The Erosion of Homeownership and Minority Wealth*

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Abstract

Since the Great Recession, the traditional path to wealth creation through home ownership has stalled and worsened for many minority households. One potential and largely unexplored driver of this trend is the growing presence of institutional investors that purchase single-family homes and convert them to permanent rentals. We find that large institutional investors alone have decreased homeownership rates in Black neighborhoods in high growth southern cities like Charlotte, North Carolina by 4 percentage points. Using a granular spatial difference-in-differences estimator, we show that an institutional investor purchase leads to a 2% decline in neighboring property values. This effect is almost exclusively limited to majority Black suburban neighborhoods. These property value declines are also associated with commonly hypothesized social spillovers from the loss of homeownership, namely increases in crime and decreases in property maintenance and political participation.

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1 Introduction

Two concerning trends for minority households have developed over the last decade. First, the century long progress in decreasing the wealth gap between Black and White Americans has stalled, and since the Great Recession, it has even grown among middle income Americans (Kochhar and Cilluffo, 2017; Derenoncourt et al., 2022). Second, homeownership rates have declined during this time period, but have fallen faster for Black families (Haughwout et al., 2020). Since home equity is an important source of wealth for most American households, there is a direct link between homeownership and the racial wealth gap (Taylor et al., 2011). Further compounding this loss of wealth is the consistent finding that Black homeowners receive lower returns on their housing investment than White homeowners (Mayock and Malacrida, 2018).

One potential and largely unexplored contributor to these racial gaps in homeownership and wealth is the role of institutional investors purchasing single-family homes and converting them to permanent rentals. This trend began in the wake of the Great Recession with a large stock of foreclosures but it has expanded to non-foreclosures and is fueled by the securitization of income producing properties into Real Estate Investment Trusts (REITs).[1] Institutional investors tend to target single-family suburban homes built in the 1990s and 2000s. This investment strategy primarily affects Black households, as they moved to these neighborhoods when older housing stock and centrally located areas became the focus of gentrification (Frey, 2018; Bartik and Mast, 2022; Ferreira et al., 2023).

In this paper, we highlight the role of institutional investors in the last decade, with a focus on neighborhood spillovers and racial disparities in investment activity. Beyond descriptive evidence, we provide causal estimates of both how an institutional home purchase impacts neighboring property prices and the cause of these impacts by incorporating microdata on transactions, home maintenance, crime reports, building code violations, and voting records. Prices help quantify the changes in home equity, while non-price outcomes elucidate the mechanisms through which the conversion of properties to rentals impacts these prices.

Our analysis combines rich property- and neighborhood-level data with a method to determine institutional investors created and validated by Tyler Dukes and Rago (2022); this allows us to identify ownership even when institutional investors attempt to obfuscate it through subsidiaries and corporate holding companies. To generate casual estimates of price effects on neighboring properties, we follow a number of papers in the foreclosure contagion literature that incorporate granular definitions of treated and control areas around the focal property (e.g., Harding et al. (2009); Campbell et al. (2011)). Given the spatial pattern of institutional investing and the scale of spillovers highlighted in our results, we limit our analysis to transacted sales that are within 500 feet of an institutional investor purchase and compare them to neighbors that are slightly further away. This research design limits concerns about any unobserved neighborhood attributes confuding our results as well as controls for any larger market demand effects from institutional investors. Subsequently, we apply this difference-in-differences (DID) model to a number of other outcomes typically associated with spillovers of homeownership,

[1] Institutional investors focus on immediately converting and occupying the home purchased with a renter to help securitize the income flows from the property. This is distinct from local "flippers" who exhibit significant heterogeneity in if and when they convert a single-family home to a rental property.

namely building permit activity, building code violations, reported crimes, and political participation. Lastly, we examine larger neighborhood dynamics with alternative definitions of a community.

We first document that 7% of all single-family homes sold between 2011 and 2021 in Mecklenburg County, North Carolina were purchased by institutional investors. If one focuses on neighborhoods targeted by these firms at any point during our study period, this becomes almost 12% of all homes sold. 77% of these transactions occur in neighborhoods that have an above median share of Black residents. Furthermore, these transactions lowered overall homeownership rates for existing homes by 2 percentage points in the county and 4 percentage points in Black neighborhoods specifically, which is a decrease that approaches the overall loss in homeownership due to the Great Recession. Focusing on what happens to the individual properties targeted by institutional investors, we find that over 80% become and remain non-owner occupied, which is consistent with a larger business plan of securitizing longer-term rental income. After a home is purchased by an institutional investor, the household is also less likely to vote or be issued a building permit and more likely to receive a nuisance code violation, all relative to other homes purchased in the same time period and neighborhood.

In terms of spillovers to neighboring property values, we find that transacted homes within 250 feet of an institutional investor purchase saw a price decline of 2%, while homes between 250 and 500 feet saw a price decline of 1%; both of these effects are relative to transacted homes located between 500 and 1,000 feet of the investor purchase. These price declines are limited to Black neighborhoods and stronger for institutional investor purchases made later in our study period. In order to examine if these price declines are unique to institutional investors or are a feature of any type of investor who becomes a landlord, we create a sample of local investors who make likely rental conversions. These local landlords generate smaller imprecise negative price effects, which suggests a role for both rental conversions and institutional investors in our main effects. We also find limited evidence to support the notion that Homeowners Associations (HOAs) are able to limit the presence of institutional investors or mitigate their price impacts. Lastly, our results are robust to different definitions of a neighborhood, including census blocks, neighborhoods defined by local stakeholders, and alternative distance bands around the institutional investor purchase, as well as to a repeat sales model and various assumptions on geographic and property controls.

Directly examining the social spillovers shows impacts on reported crime, building permits, code violations, and rentals in the neighborhood. More specifically, and consistent with negative spillovers from the loss of homeownership, we see increases in property crime (per parcel) of 2%, increases in violent crime of 3%, and increases in drug crime of 9%. We also see a 39% decrease in building permits and an 8% increase in nuisance code violations. We further find decreases in neighbor's owner-occupancy rates consistent with a 2% increase in rental properties. Lastly, we test for the role of political participation and we find a 1% decrease in the number of registered voters.

This paper contributes to several literatures. First, our results speak to a broader literature on the social benefits of homeownership. Beyond the ethos of homeownership being part of the "American Dream," policymakers, scholars, and citizens tout a number of potential benefits

that extend beyond the homeowner, including property maintenance and renovations (Coulson and Li, 2013; Harding et al., 2000), political participation (DiPasquale and Glaeser, 1999; Fischel, 2002), crime reductions (Disney et al., 2020), and cognitive and behavioral benefits for children (Haurin et al., 2002). As such, the US has provided significant financial incentives to promote homeownership, and studies show a positive relationship between homeownership and neighborhood prices (Coulson and Li, 2013) and various measures of social capital (DiPasquale and Glaeser, 1999). More recent work provides estimates of the social benefits of homeownership using policy experiments, where public or subsidized housing residents in the US (Engelhardt et al., 2010), UK (Disney et al., 2020), and Israel (Hausman et al., 2022) are given discounted purchases prices or subsidized downpayments to purchase their current rental units. In contrast to this existing literature, we study the opposite phenomenon (the loss of homeownership) as well as offer new evidence on the larger social benefits of homeownership in a context where ownership is commonly attained in the US, namely among middle income households and entry-level suburban housing.

Second, a related body of work examines foreclosures to better understand how shocks are spatially transmitted through the housing market. This literature provides strong evidence that foreclosures are associated with price declines and that there are two main sources of effects from foreclosure spillovers (Campbell et al., 2011). First, scholars find evidence of neighborhood disamenities that are a result of the foreclosure process which leads to under-maintenance and vacancy (Harding et al., 2009; Gerardi et al., 2015). Second, Anenberg and Kung (2014) highlight competition effects, whereby the increased supply of foreclosed properties may be depressing transacted prices. This work is consistent with a small spatial scale upon which these neighborhood disamenities operate, typically less than 1,000 feet. However, it also highlights the potential for competition from institutional investors that would, if anything, increase the prices of neighboring properties in the current context.

Lastly, we contribute to the literature on institutional investors by focusing on individual property impacts and social spillovers in neighborhoods instead of overall market effects. We also examine a long time horizon that includes more recent investor activity, as opposed to only the time period immediately following the Great Recession. This previous work has found mixed results on prices. For example, Brunson and Buttimer Jr (2020) and Smith and Liu (2020) find that private equity firms acquire houses at a discount, which potentially carries a negative signal to other smaller-scale investors and individual buyers, while other studies, such as Mills et al. (2019) and Ganduri et al. (2023), report that institutional investors help boost surrounding housing prices. The presence of rentals in traditionally ownership neighborhoods may also have benefits in accessing higher quality schools and other neighborhood amenities for households whom ownership is unaffordable or undesirable (Mayock and Vosters, 2022; Ihlanfeldt and Yang, 2021).

Our results have important policy implications. The focus of institutional investors on minority neighborhoods and the potential for HOAs to mitigate their negative effects highlights the importance of local governance. For example, limiting negative spillovers onto minority neighborhoods may require stronger neighborhood organizations to thwart institutional investors from converting these homes to rentals and eroding home equity. Some HOAs have recently

implemented policies that limit the ability of homebuyers to convert single-family homes by requiring a waiting period before a purchased home can become a rental (Dukes et al., 2022). Furthermore, a number of states have debated restrictions on the ability of corporate entities to purchase and permanently rent single-family housing units.[2] Lastly, Congress recently proposed *H.R.9246 - Stop Wall Street Landlords Act of 2022*, which denies certain tax benefits to large investment firms that have asset investments of over $100 million in single-family homes in a transaction year.[3] More broadly, our results suggest that homeownership is still important, as it generates positive social spillovers to neighborhoods, and historical efforts to broaden homeownership through cheaper financing and tax advantages for home purchases may be beneficial.

2 Background and Identifying Institutional Investors

After the housing market collapse during the Great Recession, institutional investors bought up many single-family homes as part of a new real estate investment strategy. Instead of flipping these properties, investors held onto them, making money from rental payments and fees and securitizing the rental income flows into Real Estate Investment Trusts (REITs). These REITs provided a new source of funding for institutional investors to raise even more funds to expand their purchasing activity. While the true extent of corporate investment in single-family homes is not entirely known, the beginning of this trend started with the purchase of single-family homes by large "buy-to-rent" private and publicly held firms, with a focus on foreclosures (Mills et al., 2019). This investment strategy has expanded to market rate properties and across a number of cities and neighborhoods in the last decade. Recent work by academics (Charles, 2020; An, 2023a) and journalists (Tyler Dukes and Rago, 2022) highlight the experience of a few cities where corporate buying has grown exponentially in recent years. However, direct evidence on the scope and presence of these firms is obfuscated by the complex ownership entities used to purchase homes, as it is often very difficult to determine the precise owner of a given property.

These institutional investors benefited from an ample supply of single-family homes in the market, tightened credit access for individual homebuyers, and a decrease in overhead costs stemming from high-tech property management and purchasing strategies (An, 2023b). In general, these firms have two broad principles when purchasing properties. First, they regularly target specific zip codes and buy properties when they become available using all cash and quick closings. Second, these firms tend to operate in the central price and size ranges of the market, purchasing properties closer to starter-home types rather than luxury style homes (Mills et al., 2019). For example, from Tyler Dukes and Rago (2022) describing North Carolina, "Investors were typically buying homes in the $200,000 to $400,000 range, where first-time homebuyers typically look...It tends to hurt the competition [for buying homes] because these are cash offers and it doesn't involve building wealth." Additionally, institutional investors often

[2]The following quote from Tyler Dukes and Rago (2022) highlights the tension between neighborhood residents and rental home conversions by larger companies: "Since 2017, the state attorney general and the Real Estate Commission have received at least 80 consumer complaints against large corporate landlords in the state [of North Carolina], including Harrison's, according to documents obtained through public records requests. Of those, 29 complaints were made against American Homes 4 Rent, 23 against Progress Residential, 15 against Invitation Homes, seven against Conrex Property Management and 6 against FirstKey Homes."

[3]See here for details on *H.R.9246 - Stop Wall Street Landlords Act of 2022*.

purchase based on specific neighborhood conditions, such as lower value homes, higher share of non-Hispanic Black, lower poverty rate, and higher share of foreclosed properties in 2008, all compared to the metro average (Charles, 2020).

In this project, we utilize data on institutional investor purchases in North Carolina to examine the impacts of large-scale and rapid changes in home ownership. This data is part of an investigative journalist series by Tyler Dukes and Rago (2022) and is supplemented with ownership records from County Tax Assessors. Specifically, to determine which property was purchased by an institutional investor, property records were first manually searched to create a database of parent companies and their most common subsidiaries. This process was then augmented within a machine learning model to find other name variations. Data from the UNC-Charlotte Urban Institute was used to add more names, as was North Carolina court data on eviction proceedings, state utility data, and corporate registration data from OpenCorporates. The Anti-Corruption Data Collective supplied additional property transaction data through a research agreement with Zillow. Almost all subsidiaries identified were matched to corporate registrations filed with the N.C. Secretary of State's Office to verify connections with their parent companies. The list of investor subsidiaries was then matched with property owner names recorded by North Carolina OneMap. Properties were removed if their zoning, land use, or building description did not align with the definition of a single-family home from the Census.

Given the numerous title changes that accompany a home purchase by an institutional investor, we assign the timing of the property purchase event based on the first transaction when the annual snapshots of county assessor records indicate the ownership changed to an institutional investor. We focus on non-zero transactions in the main analysis, but we include transactions with a zero price in the appendix and the results are similar. Companies with fewer than 100 properties in their portfolios were not included in the main analysis in order to focus on large scale or non-local investors, but the results are similar if we include smaller investors, as they represent such a small share of overall activity. Appendix Figure A.1 highlights the complex corporate structure of the institutional investors and that it has grown over time, with the typical institution using on average 22 subsidiaries to purchase homes.

Due to the complexity of identifying these institutional investors across states and the additional data requirements for our outcomes, we focus on one of the most active counties for this investment activity, Mecklenburg County, North Carolina, which saw over 9000 homes purchased by institutional investors between 2011 and 2021; this represents 7% of all single-family homes sold in the county during this time period. If one focuses on neighborhoods targeted by these firms at any point between 2011 and 2021, this becomes almost 12% of all homes sold. 77% of these transactions occur in census block groups that have an above median share of Black residents. Furthermore, these transactions lowered overall homeownership rates by almost 2 percentage points in the county. Figure 1 highlights that most homebuying occurred in the suburban portions of the county in the north, southeast, and southwest, which are predominantly Black neighborhoods with single-family, owner-occupied housing. Note that there is limited

investment activity in the wealthier and White southern parts of the county.[4] Figure 2 further highlights that investors often target neighborhoods with a higher share of Black residents.

