Estimates of the Size and Source of Price Declines Due to Nearby Foreclosures *

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Abstract

Using new data on real estate listings, we provide new evidence that foreclosures have a causal effect on nearby house prices and disentangle the effect into two sources: competition and disamenities. We identify the causal effect by showing that sellers respond to new REO listings in the exact week of listing, not a week before and not a week after. We disentangle competition and disamenity effects by examining the spillover effect across various stages of the foreclosure process. We find that competition effects are important in all areas, but only find evidence for disamenity effects in high density, low price neighborhoods.

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1 Introduction

The goal of this paper is to advance our understanding of foreclosure spillover effects in two important dimensions.[1] First: Do foreclosures contribute to, or are they merely a symptom of, neighborhood price declines? The direction of causality is not clear *a priori* because neighborhood price declines can lead to more homeowners being underwater, which leads to more foreclosures.

Second: If foreclosures are indeed a contributing factor to neighborhood price declines, then what is the mechanism through which the spillover effect operates? There are two main mechanisms to consider. The first, which we call a "disamenity effect", is that foreclosed properties may, through neglect and vacancy, create an eyesore or attract crime and vandalism, creating a negative externality for nearby homes. The second, which we call a "competitive effect", is that foreclosures increase the supply of homes on the market, which would lower prices in a standard model of differentiated products competition.[2] This effect may be especially strong if banks price their homes more aggressively because they are more motivated to sell than the typical non-bank seller (Springer [1996], Campbell et al. [2011]). Neither of the two effects should be taken as given. A disamenity effect may not arise if banks, who ultimately need to sell the foreclosed property, have an incentive to maintain the condition of the property. A competitive effect may not arise if search frictions in the housing market (Wheaton [1990], Krainer [2001], Novy-Marx [2009]) negate the predictions of standard models of price competition.[3] The extent to which each of these mechanisms is important is therefore an empirical question.

The answers to these questions are of interest to both academics and policymakers. For academics, understanding foreclosure spillovers can lead to

[1] By spillover effect we mean any effect that a foreclosed property has on neighboring property values or seller behavior. Our definition of spillovers is not limited to externalities but may also include effects due to price competition or the revelation of new information.

[2] The additional supply is not typically offset by an increase in demand because the delinquent buyer is unlikely to re-enter the housing market as a buyer. See Molloy and Shan [2012] for empirical evidence.

[3] For example, Turnbull and Dombrow [2006] find evidence that more supply can induce more buyers to shop for homes, potentially offsetting the competition effect.

a better understanding of how shocks are spatially transmitted through the housing market.[4] For policymakers, foreclosure spillovers are sometimes used to justify government intervention into housing and mortgage markets, and understanding their nature can guide the appropriate type of intervention.[5]

Most of the existing literature, which we summarize in detail below, has approached these questions with micro data on housing *transactions*. With these data, the literature has provided strong evidence that foreclosures are correlated with price declines, but the direction of causality is not definitive, and there is little and mixed evidence on the source of the spillover effect. To advance on these questions, we supplement housing transaction data with new data on housing *listings* from the Multiple Listing Service (MLS). Our listings dataset covers close to the universe of residential properties listed for sale, including REO listings[6], in the San Francisco, Washington D.C., Chicago, and Phoenix metro areas from January 2007 to June 2009.

There are two main advantages to using listing data. First, there are many more list prices at any given time in any given geography than there are sales prices. With higher frequency data, we can partial out potential sources of endogeneity by looking at the effects of foreclosures over narrower time periods (weekly) and finer geographies relative to the rest of the literature. Second, we observe the dates that both REO and non-REO properties are on the market for sale, including the initial listing date and the exit date. Existing studies typically only have access to the foreclosure date and the sale date for REOs, and only the sale date for non-REOs. We describe below how we supplement the information on transaction dates with the new information on listing dates to disentangle the separate spillover transmission mechanisms.

