Tracing the Source of Liquidity for Distressed Housing Markets *

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Abstract

Policymakers have been working to stabilize distressed neighborhoods since the 2008–2010 foreclosure crisis. We show that profit-seeking institutional investors purchased distressed properties and aided the recovery of local housing markets. Using a quasi-natural experiment in which investors purchased pre-packaged home portfolios from the GSEs, we find that average properties located within 0.25 miles of bulk-sold properties sell for 1.4% *higher* than homes located farther away. The spillover effect is greater for foreclosed homes (4.3%), homes that are similar to the bulk-sold homes (2.5%), and homes in highly distressed neighborhoods (7.4%). Our results show that institutional investors provided valuable liquidity to the distressed housing markets, and the asset-pooling design by the GSEs helps channel this liquidity to the most needed areas.

Keywords: Real Estate, Foreclosure, Institutional Investor, Liquidity

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1 Introduction

7.8 million homes were foreclosed in the decade since the onset of the financial crisis between 2007–2016. During this period the inventory of foreclosed properties for sale peaked in 2011 at 1.6 million, which was 20% of all foreclosed homes.[1] This large wave of foreclosures not only resulted in substantial price discounts for the affected properties (Clauretie & Daneshvary 2009, Campbell et al. 2011), but also depressed the value of nearby non-foreclosure properties (Harding et al. 2009, Lin et al. 2009, Frame 2010, Campbell et al. 2011, Anenberg & Kung 2014, Gerardi et al. 2015, Fisher et al. 2015). As a result, policymakers have been working to respond to the foreclosure crisis and to stabilize the neighborhoods that were adversely affected by the large inventories of foreclosed properties and their negative externalities.[2] Interestingly, the foreclosure crisis also saw the rise of institutional investors in the residential real estate market (Allen et al. 2018, Mills et al. 2019). For instance, since 2010, institutional investors such as, Blackstone Group and Starwood Capital, have spent billions of dollars buying distressed properties and have increased their holdings of single family homes 30-fold between 2010–2016. These investors acquired residential properties and turned them into rental properties. Single family rental has grown to become widely recognized as a new asset class for institutional investment over this period (Eisfeldt & Demers 2015).[3]

In this paper, we study the effect of institutional investment on the local real estate market. Specifically, we focus on how the purchase of distressed properties by institutional investors affects neighborhood home values. We find causal evidence that properties that are purchased by institutional investors have a positive spillover effect on nearby home values – i.e., homes that are closer to the properties purchased by institutional investors can subsequently sell at a higher price. This spillover effect is economically significant: a purchase of foreclosed property by institutional investors within a 0.25-mile distance increased home value by \$1.33 per square foot (sqft), or by 1.4% in total value for an average property relative to properties that are further away. Further, this positive spillover effect is greater for foreclosed transactions (\$4.25 per square foot or 4.3% of total value), and properties similar to those purchased by institutional investors (e.g., \$3.75 per square foot or 2.5% of total value for same-age properties). Importantly, our results further

[1] https://www.corelogic.com/research/foreclosure-report/national-foreclosure-report-10-year.pdf
[2] See https://www.hudexchange.info/programs/nsp/
[3] https://www.wealthforge.com/insights/single-family-rental-market-new-asset-class/

indicate that the positive spillover effect is particularly pronounced for properties in the most distressed neighborhood ($8.40 per square foot or 7.4% of total value). Our results suggest that, while institutional investors pursued an emerging investment opportunity in the distressed residential real estate market, they also provided valuable liquidity to the sector. This liquidity provision is especially important when credit markets are tight and when other potential buyers have impaired credit.

Ex ante, the effect of institutional purchases of distressed properties on the local real estate market is not obvious. For instance, the extant literature suggests two main channels for the negative price spillover effects of distressed properties, namely the "supply effect", and the "disamenity effect". The *supply effect* suggests that foreclosed properties drive down neighboring house prices because of the increased supply of properties available for sale. The supply effect is further amplified if impatient sellers such as banks drive down prices by competing to sell in order to minimize the holding costs of their real estate owned (REO) inventory (Anenberg & Kung 2014, Rajan & Ramcharan 2016, Ramcharan n.d.).[4] The *disamenity effect* suggests that poor maintenance of foreclosed properties causes blight and reduces neighboring property values due to "physical externalities" (Gerardi et al. 2015, Fisher et al. 2015). On the one hand, institutional investors purchasing distressed properties can create a positive spillover effect on nearby home values through the supply effect by reducing the supply of properties available for sale. On the other hand, institutional investors can also create negative price externalities by forcing deeper discounts using their stronger bargaining power. In terms of the disamenity effect, the role of institutional investors is also unclear: They can create negative price spillover through the disamenity channel if they have lower incentives than the previous owners, such as banks, to maintain a property after its purchase. This is especially likely if institutional investors can purchase the distressed properties at deep discounts with the purpose of exploiting short-term arbitrage opportunities. However, institutional investors can also improve the quality of the purchased properties with the intention of turning them into income-producing properties and thereby creating a positive price spillover effect through the disamenity channel.

Furthermore, a selection effect can confound the empirical identification of institutional invest-

[4] Holding costs can include property maintenance, property taxes, and home insurance.

ments on the local real estate market (Haughwout et al. 2011, Bayer et al. 2015, Chinco & Mayer 2015, Bhutta 2015). For instance, institutional investors can cherry-pick their investment properties in areas which have the greatest potential for house price growth and neighborhood recovery. To mitigate such a selection effect, we use the REO-to-Rental Pilot Initiative by the Federal Housing Finance Agency (FHFA) in 2012 as a quasi-natural experiment to estimate the causal effect of institutional investments on the local real estate market.[5] The REO-to-Rental pilot program was aimed at clearing the national backlog of foreclosed homes by selling the foreclosed properties in bulk to institutional investors. Importantly, investors were not allowed to cherry-pick individual properties and as a result had to bid on pre-packaged pools of foreclosed properties.[6] Therefore, while institutional investors could still account for their expectations of house price growth at broader geographic levels such as, the county or the zip-code level, they did not have the same ability to select at the highly local levels such as, the block or the neighborhood level. Thus, the properties in the highly local neighborhoods around the pre-packaged bulk-sale properties are less likely to be confounded by the potential selection effect of the institutional investors.

We design a difference-in-differences (DiD) test around the pilot bulk-sale transactions to examine the treatment effect of institutional investment on nearby property transactions. First, we focus on the transactions within a short-time window of (-6, 6) months around three pilot bulk transactions in June 2012 and retain only transactions that are located within a 0.5-mile radius of a bulk-sold property. Next, we define a transaction as "treated" if the transacted property is located within a 0.25-mile radius (inner ring) of a bulk-sold property. Property transactions that are located within 0.25–0.50 miles (outer ring) of a bulk-sold property serve as our "control" transactions. Consequently, our DiD design exploits the heterogeneous treatment effect of institutional investment on nearby property transactions across distance such that we expect the effect to be the strongest for the closest neighboring property transactions. We also control for time-varying local fixed effects (e.g. county and census tracts) to mitigate the potential selection by institutional investors who can choose to account for house price trends at the broader local level.

Our estimates show that, in the six-month period after the bulk transactions, properties that

[5] https://www.fhfa.gov/Media/PublicAffairs/Pages/FHFA-Announces-Interested-Investors-May-PreQualify-for-REO-Initiative.aspx

[6] The properties that were pre-packaged and sold in bulk mainly belonged to Fannie Mae. See: https://www.gpo.gov/fdsys/pkg/CHRG-112hhrg75726/pdf/CHRG-112hhrg75726.pdf

were closer to the bulk-sold properties (i.e., the treated group) had a greater increase in home value by \$1.33 per square foot, or 1.4% of total value, compared with those located farther from the bulk-sold properties. This result is robust to alternative specifications. For example, our results remain significant after controlling for both backward-looking and forward-looking local characteristics such as, the number of foreclosures and regular property transactions in the inner and outer rings. Our results are qualitatively similar when we use alternative definitions of treatment variables and dependent variables (e.g. total house price, hedonic adjusted and unadjusted house price). Moreover, the spillover effect of institutional investments remain significant even in the long run when we include transactions upto two years after the bulk-sale event. Overall, our baseline results suggest that institutional investments create a significant positive spillover on nearby property prices.

We find that the positive spillover effect of institutional investments is significantly stronger for nearby foreclosed transactions, especially for foreclosed properties that had been listed on the market for a longer time. This result is consistent with the supply effect which is particularly valuable for fire sales (e.g., Shleifer & Vishny 2011). Moreover, the spillover effect is stronger if the transacted properties are of the same age and similar size as the nearby bulk-sold properties. These results further support the supply effect which should be stronger if the transacted properties and the bulk-sale properties are closer substitutes (Anenberg & Kung 2014).

We also look into the role of institutional investors and the asset-pooling design for the efficacy of the FHFA initiative. The asset-pooling design requires investors to accept some of the less desirable properties as they bid for homes that have better growth prospects. As such, liquidity can be channeled to properties that would have been harder to sell if they were listed individually. Moreover, this design requires the participation of institutional investors who can afford to purchase at a large scale. Therefore, both asset pooling and institutional participation are crucial for a successful liquidity provision to the distressed housing markets.

We examine the efficacy of asset-pooling mechanism and institutional participation in two ways. First, we test whether the spillover effect of the bulk-sale event is stronger for areas that had the weakest recovery prospects. While we are not able to identify areas that were expected to be distressed *ex ante*, we use the *ex-post* decline in neighborhood value after the bulk transaction as an empirical proxy. Our estimates suggest that the transaction price for a focal property is higher by \$8.41 per square foot (or 7.4% in total value) after the bulk-sale event if it is located in the bottom-quintile neighborhood in terms of the post-bulk-sale change in neighborhood home value. This effect is more than six times as large as the average effect documented in the baseline specification, suggesting that the bulk transaction is particularly helpful in providing liquidity to areas that investors might be least willing to select. Second, we perform a similar estimation of spillover effect in a sample of foreclosed property transactions by individual buyers in the same counties as those in the bulk transactions during the same time (i.e., June 2012). We find that the average spillover effect is not statistically significant in the sample of individual foreclosed transactions. This is consistent with the important role of asset-pooling design and institutional participation: as individual investors cherry pick foreclosed homes to maximize their investment returns, the spillover effect they create is also muted.

Finally, to confirm the external validity of our findings, we use a large sample of single family homes that were purchased by institutional investors from 2008 to 2016 in 33 MSAs to show that institutional investments in the residential real estate markets are associated with higher subsequent property transaction prices within the same neighborhood. These additional results provide further support on a larger scale for the positive spillover effect of institutional investments in the real estate market.

Our results contribute to the policy debate in dealing with the recent foreclosure crisis that had plagued many communities across the US (e.g., Mian et al. 2015, Agarwal, Amromin, Chomsisengphet, Landvoigt, Piskorski, Seru & Yao 2017, Agarwal, Amromin, Ben-David, Chomsisengphet, Piskorski & Seru 2017, Di Maggio et al. 2017, Piskorski & Seru 2018). Policymakers were faced with the challenge of dealing with the large inventories of foreclosed properties in areas with declining property values. This was particularly challenging as the financial crisis had tightened the overall credit conditions in the economy and impaired the credit of potential families and buyers that could purchase the foreclosed homes. Our evidence shows that the recent growth of institutional investment in the residential real estate market resulted in the provision of much-needed liquidity to this sector that was plagued by the foreclosure crisis. Thus, initiatives such as the REO-to-Rental pilot programs which partner with institutional investors to purchase distressed properties can be effective tools to reduce the inventory of foreclosed properties alongside other government led programs such as the Home Affordable Modification Program (HAMP), and the Neighborhood Stabilization Program (NSP). However, in contrast, initiatives such as the REO-to-Rental programs are more market-driven as opposed to the HAMP and the NSP which were mainly through grants distributed by the government.

