	131,401	131,401

Note: *p<0.1; **p<0.05; ***p<0.01

This table presents estimates of the diff-in-diff regression of denial rates for conventional owner-occupied mortgage applications for single family homes around institutional investor mergers in a census tract. Panel A displays the results for home purchase mortgages, while Panel B displays the results for refinancing mortgages. The sample includes neighborhoods where acquiring and/or target firms owned properties prior to the merger. In Column 1, $Treat$ is a binary variable that equals 1 if both acquiring and target firms owned property in a census tract prior to the merger. In Columns 2 and 3, $Treat$ is a binary variable that equals 1 if acquiring firms gained at least 1% or 2% market share post-merger, respectively. In Column 4, $Treat$ is a continuous variable that equals the single family market share percentage consolidated by acquiring firms in a census tract. Controls for each census tract include log total population, log median income, poverty rate, employment rate, labor force participation, homeownership, minority percentage, and population ratios by age and education. t-statistics are reported in parentheses. All columns include census tract and county by year fixed effects. Standard errors are clustered by census tract.

Table A2: Entrepreneurship And Homeownership

	Log(1+Businesses)			
	$I(\Delta(MarketShare) > 0)$	$I(\Delta(MarketShare) \geq 1)$	$I(\Delta(MarketShare) \geq 2)$	$\Delta(MarketShare)$
	(1)	(2)	(3)	(4)
Treat × Post × Q1	-0.039^{***}	0.001	0.032	0.006
	(0.008)	(0.017)	(0.028)	(0.009)
Treat × Post × Q2	-0.015^{*}	0.041^{***}	0.046^{***}	0.014^{***}
	(0.008)	(0.012)	(0.014)	(0.005)
Treat × Post × Q3	0.008	0.055^{***}	0.093^{**}	0.030^{***}
	(0.009)	(0.017)	(0.037)	(0.008)
Treat × Post × Q4	0.033^{***}	0.036^{**}	0.035^{*}	0.022^{***}
	(0.008)	(0.015)	(0.020)	(0.006)
ZIP Controls	X	X	X	X
County × Year FE	X	X	X	X
ZIP FE	X	X	X	X
Observations	34,291	34,291	34,291	34,291
Adjusted R^2	0.989	0.989	0.989	0.989

Note: *p<0.1; **p<0.05; ***p<0.01

This table presents estimates of the diff-in-diff regression of small business registrations around institutional investor mergers in a zipcode, divided by homeownership. The dependent variable is the number of small businesses registered in a zipcode, excluding nontradables, construction, and F.I.R.E. industries. $Q1$ through $Q4$ are dummy variables that indicate whether the homeownership rate of a neighborhood falls within the respective quartile of neighborhoods in the sample. In Column 1, $Treat$ is a binary variable that equals 1 if both acquiring and target firms owned property in a zipcode prior to the merger. In Columns 2 and 3, $Treat$ is a binary variable that equals 1 if acquiring firms gained at least 1% or 2% market share post-merger, respectively. In Column 4, $Treat$ is a continuous variable that equals the single family market share percentage consolidated by acquiring firms in a zipcode. Controls for each zipcode include log total population, log median income, poverty rate, employment rate, labor force participation, homeownership, minority percentage, and population ratios by age and education. t-statistics are reported in parentheses. All columns include zipcode and county by year fixed effects. Standard errors are clustered by zipcode.

Table A3: Institutional Mergers and New Business Loans for Homeowners

Dependent Variable:	$Log(1 + \text{New Business Loans})$			
	$I(\Delta(MarketShare) > 0)$	$I(\Delta(MarketShare) \geq 1)$	$I(\Delta(MarketShare) \geq 2)$	$\Delta(MarketShare)$
	(1)	(2)	(3)	(4)
Panel A: Loan Numbers				
Treat × Post	0.029**	0.028*	0.008	0.011*
	(0.011)	(0.016)	(0.023)	(0.006)
Adj. R^2	0.286	0.286	0.286	0.286
Panel B: Dollar Volume				
Treat × Post	0.331**	0.398**	0.173	0.166**
	(0.134)	(0.195)	(0.273)	(0.071)
Adj. R^2	0.241	0.241	0.241	0.241
ZIP Controls	X	X	X	X
County × Year FE	X	X	X	X
ZIP FE	X	X	X	X
Observations	30,578	30,578	30,578	30,578

Note: *p<0.1; **p<0.05; ***p<0.01

This table presents estimates of the diff-in-diff regression of SBA 7(A) loan origination activity to homeowner addresses around institutional investor mergers in a zipcode. Lending data excludes loans to industries in non-tradables, construction, and F.I.R.E industries, and for businesses employing more than 10 people. Panel A displays the results for the logarithm of one plus the number of loans originated, while Panel B displays the results for the logarithm of one plus the dollar volume. The sample includes neighborhoods where acquiring and/or target firms owned properties prior to the merger. In Column 1, $Treat$ is a binary variable that equals 1 if both acquiring and target firms owned property in a zipcode prior to the merger. In Columns 2 and 3, $Treat$ is a binary variable that equals 1 if acquiring firms gained at least 1% or 2% market share post-merger, respectively. In Column 4, $Treat$ is a continuous variable that equals the single family market share percentage consolidated by acquiring firms in a zipcode. Controls for each zipcode include log total population, log median income, poverty rate, employment rate, labor force participation, homeownership, minority percentage, and population ratios by age and education. t-statistics are reported in parentheses. All columns include zipcode and county by year fixed effects. Standard errors are clustered by zipcode.

Table A4: Institutional Mergers and New Business Loans for Non-Homeowners

Dependent Variable:	$Log(1 + \text{New Business Loans})$			
	$I(\Delta(MarketShare) > 0)$	$I(\Delta(MarketShare) \geq 1)$	$I(\Delta(MarketShare) \geq 2)$	$\Delta(MarketShare)$
	(1)	(2)	(3)	(4)
Panel A: Loan Numbers				
Treat × **Post**	0.029	0.033	0.026	0.030***
	(0.017)	(0.025)	(0.036)	(0.010)
Adj. R^2	0.560	0.559	0.560	0.560
Panel B: Dollar Volume				
Treat × **Post**	0.210	0.285	0.528	0.294**
	(0.168)	(0.262)	(0.406)	(0.115)
Adj. R^2	0.417	0.417	0.417	0.417
ZIP Controls	X	X	X	X
County × Year FE	X	X	X	X
ZIP FE	X	X	X	X
Observations	30,578	30,578	30,578	30,578

Note: *p<0.1; **p<0.05; ***p<0.01

This table presents estimates of the diff-in-diff regression of SBA 7(A) loan origination activity to non-homeowner addresses around institutional investor mergers in a zipcode. Lending data excludes loans to industries in non-tradables, construction, and F.I.R.E industries, and for businesses employing more than 10 people. Panel A displays the results for the logarithm of one plus the number of loans originated, while Panel B displays the results for the logarithm of one plus the dollar volume. The sample includes neighborhoods where acquiring and/or target firms owned properties prior to the merger. In Column 1, $Treat$ is a binary variable that equals 1 if both acquiring and target firms owned property in a zipcode prior to the merger. In Columns 2 and 3, $Treat$ is a binary variable that equals 1 if acquiring firms gained at least 1% or 2% market share post-merger, respectively. In Column 4, $Treat$ is a continuous variable that equals the single family market share percentage consolidated by acquiring firms in a zipcode. Controls for each zipcode include log total population, log median income, poverty rate, employment rate, labor force participation, homeownership, minority percentage, and population ratios by age and education. t-statistics are reported in parentheses. All columns include zipcode and county by year fixed effects. Standard errors are clustered by zipcode.