Our identification strategy is based on exploiting the precise timing of when an REO listing occurs. We find that sellers are more likely to adjust list prices

[4]See Rossi-Hansberg et al. [2010] for a recent example of this literature.

[5]See, for example, the motivation for the Obama administration's Homeowner Affordability and Stability Plan. http://www.treasury.gov/press-center/press-releases/Pages/20092181117388144.aspx

[6]REO stands for Real Estate Owned. This is how properties are classified after the foreclosure sale is completed and the property is owned by the lender.

downwards in the exact week that a new, nearby REO enters the market, but they are no more likely to adjust list prices in the week before or the week after entry. If the specific timing of whether the REO was listed this week, or one week prior or one week after, is random, then we have captured a causal effect. The results are consistent across all the metro areas in our data, suggesting that the effects are general and not unique to any specific housing market. Moreover, we find that the effect of a new non-REO listing is comparable to the effect of a new REO listing, suggesting that the price movements identified around the listing date are related to competition.

Having identified a causal effect on listing behavior, we estimate the local effects of foreclosures on sales prices over specific periods in the foreclosure process: namely, the periods before the foreclosure sale, between the foreclosure sale and the REO listing, between the REO listing and the REO sale, and after the REO sale. We estimate the size of the local effect during a specific time period by comparing the sales prices of homes within 0.1 miles of the foreclosure, sold during the time period in question, to the homes sold in the previous period. Following Campbell et al. [2011], we use the changes in prices for homes sold in a wider ring around the foreclosure to control for potential pre-existing local trends.

By estimating the spillover effect at specific periods in the foreclosure process, we can isolate the competitive and disamenity effects. Any price differential centered around the listing date should be due to competition because this is when the REO begins competing with nearby listings for buyers. It should not be due to a disamenity effect because any disincentives to maintaining the property should have begun to emerge either around the date of eviction or around the date in which the borrower realizes that default is inevitable, both of which are usually many months before the listing date. Because disamenity effects are expected to arise before the REO listing date, we test for disamenity effects by looking for downward price trends in the pre-listing period.

We find that homes sold within 0.1 miles of an REO sell at a 1.6 percent lower price during the REO listing period relative to before the REO listing period. These price declines are temporary; after the REO sells, prices recover to pre-listing levels within six months. The timing of the sales price response to non-REO listings is similar to the response to REO listings, although the magnitude of the spillover effect from REO listings is stronger. We also find that the spillover effects of REO and non-REO listings are larger when the listings are more substitutable with the nearby properties. These latter results provide further evidence that the spillover effect identified during the listing period is a competitive effect rather than a disamenity effect.

In the periods of time leading up to an REO listing, we find that the size of the spillover effect depends on the foreclosure's neighborhood type. In low price, high housing density neighborhoods (about 1/4 of neighborhoods in our sample) we find a spillover effect that grows smaller the further away in time one gets from the list date, and is largest right before the list date. This is consistent with a disamenity effect that grows larger as the property is held vacant or neglected for longer periods of time. In these neighborhoods, the disamenity effect at its largest is comparable in magnitude to the competitive effect. In all other neighborhoods, we find no evidence of a spillover effect occuring before the REO list date. To further establish that the REO spillover effect we estimate in the pre-listing period captures a disamenity effect (and not a competitive effect), we show that non-REOs in the pre-listing period have no effect on nearby home prices in *any* neighborhood type. As a whole, our results support the existence of competitive effects, but we only find evidence for disamenity effects in specific types of neighborhoods.