Furthermore, the problems due to the foreclosure crisis were compounded as the eviction of many homeowners from their foreclosed properties had created a simultaneous demand for renting. However, this demand for renting remained unfulfilled due to the large inventories of unsold and unoccupied foreclosed properties. Therefore, the prospects of purchasing distressed properties at a discount and converting them to rental properties that pay off as long-term investments can be attractive opportunities for institutional investors (Eisfeldt & Demers 2015), and incentivize them to provide liquidity to the sector. Arguably, such opportunities have led to the exponential growth and institutionalization of the single family rentals.[7] Initiatives such as the REO-to-Rental pilot programs can be appealing for institutional investors who can profit from converting foreclosed homes to income-generating rental properties while at the same time reducing vacancies and rehabilitating distressed neighborhoods and homes with renters. A number of recent studies have examined the post-crisis surge of institutional investment in single family rental and its relation to the local housing markets (Smith & Liu 2017, Allen et al. 2018, Mills et al. 2019, D'Lima & Schultz 2019). Our study contributes to this line of research both by utilizing the REO-to-Rental pilot program to precisely identify liquidity provision by institutional investors, as well as by gauging the efficacy of this policy initiative.

Our results also contribute to the literature on pooling versus selling assets separately (e.g., DeMarzo 2004). Foreclosed properties can sell at deep discounts on an average if some informed investors have the ability to selectively purchase higher quality foreclosed properties, which then increases the adverse selection problems for the uninformed buyers (Lambson et al. 2004, Chinco & Mayer 2015). As a result, the bulk-sale event, in which properties were pre-packaged and pooled before they were sold to institutional investors, can potentially mitigate such adverse selection issues and increase the ease of selling properties even in the most distressed areas. Consistent with this

[7] For example, see the report by three housing activist groups on the rise of corporate landlord: `https://righttothecity.org/cause/rise-of-the-corporate-landlord/`

idea, our results indicate a significant positive price spillover effect from the bulk-sold properties even in the areas where the real estate market had not recovered significantly in the year following the bulk-sale event.[8]

Finally, in response to the foreclosure crisis local municipalities and counties had enacted laws such as the vacant property registration ordinances (VPROs) which require the up-keep of vacant foreclosed properties.[9] The owners of the foreclosed properties, such as banks, are liable to fines and criminal penalties if the VPRO requirements are not satisfied (Immergluck et al. 2012). Our results are important in this context because they indicate that the positive price spillover effect from clearing the foreclosure inventory is mainly driven by the supply effect channel as opposed to the disamenity effect channel. Thus, our results suggest that policy efforts in clearing the stock of foreclosed properties should be prioritized over maintaining the quality of foreclosed properties.

The paper proceeds as follows. Section 2 describes our data and the empirical methodology. Section 3 discuss our identification strategy. Section 4 presents our empirical results and finally Section 6 concludes.

2 Data

2.1 Data source

We obtain data on real estate transactions and assessment for all the states in the U.S. from Zillow. The transaction data contains detailed information such as sales price, transaction date, buyer and seller's identity, mortgage and foreclosure information, among others. The version of our data extract covers transactions up to July 2017. The assessment file includes information collected from local tax assessors' data as of 2016. It provides detailed characteristics of individual properties, including full street address with geocode, number of bedrooms, bathrooms, lot size and building area, and year built, among others. The two datasets can be merged based on a unique parcel ID for each property. While the database provides information on various types of properties, our analysis focus solely on single family homes.

[8] Favara & Giannetti (2017) show that areas with a higher concentration of outstanding mortgages experienced smaller house prices declines during the crisis. Our evidence provides a potential explanation for this result. That is, bulk sales are more likely to take place in areas with concentrated ownership.

[9] e.g., https://www.communityprogress.net/tool-1–vacant-property-registration-ordinances–pages-257.php

2.2 Institutional investors in single family homes

In this subsection we describe our approach to identify institutional investors in single family rental (SFR) and their properties based on information from the 2016 assessment file. We first exclude properties owned by individuals, then identify 2,097 company owner mailing addresses that are associated with at least 100 properties.[10] We manually search each address on the internet to find the identity of the owners, and filter out those not in the SFR business such as home builders, property management companies, and government agencies. We consolidate different addresses that are associated with the same company. At the end, we find 166,635 single family homes owned by 26 institutional SFR investors as of 2016. A report by Amherst Capital Market in 2016 states that there were around 190,000 single family homes owned by institutional SFR investors.[11] Thus our approach covers a majority of the sector. The assessment file also provides the last transaction record for the properties, through which we identify the timing of each SFR investment.

[Figure 1 here]

Figure 1, subfigure (a) shows the number of home purchases by SFR investors from 2007 to 2015. The figure shows that the SFR market became active in 2012 and grew exponentially since then. Table 1, Panel A lists the top institutional SFR investors that own at least 1,000 properties. The top four institutional investors: Invitation Homes, American Home 4 Rent, Starwood Waypoint, and Progress Residential, account for close to 70% of the market.[12] Thus, this is a highly concentrated market. The SFR market is also concentrated geographically. Figure 1, subfigure (b) and Table 1, Panel B show the geographic distribution of SFR properties at the MSA level. The data show that SFR investments are concentrated in 17 states, among which Georgia, Florida, Arizona, Texas, North and South Carolina, Nevada, and California account for close to 90% of the market. These areas were also among the most distressed real estate markets during the financial crisis in 2008.

[Table 1 here]

[10] The database also provides information on the name of company owners. These names, however, are often recorded with typos, abbreviations, or as various subsidiaries of a parent company. We therefore use mailing address to identify unique investors.

[11] Source: `https://www.amherstcapital.com`.

[12] On August 10, 2017, Invitation Homes and Starwood Waypoint Homes announced a merger, creating the nation's largest SFR investor. Source: `https://www.reuters.com/article/us-starwood-waypoin-m-a-invitation-homes-idUSKBN1AQ1B1`

In Figure 2 we plot the empirical relation between single family home purchases by institutional investors and future neighborhood home prices measured by Zillow's monthly neighborhood home value index. Subfigure (a) shows that institutional investment is negatively related to neighborhood home value in the subsequent two years. However, Subfigure (b) shows that institutional investment is positively related to neighborhood home value in the third and fourth years afterwards. This pattern suggests that institutional investors may have selectively invested in housing markets that are more distressed in the short run (Allen et al. 2018), but have greater growth potential in the long run. Another possible interpretation is that institutional investment has causally increased the local real estate prices in the long run. However, to distinguish between the various selection and treatment effects that might be mutually driving the empirical relation, we employ a identification strategy which we discuss in details in the following sections.

[Figure 2 here]

2.3 REO Bulk Transaction Pilot Program

To identify the causal effect of SFR investment on the local distressed real estate markets, we exploit institutional investment through FHFA's REO bulk sale initiative. The program allows qualified investors to purchase portfolios of distressed properties with the requirement of turning these purchased properties into income producing properties for a specified number of years. The initiative was targeted to the hardest-hit areas to test a new asset disposition model. The goal of this pilot was to determine whether this disposition model would generate private investment in single-family rental housing efficiently and effectively to stabilize local markets.[13] Fannie Mae discloses the three REO bulk transactions under the pilot phase, all of which were sold through an auction on June 25, 2012.[14] Under this program, SFR investors can only bid on REO portfolios prepackaged by Fannie Mae and are thus not allowed to cherry-pick on specific properties. Hence, the properties in these portfolios represent a sample that, at least at the individual property level, is not subject to the endogenous selection by institutional investors. We find the list of portfolio properties from the operation agreements, and match the properties with the geocode based on the full street address and identifying neighboring home transactions from six months before to six

[13] https://www.fhfa.gov/PolicyProgramsResearch/Policy/Pages/Real-Estate-Owned-(REO).aspx

[14] https://www.fhfa.gov/PolicyProgramsResearch/Policy/Pages/Real-Estate-Owned-(REO).aspx

months after the auction date. There are 1,763 properties in total that were sold through these three transactions.

[Table 2 here]

Table 2 presents a brief summary of the three transactions, which sell seven pools of properties to three winning bidders. The three transactions cover 1,763 properties located in Florida, Illinois, Arizona, California, and Nevada. All seven portfolios have vacancy rate around 32%–38%. The three portfolios of Florida and one portfolio of Illinois properties were transacted at valuations that were 4.2% and 13.8% below third party valuations, while three portfolios of properties in Arizona, California, and Nevada were transacted above third party valuation by 12.3%. The winning bidders are subject to a 90-day sale prohibition and, for each sub-portfolio, a limit of 10% of the total properties that can be sold in each of the subsequent three years from the closing date.[15]

Figures 3 presents the geographic distribution of REO properties involved in the three bulk transactions. The maps show that, except for those in Nevada, the properties for each pool are scattered across multiple counties within the states. This is consistent with the intention of FHFA to avoid investors from cherry-picking on specific properties.

[Figure 3 here]

2.4 Sample selection

We start with the five states involved in the three bulk sale transactions: Florida, Illinois, Arizona, California, and Nevada. We apply the following filters to the sample. First, to examine the spillover effect of institutional SFR investment, our analysis focuses on transactions that do not involve institutional SFR investors. Thus, we include only single family home transactions between individual buyers and sellers with no missing sales amount.[16] We exclude deed types (e.g., quitclaim deeds) that are typically used for intra-family transfers or gift transfers, and remove partial interest transfers. Further, we require an observation to have no missing transaction date, zip-code, building area, buyer/seller identity, and remove erroneous records such as those with built year later than

[15] See `https://www.fhfa.gov/PolicyProgramsResearch/Policy/Documents/SFREO2012DealTermF0.pdf`

[16] In fact, majority of the SFR investments cannot be found in the transaction file. This is possibly because majority of these properties were purchased through foreclosure auctions or lenders' REO inventory.

transaction year. To avoid the result from being driven by outliers, we also exclude homes with size smaller than 500 square feet, and truncate the sample at the 1st and 99th percentile of sales price for each state.

2.5 Hedonic Regression

We follow the literature and apply hedonic regression to account for price differences due to house quality and local homeowners' preferences (e.g., Campbell et al. (2011)). Specifically, we run the following regression for each year and MSA separately:

$$Price_i = \alpha_1 + \gamma_1' X_i + Zipcode_i + Month_i + \epsilon_i. \tag{1}$$

$Price_i$ is either price per square foot or log total price for transaction i, X_i refers to a set of house characteristics similar to previous studies (e.g., Campbell et al. (2011)). The independent variables include: fourth-order polynomials of building area in square feet, fourth-order polynomials of house age and remodel age, four binary variables indicating house with one bathroom, one and half bathrooms, two bathrooms, and more than two bathrooms, and four binary variables indicating houses with one bedroom, two bedrooms, three bedrooms, and more than three bedrooms.[17] We also include zipcode and month fixed effects. The residuals from the regressions are the adjusted unit home prices that we use for further analysis.

3 Identification strategy

This section discusses the challenges in identifying the causal effect of REO bulk sales on local distressed real estate markets. We have the data on the precise locations of the REO properties that were bulk-sold, as well as the locations and the prices of other regular arms-length property transactions. Therefore, an intuitive approach is to test whether properties that are close to the bulk-sold properties transact at different prices compared to the properties that are farther away

[17] We do not include are more specific property features such as garage, heating and cooling system, roof and building shape as some other studies do (e.g. Adelino et al. (2012)) because those variables are not well populated.

from the bulk-sold properties. However, there are two potential endogeneity concerns with this approach. First, the quality and characteristics of the homes that are close to the bulk-sold properties could be different from the homes that are far away from the bulk-sold properties resulting in different transaction prices. Second, house prices could trend differently for areas that are close and areas that are far away from the bulk-sold properties because they are exposed to different local economic shocks. To mitigate the first endogeneity concern, we employ a hedonic regression as defined in Equation 1 to strip away price differences which result from differences in house characteristics and local homeowners' preferences. We then use the residual from the hedonic regression as a measure of house price in our analysis.