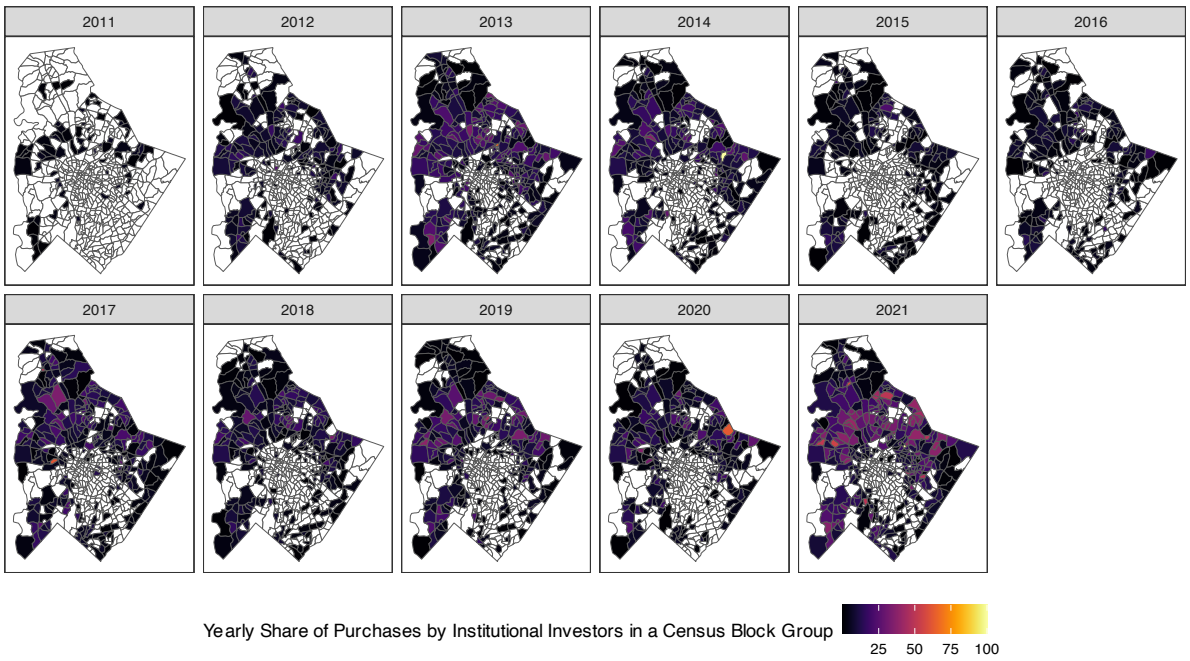


Figure 1: Share of Yearly Purchases by Institutional Investors in Mecklenburg County, NC

Notes: This figure matches the location of properties that were purchased by institutional investors to their respective census block groups. This is then aggregated to yearly shares of total transactions in each census block group that are attributed to investors. Lighter colors have a higher share of institutional activity in that year, and white census block groups have no institutional investor purchases. Figure A.2 repeats this exercise but with the yearly count of institutional investor purchases in a census block group.

To highlight the scale of institutional investor activity, Figure 3 shows the number and share of total purchases by investors during our study period. They became quite active in 2013, purchasing almost 17% of all transacted homes in neighborhoods ever targeted by investors, and more recent years have seen this activity continue to expand. To get a sense of the institutions behind these investments, Appendix Figure A.6 shows the yearly number of purchases by individual investors, where the market share is concentrated among six large public companies and private equity organizations such as Invitations Homes, American Homes 4 Rent, and Progress Residential; these companies have a presence in a number of US cities.[5] In terms of legal ownership, Appendix Figure A.7 shows how the owner type changes by event time relative to the year of purchase, where there is a large shift in ownership from individuals to non-North Carolina corporations. Lastly, the general trends towards rental units is confirmed in Appendix Figure A.8, which shows increases in non owner-occupied single-family homes from 2011 to 2021.

[4] Appendix Figure A.3 shows the share of Black residents by census geography. Appendix Figure A.4 shows the distribution of house vintage, where the majority of homes were built in the early 2000s; this makes Charlotte a desirable location for institutional investment.

[5] Websites for American Home 4 Rent (https://www.amh.com/communities) and Invitation Homes (https://www.invitationhomes.com/) highlight that they currently manage rental properties across 15 states.

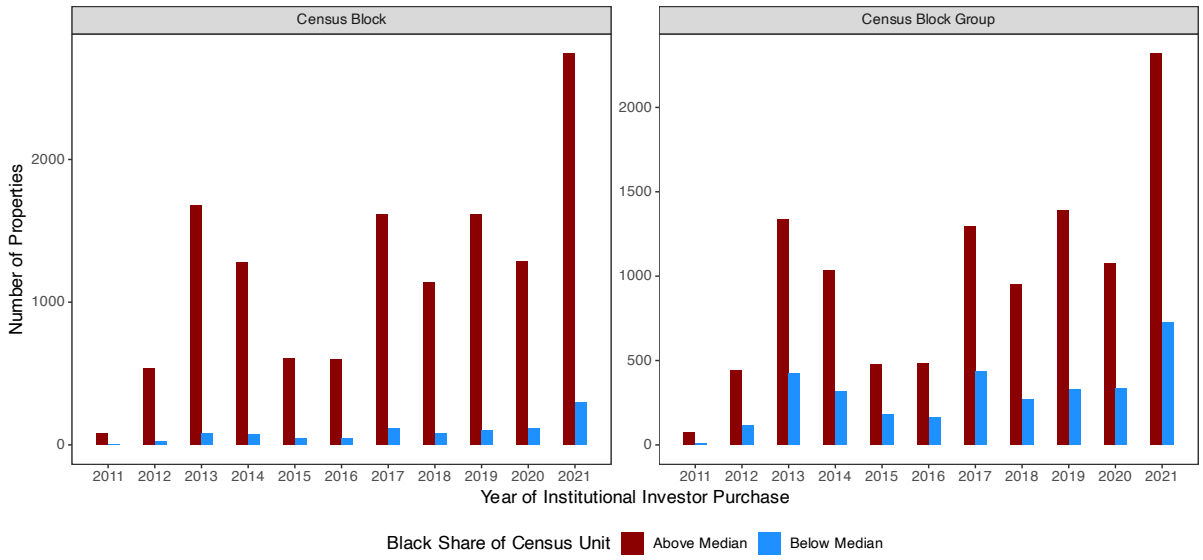


Figure 2: Institutional Investor Purchases by Year and Black Share of Census Geography

Notes: Each bar captures the total number of institutional investor transactions by year and above/below median share of residents that are Black. Figure A.5 repeats the exercise using shares of total transactions.

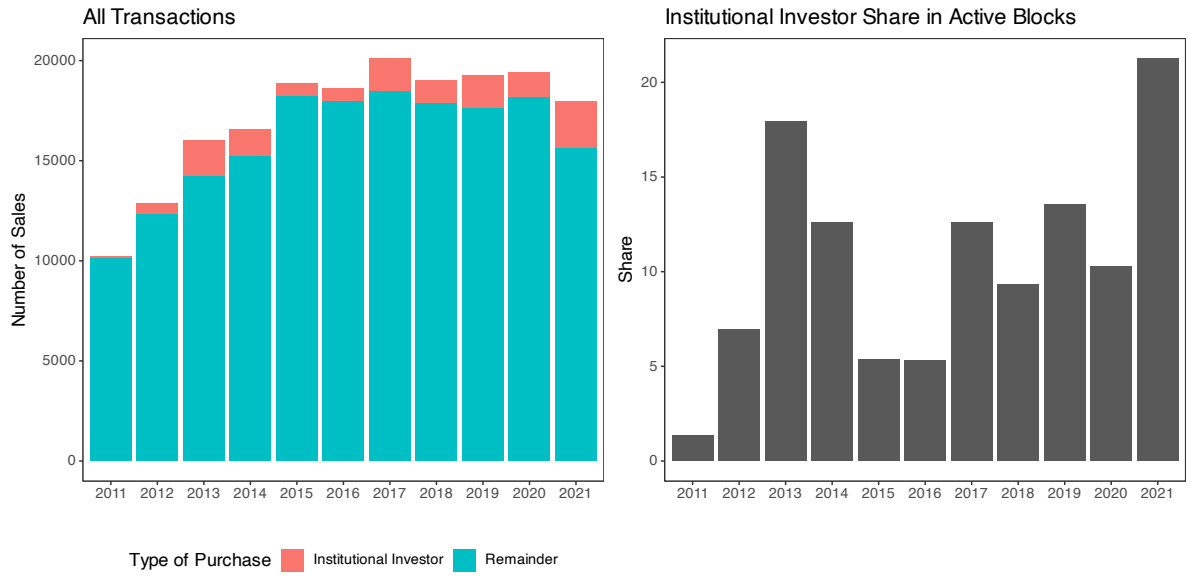


Figure 3: Total Sales by Type and Year

Notes: The left panel captures the total number of transactions where a new owner moves into the property in a given year, broken down by whether it was a standard transaction or a parcel purchased by an institutional investor. The right panel only looks at census blocks with at least one institutional purchase during our sample period and examines what share of yearly transactions were attributed to institutional investors.

3 Data

In order to examine the impacts of institutional investor purchasing activity, we incorporate a number of local government administrative records. Specifically, we combine information from annual snapshots of county tax assessor records to highlight the property attributes of each transacted home over our study period of 2011 to 2021; this also contains the transaction history of each property. We augment these records with demographic data from the census at the block-level and neighborhood characteristics at the block group-level. We also create a unique variable for the presence of an HOA in a given census block using methodology from Clarke and Freedman (2019). Their approach is based on mortgage indicators for the presence of an HOA and allows for different thresholds in defining HOA presence. We classify an area to be managed by an HOA if the majority of home purchases with mortgages in a census block contain an indicator for an HOA.

We then estimate a linear probability model based on a sample of all arm's length single-family home transactions during our sample period to explore what house attributes predict institutional investor purchases. Table 3 provides a series of estimates with increasingly granular geographical fixed effects, where the dependent variable is equal to one when the home is purchased by an institutional investor. The results highlight some of the same property attributes found in the existing literature to predict institutional investor homebuying, such as a preference for newer and smaller homes. One important property attribute to consider is the quality of the home, which is provided by dummies for different building quality used by the county assessor. Unlike some jurisdictions, this variable is well populated and varies across 43 unique grades that we aggregate to Below Average, Average, and Very Good/Excellent. We omit Below Average and show that homes purchased by institutional investors are more likely to be of high quality. This result is important because buy-to-rent investors in our sample and time periods are targeting higher quality homes, which is quite different than the foreclosure buying strategy exhibited by institutional investors in the years immediately following the Great Recession. We also show that census blocks with a higher share of residents that are Black see more institutional investor activity. Finally, we highlight that homes located in an HOA are less likely to be purchased by institutional investors, which is consistent with descriptive evidence that HOAs have more recently begun to enact policies to limit quick rental conversion.

Table 2 separately compares attributes of census blocks and individual properties that ever had an institutional homebuyer with ones that did not. Panel A shows that census blocks with institutional investors are poorer and have a higher share of Black households. Rent and population are quite similar, but blocks with institutional investors have considerably more transactions. Panel B shows that institutional investor purchases are newer and smaller and transact at lower average prices.

We later examine how institutional investors impact neighboring home prices and a number of outcomes commonly associated with homeownership.[6] Specifically, we incorporate property-level data on home prices, owner-occupied homes, building permits, code violations, reported

[6]Policymakers, scholars and citizens tout a number of potential benefits that extend beyond the homeowner, including property maintenance and renovations (Coulson and Li, 2013; Harding et al., 2000), political participation (DiPasquale and Glaeser, 1999; Fischel, 2002) and less criminal activity (Disney et al., 2020).

Table 1: Which Property and Neighborhood Attributes Predict Institutional Purchases?

	Geo Covs	Zip FE	Zip-by-YearQt FEs	CBG FE	CB FE	CB-by-YearQt FE
	(1)	(2)	(3)	(4)	(5)	(6)
Built in 1970s	0.003	0.002	0.002	0.0004	0.002	0.004
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)
Built in 1980s	0.018***	0.014***	0.014***	0.015***	0.016***	0.019***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.005)
Built in 1990s	0.041***	0.033***	0.032***	0.030***	0.026***	0.032***
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.006)
Built in 2000s	0.066***	0.058***	0.058***	0.052***	0.046***	0.056***
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.008)
Built in 2010s	−0.021***	−0.032***	−0.032***	−0.031***	−0.024***	−0.010
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.008)
Sqft = [1000,2000)	0.023***	0.014***	0.014***	0.004	0.003	0.003
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.010)
Sqft = [2000,3000)	−0.016***	−0.024***	−0.024***	−0.028***	−0.016***	−0.028***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.011)
Sqft >= 3000	−0.040***	−0.046***	−0.046***	−0.047***	−0.031***	−0.047***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.011)
Bld Quality = Average	0.016***	0.013***	0.013***	0.006*	0.004	0.007
	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.005)
Bld Quality = Good/Excellent	0.010***	0.016***	0.016***	0.008***	0.003	0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)
HOA	−0.028***	−0.020***	−0.020***	−0.008**		
	(0.004)	(0.004)	(0.004)	(0.004)		
Perc. Black in Block	0.130***	0.102***	0.105***	0.097***		
	(0.007)	(0.008)	(0.008)	(0.009)		
Med HH Income (10k)	−0.0001	0.0003	0.0003			
	(0.0003)	(0.0003)	(0.0003)			
Dep. Var Mean	0.058	0.058	0.058	0.058	0.058	0.058
Observations	144,194	144,194	144,194	144,194	144,194	144,194
R^2	0.132	0.138	0.165	0.156	0.213	0.680

Notes: *p<0.1; **p<0.05; ***p<0.01
The outcome variable is an indicator for institutional purchase
Column headings provide geographic FEs and all models include year by quarter of sale FEs
All models include indicators for building type, No. of stories, fireplaces, baths, lot size
Reference category for all variables is oldest, lowest, or worst
Sample limited to those with a sale price of 20,000 to 2,000,000

Table 2: Summary Statistics by Institutional Investor Status

Panel A: Characteristics of Census Blocks

	Ever Institutional Investor (N=2814)		Never Institutional Investor (N=5059)	
	Mean	Std. Dev.	Mean	Std. Dev.
Median household income	62143.3	27754.6	71474.4	41093.6
Percent Black	36.4	26.0	26.6	28.1
Population	2232.0	927.2	1784.5	825.6
Median gross rent	1081.0	322.7	993.1	322.1
Rent burden	31.9	10.1	30.3	10.5
Total sales	55.5	75.2	19.0	26.5
Total institutional investor sales	4.8	8.0	0.0	0.0

Note: N is number of census blocks in each category and outcomes are from the 2013 ACS.

Panel B: Characteristics of Properties

	Ever Institutional Investor (N=14963)		Never Institutional Investor (N=163447)	
	Mean	Std. Dev.	Mean	Std. Dev.
Year built	1999.8	11.5	1990.1	30.7
Sales price	200887.5	595874.7	336560.5	336814.9
Total baths	2.4	0.4	2.5	0.9
Total square footage	1918.6	553.8	2359.5	1093.8

Note: N is number of properties sold in each category between 2011 and 2021.

crimes, and registered voters. Data on prices and owner-occupancy are from the county assessor and we define owner-occupancy based on if the property address is the same as the mailing address of the owner. Building permits and code violations are linkeable to property records based on geographical coordinates, parcel numbers or addresses and made available from Mecklenburg County; our analysis focuses on building permits that are associated with building improvements and maintenance and thus excludes permits for new construction.[7] Our primary code violation of interest is related to nuisance codes, which are the vast majority of code violations and often a result of poorly maintained properties with about 1/3 being initiated by complaints. These violations are widespread in specifics but often involve overgrown lawns, furniture or cars on yard, improper trash storage, or blocking public right-of-ways. Reported crimes are made available by the Charlotte-Mecklenburg Police Department (CMPD) and we classify crimes as property, violent, or drug-related. We spatially assign each crime to its closest parcel for the non-price analysis. Finally, we incorporate publicly available voter registration records from the state of North Carolina. The records provide multiple snapshots per year and we focus on the snapshot made available closest to the November election. These data provide information on residential address, race, political affiliation, age, date registered in NC, and state of birth.

3.1 Impact of Institutional Investor Purchase on an Individual Property

We begin our analysis by examining if and how individual homes targeted by institutional investors change in response to this purchase. We first test if the home stays owner-occupied

[7]This type of construction activity includes work on roofs, HVAC, plumbing, electrical as well as structural additions and any major structural or interior changes to a home including kitchen or bathroom renovations.

after the institutional investor purchase which is consistent with conversion to a permanent rental. We then examine how these properties are maintained and if registered voters are more or less likely to live in these homes after institutional investor purchases.

Formally, we estimate the impact of an institutional investor purchase on own-property outcomes using

$$Y_{it} = \sum_{t=-5, t\neq -1}^{5} \rho_t [\text{Investor Purchase}]_{it} + \gamma_i + \tau_t + \nu_{it}, \tag{1}$$

where Y_{it} is an outcome of interest for housing unit i in year t, event time is measured relative to the year a housing unit i is purchased by an institutional investor, and γ_i and τ_t are housing unit and year fixed effects, respectively. The key coefficients of interest are ρ_t, which capture the year-by-year differences in within-property changes among housing units that experienced an institutional investor purchase compared to never- and not-yet-treated housing units.