Table A5: Entrepreneurship By Retiree Population

	Log(1+Businesses)			
	$I(\Delta(MarketShare) > 0)$	$I(\Delta(MarketShare) >= 1)$	$I(\Delta(MarketShare) >= 2)$	$\Delta(MarketShare)$
	(1)	(2)	(3)	(4)
Treat × Post × Q1	0.029***	0.077***	0.082***	0.025***
	(0.009)	(0.013)	(0.017)	(0.005)
Treat × Post × Q2	−0.009	−0.005	−0.004	0.00002
	(0.008)	(0.012)	(0.017)	(0.005)
Treat × Post × Q3	−0.021**	0.001	−0.038	−0.001
	(0.008)	(0.019)	(0.033)	(0.010)
Treat × Post × Q4	−0.033***	0.021	0.080*	0.010
	(0.009)	(0.023)	(0.043)	(0.013)
ZIP Controls	X	X	X	X
County × Year FE	X	X	X	X
ZIP FE	X	X	X	X
Observations	34,291	34,291	34,291	34,291
Adjusted R^2	0.989	0.989	0.989	0.989

Note: *p<0.1; **p<0.05; ***p<0.01

This table presents estimates of the diff-in-diff regression of small business registrations around institutional investor mergers in a zipcode, divided by the percentage of the population that is at least 65 years old. $Q1$ through $Q4$ are dummy variables that indicate whether the retiree population of a neighborhood falls within the respective quartile of neighborhoods in the sample. The dependent variable is the number of small businesses registered in a zipcode, excluding nontradables, construction, and F.I.R.E. industries. The sample includes neighborhoods where acquiring and/or target firms owned properties prior to the merger. In Column 1, $Treat$ is a binary variable that equals 1 if both acquiring and target firms owned property in a zipcode prior to the merger. In Columns 2 and 3, $Treat$ is a binary variable that equals 1 if acquiring firms gained at least 1% or 2% market share post-merger, respectively. In Column 4, $Treat$ is a continuous variable that equals the single family market share percentage consolidated by acquiring firms in a zipcode. Controls for each zipcode include log total population, log median income, poverty rate, employment rate, labor force participation, homeownership, minority percentage, and population ratios by age and education. t-statistics are reported in parentheses. All columns include zipcode and county by year fixed effects. Standard errors are clustered by zipcode.

Table A6: Job Creation Post-Merger - Zipcode Level

Dependent Variable:	*Log*(1 + *Jobs*)			
	$I(\Delta(MarketShare) > 0)$	$I(\Delta(MarketShare) \geq 1)$	$I(\Delta(MarketShare) \geq 2)$	$\Delta(MarketShare)$
	(1)	(2)	(3)	(4)
Panel A: Income				
$0 − 1250	−0.006	0.018***	0.032***	0.008***
	(0.007)	(0.007)	(0.009)	(0.002)
$1251 − 3333	0.004	0.025***	0.031***	0.010***
	(0.007)	(0.006)	(0.008)	(0.003)
	(0.003)	(0.005)	(0.006)	(0.002)
$3333+	−0.008	0.018***	0.023**	0.007***
	(0.007)	(0.007)	(0.009)	(0.002)
Panel B: Education				
Less Than Highschool	0.003	0.018***	0.016**	0.006***
	(0.006)	(0.006)	(0.008)	(0.002)
Highschool	−0.008	0.015***	0.019**	0.005**
	(0.006)	(0.006)	(0.007)	(0.002)
Some College	−0.008	0.010*	0.012	0.003
	(0.007)	(0.006)	(0.007)	(0.002)
Bachelor's and higher	−0.013*	−0.001	0.001	−0.003
	(0.006)	(0.006)	(0.008)	(0.002)
ZIP Controls	X	X	X	X
County × Year FE	X	X	X	X
ZIP FE	X	X	X	X
Observations	110,476	110,476	110,476	110,476

Note: *p<0.1; **p<0.05; ***p<0.01

This table presents estimates of the diff-in-diff regression of the number of primary private jobs around institutional investor mergers in a zipcode. Panel A divides jobs by income, while Panel B divides jobs by educational requirements. The sample includes neighborhoods where acquiring and/or target firms owned properties prior to the merger. In Column 1, $Treat$ is a binary variable that equals 1 if both acquiring and target firms owned property in a zipcode prior to the merger. In Columns 2 and 3, $Treat$ is a binary variable that equals 1 if acquiring firms gained at least 1% or 2% market share post-merger, respectively. In Column 4, $Treat$ is a continuous variable that equals the single family market share percentage consolidated by acquiring firms in a zipcode. Controls for each zipcode include log total population, log median income, poverty rate, labor force participation, homeownership, minority percentage, and population ratios by age and education. t-statistics are reported in parentheses. All columns include zipcode and county by year fixed effects. Standard errors are clustered by zipcode.

Table A7: Institutional Mergers and Housing Costs accounting for staggered treatment

Panel A: Housing Prices				
Group	**ATT**	**SE**	**95% Confidence bands**	
All Groups	0.0379^{**}	0.01	0.0184	0.0575
2016	0.0466^{**}	0.0138	0.0157	0.0775
2017	0.0165	0.0122	−0.0108	0.0439

Panel B: Rental Prices				
Group	**ATT**	**SE**	**95% Confidence bands**	
All Groups	0.0257^{**}	0.0074	0.0111	0.0402
2016	0.0262^{**}	0.0071	0.0118	0.0407
2017	−0.0424	0.0295	−0.1021	0.0172

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table A8: Institutional Mergers and Small Business Registration accounting for staggered treatment

Panel A: All Small Businesses				
Group	**ATT**	**SE**	**95% Confidence bands**	
All Groups	0.037**	0.0031	0.031	0.043
2016	0.0373**	0.0030	0.0312	0.0435
2017	−0.0017	0.0151	−0.0328	0.0294

Panel B: Minus Construction				
Group	**ATT**	**SE**	**95% Confidence bands**	
All Groups	0.0325**	0.0035	0.0256	0.0394
2016	0.0329**	0.0033	0.0259	0.0399
2017	−0.0155	0.0161	−0.0503	0.0194

Panel C: Minus Construction and Non-Tradables				
Group	**ATT**	**SE**	**95% Confidence bands**	
All Groups	0.0248**	0.0039	0.0171	0.0325
2016	0.0253**	0.0036	0.0175	0.033
2017	−0.0356	0.022	−0.0826	0.0115

Panel D: Minus Construction, Real Estate, Finance, and Non-Tradables				
Group	**ATT**	**SE**	**95% Confidence bands**	
All Groups	0.0245**	0.0038	0.0171	0.032
2016	0.0251**	0.00036	0.0176	0.0325
2017	−0.0349	0.0246	−0.0857	0.0159