To mitigate the second potential concern we first focus on a short window of [-6,+6] months around June, 2012, which is the month in which the REO bulk sale transactions took place through an auction. Next, similar to Campbell et al. (2011) and Anenberg & Kung (2014), we use a DiD approach that compares home prices before and after the REO bulk sale transactions, and the prices of homes that are close to bulk-sold properties with those that are farther away. We define "close" as being within a radius of 0.25 miles and "far" as being within a radius of 0.50 miles. The changes in the prices of homes that are within 0.5 miles from the bulk-sold homes act as a control for any potential local confounding house price trends. Further, recall that the bulk-sold REO homes were prepackaged by Fannie Mae and then offered to investors. Therefore, the portfolio of bulk-sold properties, at least at the individual property level and the hyperlocal regions around it (i.e within 0.5 miles), are less likely to be subject to the endogenous selection by investors. However, this does not rule out the possibility that the investors accounted for house price trends at a broader geographic level such as the county level before purchasing Fannie Mae's prepackaged REOs in bulk. Therefore, to mitigate this endogenous selection at the broader geographic level, we control for time-varying county house price trends using *County*$\times$*Year-month* fixed effects and time-invariant census-tract level unobserved factors using *Census-tract* fixed effects.

3.1 Empirical specification

To examine the effect of the REO bulk sale to investors on local real estate markets we estimate the following regression specification:

$$P_{i,t} = \alpha + \beta_1 Post_t \times BS_i^{Close} + \beta_2 BS_i^{Close} + f(\mathbf{X}_{i,t}) + \gamma_{c,t} + \delta_s + \varepsilon_{i,t} \tag{2}$$

where i, and t index a single family home, and year-month, respectively. $P_{i,t}$, which is the dependent variable of interest, is the transaction price for the single family home i that is sold at time t through a regular transaction that is neither related to the bulk transactions nor purchased by other institutional investors. We refer to transaction i as the *focal* transaction. For our analysis $P_{i,t}$ is measured using the adjusted transaction price per square foot or log total price, which are the residuals obtained from the hedonic regression defined in Equation 1. The sample used for the baseline test includes transactions in the six months before and six months after the bulk transactions, excluding the event month (June, 2012). $Post_t$ is an indicator variable equal to 1 for all year-month observations from July, 2012 onwards, and equal to 0 prior to June, 2012. BS_i^{Close} is a binary treatment variable which takes the value of 1 if the closest bulk-sold REO is within 0.25 miles from focal transacted property, and is 0 otherwise. Figure 4 illustrates the binary definition of treated and control group using five census tracts in Maricopa County, AZ.

[Figure 4 here]

The main coefficient of interest is β_1, which is associated with the variable $Post_t \times BS_i^{Close}$. Thus, β_1 captures changes in the sales price of homes that are located within 0.25 miles of a bulk-sold REO, relative to homes that are located within 0.25–0.50 miles of a bulk-sold REO, from the pre- to the post-REO bulk sales transaction event period. $\mathbf{X}_{i,t}$ is a vector of time-varying control variables such as the number of foreclosed homes close to the transacted property i. $\gamma_{c,t}$ represents *County*$\times$*Year-month* fixed effects, which control for time-varying factors at the county level that drive house price trends. δ_s represents *Census-tract* fixed effects, which control for time-invariant differences in house prices at the census-tract level. $\varepsilon_{i,t}$ is the error term.

4 Empirical Results

4.1 Summary statistics

Table 3 shows the summary statistics for the focal transaction properties that are close to the bulk-sold REOs and those that are far away. Panel A shows the summary statistics for property transactions before the REO bulk sale event. The treated group consists of property transactions that are within 0.25 miles of the bulk-sold REOs, while the control group consists of properties that are within 0.25–0.50 miles of the bulk-sold REOs. The statistics show that the transacted properties that are close to the bulk-sold REOs have a lower price and price per square foot compared to the transacted properties that are farther away from the bulk-sold REOs before the REO bulk sale event. This is consistent with the negative price spillovers of REOs which are strongest at short distances and dissipate as distance increases (Anenberg & Kung (2014), Gerardi et al. (2015)). It is also consistent with Fannie Mae's statement that their REO-to-Rental pilot program was targeted at the disposition of their REO inventory in the hardest-hit areas. For instance, the discount in house prices within 0.25 miles of the bulk-sold REOs relative to those within 0.25–0.50 miles from bulk-sold REOs is 11.6% in terms of total price, and 5.7% in terms of price per square foot. The properties that are closer to the bulk-sold REOs are also smaller and newer. Differences along these dimensions are an important consideration for the spillover effects of foreclosures and REOs through the disamenity channel to the extent that smaller and newer properties require lower maintenance. On the other hand, it is likely that smaller homes with fewer amenities are an indication of lower income neighborhoods. If lower income neighborhoods suffered greater wealth shocks during the great recession, then individuals in these neighborhoods may not have the financial means to maintain their home leading to a greater disamenity effect on nearby properties.

[Table 3 here]

While we can control for some of these observable differences between the focal properties that are close versus far away from the bulk-sold REOs, a potential concern is that such properties may also differ on unobserved dimensions that drive home price changes around bulk-sale events and thus confound our interpretation. Arguably, this potential concern is likely to be more important if the homes sold in the control and treated group in the pre-bulk-sale period differ substantially even on observable dimensions when compared to the homes sold in the post-bulk-sale period. To perform this sanity check, in Panel B of Table 3 we report the characteristics of traded homes in the treated and control groups in the post-bulk-sale period. By comparing home characteristics in Panels A and B, we see that the properties which were sold within the treated and the control group were similar on observable dimensions such as building area, lot size, age and the number of bedrooms and bathrooms, before and after the REO bulk sale event. However, the transaction prices were higher in the post-REO bulk sale period. To the extent that these higher prices are affected by unobserved characteristics, and to the extent that these unobserved characteristics affect the treated and control groups similarly across time, we can difference out such price effects using our DiD specification.

4.2 Effect of REO bulk sales: Baseline results

Table 4 shows the baseline results for our focal property transactions using a (-6,6) month event window around the bulk-sale event month of June, 2012. The dependent variable is the adjusted transaction price per square foot or log total price of the focal property. The coefficient of interest in Column (1) is associated with the interaction term $\textit{Post-sales} \times \mathbb{I}(\textit{Distance} < 0.25)$. $\mathbb{I}(\textit{Distance} < 0.25)$ is an indicator variable that takes the value of 1 if the closest bulk-sold REO is within 0.25 miles from focal transacted property, and is 0 otherwise. The point estimates in Column (1) indicate that a purchase of foreclosed property by institutional investors within a 0.25-mile distance from a bulk-sold property (the treated group) increased home value by $1.33 per square foot more than those located further away from a bulk-sale property (the control group). The estimates in Column (2) suggests that the treated group experienced a 1.4% increase in total value relative to properties that are further away. Importantly, the specifications in Columns (1) and (2) include *County*×*Year-month* fixed effects, which control for confounding time-varying house price trends at the (broader) county-level, and *Census-tract* fixed-effects, which control for time-invariant secular house price trends at the (narrower) census-tract level. This evidence is consistent with prior literature that documents a strong negative spillover effect of REOs on nearby properties (Anenberg & Kung (2014), Gerardi et al. (2015)). However, the coefficient associated with *Post-*

$sales \times \mathbb{I}(Distance < 0.25)$ is positive and statistically significant, which suggests that transacted properties that are closer to the bulk-sold REOs experienced a greater price increase *after* the REO bulk sale event relative to the control group. These results are consistent with one of the goals of the REO pilot program, which was to stabilize the local real estate market.

[Table 4 here]

4.3 Effect of REO bulk sales: Robustness

We show that our baseline results in Table 4 survive placebo tests and are also robust to using alternate model specifications or variable definitions.

[Figure 5 here]

First, we examine the parallel-trend assumption for our DiD specification. In Figure 5, we plot the difference in adjusted price per square foot between the treated (i.e. those within 0.25 miles from bulk-sold properties) and the control group (those in 0.25-0.5 mile from bulk-sold properties) from 6 months before to 6 months after the REO bulk sale events. The figure shows that the difference increased only after the bulk sale events. We also perform a placebo test using a pseudo bulk-sale event month of December, 2012, which is six months prior to the actual bulk sale event month of June, 2012. We reestimate the specifications in Table 4 using a (-6,6) month event window around the placebo event month in Table 5. In the absence of the bulk-sale event we find no evidence for a significant differential trend in transaction prices for homes that are close versus far way from the bulk-sale properties. In fact, the transaction prices for homes that were close to the bulk-sale properties during our placebo period seem to decrease over time as evidenced from the negative coefficient associated with the interaction of *Post-sales (placebo)* and the proximity measure in Column (1) and (2). This evidence is consistent with the lasting negative spillover effects of the unsold foreclosed homes on neighboring house prices (Harding et al. 2009, Anenberg & Kung 2014, Gerardi et al. 2015).

[Table 5 here]

In Table 6 we also show that our results are robust to controlling for the number of foreclosures and other home transactions around the focal property within the inner and outer circle of 0.25

miles and 0.50 miles, respectively. This allows us to control for house price trends due to other real estate events and transactions in the vicinity of the focal property that can affect the focal property's transaction price. For instance, the neighborhood around a focal property could be experiencing an increase in the number of home sales or a reduction in the number of foreclosures that is unrelated to the bulk-sale event. The spillover effects resulting from such changes can potentially confound our results. Thus, we control for the number of real estate events before and after the focal transaction to flexibly control for the variation in spillover effects across focal transactions resulting from differential changes in the market condition of the surrounding area.

[Table 6 here]

Table 6 shows that the coefficient on $\textit{Post-sales} \times \mathbb{I}(\textit{Distance} < 0.25)$ is slightly smaller and is estimated with larger standard errors because its effect has been absorbed to some extent by the number of foreclosures and other home transactions in the inner and outer ring around the focal property. However, we find that the positive spillover effect of the REO bulk sale event on nearby property transactions is robust even after flexibly controlling for the number of real estate events prior to and after the focal transaction. Our point estimates suggest that the transaction prices for the treated group are higher by \$1.20 per square foot (or 1.2% in total value) relative to the control group. We also find that an additional foreclosure within 0.25 miles in the six months prior to the bulk-sale event is related to lower home value by \$0.11 per square foot, consistent with the previous studies (see Lin et al. 2009, Campbell et al. 2011, Anenberg & Kung 2014, Gerardi et al. 2015)). Furthermore, we find that an additional home sales within 0.25 miles in the six months prior to the bulk-sale event has a *positive* spillover effect of \$0.23 per square foot. This result is consistent with the positive spillover effect due to the reduction in the supply of homes available for sale in the vicinity of the focal property. The positive spillover from the bulk-sale event is also consistent with the supply effect because the bulk-sale event also reduces the supply of homes available for sale in the vicinity of the focal property. Our estimates also show that neither foreclosed or regular transactions that happen outside of the 0.25 mile radius from a focal transaction affect its price. Thus the short-term spillover effect is highly local.