The left panel of Figure 4 presents an event study from estimating Equation (1) for whether or not the property remains owner-occupied. We see an initial drop of 50 percentage points in the first year of the institutional investor purchase. This is followed by a total decrease of 80 percentage points in the probability of the house being owner-occupied for the five years after purchase, consistent with a home being converted from an ownership unit to a permanent rental. Note that local home flipping activity would likely see a reversion back to owner-occupied soon after the initial investor purchase. The right panel of Figure 4 provides evidence that properties obtained by institutional investors have tenants that are 30% (0.51/1.7) less likely to be registered voters, which is consistent with lower voter participation by renter households (Hall and Yoder, 2022).

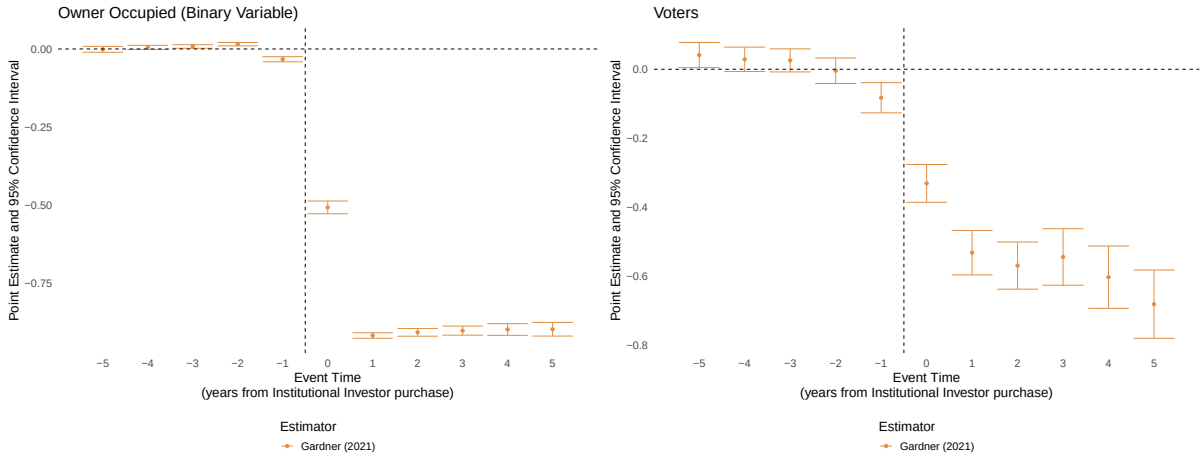


Figure 4: Event Study for Parcel Outcomes after Institutional Purchase

Notes: These event studies are run at the parcel-level, where the outcome is either whether the property had a owner-occupant that year or not in the left panel and the number of registered voters at the parcel in the right panel. Year and parcel fixed effects are included in both models. We limit our sample to properties that are owner-occupied prior to purchase by institutional investors. The mean number of voters per parcel is 1.7.

Figure A.9 again estimates Equation (1) but for building permits and violation outcomes. In the left panel, permits related to property improvements are 62% (0.0037/0.006) less likely

over the entire post period. The right panel of Figure A.9 highlights an outcome that relates to overall property maintenance and adherence to city codes. Specifically, this figure shows that the presence of a nuisance code violation is 91% (0.039/0.043) more likely in the year of acquisition; there is no effect in subsequent years. These results are consistent with the presence of renters who are less politically active and in homes with lower levels of property maintenance after the institutional investor purchase. Furthermore, these results provide evidence that the timing of purchase is unrelated to existing levels of maintenance and political participation.

4 Methods

In order to implement our primary analysis, we need a measure of neighborhood spillovers for a given institutional investor purchase. Related research provides guidance on identifying spatial spillovers in home prices (Harding et al., 2009; Campbell et al., 2011), crime (Ellen et al., 2013; Cui and Walsh, 2015), and health at birth (Currie et al., 2015) and highlights that a spatial DID model using granular measures of geography can generate casual estimates of these spillovers. Consistent with this literature, we focus on a ring analysis around each institutional investor purchase and assign treatment based on proximity to our subject property.

Determining the spatial extent of treated versus control areas is a challenging endeavor. We must first determine an appropriate control group that is equally affected by any neighborhood shocks and represents a set of homes that are part of the same choice set for residents. A valid control group in this context must also limit any influence of unobservables that may vary temporally or not. If one selects control and treatment areas that are at a sufficiently small distance from each other, we can reasonably assume that in the absence of treatment (the institutional investor buying a home and converting it to a rental property), residents are just as likely to select control or treated areas when sorting into neighborhoods. We adopt a similar assumption as Bayer et al. (2008) and Bayer et al. (2022) in that residents can sort on specific neighborhoods and property attributes, but they cannot sort on an individual home given the idiosyncratic nature of when a home becomes available for sale or rent.

Our main empirical model most closely follows Currie et al. (2015) and implements a standard spatial DID strategy where each institutional investor purchase has a set of nearby properties that are considered treated and a set further away that are considered controls. Correspondingly, our data adapts a panel structure where we observe yearly outcomes for a given distance band (treated versus control) around a specific institutional investor transaction. This setup follows a stacked DID approach, which avoids a number of concerns with standard two-way fixed effects DID models, but our results are also robust to various DID estimators (Roth et al., 2023). More formally, we estimate

$$\begin{aligned} Y_{jdt} = \theta_0 + \theta_1 \text{Near}_{jd} + \theta_2 [\text{Investor Purchase}]_{jt} + \\ \theta_3 (\text{Near}_{jd} \times [\text{Investor Purchase}]_{jt}) + \eta_j + \tau_t + \varepsilon_{jdt}, \end{aligned} \tag{2}$$

where Y_{jdt} denotes a given outcome near institutional investor purchase j, within distance group d, in year t. For each investor purchase j, there are two observations per year, where one consists of outcomes "near" the investor purchase, such as within 250 or 500 feet, and

the second observation consists of outcomes within a farther distance band, often 500 to 1,000 feet. The variable Near_{jd} is an indicator equal to one for observations from the near category, regardless of whether the investor purchase has occurred. The variable $[\text{Investor Purchase}]_{jt}$ is an indicator equal to one if an investor purchase occurs in or after year t and is zero otherwise. Note that it is equal to one for both distance groups associated with the investor purchase and drops out due to the year fixed effect. We also include match group (distance bands around each institutional purchase) or match group-by-treatment fixed effects, η_j, to control for all time-invariant determinants of house prices in a given buffer zone, as well as year fixed effects, τ_t, to account for trends in our outcomes over time. Our parameter of interest is θ_3, which captures the differential impact of an investor purchase on nearby properties relative those that are farther away.

Count outcomes (Y_{jdt}) simply reflect the number of outcomes such as building permits, reported crimes, or voters in a given year (t) by distance band (d) around a specific institutional investor purchase (j). For price outcomes, we condition on property attributes first to limit any comparisons between different types of homes sold, but given the focus of institutional investors on suburban homes, our results are not affected by this. However, to be consistent with how we later quantify count outcomes, we take the logarithm of transacted home prices conditioned on year of sale and property attributes and average the residuals to a property sale's respective year (t) by distance band (d) around a specific investor purchase (j).

It is important to discuss how granular we can define the treated and control areas, as there is a tradeoff between providing sufficient power to detect any effects and supporting our primary assumption of parallel trends. Note that given the nature of assigning distances between parcels that are polygons, we introduce some measurement error because we calculate from the centroid of a parcel; this noise limits our ability to analyze very small distances (e.g. 100 feet). On the other hand, when the treated area is sufficiently large, there are two main concerns. First, we want to limit treatment effects from influencing our control areas. This is primarily achieved by expanding our treated area until the average treatment effects begin to fade away; we find that this occurs at approximately 500 feet from the investor purchase. We also implement a number of models with "donut" holes, such as removing properties between 250 and 500 feet and using control properties from 500 to 1,000 feet. Second, we want to limit any effects from other institutional purchases contaminating our results. Appendix Figure A.10 shows that in a given year, the median distance between a given institutional purchase and its nearest neighboring institutional purchase is 513 feet.

The fact that institutional investors purchase homes close together makes sense from an investment and maintenance standpoint, but may be concerning for spatial analysis. However, in practice, neighboring properties that are both purchased by an institutional investor will not be problematic, as the set of neighbors will be the same. The primary concern then is if a property in the treated area is also in the control area for another investor purchase in the same year. This phenomena would mute any results toward zero since the same parcel would contribute to estimates of both the treated and control areas. We examine this in Appendix Figure A.11, which provides the share of all properties in the control and treated areas that are also located in a treated or control ring of another investor purchase in the same year. It shows

that larger treated areas generate more overlapping parcel assignments. More specifically, the smallest treated area contains between 10% and 18% overlap while the largest contains almost 40%. In order to limit overlap, this analysis suggests that the optimal treated areas should extend out to either 250 or 500 feet. Note that any estimated effect sizes could be scaled up by this overlap share to correct for the fact that these overlapping properties are attenuating our effects toward zero.

Given the examination of the spatial distribution between institutional investor purchases above, we limit our analysis to control rings that do not extend more than 1,000 feet from an investor purchase and consider three treatment rings: 1) 0 to 250 feet, 2) 250 to 500 feet, and 3) 0 to 500 feet. However, we adopt a number of robustness checks that vary the treated and control rings in several ways and showcase that the main results are quite consistent. This spatial scale is reasonable, as residents more than one block away are less likely to notice the change in tenants or any change in upkeep associated with an institutional purchase.

In order to test the validity of our spatial DID model, we expand Equation 2 to estimate

$$Y_{jdt} = \alpha + \beta \text{Near}_{jd} + \sum_{t=-5, t\neq -1}^{5} \gamma_t [\text{Investor Purchase}]_{jt} + \sum_{t=-5, t\neq -1}^{5} \delta_t (\text{Near}_{jd} \times [\text{Investor Purchase}]_{jt}) + \eta_j + \tau_t + \varepsilon_{jdt}, \quad (3)$$

where the variables are defined as before, but now there are a series of indicator variables that capture the time period relative to the year of the institutional investor purchase. To validate that our model is not simply capturing neighborhood trends that are reflected in the probability that a home is sold, we later report results from a falsification test where non-investor property transactions are assigned to be investor purchases and find no price effects. Additionally, to address any concerns that the timing of investor purchases reflects general trends in home prices or other neighborhood outcomes, we randomly assign the timing of treatment to our institutional investor properties 1000 times and again find small and insignificant price impacts.

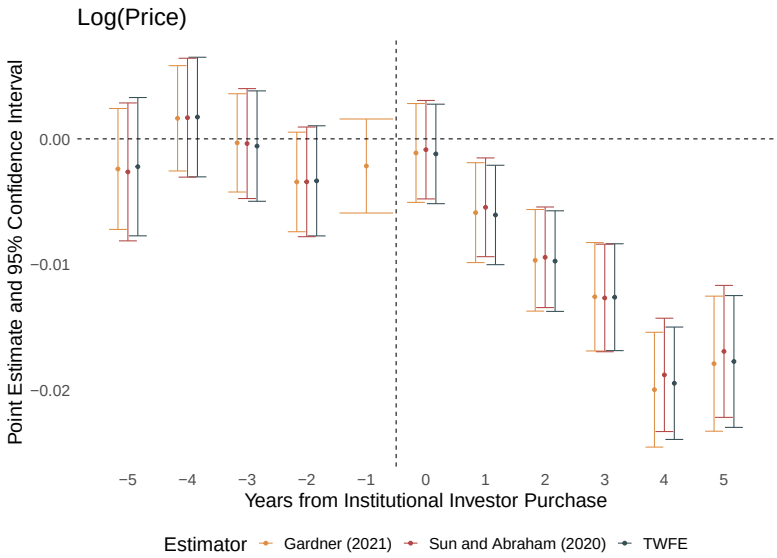
5 Impacts on Neighboring Home Prices

Our primary goal is to understand the price impacts of an institutional investor purchase on nearby properties and we therefore first estimate Equation 3 for the logarithm of the transacted sales price.[8] The results are in Figure 5 and show a significant dynamic decrease in prices for homes within 500 feet of an investor purchase relative to those 500 to 1000 feet from one, regardless of which DID estimator is used; this reaches approximately 2% within five years of the investor purchase. Appendix Figure A.12 shows that prices decline more rapidly for properties closer to the private equity purchase (0 to 250 feet) than those just a little farther away (250 to 500 feet). These event studies also provide support for the identifying assumption that the

[8] As described in Section 4, our main results residualize price by first estimating a standard hedonic model, where we regress the logarithm of the transacted sales price on property attributes and census block group and year fixed effects. This conditional price is largely unnecessary given our focus on suburban homes with homogeneous housing stock, but it does have the potential to generate more precise estimates by limiting outliers.

average untreated potential outcomes for the treated properties in a given time period would have followed parallel trends in outcomes for control properties for all post-treatment periods.

Figure 5: Impact of Institutional Purchases on Prices of Nearby Homes



Notes: To estimate this event study, we first regress the logarithm of transacted price on individual property attributes and year and census block group fixed effects. Residuals from this regression are then averaged by match group and treatment year, and a DID model is estimated with year and match group fixed effects. The treatment group is 0 to 500 feet from an institutional investor purchase, while the control group is 500 to 1000 feet.

We provide a number of robustness checks. First, the top panel of Appendix Figure A.13 shows that our results are not sensitive to the choice of first stage fixed effects. Of note, we include a model with year, census block, and parcel fixed effects, which is equivalent to a repeat sales estimator, and find identical results to our main specification. The bottom panel of Appendix Figure A.13 shows that our results are not sensitive to the choice of second stage fixed effects in Equation 3. Across all these different fixed effects models, we find a lack of pre-trends and similar outcome dynamics. Furthermore, the insensitivity to the choice of fixed effects suggests that our research design is well suited to address concerns of unobservable differences between treated and control areas prior to the investor purchase. We still verify that our choice of distance bands is appropriate to capture the full effects of the investor purchase, limit concerns about unobservables, and limit the extent of overlap between treated areas of one purchase and control areas of another purchase. To do this, we estimate Equation 3 with various distance bands in the top panel of Appendix Figure A.14. This figure highlights that using treated areas that are too small or too large generate invalid results. The bottom panel of Appendix Figure A.14 finds that the results are slightly larger when using a donut design to separate out spillovers from treated areas onto control ones.

To more clearly highlight the magnitude of these results, we estimate Equation 2 in Table 3. Column (1) shows that average decline in price in the post-period is approximately 1% for transacted homes within 500 feet of an institutional investor purchase relative to those 500 to 1000 feet from one. Table A.1 tests if very close neighbors experience larger effects than those slightly further away relative to the same control properties. The results are consistent with a spatial decay of effects, as there is a 1.9% decline in prices within 250 feet of an institutional

purchase and a 1.1% decline between 250 and 500 feet. We then run a subsample analysis, which finds that these negative effects are concentrated among Black neighborhoods (Column (2)); they also experience the most institutional investors purchases, as seen in Figure 2. We also find limited evidence to support the notion that Homeowners Associations (HOAs) are able to broadly limit the presence of institutional investors or mitigate their price impacts (Column (5)). Lastly, when we look at the timing of institutional investor activity, we see the effects are generated by purchases made after 2015 (Column (7)). This is consistent with an expansion of institutional investor activity and may highlight greater concern among residents about the longer-term effects of these purchases.[9]

Table 3: Impact of Institutional Investor Purchase on Prices

	log(Sale Price)						
	Full Sample	Black	White	Not HOA	HOA	2015 and Before	After 2015
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
NearXPost	-0.008^{***}	-0.007^{***}	-0.002	-0.006^{***}	-0.006^{**}	-0.001	-0.014^{***}
	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)
Observations	157,067	118,455	38,732	137,910	19,157	51,833	105,354
R^2	0.226	0.217	0.261	0.220	0.221	0.230	0.214

Notes: *p<0.1; **p<0.05; ***p<0.01
In the first stage, price is regressed on property attributes and
year and census block group fixed effects.
Regression residuals are averaged by match group and treatment year,
then in the second stage, DiD estimation is run with
match group-by-treatment and year fixed effects separately on each subsample.
The treatment group is 0 to 500 feet from an institutional investor purchase,
while the control group is from 500 to 1000 feet.