Note: *p<0.1; **p<0.05; ***p<0.01

Table A9: Non-insitutional investor keywords

Type	Keywords
Government Agencies	"City of", "County of", "Fannie Mae", "Federal Deposit Insurance", "FDIC", "Federal Home Loan Banks", "Federal Housing Administration", "FHA", "Freddie Mac", "Small Business Administration", "SBA", "State of", "US Department of Housing and Urban Development", "HUD", "Township", "Veterans Administration", "Veterans Affairs", "Federal National Mortgage Assn", "Community Assn", "FNMA","FHLMC", "ˆRedevelopment", "Natl Assn", "Community Association", "Housing Commission","Land Bank Authority", "Public Schools", "Intercity Escrow", "Housing Authority","County Housing", "Dept Transportation", "Conservancy", "Department of", "Hsng Dev Agcy", "Neighborhood Development","Federal", "Authority", "Autho", "And Economic", "Government", "County", "Dept Of", "Veterans", "CHFA", " FCU", "United States of America", "Secretary","Mortgage Assn", "Cmty Svcs",
Nonprofits	"Affordable housing", "Baptist Church", "Catholic", "Christian church", "Church of", "Community development fund","Community housing works", "Community land trust", "Episcopal", "God", "Gospel", "Habitat for Humanity", "Methodist", "Neighborhood redevelopment", "Neighborhood rehab", "Presbyterian"
Banks and Lending	"American Bank", "Bank of America", "CWABS","CWALT", "CWMBS", "Everbank", "Bank of New York", "Bank One","Bank Of","Banco ", "Bankers Trust", "Bear Sterns", "Capital One", "Chase", "Citi Bank", "Citi Mortgage", "Citybank", "Citigroup", "Citizens Bank", "Coast Bank", "Commerce Bank", "Commercial Bank", "Countrywide", "Credit Suisse", "Credit Union", "Deutsche Bank", "E Trade", "Flagstar Bank", "First Union", "Household Fin", "HSBC", "IndyMac", "JP Morgan", "Lasalle Bank", "Lehman Brothers", "Loan & Thrift", "Morgan Stanley", "Mutual Bank", "National Bank", "Norwest Bank", "Old Kent Bank", "Pacific Bank", "ProvidentBank", "Regions Bank", "RBC Bank", "Silverton Bank", "Sovereign Bank", "Standard Bank", "State Bank", "Sterling Bank", "Suntrust", "Wachovia", "TCF Bank", "Treasury Bank", "Union Bank", "United Texas Bank", "View Bank","Virtual Bank", "Washington Mutual", "Wells Fargo", "World Food Bank", "World Savings Bank", "MTG", "US Bank", "Nationstar Mortgage", "Ellington Loan Mortgage", "Security National", "Ocwen", "Mortgage Electronic", "IMORTGAGE", "Residential Funding", "MorEquity", "US BK NA","PNC Bank", "Bank National Assn", "Ellington", "Mortgage Capital", "Insurance Corp", "Savings Fund", "Loan Services", "Quicken", "Rocket Mortgage", "Green Tree","Pennymac", "NATIONAL RESIDL NOMINEE SVCS", "U S Bank", "Pacific Coast", "Wilmington", "ˆAjax", "Ameriquest","Mortgage Series", "EMC Mortgage", "First Guaranty", "Metlife"
Construction	"Ashton Residential", "Ambrosia Homes", "Arvida of JMB", "Bowen Family Homes", "Centex Homes", "Continental Homes", "CP Morgan", "Coscan Washington", "Construction", "Dalton", "David Weekley Homes","Dell Webb Community", "DR Horton", "GL Homes", "Greystone Nevada", "Hedgewood Properties", "Highland Home", "Homeland Legacy", "Homes of Charlotte", "John Wieland HMS", "KB Homes", "Legacy Communities", "Legend", "Lennar", "Lewis Homes", "Levitt Homes", "Long Lake", "McCar Homes", "Melody Homes", "Mercedes Homes", "Mercedea Homes", "Meritage Homes", "Minto Communities", "Morrison Homes", "Mulvaney Homes", "NVRL Permabilt", "Pulte Home", "Quadrant Corp", "RH of Texas", "Rottlund Co", "Richardson", "Housing Group", "Richport Prop", "Ryan Homes", "Ryland GRP", "Scenic Homes", "Shapell Industries", "Shea Homes", "Toll Brothers", "Watt Homes", "Westbrooke Homes", "Western Pacific Housing", "William Lyon Homes", "Woodside Homes", "Chathambilt Homes", "NVR", "Builders", "True Homes", "Weekley Homes", "Renovations", "Mercerders", "Dalton", "Packaging Equipment","Built", "Builder", "S & S Homes",
Financial Institutions	"Argent", "Arizona Equity", "Consult", "Cardinal Capital", "Ace ", "American Residential Equities", "Life ins", "Andesite", "Arch Bay", "Arizona Equity", "Auction", "Asset Management","Asset Mgmt", "Bekshire", "Cascade F", "Elizon", "ˆEqui", "ˆFrontier", "ˆSpring"
Others	"AS I", "Alder S", "ASE 1", "ASE 2", "ASE 3", "Elite Home", "Relocation", "Mobility", "Cartus", "Cendant Mob", "Global", "Homesales", "Right Resid", "Realty Opening", "HOA", "Homeowners Association", "Opendoor", "Zillow", "SPH Property"