We perform further robustness checks and report the results in the Appendix. In Table A1, we control for unobservable time varying neighborhood characteristics using census-tract × year-quarter fixed effects.[18] The DiD estimate remains statistically significant and similar in magnitude to the baseline estimates. We also show that the effect of the bulk-sale event lasts over longer time-periods when we expand our sample along the time dimension. In Table A2 we consider (-6, 24) month event window around the bulk-sale event instead of the (-6, 6) month event window in our baseline results. In Columns 1 and 2, when the sample includes transactions within 0.5 miles from the bulk-sale properties, the difference in total house price between the treated and control transactions is only 0.6% and significant at 10% level. A possible explanation for this result is that, in the longer term, the spillover effect went further away from the bulk sale properties. To test this conjecture, we expand the sample by including transactions within 1 mile from a bulk-sale property. We maintain the definition of our treatment variable such that a proximate home transaction ("treated") is still defined to be within 0.25 miles of a bulk-sale property. Consequently, the set of "control" house transactions increases due to the expansion of the outer ring from 0.5 miles to 1-mile. Our results in Columns 3 and 4 show that, over a two-year horizon, the average impact of the bulk-sale event on properties located within a 0.25-mile distance is 1.2% relative to neighboring properties that are outside the 0.25-mile but within the 1-mile distance. This results supports our interpretation that the liquidity effect spreads further away from the bulk-sale transactions over time.

Table A3 shows that our results are robust to using alternate measures of a focal property's proximity to a bulk-sale transaction. In Column (1) we use the negative log distance between the focal transaction and the nearest bulk-sale transaction in miles. The coefficient for the interaction term between *Post-sales* and proximity is significantly positive, suggesting that houses closer to the bulk-sale properties experienced a greater appreciation after the bulk-sale transactions. In Column (2) we count the number of bulk-sold properties within 0.25 miles of the focal transaction as opposed to our baseline specification which considers just the closest bulk-sale REO within the 0.25 miles. Our point estimates suggest that the presence of an additional bulk-sold property within 0.25 miles increases the transaction price of the focal property by 0.7% after the bulk transaction. In Column (3) we show that the number of bulk-sold properties within 0.25–0.50 miles of the focal property have no effect on the transaction price of the focal property. This suggests that only the close

[18] To preserve statistical power while controlling for unobservables at a highly local level, we use time fixed effects at the year-quarter as oppose to the year-month level for this test.

bulk-sale REOs have spillover effects on the focal property transaction which is consistent with the hyper-local spillover effects of neighboring transactions documented in the extant literature. In Column (4) we use a weighted number of bulk-sale properties within 0.25 miles of a focal property such that a bulk-sale property that is closer to the focal property receives a higher weight.[19] Our results are stronger when we use the weighted number of bulk-sale properties. We find that house prices increase by 3% when there is an additional bulk-sale property at the same location as the focal property. The significantly stronger effect of the bulk-sale event using our weighted approach makes it more likely that our results are capturing the effect due to the bulk sales as opposed to other confounding neighborhood events that can affect the focal property's transaction price.

5 Effect of REO bulk sales: Heterogeneous treatment effects

In this section we examine the heterogeneous effect of the bulk sale event on different kinds of focal properties. We exploit the cross-sectional variation across the focal properties along two main dimensions, namely, (a) whether the focal property is itself a distressed sale, and (b) how similar the focal property is to the closest bulk-sold REO property. For instance, a focal property undergoing a distressed sale, such as a foreclosure, should be more likely to benefit from nearby REO bulk-sale transactions because they reduce the inventory of the listed homes for sale. As a result, the distressed focal property should be able to undergo a sale more quickly and with a lower discount when buyers are competing among a fewer number of properties after the bulk-sale event as opposed to a larger number of properties before the bulk-sale event.

The positive spillover effect on distressed focal properties is also more likely if the markets for regular and distressed sales are segmented. That is, the buyers who have a higher demand for foreclosed properties, such as real estate investors, should be willing to purchase the foreclosed properties at a higher price after the bulk-sale event because the bulk-sold properties themselves were foreclosed REO properties. Similarly, to the extent that buyers' demand is home-specific, the positive spillover effect on focal property's transaction price should be higher when the focal

[19] A bulk-sale property that is at a distance of d from the focal property is assigned a weight of $\frac{0.25 - d}{0.25}$. Thus, a bulk-sale property that is located exactly at the focal property is assigned a weight of 1, and a bulk-sale property that is located 0.25 miles from the focal property (i.e., on the periphery of the inner ring of radius 0.25 miles) is assigned a weight of 0.

property and the closest bulk-sold property are closer substitutes. Thus, the positive spillover effect on the focal property's transaction price should be greater if it is more similar to its closest bulk-sold property on dimensions such as size (e.g., square footage), type (e.g., single family vs. condo), and age.

5.1 Heterogeneous treatment effect on foreclosed properties

We classify focal property transactions as distressed sales if they were foreclosed before the sale. Foreclosed properties, which are repossessed by lenders (usually banks), can be sold in foreclosure auctions. However, if the foreclosure auction is unsuccessful then the foreclosed property is transferred to the lender who then subsequently lists and sell property (REO sale). In the former case the time between the foreclosure of the focal property and its sale is shorter than in the latter case of an REO sale. Conditional on a property's foreclosure, a longer time between the foreclosure event and the sale can potentially signal the difficulty of the lender in selling the repossessed distressed property. Thus, we consider both whether, and how long, a focal property transaction has been in foreclosure prior to its sale. Table 7, Panel A shows that the price spillover effect of the bulk-sale event on a foreclosed focal property transaction is greater than a regular (non-foreclosed) focal property transaction. A foreclosed focal property that is within 0.25 miles of a bulk-sale property experienced an increase in home value by $4.25 per square foot or 4.3% of total value after the bulk-sale event relative to those located within 0.25–0.50 miles of a bulk-sale property. In fact, the spillover effect of the bulk-sale event on a foreclosed focal property is more than twice the spillover effect for an average treated focal property.

[Table 7 here]

In Panel B of Table 7, we consider only foreclosed transactions in the sample, and test the spillover effect of the bulk-sale events on foreclosed focal property transactions conditional on the time between the foreclosure event and sale of the property. However, we consider only the set of foreclosed transactions in Panel B. We find that the positive spillover effect due to the bulk-sale event is greater for distressed focal properties that were on the market for a longer time. Our point estimates suggest that for a foreclosed focal property that was on the market for 1% longer than the average time it takes to sell a foreclosed house increases the DiD estimate, and hence the treatment effect of nearby bulk transactions, by 0.63%.

5.2 Heterogeneous treatment effect on similar properties

In Table 8 we consider whether the spillover effect of the bulk-sale event is greater for focal properties that are close substitutes to the bulk-sale properties. We consider two properties to be close substitutes if they are of the same type (e.g., single family vs. condo) or if they have a similar size (e.g., square footage), or age. As all our focal properties are single family homes, a focal property is classified to be of similar type when the bulk-sale property is also a single family home. For comparing along the age and size dimensions we define a continuous measure of similarity as the negative of the absolute difference between the age and size of the focal property and its nearest bulk-sale property.[20]

[Table 8 here]

In Table 8, Columns (1) and (2) a focal property is classified as "similar" if the focal property and its closest bulk-sale property are of the similar size. Our point estimates suggest that the transaction price for a focal property is higher by \$2.29 per square foot (or 1.6% in total value) after the bulk-sale event relative to other focal property transactions if it has the same size as its closest bulk-sold property within 0.25 miles. In Columns (3) and (4), we define the similarity between two properties in terms of their age. The coefficient estimates indicate that the transaction price for a focal property is higher by \$3.75 per square foot (or 2.5% in total value) after the bulk-sale event relative to other focal property transactions if it is of the same age as its closest bulk-sold property within 0.25 miles. In Columns (5) and (6), we define a bulk-sale property as similar to the focal property if it is also a single family house. Although weaker, our results suggest that a focal property that is similar in its type to its closest bulk-sold property transacts at a higher price after the bulk-sale event relative to other focal property transactions.

[20] We compute the absolute difference in terms of percentage for the size dimension, and levels for the age dimension.

5.3 Heterogeneous treatment effect for areas with low house price growth

In Table 9 we test whether the spillover effect of the bulk-sale event is stronger for the focal properties in areas with the weakest recovery prospects. This test is particularly relevant for assessing the efficacy of selling REOs in pools because it captures properties that would have been harder to sell if they were listed individually. While we are not able to identify areas that were expected to be distressed ex ante, we use the *ex-post* decline in neighborhood value after the bulk transaction as an empirical proxy. We classify the neighborhoods in which the bulk-sold properties are located into quintiles based on house price growth over the one year after the bulk-sale event using a neighborhood-level house price index from Zillow.com. We define neighborhoods using Zillow's neighborhood boundary files which is a database of 17,300 neighborhoods as defined by Zillow.[21] Our sample drops by roughly half because Zillow classifies neighborhoods only in the largest cities in the US. Subsequently, we compare the heterogeneity of the positive spillover effect from the bulk-sale event in more distressed real estate markets – i.e., neighborhoods with the lowest house price growth after the bulk-sale event (the first quintile) – compared to less distressed real estate markets (the second to fifth quintiles). Our point estimates suggest that the transaction price for a focal property is higher by \$8.41 per square foot (or 7.4% in total value) after the bulk-sale event if it is located in the bottom-quintile neighborhood in terms of the post-bulk-sale change in neighborhood home value. This effect is more than six times as large as the average effect documented in the baseline specification, suggesting that the bulk transaction is particularly helpful in providing liquidity to areas that investors might be least willing to select.

[Table 9 here]

Our results in Table 9 provide evidence against the selection effect which suggests that our positive spillover effect is confounded by the decision of the institutional investors to purchase the bulk-sale properties because they expected high house price growth in the areas where the bulk-sale properties were located. Further, the results in Table 9 are consistent with the results in Table 7 which show that the positive spillover effect of the bulk-sale event on nearby house prices was greater for the more distressed properties. More importantly, our results also show the

[21] See `https://www.zillow.com/howto/api/neighborhood-boundaries.htm`

effectiveness of selling the REO properties in bulk by pooling properties in the more distressed areas with the less distressed areas as opposed to selling them individually. First, in addition to lower observable quality, REO properties are also likely to have lower unobservable quality which can lead to adverse selection issues. Moreover, such adverse selection problems can be compounded if some investors are informed and can selectively purchase higher quality REO properties leaving the poorer quality REO properties for the uninformed investors (DeMarzo 2004). Thus, a bulk-sale event, in which properties are pre-packaged and sold, can potentially mitigate such adverse selection issues thereby increasing the ease of selling the remaining REO properties even in the most distressed areas. Our results in Table 9 are consistent with this interpretation of bulk-sales easing the adverse selection issues in the most distressed areas.

5.4 Comparison with individual investors

In this section, we compare the spillover effects between the bulk-sold properties and the properties sold individually (i.e., individually-sold properties) to investors. The asset-pooling design of the REO bulk-sale initiative does not allow investors to cherry pick and thus requires investors to accept some of the less desirable REO properties. However, without bulk sales, investors can cherry pick properties to maximize their investment returns, which subjects the remaining pool of unsold properties to adverse selection. Further, the degree of adverse selection is likely greater in the most distressed areas where houses tend to be of poorer observable and unobservable quality. Thus, while individually-sold properties can also generate a positive price spillover on neighboring homes due to the liquidity effect, this effect can be mitigated by the adverse selection effect which depress neighboring property prices. Consequently, we should expect the spillover effect for individually-sold properties to be weaker than bulk-sold properties. To test our conjecture, we perform a similar estimation of spillover effect in a sample of foreclosed property transactions in the same counties as those in the bulk transactions during the same time (i.e., June 2012), but for which the buyers were individual investors who bought one or a few properties at a time. In Table 10, our estimates show that the average spillover effect is not statistically significant in the sample of individual foreclosed transactions. We also do not find a significant spillover effect in the most distressed areas in the sample. These findings are consistent with our interpretation that the efficacy of the REO program requires: first, the participation of institutional investors to allow large-scale purchases; second, the pre-packaging of portfolio prior to transactions to avoid cherry picking.[22]

[Table 10 here]

6 Conclusion

In this paper, we document the emergence of single family rental as a new asset class for institutional investors and examine the role of institutional investment in the local real estate markets. We find causal evidence that properties that are purchased by institutional investors have a positive spillover effect on nearby home values. This positive spillover effect is greater for nearby homes that are more distressed and homes that are close substitutes to the properties purchased by institutional investors.