Appendix Figure A.16 shows that our results are stable whether we include or exclude smaller institutional investors, which is likely due to the fact that over 80% of all institutional investor activity in our sample is by the six largest firms. In order to further test whether our price effects only occur for institutional investors, we examine local flippers, who convert owner-occupied homes to non-owner-occupied homes. This proxy for rental conversion properties should generate effects due to renters without any impact from institutional landlords. This figure shows negative price effects that are smaller than our main estimates and considerably nosier. The results are too imprecise to be conclusive, but are suggestive that our main effects occur from both tenants becoming renters and having a property managed by an institutional investor.

Lastly, we verify that our price effects are unrelated to the two dimensions of variation in our model, spatial and temporal, by implementing two falsification tests. First, in the left panel of Figure 6, we consider all non-institutional investor transactions from 2011 to 2022 and between $20,000 and $2,000,000 that are in census block groups with at least one institutional investor purchase. This ensures that we are considering transactions that are in similar neighborhoods to an investor purchase. We then randomize which of these eligible properties is treated as an 'institutional investor' transaction, where their actual year of sale is the new treatment year; we do this for the same number of pseudo-institutional purchases that we observe for actual

[9]The corresponding event studies for this subgroup analysis are in Appendix Figure A.15.

institutional purchases. We then estimate Equation 2, and repeat this procedure twenty-five times, where each color represents a different random sample of pseudo-institutional purchases. The treatment group is 0 to 500 feet from a pseudo-institutional investor purchase, while the control is from 500 to 1000 feet. In the right panel of Figure 6, we first randomize the year of sale among each institutional investor purchase. We then estimate equation 2 1000 times using a new random sample each time, where the figure presents the distribution of t-statistics obtained from these regressions. The dotted vertical red line is our estimated t-statistic and suggests that are main results are unlikely due to chance.

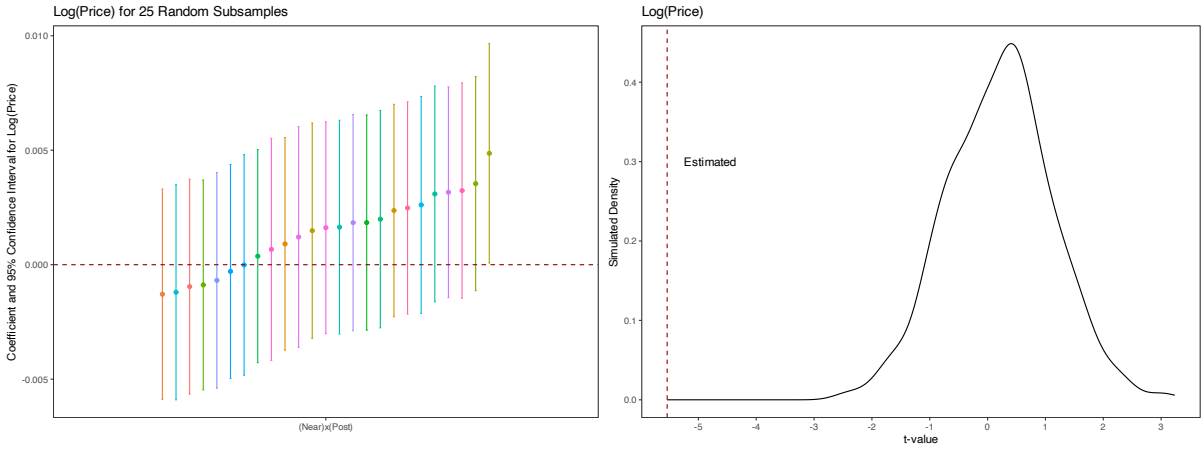


Figure 6: Permutation Tests for the Impacts of Institutional Purchases on Prices

Notes: In the left panel, we first consider all non-institutional investor transactions from 2011 to 2022 and between $20,000 and $2,000,000 that are in census block groups with at least one institutional investor purchase. We then randomize which of these is treated as an "institutional investor" transaction, where their actual year of sale is the new treatment year. In the first stage, log price is regressed on property attributes and year and census block group fixed effects. Residuals from this regression are then averaged by match group and treatment year, and a DID model is estimated with year and match group fixed effects. This procedure is repeated twenty-five times, and each color represents a different subsample. The treatment group is 0 to 500 feet from a institutional investor purchase, while the control is from 500 to 1000 feet. In the right panel, we first randomize the yearly of sale among actual institutional investor purchases. We then run equation (2) 1000 times using a new sample each time, where the figure presents the distribution of t-statistics obtained from these regressions. The dotted vertical red line is our estimated t-statistic for reference.

6 Impacts on Crime, Permits, Code Violations, and Voting

One of the primary mechanisms through which institutional investors are altering a purchased property is by converting it to a permanent rental. The speed at which this happens is not directly observable, but their business model is to purchase homes, generate sustained rental income, securitize these income flows, raise additional funds, then repeat this cycle. Quick conversion to a rental unit can impact externalities through two channels. The first is that these homes may be differently maintained or renovated than an ownership unit because the corporate entity is often only concerned about short-term cash flows. Second, the tenant has now changed from a longer-term owner to a renter. As hypothesized in the externalities of homeownership literature (Coulson and Li, 2013; Harding et al., 2000; DiPasquale and Glaeser, 1999; Fischel, 2002; Disney et al., 2020), the loss of a homeowner may erode social benefits for neighbors in the form of property maintenance, criminal activity, and political participation. We test for all these outcomes using geocoded administrative data. Since reported crimes,

building permits, and code violations are all count variables and our rings are of different sizes, we standardized these measures to be per 1,000 parcels.[10] We also highlight the changing nature of owner-occupancy as an additional outcome to examine homeownership trends more broadly.

Table 4 estimates Equation 2 for non-price outcomes using the same model as in Table 3, where transactions within 500 feet of an investor purchase are the treatment group and those from 500 to 1000 feet are the control.[11] These results show increases in property, violent, and drug crime, increases in nuisance codes, decreases in buildings permits, and decreases in owner-occupied homes. The effects for property and violent crime are increases of 2% (1.71/88.5) and 3% (0.93/27.9), respectively, while we see a 9% (0.61/6.84) increase in drug crime. The result for building permits shows a decline of about 36% (-6.74/18.8) and nuisance code violations increase by 8% (8.5/106.3). Finally, we see a decline of 1.4% (-1.11/78.2) in owner-occupancy rates. These results show that the institutional investor purchase generates significant negative spillovers to nearby neighbors.

Table 4: Impact of Institutional Investor Purchase on Non-Price Outcomes

	Property Crime	Violent Crime	Drug Crime	Building Permits	Nuisance Codes	Owner Occupied
	(1)	(2)	(3)	(4)	(5)	(6)
NearXPost	1.838**	0.954***	0.606***	−7.698***	8.827***	−1.195***
	(0.868)	(0.365)	(0.158)	(1.043)	(2.373)	(0.066)
Control Mean	88.936	28.025	6.911	18.858	106.549	76.8
Observations	219,948	219,948	219,948	219,948	219,948	219,948
R^2	0.565	0.624	0.385	0.173	0.251	0.874

Notes: *p<0.1; **p<0.05; ***p<0.01
Match group-by-treatment and year fixed effects included in all specifications.
Treatment group is 0 to 500 feet from investor purchase. Control group is 500 to 1000 feet.
All outcomes scaled per 1000 parcels, except for Owner Occupied, which is a percent.

Our final outcome of interest is political participation. Given the availability of annual snapshots of registered voters, we can follow residents as they move, as well as see newly registered voters. However, we only can see where an individual lives when they register to vote and when they update their registered voter address.[12] With these data, we seek to answer two questions: 1) does the presence of an institutional investor purchase motivate people to become more politically active by registering to vote and 2) do politically active individuals sort to and away from being neighbors of these purchases. We first generate an imperfect measure of voter type (new versus existing) to address these by splitting our sample into individuals that recently registered to vote in North Carolina (since the beginning of our study period) and those who

[10] We scale by parcel since crime is often coded to streets and includes some commercial crimes, but the results are identical if we scale by housing unit, as the vast amount of development in our rings is single-family housing.

[11] The associated event studies are in Appendix Figures A.17 and A.18. We also break down Table 4 by race in Appendix Table A.2, which shows larger and/or more significant impacts in above median Black neighborhoods. Note that for reported crimes, building permits, and code violations, we are always given spatial coordinates and thus our measure of location is slightly noisier than it is for prices. Therefore, we report results in the main text for a treated area of 0 to 500 feet from an institutional investor purchase and a control area from 500 to 1,000 feet. The disaggregated version is in Appendix Table A.3.

[12] We are limited in using voting history, given the every other year nature of active elections and that these data do not go back to the beginning of our study period.

registered to vote in North Carolina before our study period.[13]

We estimate Equation 3 for all voters, existing voters, and newly registered voters in Appendix Figure A.19. This figure highlights a broader trend of a declining number of registered voters within 500 feet of an institutional purchase. Our breakdown by existing versus new voters shows that only already registered voters are driving this decline; this is consistent with less politically active individuals moving into the neighborhood due to residential sorting. Table 5 estimates Equation 2, where Column (1) finds a small decline of 0.008 voters per housing unit (0.5%). However, when we focus only on existing voters (those first registered before 2011) in Column (2), we see larger effects, which indicates that the closest neighbors are 1.1% less likely to be registered voters. This is likely due to longer-term voters updating their residential addresses when they move, but is also consistent with a greater concentration of renters, who are typically less likely to be registered voters. When we focus on newer voters in Column (3), we see no effects and thus the presence of institutional investors does not appear to induce current neighbors to be more politically active in terms of formal voting behavior.

Table 5: Impact of Institutional Investor Purchase on Neighboring Voters

	All Voters	Existing Voters	New Voters	Share Repub	Share Dem	Share Out-of-State	Share Minority
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
NearXPost	−0.008***	−0.012***	0.001	0.001*	−0.001	−0.001**	−0.003***
	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Control Mean	1.701	1.057	0.748	0.177	0.535	0.754	0.512
Observations	176,681	176,681	163,866	175,952	175,952	175,952	175,952
R^2	0.829	0.814	0.880	0.928	0.942	0.851	0.971

Notes: *p<0.1; **p<0.05; ***p<0.01
Match group-by-treatment and year fixed effects included in all specifications.
Columns 1-3 are scaled to be per housing unit.
Columns 4-7 are based on the share of registered voters for each group.
Control group is from 500 to 1000 feet from an investor purchase.
Treated group is from 0 to 500 feet.

We also examine voter demographics based on the share of registered voters that are based on certain attributes, namely political affiliation, voters born outside of North Carolina, and race. Political affiliation can be characterized into three categories that are roughly equal in size - democrats, republicans, and unaffiliated/independent/others. These categorizations have been used in other work and the third group is considered to be politically centered (Bernstein et al., 2022). Columns (4) to (7) of Table 5 show for our various voter demographics that the only outcome of note is the minority share. This provides evidence that the share of voters that are minorities decreases in response to an institutional home purchase, but the effect is quite small; it represents a 0.6% (0.003/0.51) decline.

[13]This split is done under the assumption that existing voters update their new residential location over time, which can show if sorting is simply generating changes in our measure of voter participation.

7 Cumulative Impacts & Alternative Neighborhood Definitions

Since our analysis thus far has been quite granular, it remains unclear how larger neighborhoods evolve in the presence of institutional investor activity. We first examine this by extending our earlier price analysis to the census block and utilizing two different measures of institutional investor purchases: 1) the initial entry of an investor to a given census block and 2) the cumulative number of investor purchases to date in that block. For these models, we implement DID estimators with two types of control groups. The first is census blocks without an institutional investor purchase but that are in the same census block group as one with at least one investor purchase and the second is only not-yet-treated census blocks. Appendix Figure A.20 shows that the initial entry of an institutional purchase generates noisy negative impacts on census block prices. However, the cumulative institutional purchases effects are more precise, where each additional purchase generates 0.1% to 0.2% lower prices for home purchases in nearby census blocks. This lines up in magnitude with Table 3, as census blocks have an area that is about ten times that of our primary spatial DID rings.

We then extend our analysis to a different definition of a neighborhood, which also provides us access to outcomes not available at a more granular-level. For this exercise, we utilize Charlotte's Neighborhood Quality of Life Survey, which starts in 2012 and is collected biannually. Neighborhoods are close in size to census block groups but are defined differently across space. There are 458 unique neighborhoods and they are defined through input from citizens, community organizations, real estate professionals, and public officials to capture perceived neighborhood boundaries. This process likely better characterizes how residents perceive where they live and thus outcomes may better align with these boundaries than with those defined by the census. The outcomes represent a mix of survey and administrative data, and we focus on a subset of outcomes that are consistently reported over our sample period. These outcomes are standardized by the city of Charlotte to have a mean of 0 and a standard deviation of 1.

Figure A.21 shows the impact of cumulative private equity purchases on two sets of outcomes for this more community-driven definition of a neighborhood. The top panel is intended to mirror the outcomes we have examined in Section 6, namely building permits, housing violations, crime, and voting, and is consistent in direction and magnitude with these results. The second set of outcomes in the bottom panel of Figure A.21 shows increases in 311 call volume (any complaint to the city), animal control calls to the city, and disorder call rates. The results of this section again show that institutional investor activity generates significant negative spillovers, but with alternative (and larger) definitions of a neighborhood than those used in Section 6.

8 Conclusion

The traditional path to wealth creation through home ownership has stalled and worsened for many minority households since the Great Recession. One potential driver of this trend is the growing presence of institutional investors in the real estate market, who often purchase single-family homes and convert them to permanent rentals. We show that large institutional investors alone have decreased overall homeownership rates for existing homes by 2 percentage points in Mecklenburg County, North Carolina. This is a 4 percentage point decline for majority Black

neighborhoods, which approaches the overall loss in homeownership due to the Great Recession.

Using a granular spatial difference-in-differences estimator, we show that an institutional investor purchase generates up to a 2% decline in nearby property values. This effect exhibits spatial decay and is almost exclusively limited to majority Black suburban neighborhoods that do not have Homeowners Associations. These declines in property values are also associated with commonly hypothesized social spillovers from the loss of homeownership, namely declines in neighborhood property maintenance, increases in crime, and declines in registered voters moving into these neighborhoods.

An increase in the number of rental units in neighborhoods with primarily single-family, owner-occupied properties improves access to amenities that were previously unaffordable or unattainable for certain households, such as better schools. Therefore, institutional investor activity has the potential to improve the livelihoods of the new rental tenants. Whether this actually occurs remains an open question, but our results suggest that large-scale conversion of owner-occupied homes to rental units has significant negative spillovers to neighboring properties and the community more broadly. This then suggests that historical efforts to broaden homeownership through cheaper financing and tax advantages for home purchases may still be quite beneficial. However, it remains unknown as to whether institutional investor activity is welfare improving in the aggregate, and future research should examine its impacts on educational attainment, income, and criminal justice contact for both the individuals moving into these newly converted rental properties as well as their neighbors.

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A Additional Figures and Tables

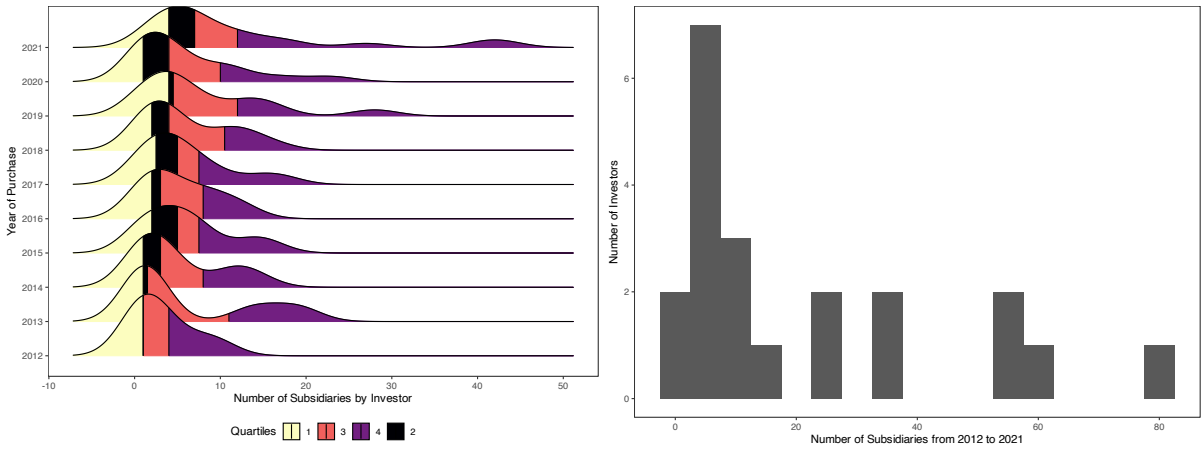


Figure A.1: Number of Subsidiaries by Investor and Year in Mecklenburg County, NC

Notes: The left panel shows the distribution of subsidiaries per investor in a given year, while the right panel shows the distribution of the number of subsidiaries per investor over the entire sample period.