Appendix B. Additional Figures

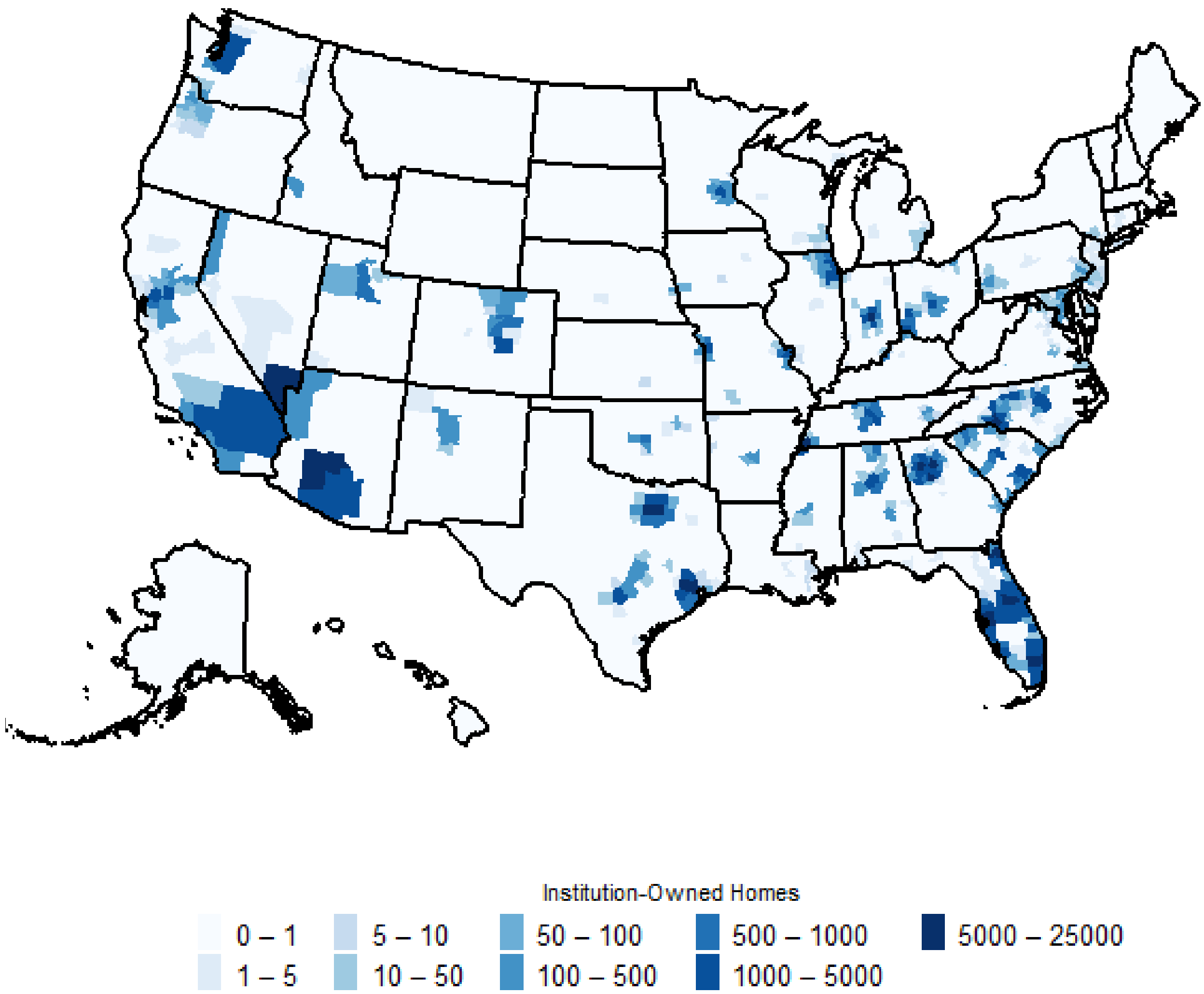
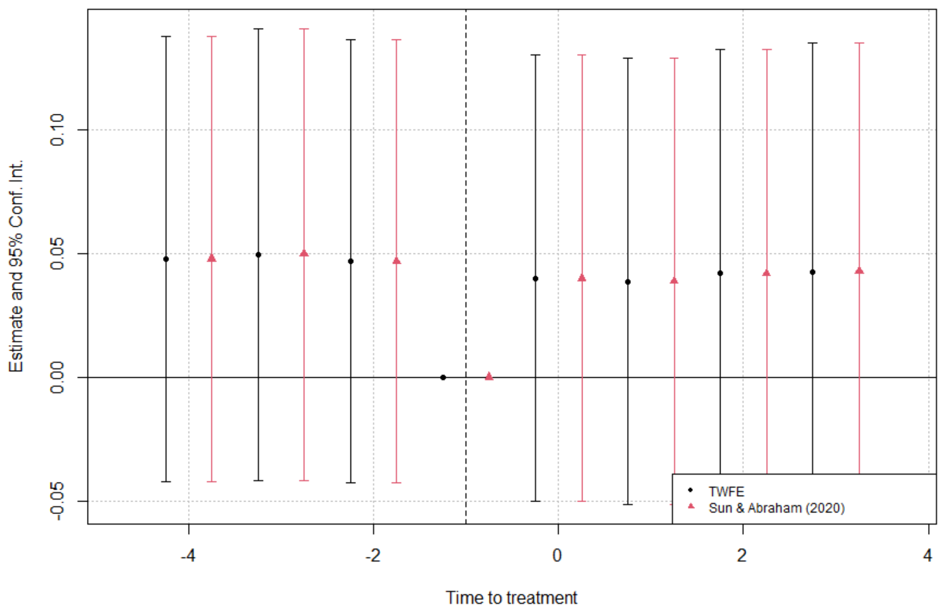
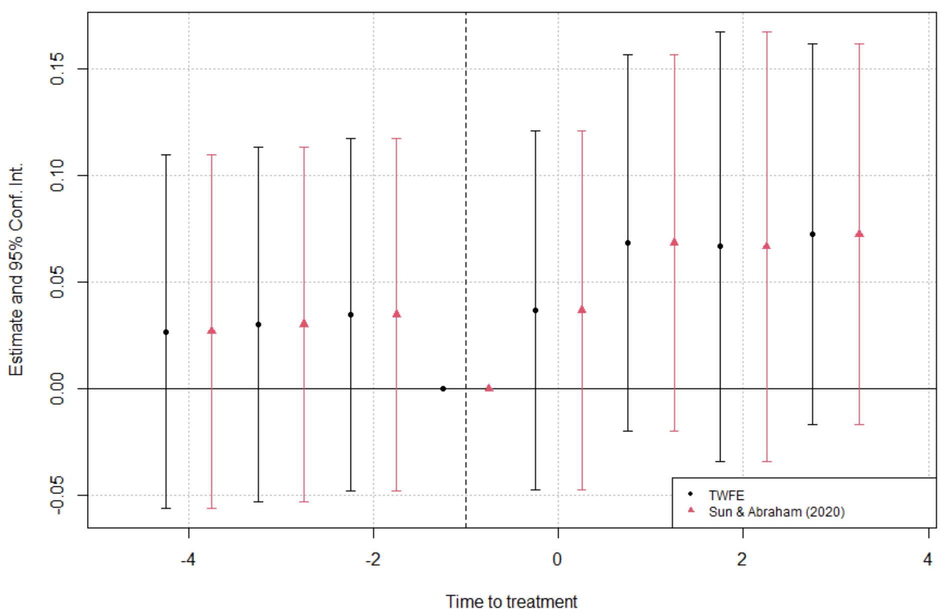


Figure A1: Institutional Investor Single-Family Ownership in 2022

This figure displays the number of properties owned by institutional investors in 2022 by county throughout the United States.



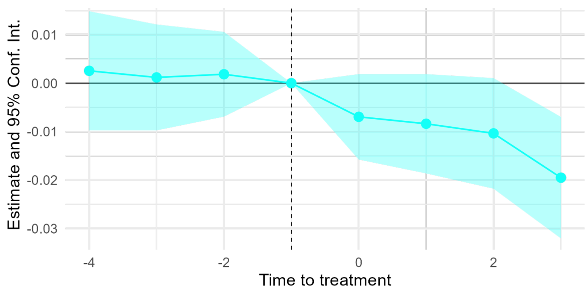
(a) All Industries



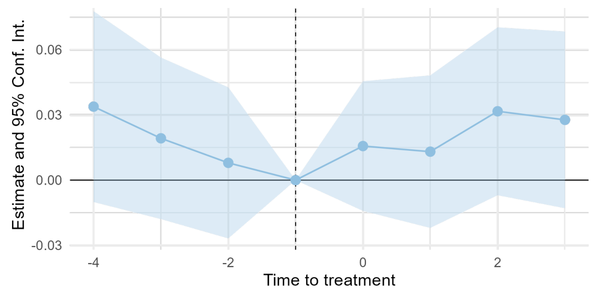
(b) Minus Non-Tradables, Construction, and F.I.R.E.

Figure A2: Differences in non-small business registrations between treated and control neighborhoods around mergers

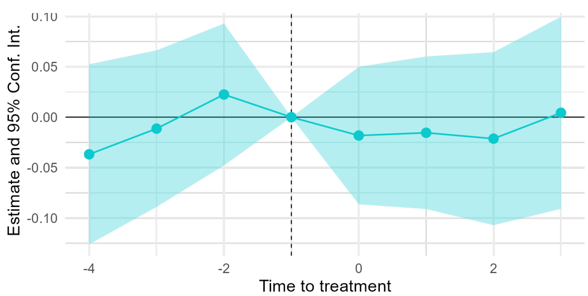
This figure presents the coefficients of a poisson regression of non-small business registration numbers between treatment and and the year in relation to institutional mergers. A business is defined as non-small if it has at least five employees. TWFE estimates are presented in black, while Sun and Abraham (2021) adjustments are presented in red. Panel A displays the estimates for all industries. Panel B displays the estimates for industries that are not non-tradable, construction, or F.I.R.E. A zip is treated if acquirers gained at least 2% market share in the zip n as a result of mergers. The horizontal axis shows four years prior to the merger to three years after the merger. The vertical axis represents the difference between treated and control zips in terms of the number of small businesses registered between treated and control zips. Each estimate is presented with 95% confidence intervals.