Our results suggest that, while institutional investors pursued an emerging investment opportunity in the distressed residential real estate market, they also provided valuable liquidity to the sector. This liquidity provision is especially important in an environment where credit markets are tight and short-term economic prospects are uncertain. We also show that the positive spillover effect is particularly pronounced for properties in the most distressed neighborhoods. This evidence supports the institutional design of selling distressed properties in prepackaged pools, which reduces adverse selection problem and channels liquidity to the most needed local areas.

[22]In unreported tests, we find the spillover effect statistically significant in models without census tract fixed effects. Thus, individual investors likely also create liquidity to the surrounding neighborhoods, but not as much as institutional investors do.

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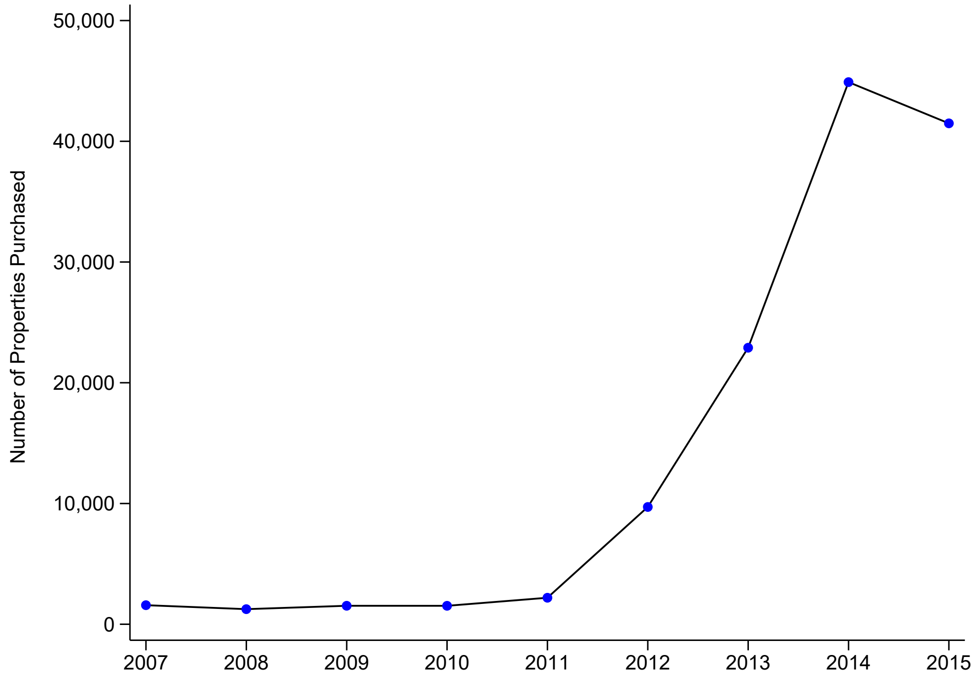
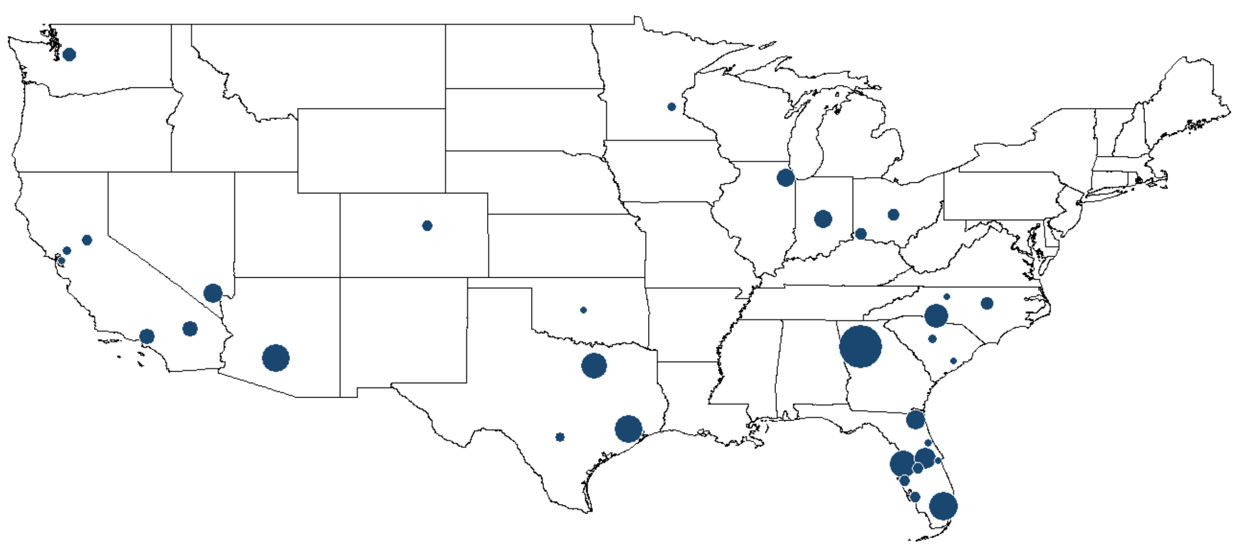
Figure 1: Growth and Distribution of SFR Holdings by Institutional Investors

The figure presents the growth and distribution of single family rentals (SFR) holdings by insitutional investors. Subfigure (a) shows the growth of SFR purchases over time from 2007–2015. Subfigure (b) shows the geographic distribution of SFR holdings as of 2015 across MSAs. The size of each dot is proportional to the market share of SFR holdings by institutional investors in a given MSA relative to the total number of SFR holdings in the U.S.



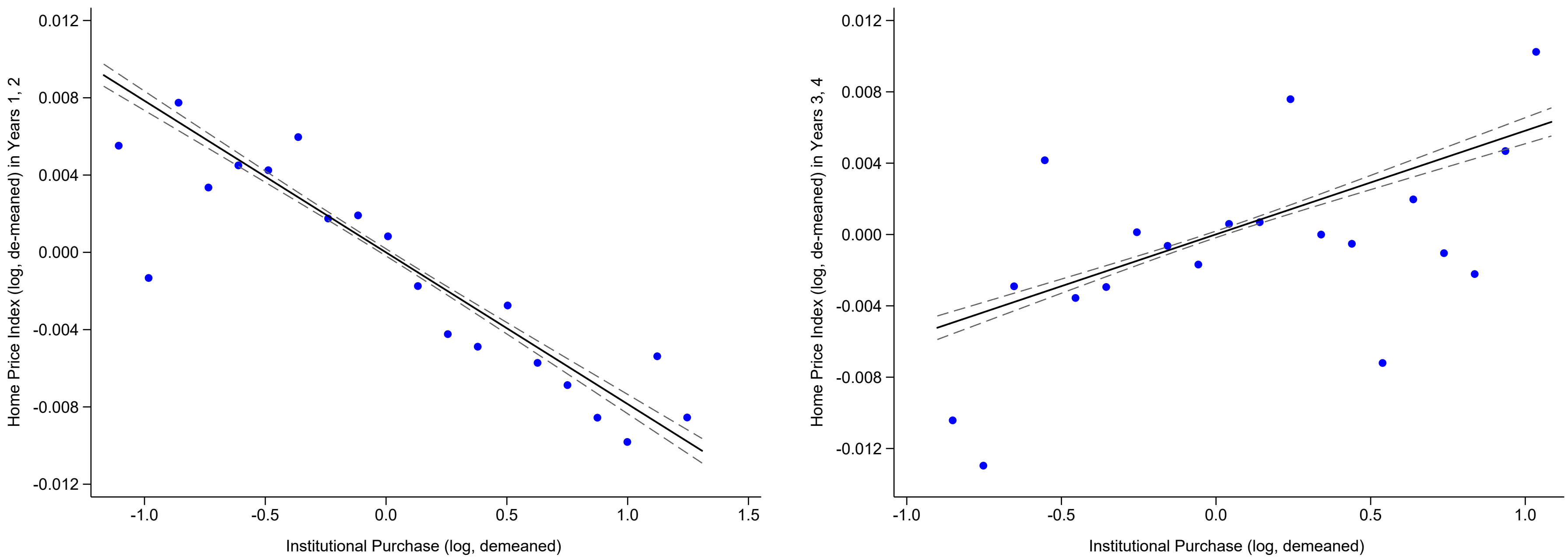
(a) *SFR Purchases by Institutional Investors 2007–2015*



(b) *Geographic Distribution of SFR Holdings by Institutional Investors*

Figure 2: Institutional Investment and House Prices

This figure presents the empirical relation between institutional investment in single family homes and future home prices in the same neighborhood. The sample covers 33 MSAs with institutional investment from 2008 to 2016. The vertical axis denotes the natural logarithm of Zillow's monthly neighorhood home value index. The horizontal axis denotes the natural logarithm of one plus the number of home purchases by institutional investors in the Zillow neighborhood in past two and four years. The variables are demeaned to account for county × year-month fixed effects and neighborhood fixed effects.

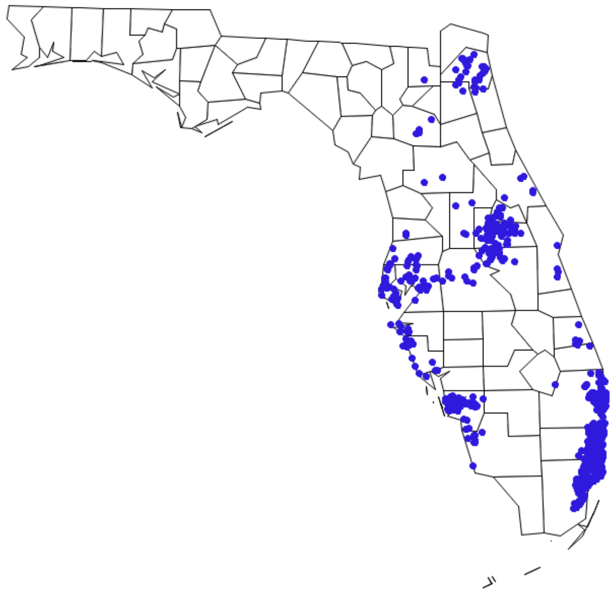


(a) *Institutional Investment and Future Home Prices (Year 1-2)*

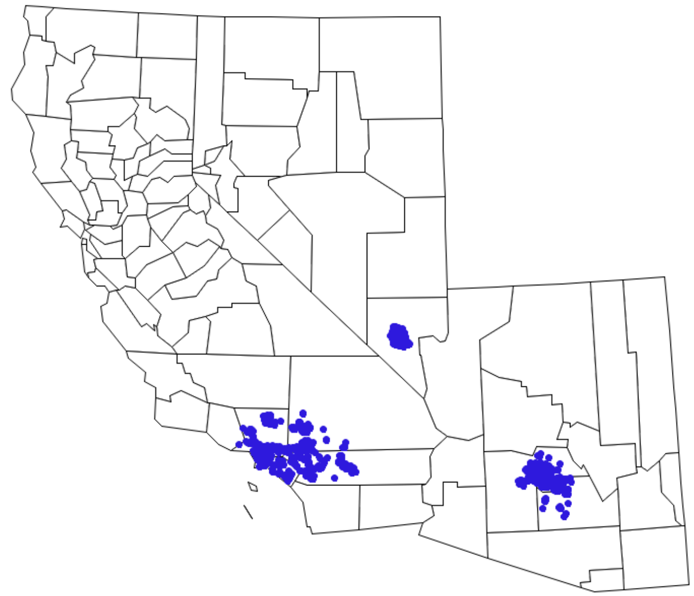
(b) *Institutional Investment and Future Home Prices (Year 3-4)*

Figure 3: Bulk Sale Transactions

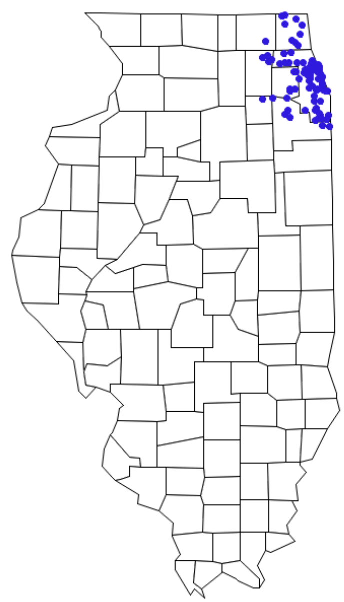
This figure presents the distribution of the bulk sale transaction that were part of the FHFA's REO bulk sale pilot program in 2012. The figure shows the geographical distribution of the three REO bulk sale transactions disclosed by Fannie Mae which were sold through an auction on June 25, 2012. The geographic distribution of three bulk sales transactions, namely, Florida, West (Arizona, California, and Nevada), and Chicago, is shown below. REO portfolios were pre-packaged and sold by Fannie Mae. As a result, institutional investors bidding in the REO bulk sale auction were not allowed to cherry-pick specific properties.