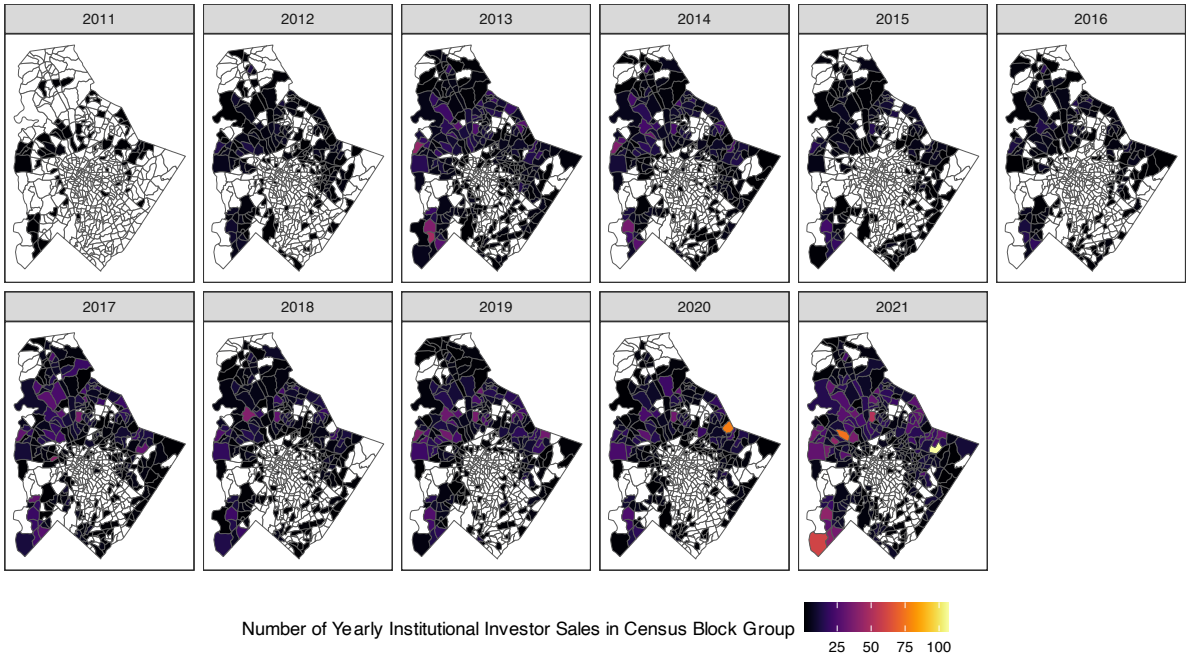


Figure A.2: Yearly Sales by Census Block Group in Mecklenburg County, NC

Notes: This figure matches the location of properties that were purchased by institutional investors to their respective census block groups. These are then aggregated to yearly totals of transactions for each census block group, and thus areas with lighter colors have more institutional activity in that year. Figure 1 shows the yearly share of all transactions purchased by institutional investors in a census block group.

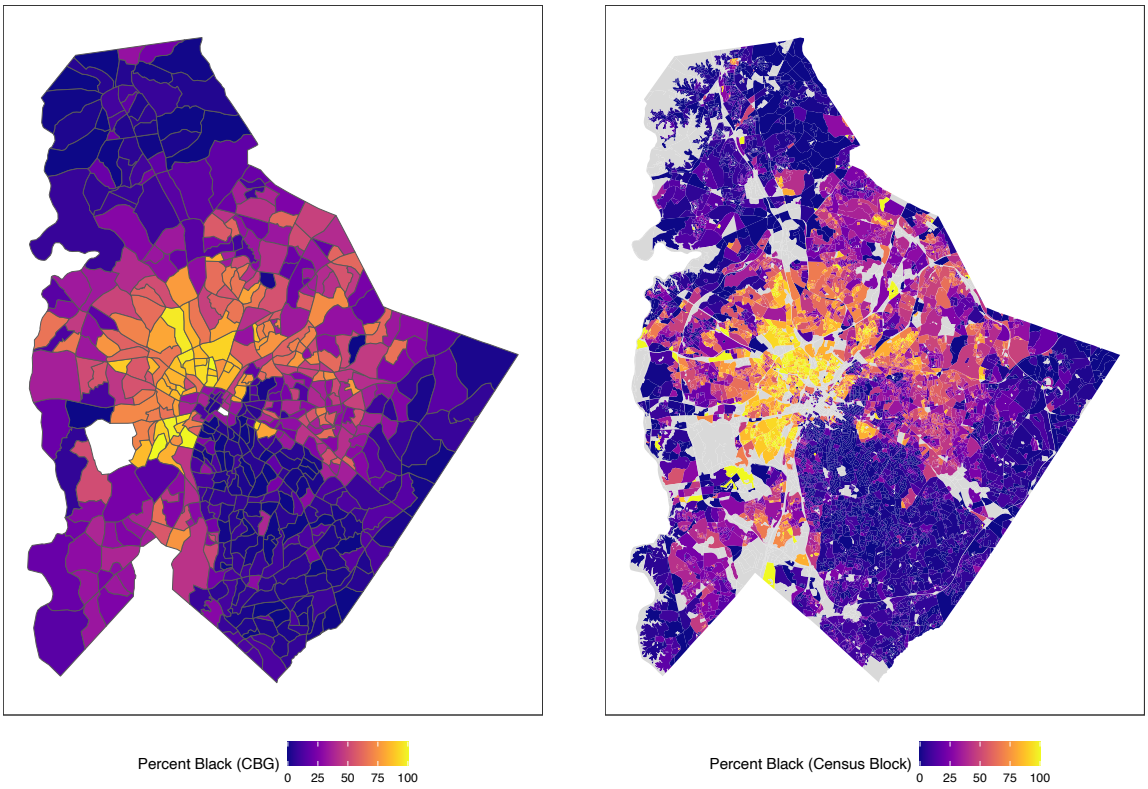


Figure A.3: Map of Census Geography by Race in Mecklenburg County, NC

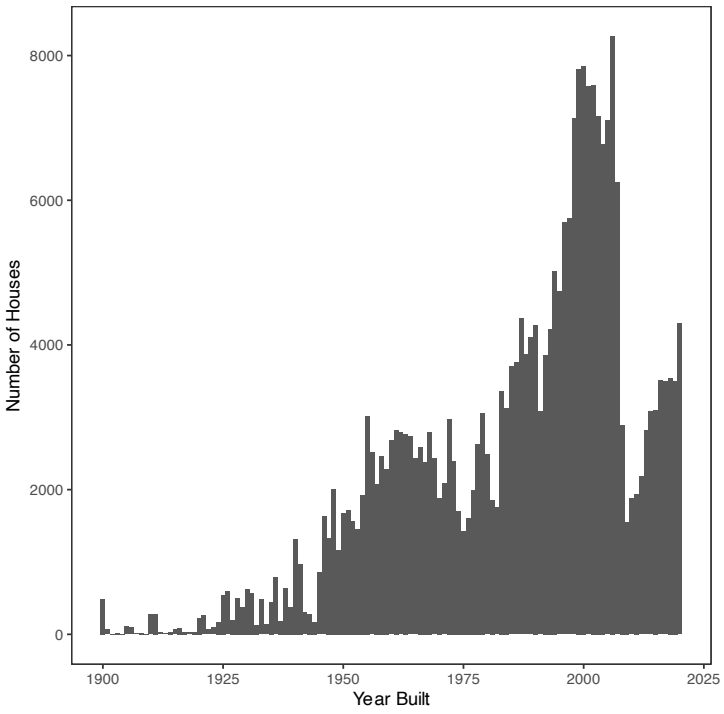


Figure A.4: Number of Houses by Year Built

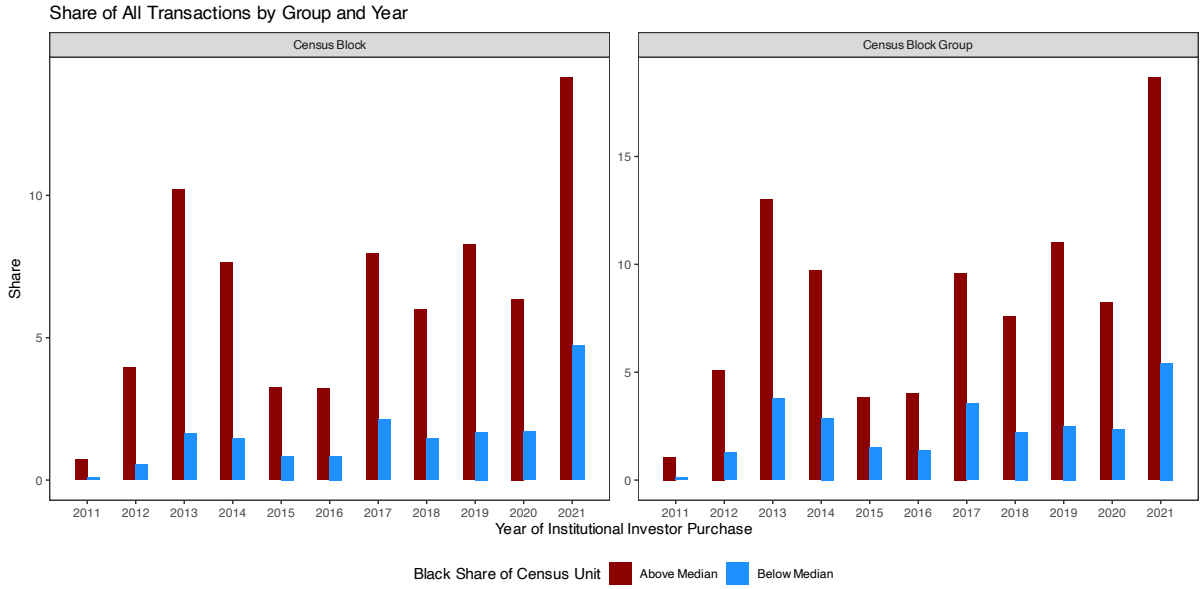


Figure A.5: Institutional Purchases by Year and Black Share of Census Geography

Note: Each bar captures the share of private equity transactions by year and census geography that is Black. Figure 2 repeats the exercise with total transactions by year and group.

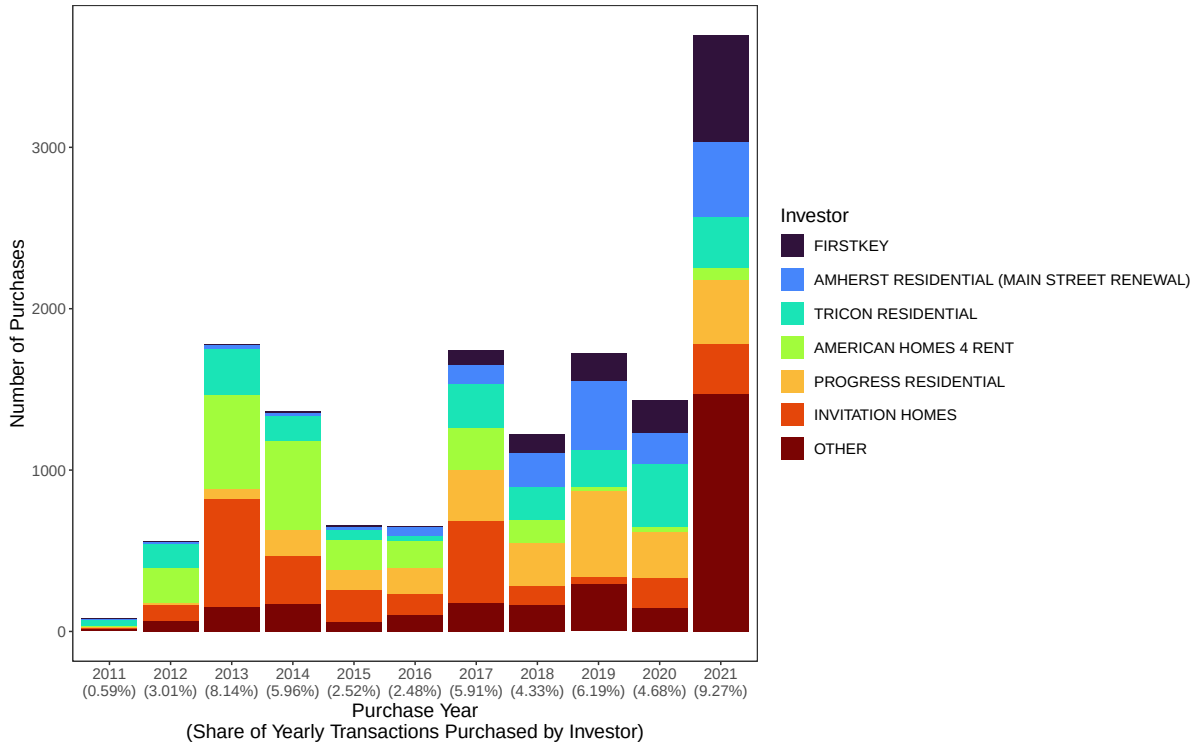


Figure A.6: Yearly Purchases by Investor in Mecklenburg County, NC

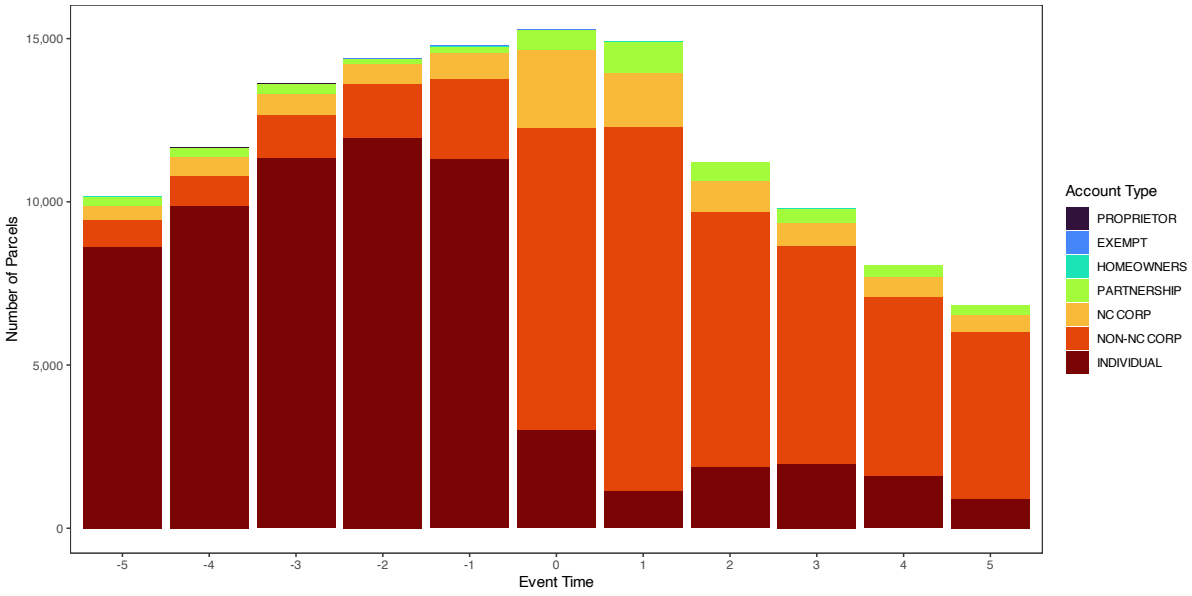


Figure A.7: Account Type by Event Time

Notes: The sample includes only parcels that eventually were purchased by a private equity investor.