Related Literature

Several recent studies on foreclosure spillovers have focused on estimating the size of the effect using a variety of methods (Immergluck and Smith [2006], Harding et al. [2009], Lin et al. [2009], Campbell et al. [2011], Mian et al. [2012], Gerardi et al. [2012]).[7] These studies are largely distinguished by how each deals with a difficult identification problem: given that price declines

[7] There is a related literature that studies the impacts of foreclosures on other outcomes, including crime (Ellen et al. [2013]), racial composition of neighborhoods (Lauria and Baxter [1999]), and health (Currie and Tekin [2011]). Goodstein et al. [2011], Guiso et al. [2009], and Towe and Lawley [2013] look at whether foreclosures lead to more foreclosures.

are a necessary condition for foreclosure, homes that are near foreclosures can have lower prices for reasons unrelated to the foreclosure itself. Early papers (Immergluck and Smith [2006], Harding et al. [2009], Lin et al. [2009]) focused on controlling for differences in house quality for homes nearby foreclosures, either through inclusion of a rich set of characteristics or through a repeat-sales approach. This approach alleviates some endogeneity concerns, but does not solve the problem of reverse causation because it is likely that areas near foreclosures are trending differently from areas further away for reasons unrelated to house quality.

In a recent prominent paper, Campbell et al. [2011] use the difference-in-differences approach discussed above to better control for differential trends. They find that an additional foreclosure lowers home prices in an inner ring around the foreclosure by about 1% relative to homes in the outer ring. However, as the authors themselves acknowledge, there is still the concern that the homes in the outer ring are trending differently from the homes in the inner ring, especially if spatial shocks in the housing market are propagated continuously over distance.

More recently, Gerardi et al. [2012] have used the repeat-sales approach of Harding et al. [2009], allowing for differential annual price trends at a finer level of geography (census-block-group) relative to Harding et al. [2009] (MSA). Their estimates of the foreclosure spillover effect are similar to Campbell et al. [2011], but in terms of establishing a causal effect, their identification strategy is also susceptible to the possibility of differential price trends within a year-census-block-group. In a unique approach, Mian et al. [2012] instrument for the number of foreclosures with the judicial/non-judicial status of a state's foreclosure law. Their identification strategy estimates a total price effect at the state or metro level over a long time period, so it includes both local spillover effects as well as other effects that foreclosures may have on aggregate house prices, such as through the feedback cycles between foreclosures and price declines and the effect of foreclosure on the foreclosed home itself.

Evidence on the transmission mechanism is relatively scant and mixed. Harding et al. [2009] have attributed the spillover to a disamenity effect because they found that the spillover effect is largest in the year preceding the foreclosure sale, and that the spillover effect is persistent even one or two years after the foreclosure date. There are two reasons why these empirical facts may arise even absent a disamenity effect. First, these facts may simply be capturing the differential price trends within an MSA for areas with many foreclosures compared to areas with few foreclosures. Second, persistence may be capturing the lag between the foreclosure sale date and the REO list date, which is when competitive effects are expected to arise. Gerardi et al. [2012] find that foreclosed properties which are reported to be in poor condition have a larger spillover effect, which is suggestive of a disamenity effect. Hartley [2010] finds that, for a sample of home sales in Chicago, foreclosures of multi-family units do not appear to exert spillover effects on nearby single-family units. Under an assumption about the segmentation of the single-family and multi-family housing markets, he interprets the entire spillover effect as a competitive effect. Mian et al. [2012] also use listing data to show that foreclosures lead to a net increase in housing inventory at the zip code level, which is consistent with a competitive effect. Mian et al. [2012] are unable to say whether an additional spillover arises through the disamenity effect.

Finally, while our paper focuses on how foreclosures affect the prices of *other* homes, foreclosure can also affect the price of the foreclosed home itself. We and other papers in the literature find that foreclosed homes sell at substantial discounts relative to the prices of non-distressed sales.[8] However, this discount is difficult to interpret because while some of the effect may be due to the foreclosure (e.g. banks have relatively high holding costs and thus sell at fire sale prices), some of it may simply reflect the fact that foreclosures tend to be of lower unobserved house quality. Harding et al. [2012] offer one approach to disentangle the source of the discount. By comparing the returns earned by purchasers of REOs to the returns earned by purchasers of nonREOs, they conclude that much of the foreclosure discount cannot be explained by banks selling homes at fire sale prices.