(a) *Florida Bulk Sale*



(b) *West Bulk Sale*



(c) *Chicago Bulk Sale*

Figure 4: Illustration of Treated and Control Properties in Maricopa County, AZ

This figure illustration the definition of treated and control observations using five census tracts in Maricopa County, AZ. The inner and outer circles have radius of 0.25 mile and 0.5 mile, respectively. The black circles indicate bulk-sale properties. The green triangles are control transactions that do not have a bulk-sale property within and 0.25-mile radius. The blue diamonds are treated transactions with $N(Distance < 0.25mi) = 1$, and the blue square is also a treated transaction with $N(Distance < 0.25mi) = 2$.

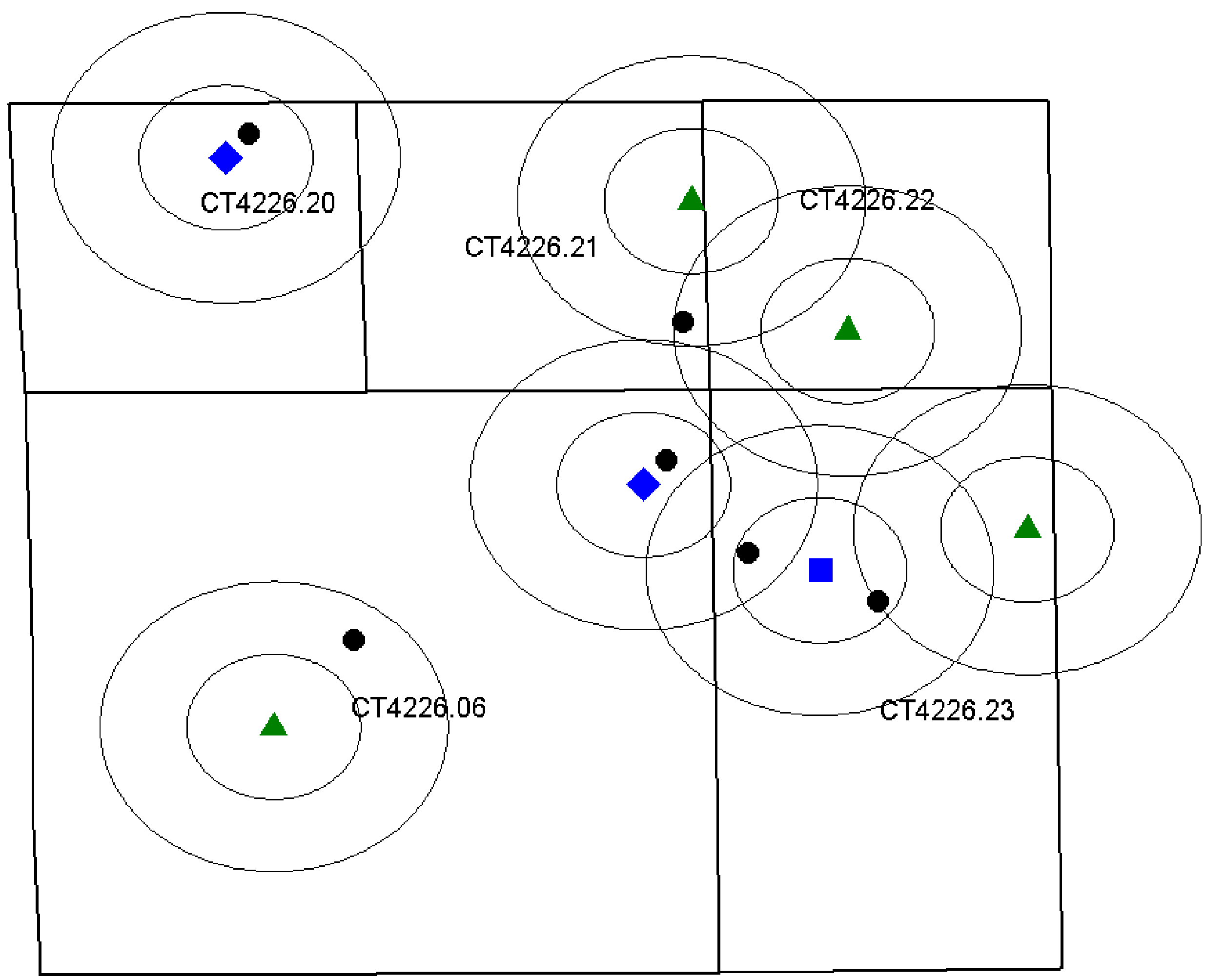


Figure 5: Transaction Prices for Treated and Control Properties Around Bulk-sold Properties

This figure shows the difference in transaction prices between the treated and control groups before and after the bulk sale event. The figure plots point estimates for the interaction between the treatment indicator and the leading and lagging monthly indicators over six months before and after the bulk sale event similar to Equation (2). The transaction price is the adjusted price per square foot which is the residual from hedonic regression in Equation (1). The treated group consists of transacted properties that are within 0.25 miles from a bulk-sold property. The control group consists of transacted properties that are within 0.25–0.50 miles from a bulk-sold property.

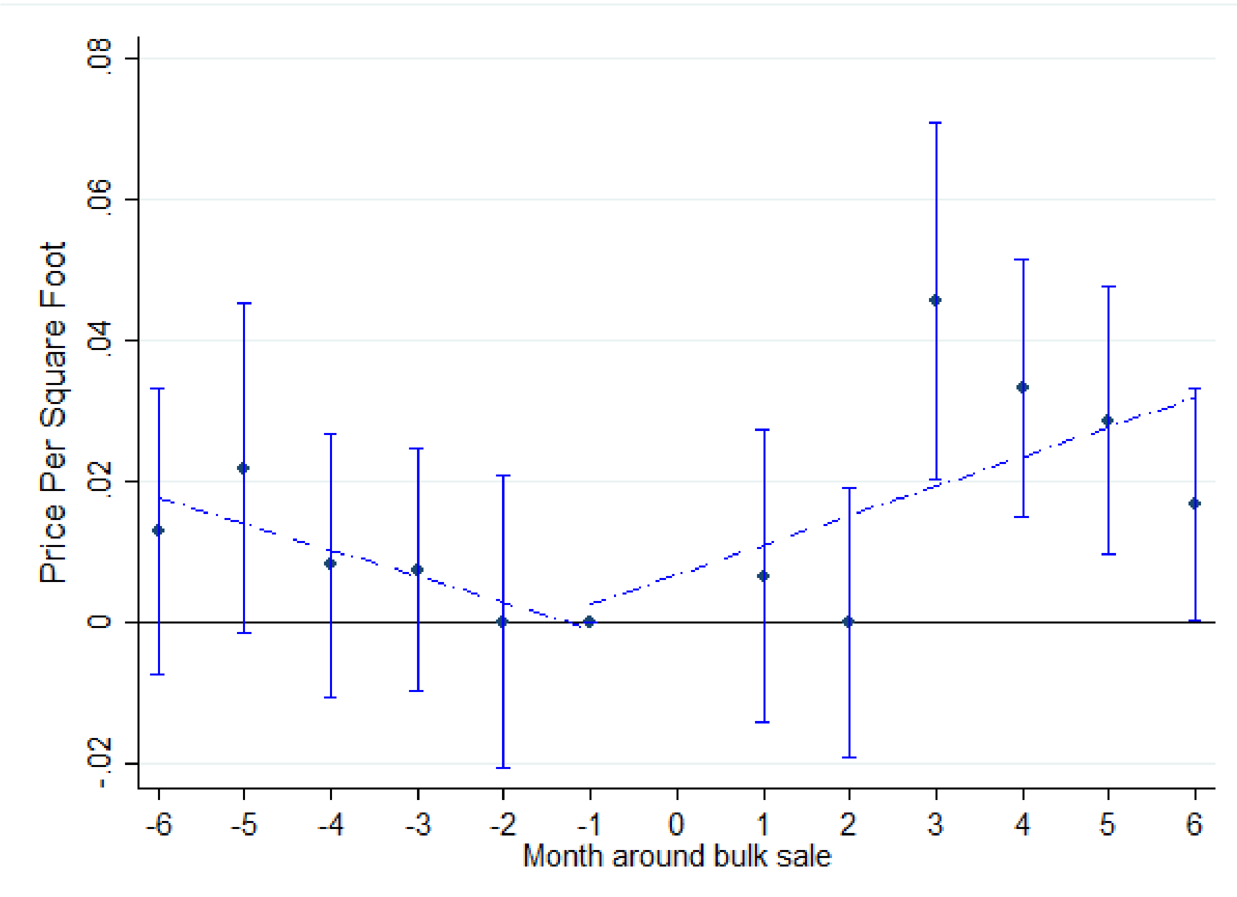


Table 1: SFR Holdings by Institutional Investors

This table shows the institutional investors and geographies with the highest SFR institutional investment. Panel A presents the top SFR institutional investors in the U.S. market which are identified as institutional investors owning at least 1,000 properties based on 2016 county assessment record. Panel B presents the top MSAs with SFR institutional investment with at least 1,000 properties owned institutional investors based on the 2016 county assessment record.

Panel A: Top SFR Institutional Investors

Rank	Investor	Number of SFR Properties
1	Invitation Homes	41,735
2	American Home 4 Rent	36,231
3	Starwood Waypoint	27,290
4	Progress Residential	13,890
5	Silver Bay	6,872
6	Main Street Renewal	5,819
7	Tricon American Homes	5,677
8	Altisource	4,256
9	Havenbrook Homes	3,568
10	Cerberus	3,440
11	Camillo Properties	2,817
12	Golden Tree Insite Partners(GTIS)	2,515
13	Connorex-Lucinda	2,434
14	Haven Homes	1,728
15	Gorelick Brothers Capital	1,717

Table 1: SFR Holdings by Institutional Investors (Cont.)