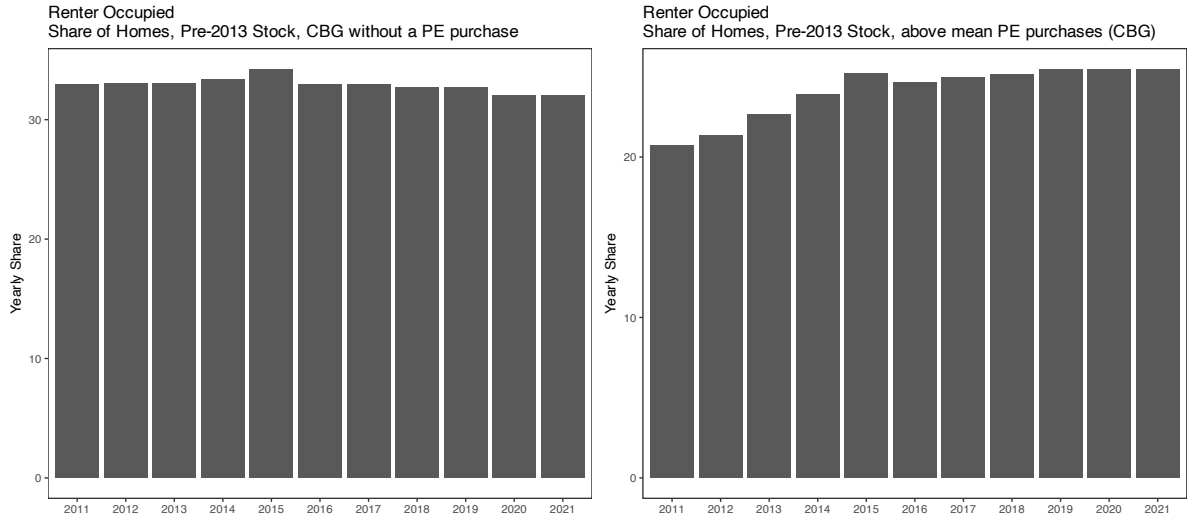


Figure A.8: Rental Share (Non-Owners, limited by purchases from institutional investors in CBG)

Notes: The average number of private equity purchases in a census block group is 39, while the median is 15 and the maximum is 315.

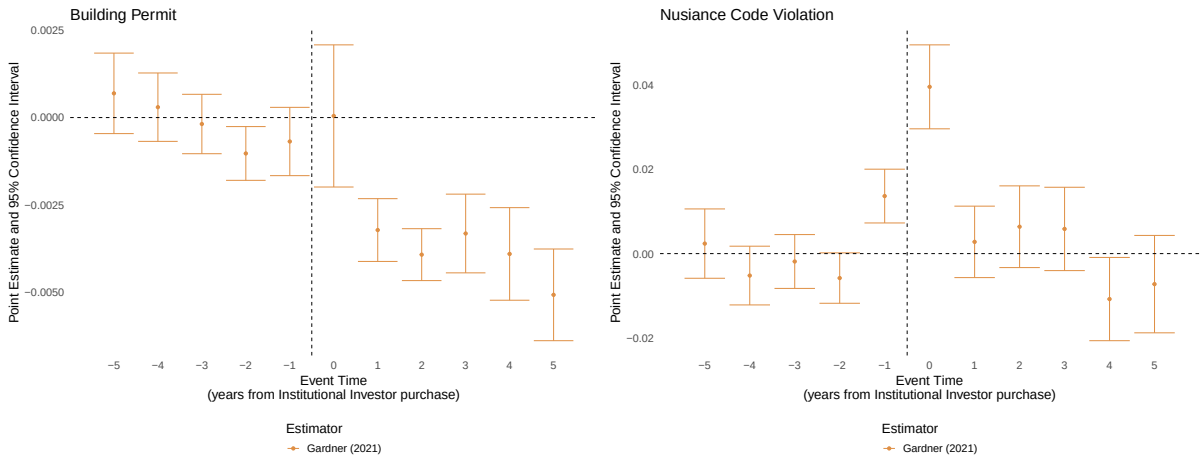


Figure A.9: Event Study for Parcel Outcomes after Institutional Purchase

Notes: These event studies are run at the parcel-level. Specifically, does the property have a building permit issued in the left panel and if there was nuisance code violation in a given year in the right panel. Year and parcel fixed effects are included in both models. We limit our sample to properties that are owner-occupied prior to purchase by institutional investors. The mean number of building permits per year is 0.006 and the mean number of nuisance code violations in our sample is 0.043.

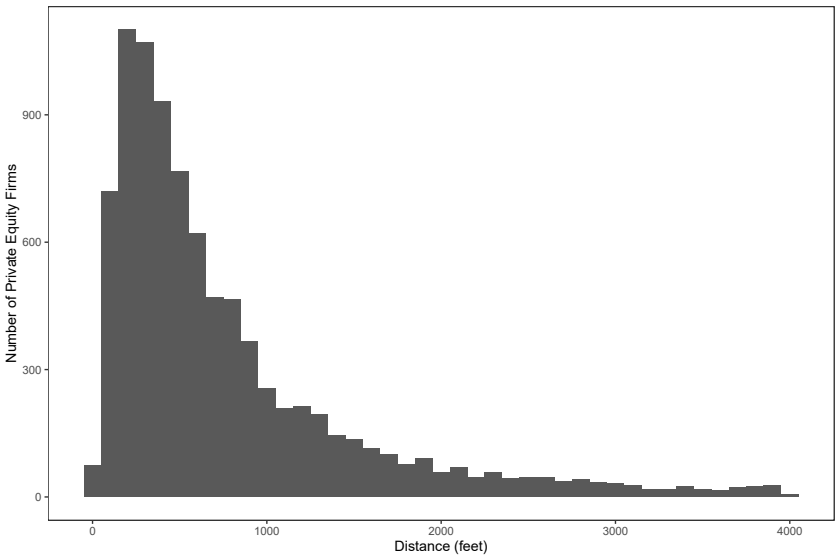
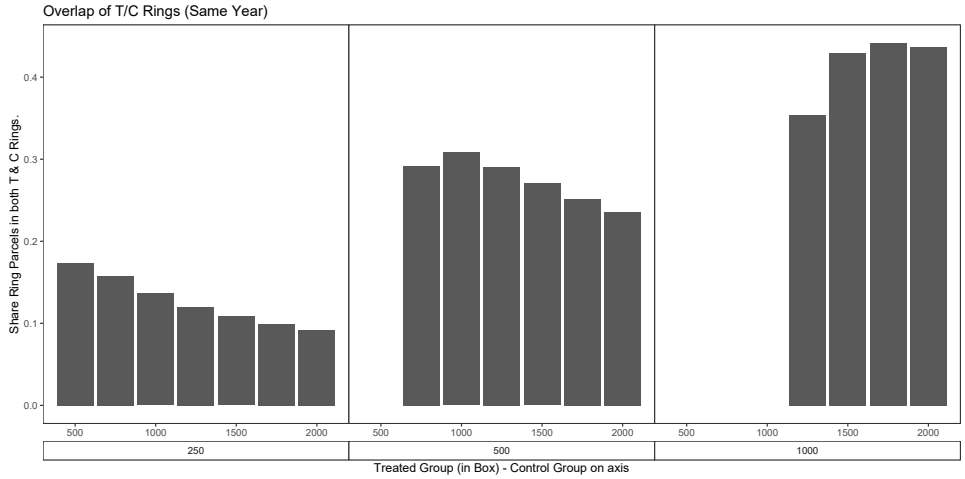
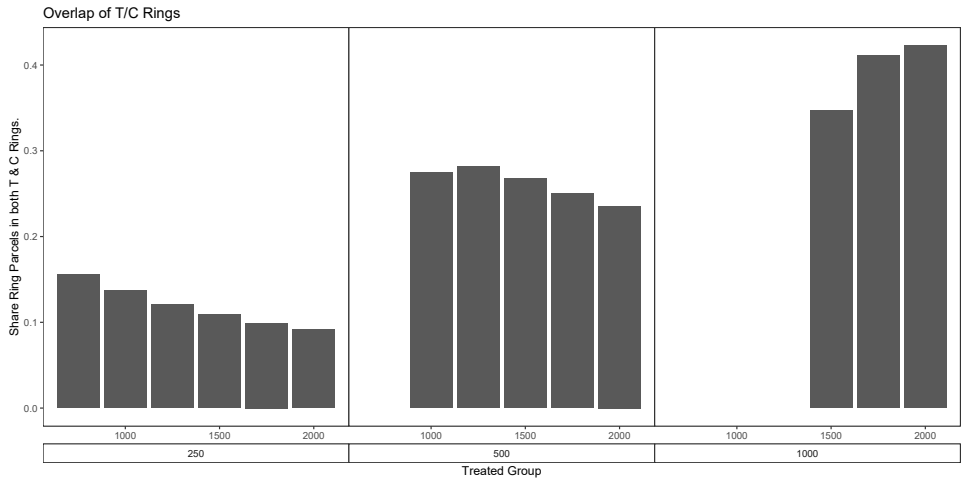


Figure A.10: Distance from Institutional Purchase to Closest Other Institutional Purchase in the Same Year



(a) Overlap, Treatment/Control Rings, Same Year

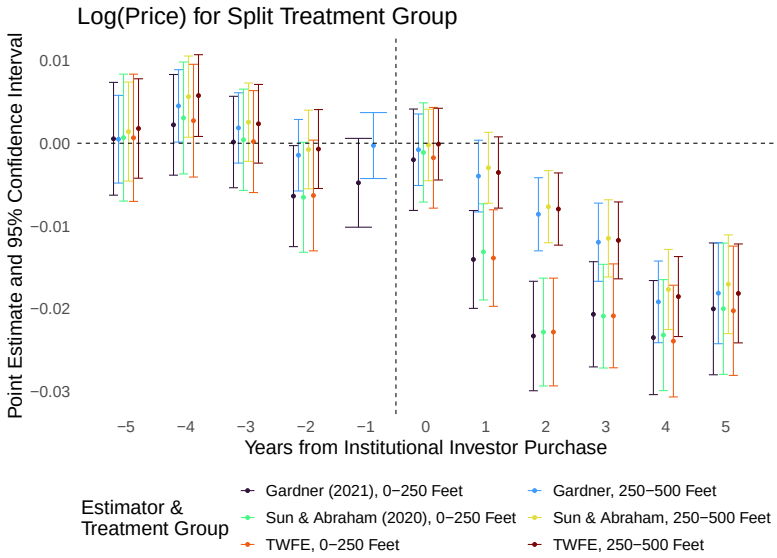


(b) Overlap, Treatment/Control Rings, Same Year w/ Donut

Figure A.11: Portion of Parcels that Occupy both Treated and Control Rings

Note: These two figures provide a sense of how often a parcel located within a treatment or control ring of a property purchased by an institutional investor is assigned to a different group (control vs treatment) due to the spatial proximity of other properties purchased by investors. The top figure provides the share of multiple assignment parcels for investors purchases in the same year. We focus on same year investor purchases since our identifying variation is partially due to temporal variation. Below the x-axis provides the treatment ring radius in a box and assigns treatment for all parcels up to a given feet in distance. The x-axis provides the control area radius from the investor property and represents the area from treatment to the given x-axis value. For example, the boxed value of 250 and the x-axis value of 1,000 would indicate a treatment area from 0 to 250ft and a control area from 251ft to 1,000 ft. The bar heights indicate the share of all properties in any ring (treatment or control) that are assigned to at least one treatment and one control ring. The second figure provides overlap when we implement a donut for the control area of 250 feet. This leave 250 feet between the edge of the treated area to the beginning of the control area. For example, a treated area of 0 to 250, then the control area would now be 500 to 1,000 ft.

Figure A.12: Impact of Institutional Purchases on Prices of Nearby Homes



Notes: To estimate this event study, we first regress the logarithm of transacted price on individual property attributes and year and census block group fixed effects. Residuals from this regression are then averaged by match group and treatment year, and a DID model is estimated with year and match group fixed effects. The treatment group is either 0 to 250 feet or 250 to 500 feet from an institutional investor purchase, while the control group is always 500 to 1000 feet.

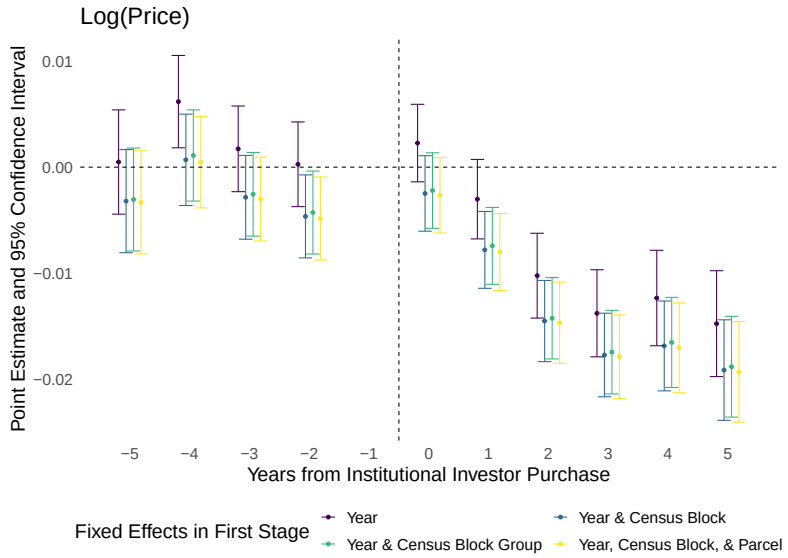
Table A.1: Impact of Institutional Investor Purchase on Prices

	log(Sale Price)						
	Full Sample	Black	White	Not HOA	HOA	2015 and Before	After 2015
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
NearXPost (0-250 Feet)	-0.019^{***}	-0.015^{***}	-0.007^{*}	-0.016^{***}	-0.011^{**}	-0.006	-0.029^{***}
	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)	(0.005)	(0.003)
NearXPost (250-500 Feet)	-0.011^{***}	-0.010^{***}	-0.003	-0.009^{***}	-0.006^{**}	-0.004	-0.017^{***}
	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.004)	(0.003)
Observations	183,533	137,245	46,442	160,013	23,520	59,319	124,368
R^2	0.262	0.256	0.285	0.258	0.248	0.273	0.248

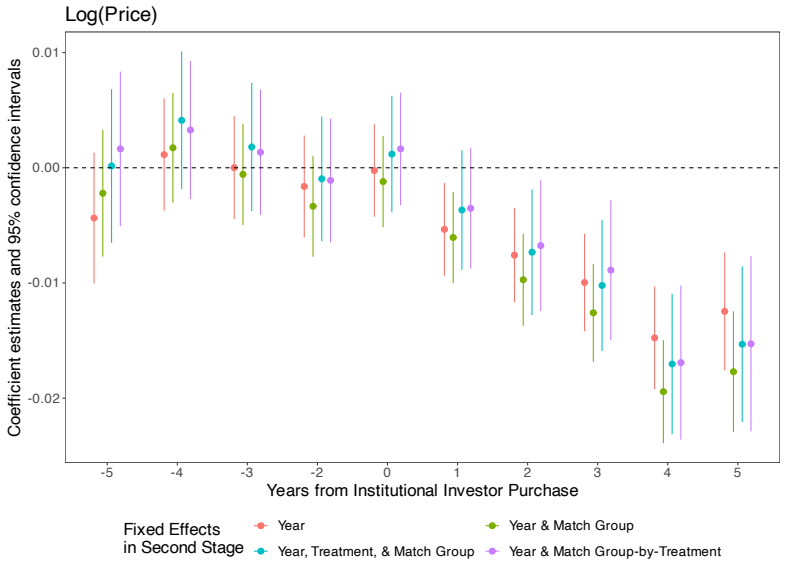
Notes: *p<0.1; **p<0.05; ***p<0.01
In the first stage, price is regressed on property attributes and
year and census block group fixed effects.
Regression residuals are averaged by match group and treatment year,
then in the second stage, DiD estimation is run with
match group-by-treatment and year fixed effects separately on each subsample.
The control group is 500 to 1000 feet from an institutional investor purchase,