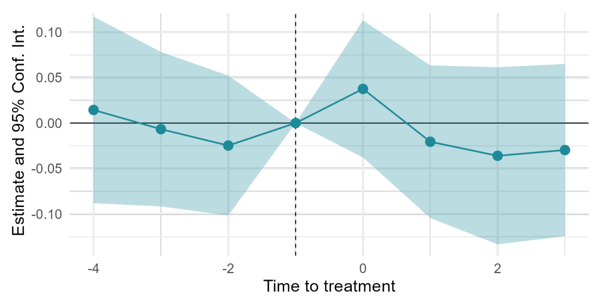
(a) Same House



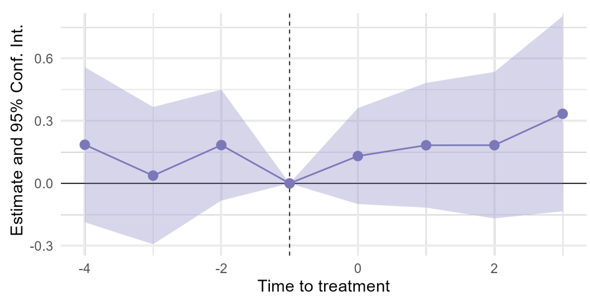
(b) Different House, Same County



(c) Different House, Different County



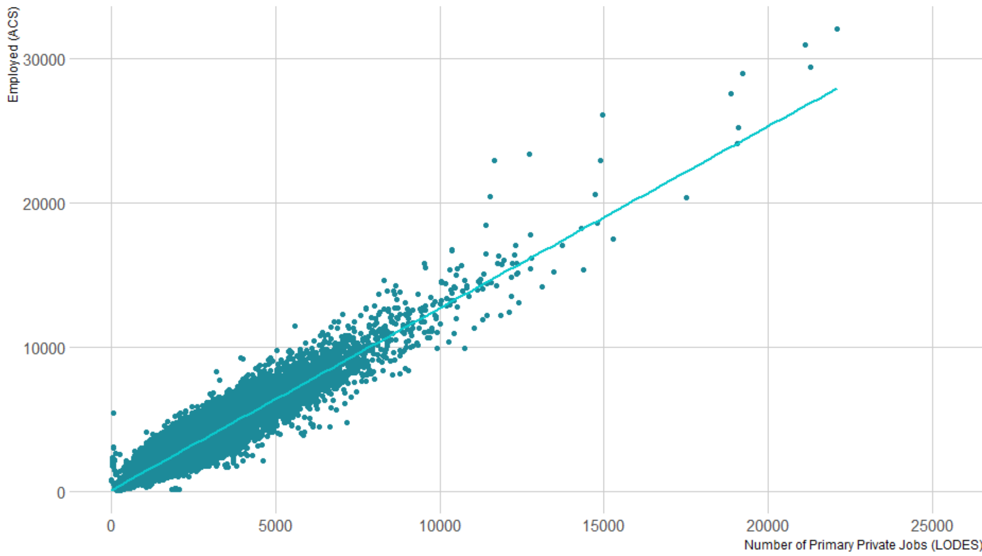
(d) Different House, Different State



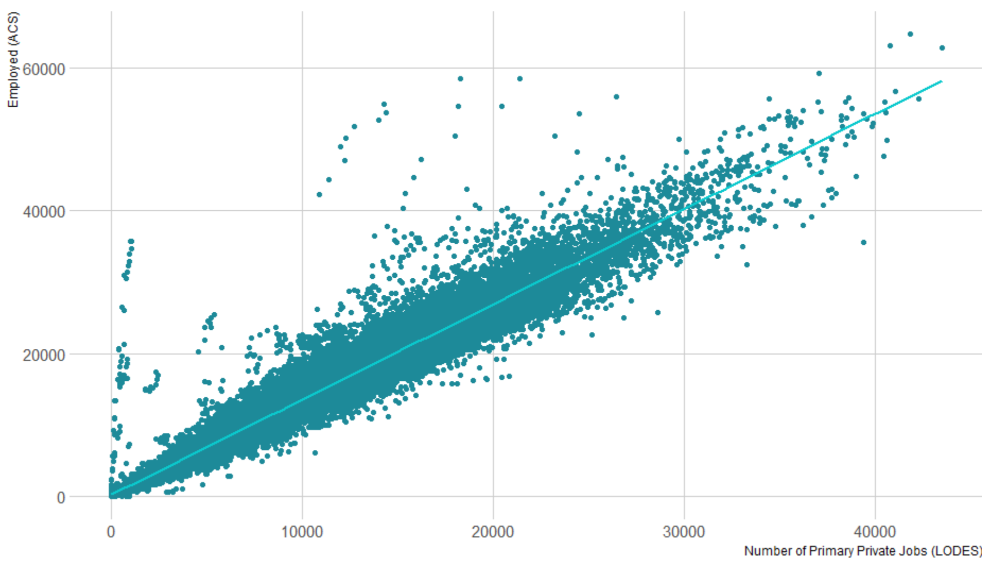
(e) Different House, Different Country

Figure A3: Differences in household median income by housing tenure between treated and control neighborhoods around mergers

This figure presents the coefficients of a regression of log median income between treatment and and the year in relation to institutional mergers, separated by housing tenure. Estimates are measured via Sun and Abraham (2021) to account for biases due to differential timing. A census tract is treated if acquirers gained at least 1% market share in the census tract n as a result of mergers. The horizontal axis shows four years prior to the merger to three years after the merger. The vertical axis represents the difference between treated and control census tracts in terms of the number of small businesses registered between treated and control census tracts. Each estimate is presented with 95% confidence intervals.



(a) Census Tract-Level



(b) Zipcode-Level

Figure A4: LODES vs. ACS Employment Numbers

This figure displays the relationship between employment numbers from LEHD Origin-Destination Employment Statistics (LODES), and those from American Community Survey (ACS). Panel A displays the relationship for employment numbers on a census tract level. Panel B displays the relationship for employment numbers on a zipcode level. The horizontal axis displays the number of private primary jobs for a neighborhood in LODES. The vertical axis displays employment numbers for the same neighborhood in ACS.