Panel B: Top MSAs with SFR Properties Owned by Institutional Investors

Rank	CBSA Title	Number of SFR Properties
1	Atlanta-Sandy Springs-Roswell, GA	29,141
2	Miami-Fort Lauderdale-West Palm Beach, FL	13,223
3	Phoenix-Mesa-Scottsdale, AZ	12,647
4	Houston-The Woodlands-Sugar Land, TX	12,628
5	Tampa-St. Petersburg-Clearwater, FL	11,392
6	Dallas-Fort Worth-Arlington, TX	10,682
7	Charlotte-Concord-Gastonia, NC-SC	9,208
8	Orlando-Kissimmee-Sanford, FL	7,269
9	Las Vegas-Henderson-Paradise, NV	6,543
10	Jacksonville, FL	6,408
11	Chicago-Naperville-Elgin, IL-IN-WI	5,925
12	Indianapolis-Carmel-Anderson, IN	5,877
13	Riverside-San Bernardino-Ontario, CA	4,204
14	Los Angeles-Long Beach-Anaheim, CA	3,987
15	Seattle-Tacoma-Bellevue, WA	3,114
16	Raleigh, NC	2,781
17	Columbus, OH	2,546
18	Cincinnati, OH-KY-IN	2,403
19	Denver-Aurora-Lakewood, CO	2,362
20	North Port-Sarasota-Bradenton, FL	2,292
21	Lakeland-Winter Haven, FL	2,218
22	Sacramento–Roseville–Arden-Arcade, CA	2,145
23	Cape Coral-Fort Myers, FL	2,098
24	San Antonio-New Braunfels, TX	1,932
25	Vallejo-Fairfield, CA	1,679
26	Columbia, SC	1,632
27	Minneapolis-St. Paul-Bloomington, MN-WI	1,561
28	San Francisco-Oakland-Hayward, CA	1,302
29	Deltona-Daytona Beach-Ormond Beach, FL	1,275
30	Oklahoma City, OK	1,137
31	Winston-Salem, NC	1,070
32	Palm Bay-Melbourne-Titusville, FL	1,058
33	Charleston-North Charleston, SC	1,020

Table 2: REO Pilot Transactions.

This table summarizes the three REO bulk transactions by Fannie Mae under the FHFA REO bulk sale pilot program. The three REO bulk sale transactions, namely, Florida, West (Arizona, California, and Nevada), and Chicago were sold through an auction on June 25, 2012. REO portfolios were pre-packaged and sold by Fannie Mae. As a result, institutional investors bidding in the REO bulk sale auction were not allowed to cherry-pick specific properties.

Transaction Name	Transaction Size (# of Properties)	Geography	Winning Bidder	Vacancy Rate	Third Party Valuation	Transacted Value (% of Third Party)
SFR 2012-1-Florida	699	Florida (Central and Northeast, Southeast, West Coast)	Pacifica L 47, LLC	32.62%	$81,527,995	95.8%
SFR 2012-1-Chicago	94	Chicago, Illinois	Cogsville Capital Partners Fund I, LP	38.74%	$13,689,012	86.2%
SFR 2012-1 West	970	Arizona, California, Nevada	Colony Homes, LLC	36.05%	$156,771,744	112.3%
Total	1763					

Table 3: Summary Statistics For Properties Around Bulk Transactions.

This table presents the average property characteristics for single family houses in the treated and control group around bulk transactions. The treated group includes properties within the 0.25 mile radius around bulk-sale properties. The control group includes properties in the 0.25–0.5 mile radius around the bulk-sold properties. Sales price is winsorized at the 1st and 99th percentiles.

Panel A: Before Bulk Transactions

	Control	Treated	Difference
Price per sqft	93.486	88.203	5.282**
			(1.63)
Total price (in 000's)	177.655	157.052	20.604***
			(3.36)
Building area (sqft)	1,922.115	1,827.302	94.813***
			(19.03)
Lot size (sqft)	8,248.365	7,896.494	351.871
			(458.21)
Age	24.004	21.134	2.870***
			(0.50)
Number of bedrooms	1.605	1.519	0.086*
			(0.04)
Number of bathrooms	2.038	2.072	-0.034
			(0.03)
Foreclosed	0.263	0.274	-0.011
			(0.01)
Observations	4,303	2,354	

Panel B: After Bulk Transactions

	Control	Treated	Difference
Price per sqft	108.914	102.531	6.383***
			(1.71)
Total price (in 000's)	204.046	182.537	21.509***
			(3.32)
Building area (sqft)	1,934.522	1,857.532	76.990***
			(18.11)
Lot size (sqft)	8,567.722	8,425.876	141.846
			(443.54)
Age	25.958	22.484	3.474***
			(0.50)
Number of bedrooms	1.766	1.769	-0.003
			(0.04)
Number of bathrooms	2.101	2.119	-0.017
			(0.02)
Foreclosed	0.193	0.179	0.014
			(0.01)
Observations	4,847	2,732	

Table 4: Change in Neighbor House Prices Around Bulk Transactions.

This table presents estimates of DiD regressions from a sample of single family house transactions in a 13-month window around the bidding date of three bulk transactions (June 25, 2012) and within a 0.5-mile radius around bulk-sale properties. The dependent variables are price per square foot and log total price adjusted from hedonic regressions. *Post-sales* is a binary variable that equals one for transactions after the bidding date of the bulk transactions, zero otherwise. $I(Distance < 0.25mi)$ is a binary variable that equals one if the distance from the nearest bulk-sale property is less than 0.25 miles. We include county × year-month fixed effects and census tract fixed effects in the regressions. We report standard errors clustered by county in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Dependent Variable:	Adjusted price per sqft	Adjusted ln(total price)
	(1)	(2)
Post-sales × I(Distance<0.25mi)	1.330**	0.014***
	(0.64)	(0.00)
I(Distance<0.25mi)	-1.673**	-0.012*
	(0.70)	(0.01)
County×Year-Month FE	Yes	Yes
Census-tract FE	Yes	Yes
N	13,593	13,593
adj.R-sq	0.623	0.556

Table 5: Placebo test: Pseudo Event Six Month Before Bulk Transactions.

This table presents placebo test DiD estimates of using a 13-month window around a pseudo event six months prior to the actual bidding date of three bulk transactions (June 25, 2012). The sample consists of transactions within a 0.5-mile radius around bulk-sale properties. The dependent variables are price per square foot and log total price adjusted from hedonic regressions. $I(Distance < 0.25mi)$ is a binary variable that equals one if the distance from the nearest bulk-sale property is less than 0.25 miles. *Post* is a binary variable that equals one for transactions after pseudo event, zero otherwise. We include county × year-month fixed effects and census tract fixed effects in the regressions. We report standard errors clustered by county in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Dependent Variable:	Adjusted price per sqft	Adjusted ln(total price)
	(1)	(2)
Post (placebo)×I(Distance<0.25mi)	-0.573	-0.002
	(0.81)	(0.01)
I(Distance<0.25mi)	-1.344*	-0.013
	(0.77)	(0.01)
County×Year-Month FE	Yes	Yes
Census-tract FE	Yes	Yes
N	11,007	11,007
adj.R-sq	0.616	0.547

Table 6: Robustness: Additional Controls.

This table presents estimates of DiD regressions from a sample of single family house transactions in a 13-month window around the bidding date of three bulk transactions (June 25, 2012) and within a 0.5-mile radius around bulk-sale properties. The dependent variables are price per square foot and log total price adjusted from hedonic regressions. $I(Distance < 0.25mi)$ is a binary variable that equals one if the distance from the nearest bulk-sale property is less than 0.25 miles. We control for additional backward-looking as well as forward-looking neighborhood characteristics in the regression. Specifically, these controls include the number of foreclosure filings (F) and the number of regular transactions (T) in the inner and outer circle within the 0.5-mile radius, in the past or in the future six months. *Post-sales* is a binary variable that equals one for transactions after the bidding date of the bulk transactions, zero otherwise. We include county × year-month fixed effects and census tract fixed effects in the regressions. We report standard errors clustered by county in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Dependent Variable:	Adjusted price per sqft	Adjusted ln(total price)
	(1)	(2)
Post-sales × I(Distance<0.25mi)	1.204*	0.012**
	(0.65)	(0.00)
I(Distance<0.25mi)	-1.045*	-0.006
	(0.58)	(0.01)
F(Dist<0.25mi)$_{m-1,m-6}$	-0.111***	-0.001***
	(0.03)	(0.00)
F(Dist<0.25mi)$_{m+1,m+6}$	-0.190*	-0.002**
	(0.10)	(0.00)
T(Dist<0.25mi)$_{m-1,m-6}$	0.234***	0.002***
	(0.06)	(0.00)
T(Dist<0.25mi)$_{m+1,m+6}$	0.120	0.001*
	(0.10)	(0.00)
F(0.25<=Dist<0.50mi)$_{m-1,m-6}$	-0.014	-0.000
	(0.02)	(0.00)
F(0.25<=Dist<0.50mi)$_{m+1,m+6}$	0.000	-0.000
	(0.03)	(0.00)
T(0.25<=Dist<0.50mi)$_{m-1,m-6}$	0.075	0.001
	(0.06)	(0.00)
T(0.25<=Dist<0.50mi)$_{m+1,m+6}$	-0.034	-0.000
	(0.04)	(0.00)
County×Year-Month FE	Yes	Yes
Census-tract FE	Yes	Yes
N	13,578	13,578
adj.R-sq	0.625	0.559

Table 7: Change in Neighbor Foreclosed Prices Around Bulk Transactions.

This table presents estimates of DiD regressions from a sample of single family house transactions in a 13-month window around the bidding date of three bulk transactions (June 25, 2012) and within a 0.5-mile radius around bulk-sale properties. Panel A includes all neighboring single family house transactions and Panel B includes only foreclosed transactions. The dependent variables are price per square foot and log total price adjusted from hedonic regressions. *Post-sales* is a binary variable that equals one for transactions after the bidding date of the bulk transactions, zero otherwise. $I(Distance < 0.25mi)$ is a binary variable that equals one if the distance from the nearest bulk-sale property is less than 0.25 miles. *Foreclosed* is a binary variable that equals one if the transaction is foreclosed, zero otherwise. *ln(Foreclosure time)* is the natural logarithm of the number of days between foreclosure filing and the transaction date. We measure foreclosure time for a property relative to the mean foreclosure time for the ease of interpretation. We include county × year-month fixed effects and census tract fixed effects in the regressions. We report standard errors clustered by county in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Panel A

Dependent Variable:	Adjusted price per sqft	Adjusted ln(total price)
	(1)	(2)
Post-sales×I(Distance<0.25mi)	0.410	0.005
	(0.69)	(0.01)
Post-sales×I(Distance<0.25mi)×Foreclosed	3.844*	0.038*
	(2.04)	(0.02)
I(Distance<0.25mi)*Foreclosed	-0.273	0.000
	(1.30)	(0.01)
I(Distance<0.25mi)	-1.626**	-0.012
	(0.78)	(0.01)
Foreclosed	-9.993***	-0.122***
	(1.37)	(0.02)
Post-sales×Foreclosed	-3.557**	-0.023
	(1.69)	(0.02)
County×Year-Month FE	Yes	Yes
Census-tract FE	Yes	Yes
N	13,593	13,593
adj.R-sq	0.634	0.577

Panel B

Dependent Variable:	Adjusted price per sqft	Adjusted ln(total price)
	(1)	(2)
Post-sales×I(Distance<0.25mi)	4.215	0.043
	(2.52)	(0.03)
Post-sales×I(Distance<0.25mi)×ln(Foreclosure time)	2.774***	0.027***
	(0.59)	(0.01)
I(Distance<0.25mi)×ln(Foreclosure time)	-0.356	0.002
	(0.50)	(0.01)
I(Distance<0.25mi)	-0.811	-0.006
	(0.75)	(0.01)
ln(Foreclosure time)	0.007	-0.005
	(0.39)	(0.00)
Post-sales×ln(Foreclosure time)	-2.806***	-0.029***
	(0.44)	(0.00)
County×Year-Month FE	Yes	Yes
Census-tract FE	Yes	Yes
N	2,574	2,574
adj.R-sq	0.695	0.610

Table 8: Similarity between Focal and Bulk-sold Properties.