Figure A.13: Robustness to Controls for Property Attributes, Repeat Sales and Fixed Effects

(a) Varying First Stage Fixed Effects for Spatial DID



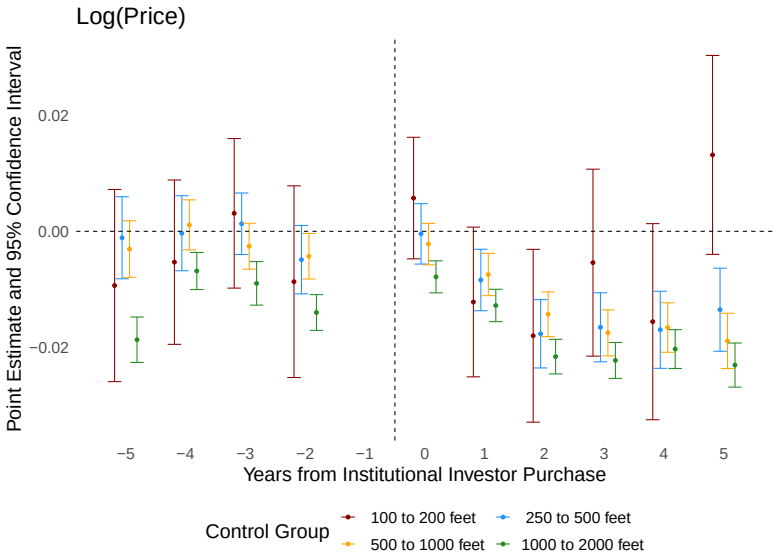
(b) Varying Second Stage Fixed Effects for Spatial DID



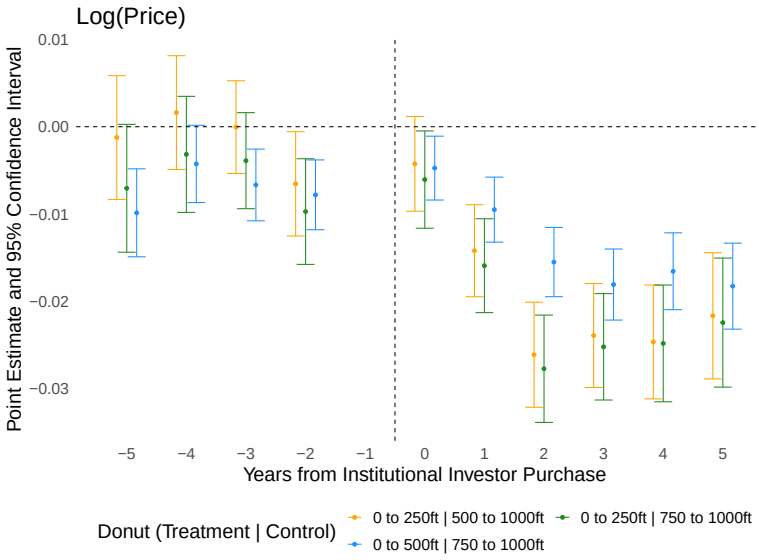
Note: The top figure provides a series of event studies for our main treatment area of 0 to 500 feet relative to 500 to 1,000 feet for models that vary in our first stage creation of conditional prices. These models always include property attributes in the regression, but span from including only year fixed effects, which is equivalent to real prices, to different geographical fixed effects to a repeat sales specification through the inclusion of parcel fixed effects. The bottom figure provides robustness to a variety of fixed effects for Equation 2 (the second stage), which vary from less granular geographical fixed effects to our main model of institutional purchase by treatment ring fixed effects.

Figure A.14: Price Results using Different Distances for Rings and "Donut" Models

(a) Different Distance Rings



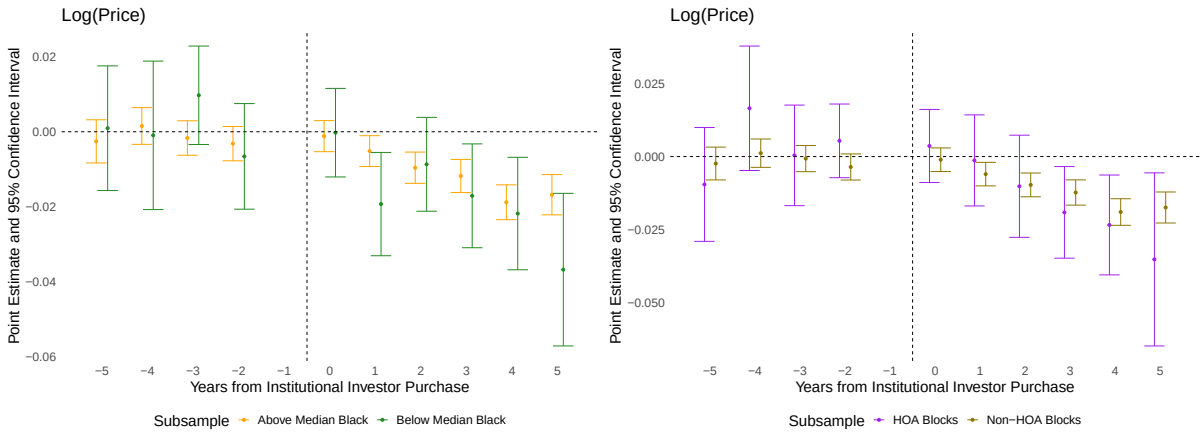
(b) Models Incorporating a "Donut" between Treatment & Control Areas



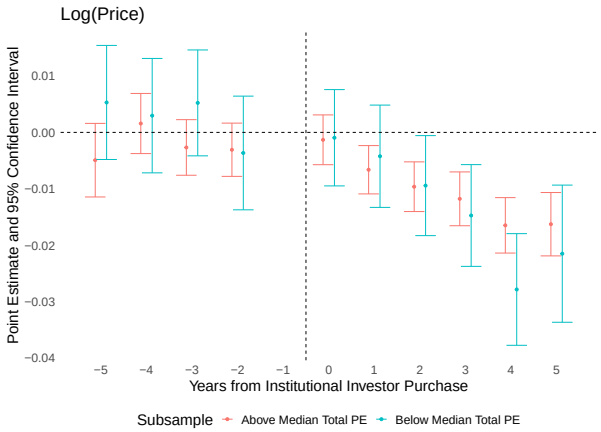
Note: The top figure provides a series of event studies that vary the distance rings for treatment and adjacent control areas. The treatment group is from 0 feet to lower bound of the respective control group. These models highlight that using treated areas too small (0 to 100 feet) and too large (0 to 1,000 feet) generate invalid spatial DID estimates. The bottom figure removes different size rings and shows the value of using a donut area to separate treated and control areas. While these models limit concerns about spillovers from treated areas onto control ones, the results they produce are similar to our primary model.

Figure A.15: Impact of Institutional Purchases on Prices of Nearby Homes (Heterogeneity)

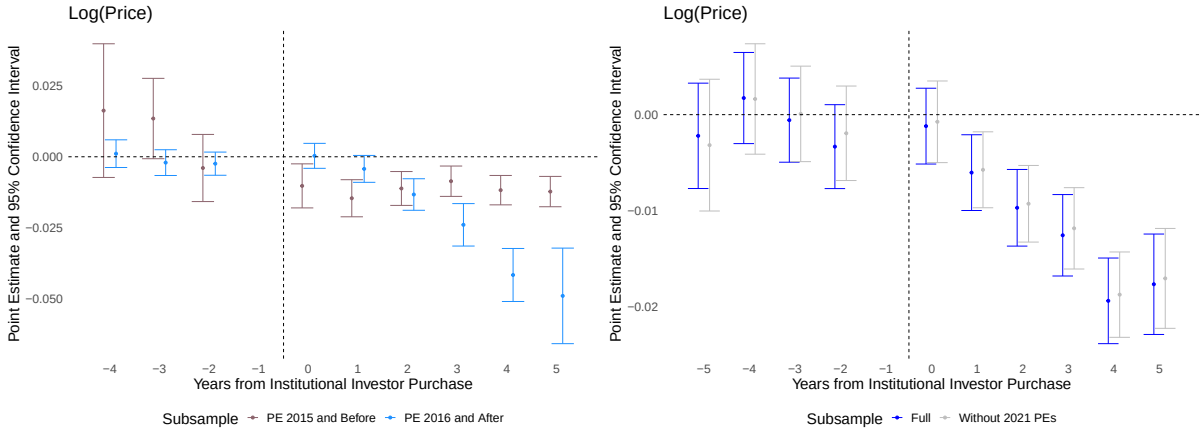
Panel A: By Race and Homeowners Association



Panel B: Timing of Institutional Purchase Activity



Panel C: By Time Period



Notes: In the first stage, the logarithm of the transacted sales price is regressed on property attributes and year and census block group fixed effects for each subsample. Regression residuals are then averaged by match group and treatment year, then in the second stage, DID estimation is run with match group-by-treatment and year fixed effects. The treatment group is 0 to 500 feet from the purchase, which is either an institutional investor or a conversion from an owner-occupied property to one that is no longer owner-occupied. The control group for all is from 500 to 1000 feet.

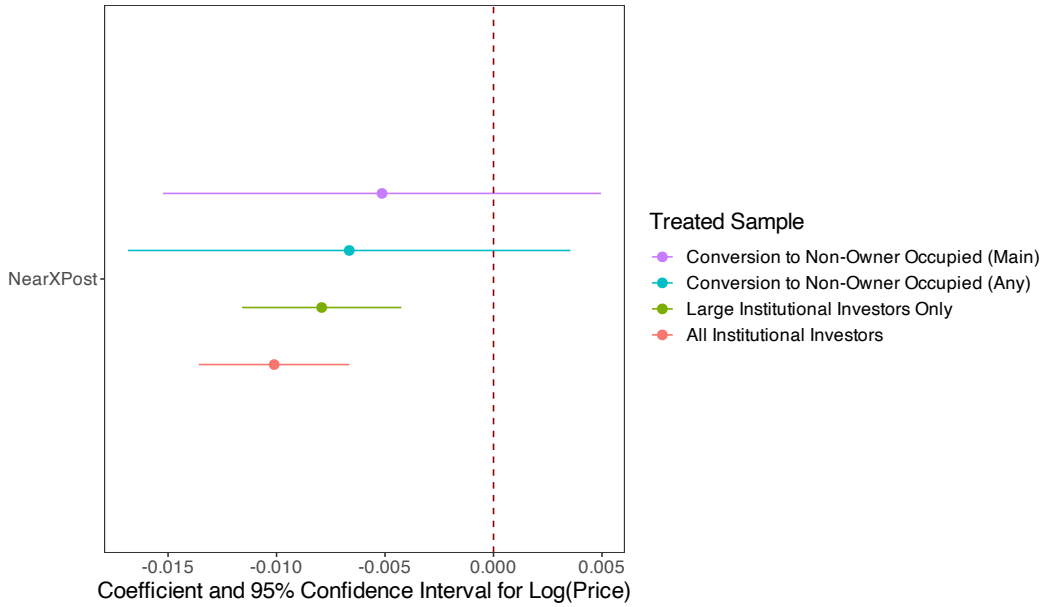


Figure A.16: Impact of Institutional Purchases versus Local Flippers on Price Outcomes

Notes: In the first stage, the logarithm of the transacted sales price is regressed on property attributes and year and census block group fixed effects. Regression residuals are then averaged by match group and treatment year, then in the second stage, DID estimation is run with match group-by-treatment and year fixed effects. The treatment group is 0 to 500 feet from the purchase, which is either an institutional investor or a conversion from an owner-occupied property to one that is no longer owner-occupied. The control group for all is from 500 to 1000 feet. Large investors are those with more than one thousand properties. The (Main) sample removes large investor transactions from the pool of control properties, while (Any) removes any investor transaction.

Table A.2: Impact of Institutional Investor Purchase on Non-Price Outcomes by Race

	Property Crime		Violent Crime		Drug Crime		Building Permits		Nuisance Codes		Owner Occupied	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
NearXPost	2.203**	0.281	1.035**	0.611	0.655***	0.419**	−4.995***	−20.611***	10.514***	−0.300	−1.094***	−1.707***
	(0.973)	(1.914)	(0.434)	(0.448)	(0.188)	(0.192)	(1.098)	(3.047)	(2.794)	(3.500)	(0.069)	(0.192)
Control Mean	97.289	50.913	31.314	13.053	7.775	2.981	15.528	34.018	116.64	60.613	75.827	81.233
Neighborhood	Black	White	Black	White	Black	White	Black	White	Black	White	Black	White
Observations	180,444	39,504	180,444	39,504	180,444	39,504	180,444	39,504	180,444	39,504	180,444	39,504
R^2	0.564	0.522	0.631	0.463	0.375	0.541	0.156	0.200	0.246	0.351	0.889	0.800

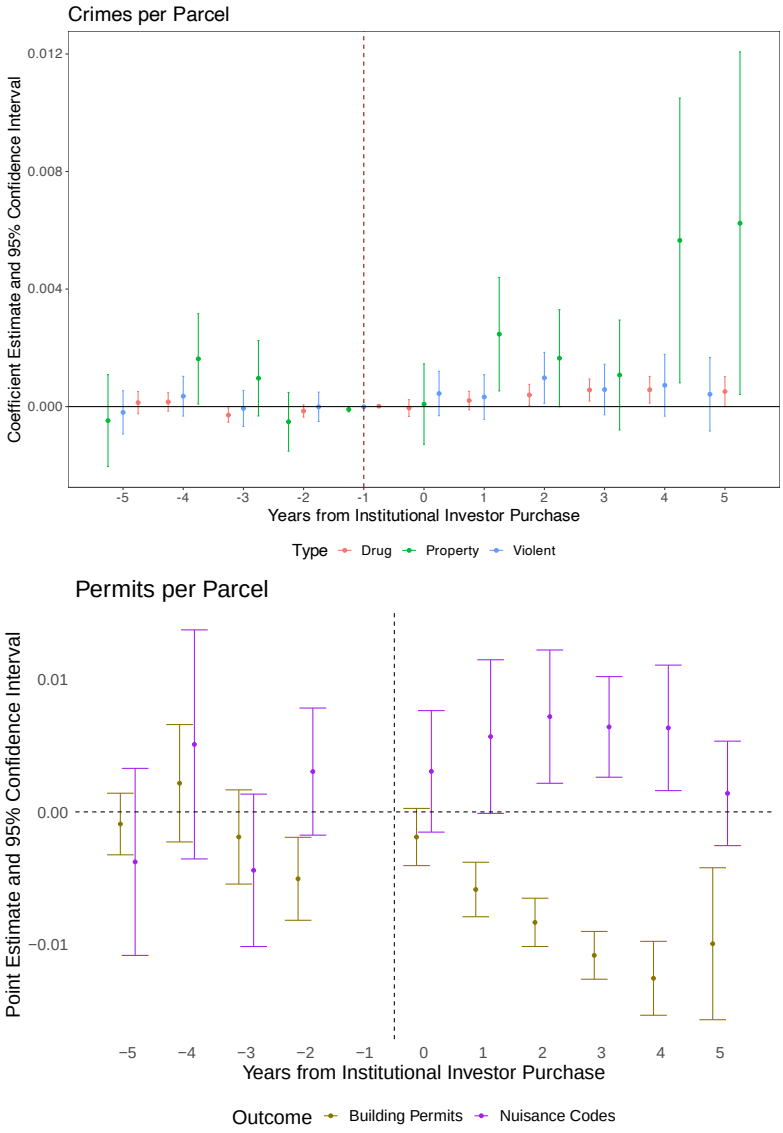
Notes: *p<0.1; **p<0.05; ***p<0.01
Match group-by-treatment and year fixed effects included in all specifications.
Treatment group is from 0 to 500 feet from institutional investor purchase. Control group is 500 to 1000.
All outcomes scaled per 1000 parcels, except for Owner Occupied, which is a percent.
Race of neighborhood is based on median share of census block group of the purchase.