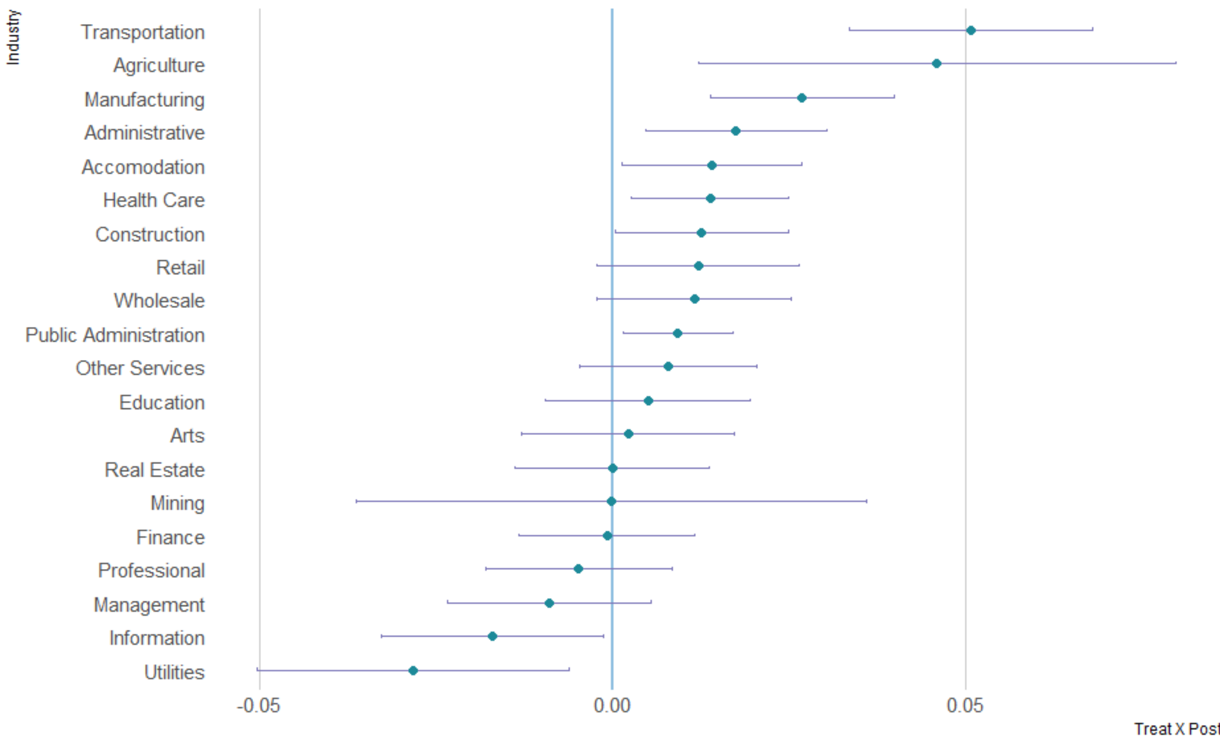


Figure A5: Differences in job numbers by industry between treated and control zipcodes around mergers

This figure presents the coefficients of a diff-in-diff regression of institutional investor mergers on the number of private primary jobs registered in a zipcode, separated by industry. A zipcode is treated if acquirers gained at least 1% market share in the zipcode n as a result of mergers. The horizontal axis shows the estimate for β_3 in Equation 13 with 95% confidence intervals. The vertical axis separates the estimates for industries as defined in LODES.

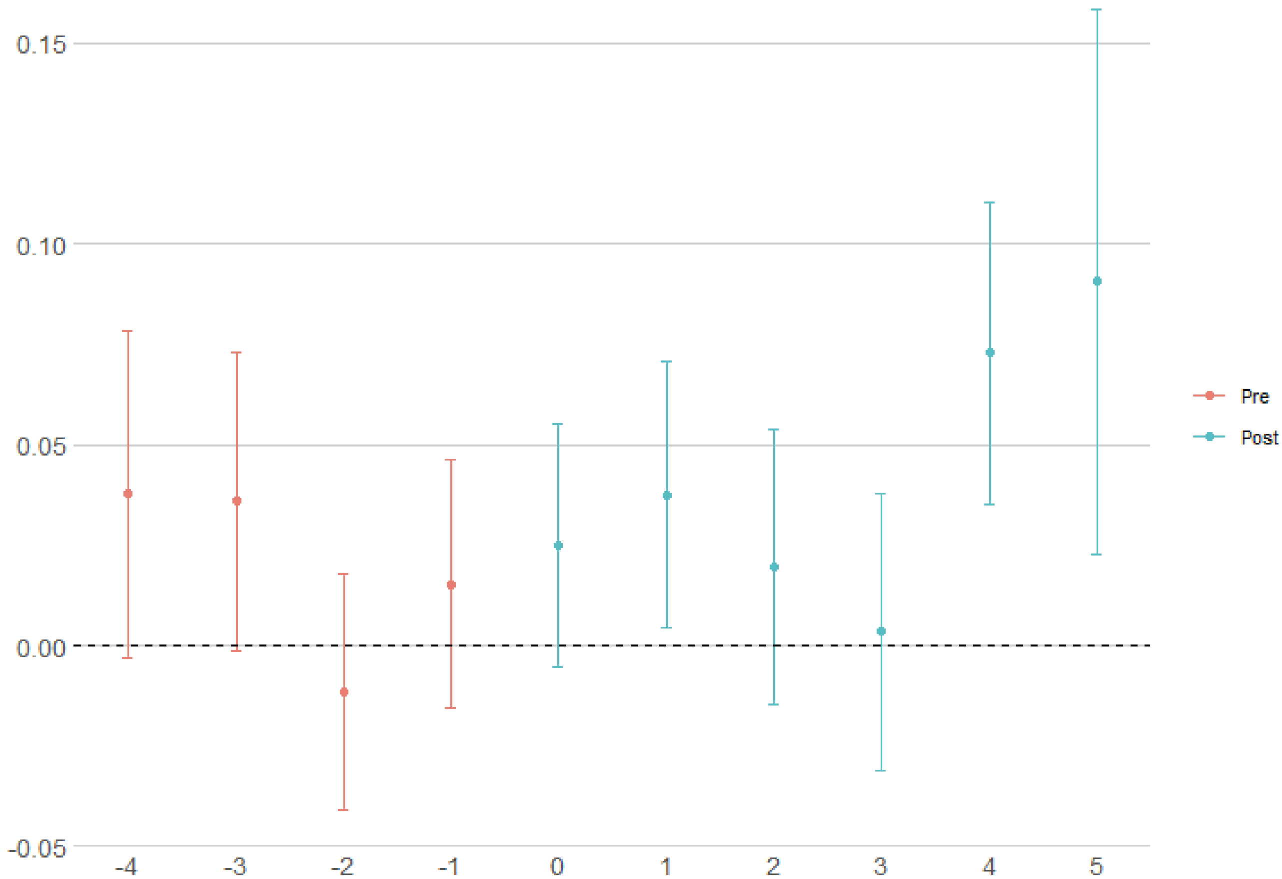


Figure A6: Differences in housing prices between treated and control neighborhoods accounting for staggered treatment

This figure presents the coefficients of a regression of house prices between treatment and and the year in relation to institutional mergers. House prices are measured via hedonic regression as described in Appendix C.2. A zip is treated if acquirers gained at least 1% market share in the zip n as a result of mergers. Estimates are measured via Callaway and Sant'Anna (2021) to account for biases due to differential timing. The vertical axis represents the difference between treated and control zips in terms of the logarithm of the respective house and rental price indexes. Each estimate is presented with 95% confidence intervals.