This table presents estimates of DiD regressions from a sample of single family house transactions in a 13-month window around the bidding date of three bulk transactions (June 25, 2012) and within a 0.5-mile radius around bulk-sale properties. The dependent variables are price per square foot and log total price adjusted from hedonic regressions. *Post-sales* is a binary variable that equals one for transactions after the bidding date of the bulk transactions, zero otherwise. $I(Distance < 0.25mi)$ is a binary variable that equals one if the distance from the nearest bulk-sale property is less than 0.25 miles. We measure similarity between the focal property and the nearby bulk-sold property in three ways. In columns 1 and 2, *Similarity* is defined as $1 - |SQFT_{Focal}/SQFT_{\text{Close BS}} - 1|$ where $SQFT$ is the building square footage of the property. In columns 3 and 4, *Similarity* is defined as $1 - |Age_{Focal} - Age_{\text{Close BS}}|$. In columns 5 and 6, *Similarity* equals one if the nearby bulk-sold property is also a single family home. We include county × year-month fixed effects and census tract fixed effects in the regressions. We report standard errors clustered by county in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Similarity:	Size		Age		Property Type	
Dependent Variable:	price per sqft (1)	ln(total price) (2)	price per sqft (3)	ln(total price) (4)	price per sqft (5)	ln(total price) (6)
Post-sales×I(Distance<0.25mi)	0.634	0.009*	3.269***	0.023***	1.800**	0.015**
	(0.80)	(0.00)	(1.00)	(0.01)	(0.69)	(0.01)
Post-sales×I(Distance<0.25mi)×Similarity	1.655***	0.007***	0.487*	0.002***	2.383	0.002
	(0.08)	(0.00)	(0.24)	(0.00)	(4.93)	(0.03)
I(Distance<0.25mi)×Similarity	-1.438***	-0.005***	-0.434***	-0.003***	3.797	0.031
	(0.08)	(0.00)	(0.13)	(0.00)	(3.31)	(0.02)
I(Distance<0.25mi)	-0.197	-0.005	-2.566***	-0.018***	-1.026*	-0.007
	(0.54)	(0.00)	(0.69)	(0.01)	(0.58)	(0.01)
Similarity	-2.740***	-0.009***	0.057	0.001	0.963	0.007
	(0.65)	(0.00)	(0.09)	(0.00)	(2.38)	(0.02)
Post-sales*Similarity	-1.548***	-0.006***	-0.172**	-0.002*	-1.514	0.003
	(0.10)	(0.00)	(0.08)	(0.00)	(2.23)	(0.02)
County×Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Census-tract FE	Yes	Yes	Yes	Yes	Yes	Yes
N	11,703	11,703	11,753	11,753	13,593	13,593
adj.R-sq	0.626	0.556	0.620	0.556	0.623	0.556

Table 9: Distressed Neighborhood and Change in Neighbor House Prices Around Bulk Transactions

This table presents estimates of DiD regressions from a sample of single family house transactions in a 13-month window around the bidding date of three bulk transactions (June 25, 2012) and within a 0.5-mile radius around bulk-sale properties. The dependent variables are price per square foot and log total price adjusted from hedonic regressions. *Post-sales* is a binary variable that equals one for transactions after the bidding date of the bulk transactions, zero otherwise. $I(Distance < 0.25mi)$ is a binary variable that equals one if the distance from the nearest bulk-sale property is less than 0.25 miles. *Bottom Quintile Neighborhood* is a binary variable that equals one if the post-bulk-sale change in neighborhood home value is in the bottom quintile. We include county × year-month fixed effects and Zillow neighborhood fixed effects in the regressions. We report standard errors clustered by county in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Dependent Variable:	Adjusted price per sqft	Adjusted ln(total price)
	(1)	(2)
Post-sales×I(Distance<0.25mi)	-0.004	0.006
	(0.71)	(0.01)
Post-sales×I(Distance<0.25mi)×Bottom Quintile Neighborhood	8.404***	0.068**
	(2.54)	(0.03)
I(Distance<0.25mi)×Bottom Quintile Neighborhood	-8.052***	-0.065***
	(2.49)	(0.02)
I(Distance<0.25mi)	-0.049	-0.001
	(0.49)	(0.00)
Post-sales×Bottom Quintile Neighborhood	-10.896**	-0.095***
	(3.90)	(0.01)
Baseline Controls	Yes	Yes
County×Year-Month FE	Yes	Yes
RegionID FE	Yes	Yes
N	7,130	7,130
adj.R-sq	0.542	0.421

Table 10: Change in Neighbor House Prices Around Individual Foreclosed Home Transactions.

This table presents estimates of DiD regressions from a sample of single family house transactions in a 13-month window around individual foreclosed homes that were bought in June 2012 in the same counties as those in the bulk transactions. The dependent variables are price per square foot and log total price adjusted from hedonic regressions. *Post-sales* is a binary variable that equals one for transactions after June 2012, zero otherwise. $I(Distance < 0.25mi)$ is a binary variable that equals one if the distance from the nearest purchased foreclosed property is less than 0.25 miles. *Bottom Quintile Neighborhood* is a binary variable that equals one if the post-transaction change in neighborhood home value is in the bottom quintile. In Panel A (B), we include county × year-month fixed effects and census tract (Zillow neighborhood) fixed effects in the regressions. We report standard errors clustered by county in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Panel A

Dependent Variable:	Adjusted price per sqft	Adjusted ln(total price)
	(1)	(2)
Post-sales×I(Distance<0.25mi)	-0.276	0.002
	(1.06)	(0.01)
I(Distance<0.25mi)	-0.295	-0.010
	(1.05)	(0.01)
County×Year-Month FE	Yes	Yes
Census-tract FE	Yes	Yes
N	16,782	16,782
adj.R-sq	0.588	0.519

Panel B

Dependent Variable:	Adjusted price per sqft	Adjusted ln(total price)
	(1)	(2)
Post-sales×I(Distance<0.25mi)	-0.431	0.001
	(0.60)	(0.00)
Post-sales×I(Distance<0.25mi)×Bottom Quintile Neighborhood	-0.318	0.006
	(5.23)	(0.02)
I(Distance<0.25mi)×Bottom Quintile Neighborhood	0.506	0.003
	(4.93)	(0.02)
I(Distance<0.25mi)	-1.062**	-0.019***
	(0.37)	(0.00)
Post-sales×Bottom Quintile Neighborhood	-3.249	-0.076**
	(4.31)	(0.03)
Baseline Controls	Yes	Yes
County×Year-Month FE	Yes	Yes
RegionID FE	Yes	Yes
N	8,022	8,022
adj.R-sq	0.524	0.396

Appendix

Variable Definitions

- I(Distance<0.25mi): a binary variable that equals one if *Distance* is less than 0.25 miles.

- *Proximity*: negative log distance from the nearest bulk-sale property in miles.

- N(Distance<0.25mi): the number of bulk-sale properties within the 0.25-mile radius.

- WN(Distance<0.25mi): the number of bulk-sale properties within the 0.25-mile radius weighted by $\frac{0.25-distance}{0.25}$.

- N(0.25mi<=Distance<0.5mi): the number of bulk-sale properties between 0.25 and 0.5 miles radius from the focal property.

- *Foreclosed*: a binary variable that equals one if the transaction is foreclosed, zero otherwise.

- *ln(Foreclosure time)*: the natural logarithm of the number of days between foreclosure filing and the transaction date.

Additional Tables

Table A1: Robustness: Controlling for Time Varying Neighborhood Effect.

This table presents estimates of DiD regressions from a sample of single family house transactions in a 13-month window around the bidding date of three bulk transactions (June 25, 2012) and within a 0.5-mile radius around bulk-sale properties. We include Census-tract × Year-quarter fixed effects to control for unobservable time-varying neighborhood characteristics. The dependent variables are price per square foot and log total price adjusted from hedonic regressions. $I(Distance < 0.25mi)$ is a binary variable that equals one if the distance from the nearest bulk-sale property is less than 0.25 miles. *Post-sales* is a binary variable that equals one for transactions after the bidding date of the bulk transactions, zero otherwise. We report standard errors clustered by county in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Dependent Variable:	Adjusted price per sqft	Adjusted ln(total price)
	(1)	(2)
Post-sales×I(Distance<0.25mi)	2.092**	0.016*
	(0.84)	(0.01)
I(Distance<0.25mi)	-1.621*	-0.012
	(0.87)	(0.01)
Census-tract×Year-Quarter FE	Yes	Yes
N	11,957	11,957
adj.R-sq	0.601	0.545

Table A2: Robustness: Long-Term Effects of Bulk Transactions.

This table presents estimates of DiD regressions from a sample of single family house transactions in a 31-month window from six months before to 24 months after the bidding date of three bulk transactions (June 25, 2012) and within a 0.5-mile radius (columns 1 and 2) or 1-mile radius (columns 3 and 4) around bulk-sale properties. The dependent variables are price per square foot and log total price adjusted from hedonic regressions. $I(Distance < 0.25mi)$ is a binary variable that equals one if the distance from the nearest bulk-sale property is less than 0.25 miles. *Post-sales* is a binary variable that equals one for transactions after the bidding date of the bulk transactions, zero otherwise. We include county × year-month fixed effects and census tract fixed effects in the regressions. We report standard errors clustered by county in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Dependent Variable:	price per sqft	ln(total price)	price per sqft	ln(total price)
	(1)	(2)	(3)	(4)
Post-sales×I(Distance<0.25mi)	0.456	0.006*	1.321*	0.012***
	(0.50)	(0.00)	(0.76)	(0.00)
I(Distance<0.25mi)	-1.893**	-0.014**	-3.206***	-0.024***
	(0.76)	(0.01)	(1.10)	(0.01)
County×Year-Month FE	Yes	Yes	Yes	Yes
Census-tract FE	Yes	Yes	Yes	Yes
Radius	0.5 mile	0.5 mile	1 mile	1 mile
N	36,608	36,608	84,760	84,760
adj.R-sq	0.636	0.569	0.630	0.570

Table A3: Robustness: Alternative Treatment Variables.

This table presents estimates of DiD regressions from a sample of single family house transactions in a 13-month window around the bidding date of three bulk transactions (June 25, 2012) and within a 0.5-mile radius around bulk-sale properties. The dependent variable is price per square foot adjusted from hedonic regressions. *Post-sales* is a binary variable that equals one for transactions after the bidding date of the bulk transactions, zero otherwise. We use the following alternative treatment variables: *Proximity* is negative log distance from the nearest bulk-sale property in miles; $N(Distance < 0.25mi)$ is the number of bulk-sale properties within the 0.25-mile radius; $WN(Distance < 0.25mi)$ is the number of bulk-sale properties within the 0.25-mile radius weighted by $\frac{0.25-distance}{0.25}$. $N(0.25mi <= Distance < 0.5mi)$ is the number of bulk-sale properties between 0.25 and 0.5 miles radius from the focal property. We include county × year-month fixed effects and census tract fixed effects in the regressions. We report standard errors clustered by county in brackets. *, ** and *** indicate significance better than 10%, 5%, and 1% respectively.

Dependent Variable:	Adjusted ln(total price)			
	(1)	(2)	(3)	(4)
Post-sales×Proximity	0.011*			
	(0.01)			
Proximity	-0.006			
	(0.01)			
Post-sales×N(Distance<0.25mi)		0.007*	0.009**	
		(0.00)	(0.00)	
N(Distance<0.25mi)		-0.011**	-0.013**	
		(0.01)	(0.00)	
Post-sales×N(Distance<0.5mi)			-0.007	
			(0.01)	
N(Distance<0.50mi)			0.006	
			(0.01)	
Post-sales×WN(Distance<0.25mi)				0.030**
				(0.01)
WN(Distance<0.25mi)				-0.019
				(0.01)
County×Year-Month FE	Yes	Yes	Yes	Yes
Census-tract FE	Yes	Yes	Yes	Yes
N	13,593	13,593	13,593	13,593
adj.R-sq	0.556	0.556	0.556	0.556