Table A.3: Impact of Institutional Investor Purchase on Non-Price Outcomes

	Property Crime	Violent Crime	Drug Crime	Building Permits	Nuisance Codes	Owner Occupied
	(1)	(2)	(3)	(4)	(5)	(6)
NearXPost (0-250 Feet)	2.011	1.174**	0.390*	−7.554***	−1.244	−1.501***
	(1.324)	(0.583)	(0.226)	(1.158)	(3.775)	(0.098)
NearXPost (250-500 Feet)	0.509	0.811*	0.505***	−4.783***	10.170***	−0.893***
	(1.023)	(0.439)	(0.185)	(0.981)	(2.453)	(0.069)
Control Mean	88.5	27.93	6.844	18.859	106.344	76.827
Observations	315,490	315,490	315,490	315,490	315,490	315,490
R^2	0.516	0.506	0.347	0.182	0.242	0.868

Notes: *p<0.1; **p<0.05; ***p<0.01
Match group-by-treatment and year fixed effects included in all specifications.
Control group is from 500 to 1000 feet from Institutional Investor purchase.
All outcomes scaled per 1000 parcels, except for Owner Occupied, which is a percent.

Figure A.17: Impact of Institutional Purchases on Crime and Permits



Notes: The treatment group for both event studies is 0 to 500 feet from institutional investor transaction and the control is 500 to 1000 feet. Match group-by-treatment and year fixed effects included and standard errors are clustered at the match group.

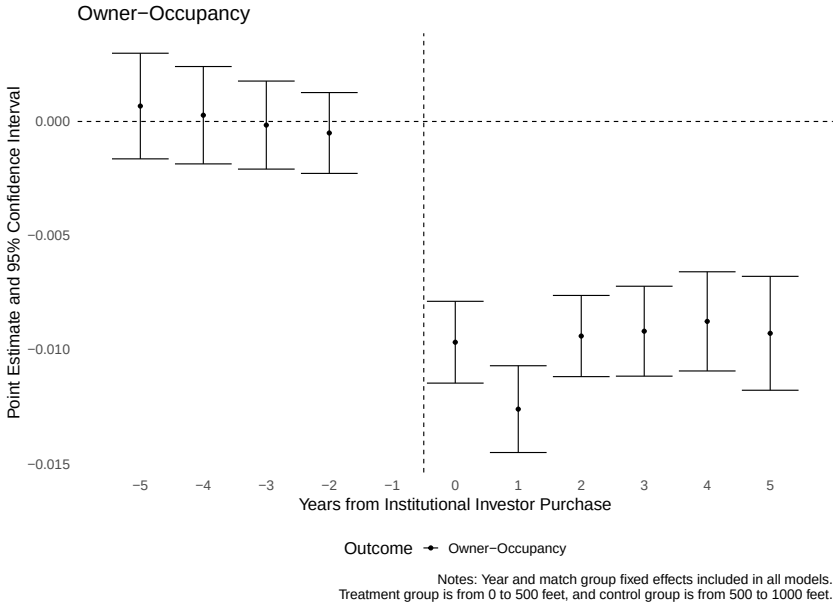


Figure A.18: Impact of Institutional Purchases on Owner Occupation

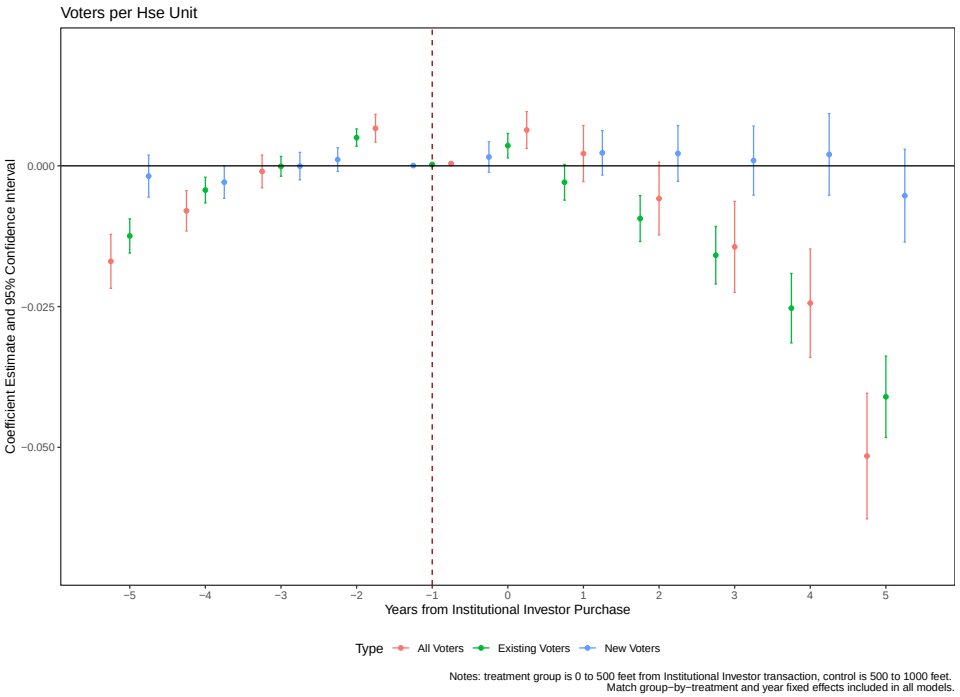


Figure A.19: Impact of Institutional Home Purchase on Registered Voters

Note: These figures are based on registered voter snapshots for each year in Mecklenburg County, NC. Existing voters are those registered voters prior to 2011 and newly voters only includes voters that registered for the first time after 2010.

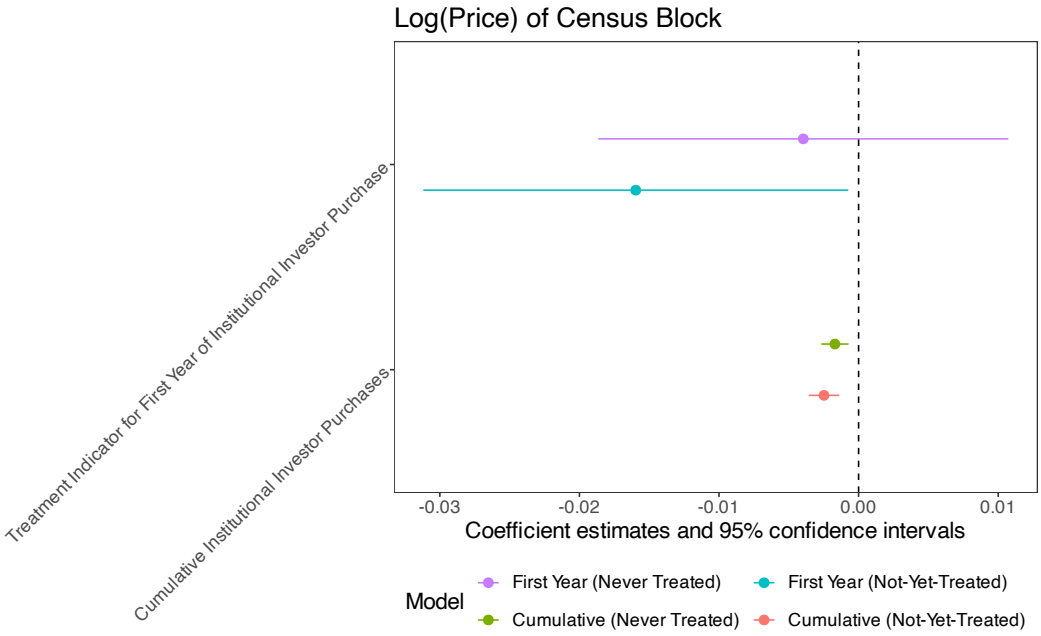


Figure A.20: Impact of Institutional Investor Purchases on Census Block-Level Prices

Notes: In the first stage, the logarithm of transacted price is regressed on property attributes and year fixed effects. Residuals from this regression are then averaged by census block and year, and the DID estimation is run with census block and year fixed effects. Standard errors are clustered at the census block. Cumulative institutional investor purchases is calculated yearly. The control group is either census blocks without an institutional investor purchase in the same census block group that has at least one investor purchase or only not-yet-treated census blocks.

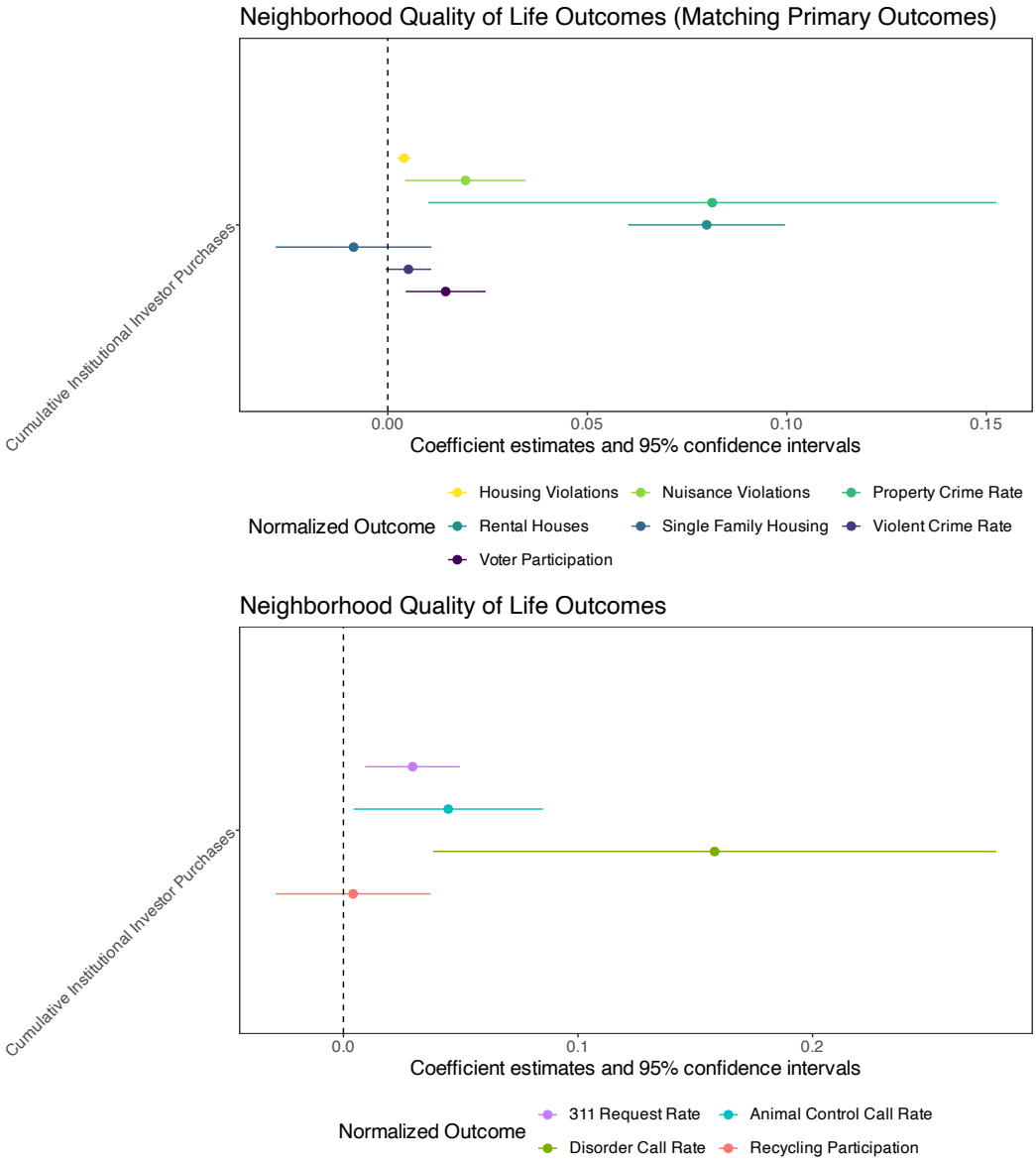
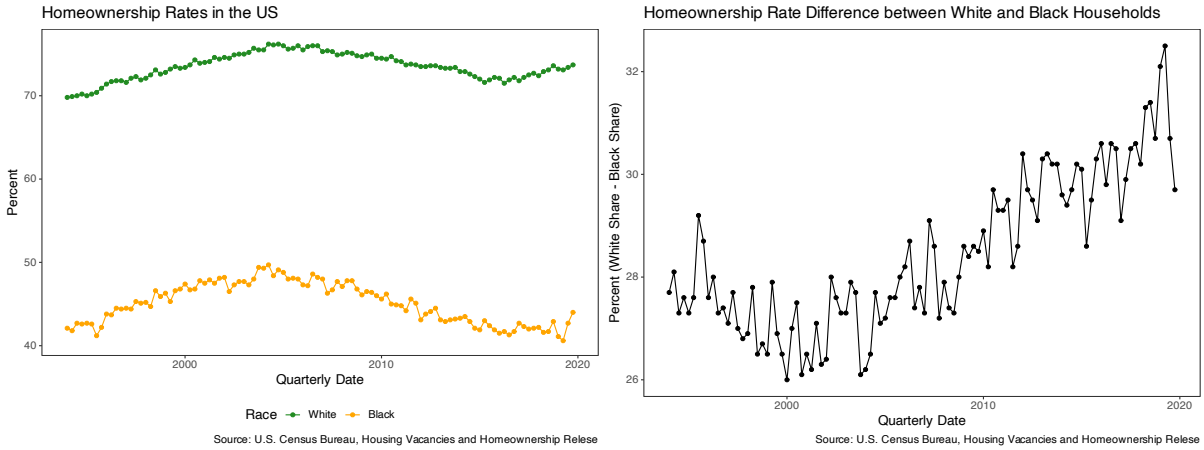


Figure A.21: Impact of Cumulative Institutional Purchases on Neighborhood Outcomes

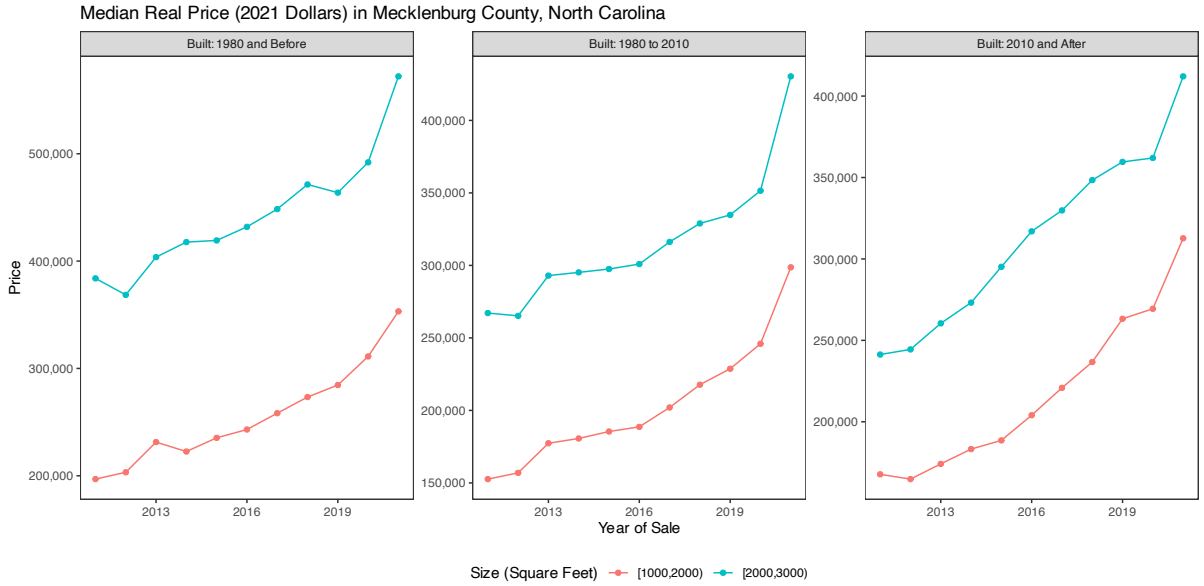
Notes: The cumulative institutional investor purchases measure is calculated yearly, and outcomes are standardized to have a mean of 0 and standard deviation of 1. Neighborhood and year fixed effects are included in all models, and standard errors are clustered at the neighborhood.

Context

Panel A: US Homeownership Rates



Panel B: Median Prices in Mecklenburg, NC



Notes: this will likely be removed and only for the presentation. Sizes outside these two ranges had too few observations.