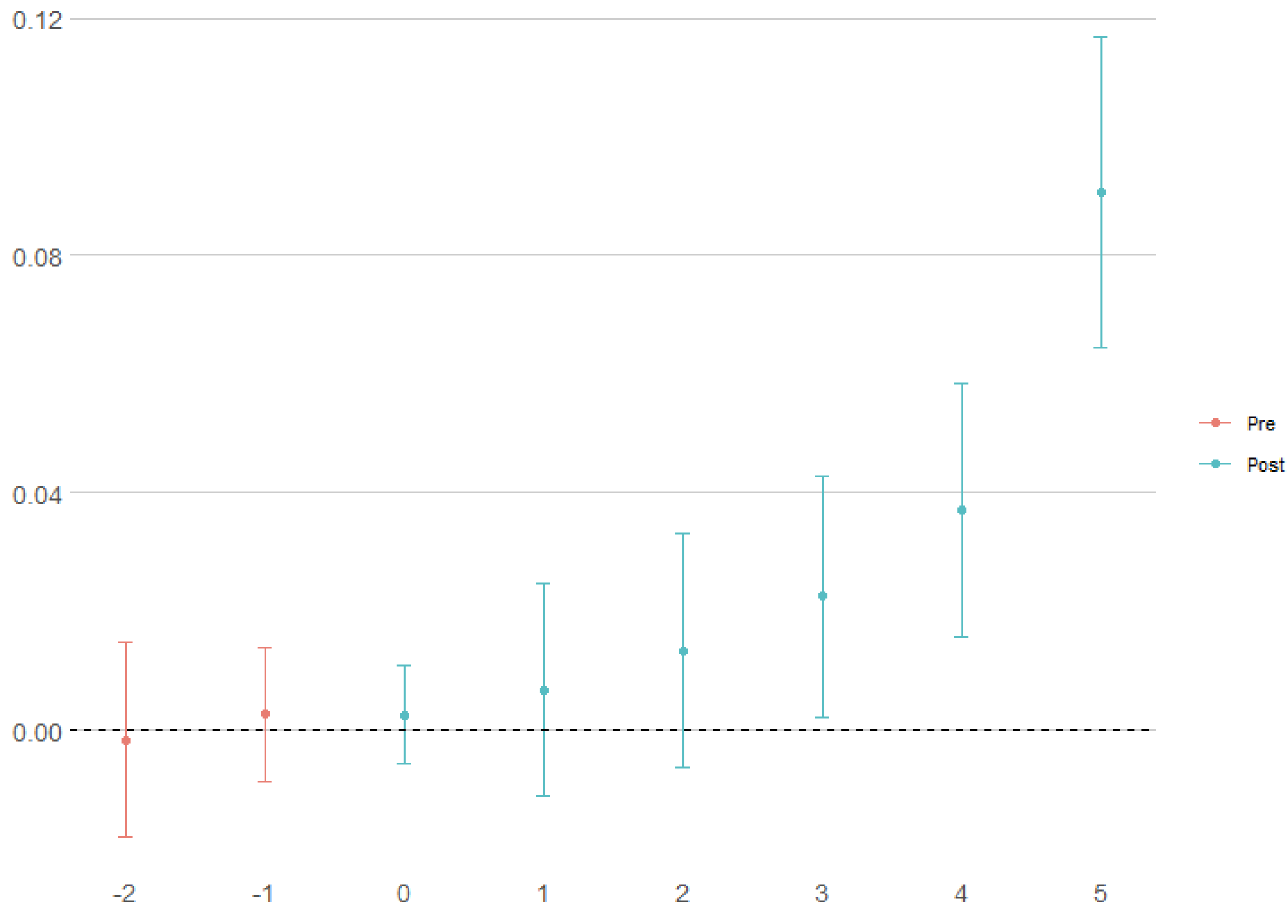


Figure A7: Differences in rental prices between treated and control neighborhoods accounting for staggered treatment

This figure presents the coefficients of a regression of rental prices between treatment and and the year in relation to institutional mergers. Rental prices are taken from Zillow (ZORI). A zip is treated if acquirers gained at least 1% market share in the zip n as a result of mergers. Estimates are measured via Callaway and Sant'Anna (2021) to account for biases due to differential timing. The vertical axis represents the difference between treated and control zips in terms of the logarithm of the respective house and rental price indexes. Each estimate is presented with 95% confidence intervals.

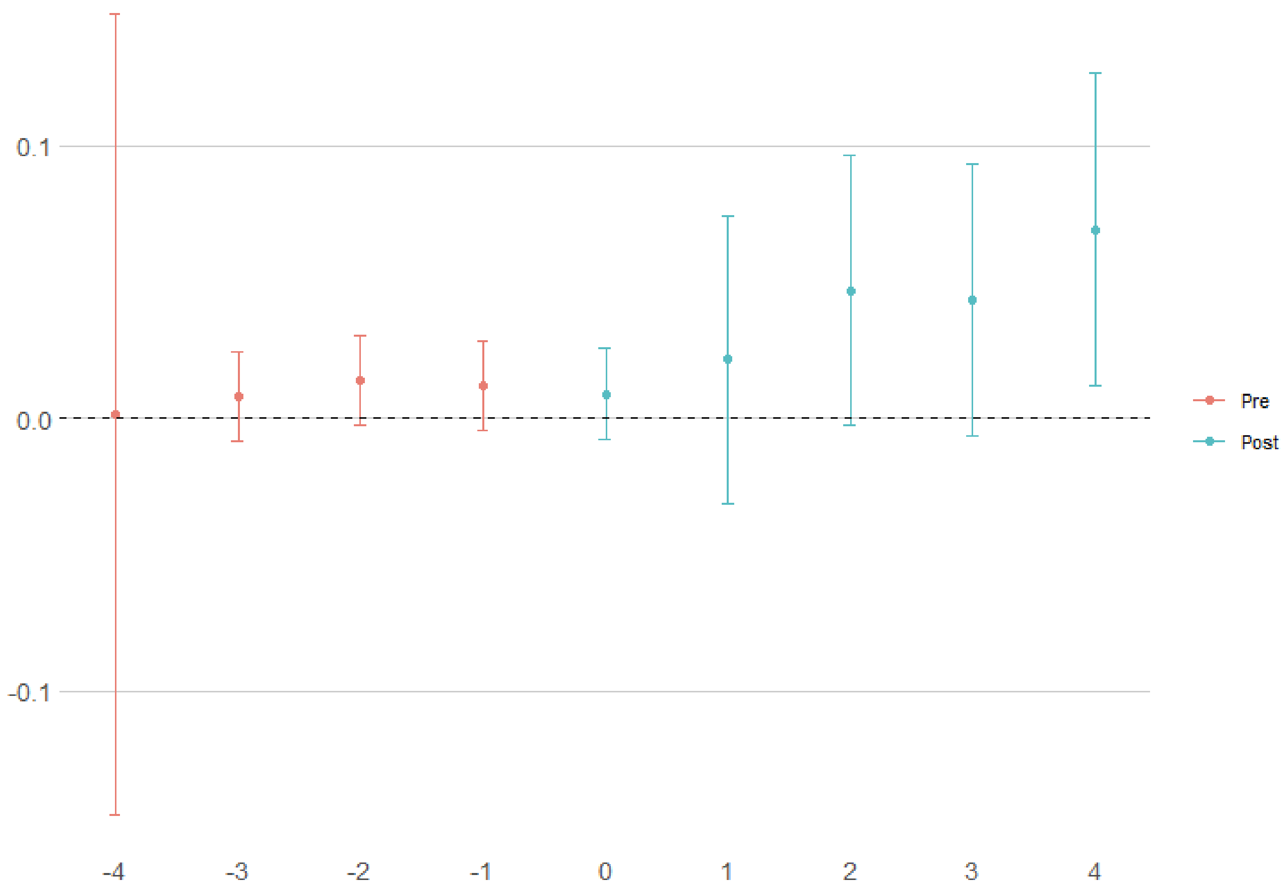


Figure A8: Differences in nontradable small business registrations between treated and control neighborhoods accounting for staggered treatment

This figure presents the coefficients of a poisson regression of small business registration numbers amongst nontradable sectors between treatment and and the year in relation to institutional mergers. A business is defined as small if it has less than four employees. Treatment effects are estimated according to Callaway and Sant'Anna (2021) to account for staggered treatment. The horizontal axis shows four years prior to the merger to four years after the merger. The vertical axis represents the difference between treated and control zips in terms of the number of small businesses registered between treated and control zips. Each estimate is presented with 95% confidence intervals.

Appendix C. Data Construction

C.1 Institutional Investor Identification

Property transaction data obtained from county tax assessor offices details the grantee name for almost every transaction. This data can be used to identify whether a property is owned by an institutional investor, as well as the exact institutional investor along with transfers of ownership between investors over time. However, there are several challenges associated with identifying institutional ownership accurately due to issues with ultimate ownership and recording mistakes made by government officials. For instance, large institutional investors often purchase properties through various subsidiaries with locally registered limited liability companies (LLCs), which can make it difficult to trace the ultimate owner of the property. For example, Invitation Homes lists over 185 subsidiaries in their 2021 SEC 10-K filing. Some subsidiaries can easily be traced back to Invitation Homes, such as "2018-3 IH Borrower L.P.", while others such as "Adalwin LLC" are harder to trace back to Invitation Homes without ownership data. Moreover, errors in data entry and spelling mistakes can further complicate the process of identifying institutional ownership. In order to correctly observe institutional ownership and consolidation, it is imperative to trace these LLCs back to their ultimate owner. In this Appendix, I detail the process I use in this paper to identify institutional investors.

First, I identify a list of the 23 most active institutional investors from 2012 to 2022. The list includes:

- Altisource Residential, a.k.a. Front Yard Residential Corporation
- American Homes 4 Rent
- American Residential Properties
- Colony American Homes
- Invitation Homes
- Silver Bay Realty Trust Corp
- Starwood Waypoint
- Tricon American Homes
- Vinebrook Homes Trust

- Progress Residential
- Cerberus Capital Management
- Home Partners of America
- Connorex-Lucinda
- Gorelick Brothers Capital
- Camillo Properties
- Lafayette Real Estate
- Golden Tree
- Havenbrook Homes
- Prager Property Management
- Reven Housing REIT
- Transcendent
- Broadtree
- Waypoint Homes (later renamed to Starwood Waypoint)

For the first 9 institutional investors that are/were publically listed, I extract the names of their subsidiaries from Exhibit 21.1 from SEC 10-K filings, matching each subsidiary to the ultimate parent by name and year. The importance of matching by name and year is that subsidiaries rarely change names after a merger. For example, both "SWAY 2014-01 Borrower, LLC" and "Fetlar, LLC" used to belong to Starwood Waypoint, but then transferred ownership to Colony Starwood after a merger, and then finally to Invitation Homes after a second merger. By matching subsidiaries to their ultimate parent by year, this approach achieves a comprehensive and accurate tracking of institutional ownership over time.

However, as private firms, the remaining institutional investors do not release subsidiary details. I thus identify subsidiaries of private firms by starting from the recorder transaction data. First, I filter out transactions that are not for residential housing, not recorded as arms-length transaction or having transferred ownership to an individual. Next, I filter out grantee names based on keywords detailed in Table A9 that identify grantees that belong to government agencies, nonprofits, banks and other lending institutions, construction firms, financial institutions, and other organizations that identify organizations as a non-institutional investor. Once I have applied all of the previous filters, I then select grantee name-address pairs that are recorded to have undertaken at least 100 transactions in a single year as the basis for all possible subsidiary names.

I first match subsidiary names for the public institutional investors, utilizing the Jaro-Winkler similarity measure between 10-K subisidiary and recorder grantee names to achieve the most accurate matches betweeen names. Fuzzy matching using the Jaro-Winkler algorithm is used to account for frequent recording mistakes by tax assessor offices. For private subsidiaries, I first match based on having an exact match between the name of the ultimate parent, or on the name of known subsidiaries. For example, Cerberus Capital Management is known to purchase properties under FirstKey Homes, or under subsidiaries with names beginning with "CSMA". Next, I use the home addresses of private institutional investors to find subsidiaries with the same address as the ultimate parent.

Once I obtain these matches, I then manually check the quality of each match by hand, using OpenCorporates to assist in matching subsidiaries to the ultimate owner. OpenCorporates is a website that contains the registration records of over 200 million companies worldwide, including registered address, agent name, agent address, home company name, home company address, controlling entity name, and controlling entity address. I utilize Opencorporates to check whether a subsidiary has been properly matched to the ultimate parent based on name, address, and listed directors. Many institutional investors list executive officers as directors for their subsidiaries. For example, Brian Buffington, the CFO of Progress Residential, is listed as a director for many subsidiaries such as "SFR Investments V Borrower 1 LLC". As an additional check to ensure as comprehensive a match as possible, I also search the Florida Division of Corporations website for possible matches. Since Florida is unique in providing a search for corporations based on address alone, I am able to identify additional subsidiaries that would otherwise be missing from OpenCorporates. In total, I get 10,658 subsidiary name-address matches from this first step. The number of matches is primarily due to idiosyncrasies in tax assessor data collection, such as spelling mistakes and address formatting, meaning that one subsidiary can have multiple name-address matches.

Once all matches are verified, I then take the addresses of all matched subsidiaries, and then find the names of all subsidiaries with the same addresses belonging to arms-length transactions for residential housing transferring ownership to non-individual grantees, again filtered based on keywords from Table A9. Similar to the previous step, I then use 10-K Exhibit 21.1 filings, OpenCorporates and the Florida Division of Corporations to manually trace each match back to the ultimate owner. I then take the names of the matched subsidiaries from this step, and repeat the process of matching based on name and address and manually verifying each match, until I get 0 additional matches from each step. In total, I am able to match 34,884 name-address pairs to institutional investors, which I then use identify transactions in the recorder data that are undertaken by a specific institutional investor. Utilizing this method, I am able to identify properties that belong to institutional investors at the end of each year from 2012 to 2022, identifying over 360,000 properties in 2022 that are owned by institutional investors.

C.2 Housing Price Index Construction

I construct a hedonic house price index at the zipcode level using ATTOM transaction data from 2007 to 2022. I restrict the sample to arms-length transactions of single-family homes, condominiums, townhouses, and co-ops, removing transactions that have \$0 recorded as the transaction amount, and winsorizing at the 99th percentile. Sale prices are normalized to 2007 real dollars. I match transaction data with tax assessor property records to directly control for housing characteristics. For each zipcode z, I construct $HPI_{z,t}$ using the following hedonic regression specification:

$$log(Price)_{h,z,t} = HPI_{z,t}Year_t + \beta X_{h,t} + \epsilon_{h,z,t} \tag{14}$$

$log(Price)_{h,z,t}$ equals the logarithm of the transaction amount for house h in zipcode z in year t. $X_{h,t}$ are the property characteristics of house t including building age, age squared, number of bathrooms, number of bedrooms, number of rooms, building area, living area size, and indicators for the presence of an attic, basement, pool, porch, patio, and deck. $Year_t$ is an indicator variable for year t. The estimates $HPI_{z,t}$ provides the normalized house price index of zipcode z in year t, giving me a zipcode by year panel dataset of house price indices.