[8] These other papers are reviewed in Clauretie and Daneshvary [2009].

2 Data

We purchased home sale and listing data from two separate data providers for four large and diverse MSA's in the United States: Chicago, Phoenix, San Francisco, and Washington DC (DC henceforth). We chose the four largest cities by population for which there was good data quality and for which we could claim represent a diverse set of housing market conditions. Appendix A.2 describes in more detail how we selected these particular cities.

For San Francisco, we observe the universe of single-family homes listed for sale on the Multiple Listing Service (MLS) from January 2007 - June 2009.[9] For the other three cities, we observe all homes types (i.e. condo, single family, and multi-family), but the listing data does not begin until October 2007. Every week, Altos Research, the provider of the listing data, records the address, list price, the latitude and longitude, and some standard house characteristics (square footage, age, bathrooms, bedrooms) for all houses listed for sale. From this information, we can infer the date of initial listing and the date of delisting for each property.[10] A property is delisted when there is a sale agreement or when the seller withdraws the home from the market. Properties are also sometimes delisted and then relisted if a sales agreement falls through. We consider a listing as new only if there was at least a 180 day window since the address last appeared in the listing data. The MLS data alone do not allow us to distinguish between delistings due to sales agreements or withdrawals, nor does it identify which listings are REO listings.

For these reasons, we supplement the MLS data with a transactions dataset from Dataquick that contains information about the universe of housing transactions in each MSA from 1988-2009. In this dataset, the key variables for this

[9] All-broker assisted listings appear on the MLS. For-sale-by-owner sales accounted for only 6 percent of arms-length sales in 2011 according to the National Association of Realtors Annual Profile of Home Buyers and Sellers. Thus, MLS listings reflect close to the universe of all homes for sale at a given time.

[10] The initial listing date is censored for properties that are already on the market when our sample begins, and the delisting date is censored for those that are still on the market when our sample ends. We account for this censoring in our analyses below. See Appendix A.3 for more details.

analysis are the address of the property, the date of the transaction, the sales price, the name of the buyer and seller, and an indicator for whether the transaction is arms length.

Using the address, we merge the listing data together with the transaction data for each of the MSAs. Appendix A.3 describes the details of the merge and how we use the variables in the transaction data to identify REO listings and sales. Appendix A.3 also describes minor restrictions to the estimation sample (e.g. excluding properties with zero square feet). Since foreclosures are typically recorded in our transaction data prior to the listing of the REO on the MLS, the availability of the transaction data back to 1988 ensures that we can accurately classify each listing as REO or non-REO regardless of the length of time between foreclosure and listing. However, not all foreclosures will appear in our listing data. In some cases, the property is transferred directly to a new owner at the time of foreclosure through an auction. Campbell et al. [2011] report that this happens in only 18 percent of foreclosures. In most cases, the lender will take control of the property after foreclosure and work with a realtor to get the property listed on the MLS.

There are several advantages to using listings data in addition to the transactions data used in previous studies. We described the two main advantages in Section 1: listings data are higher frequency, and we can observe the REO listing date. A third advantage is that we can observe the date when the buyer and seller agree on the sales price.[11] We use the agreement date as the sale date in all of our analyses since the sales price reflects housing market conditions at the time of agreement. The existing literature uses the closing date, which is the date when the buyer takes ownership of the home, to classify sales into, for example, pre- and post-foreclosure. Since closing dates lag agreement dates and the length of the lag is idiosyncratic to each transaction, the additional information provided by the agreement date reduces measurement error as well as bias in estimators that use a before and after comparison.

[11]We assume that the agreement date is the date that the property is delisted from the MLS since most realtor organizations have a system of rules and fines in place to ensure that listings are updated promptly when a sale agreement occurs. For example, see www.bareis.com for details.

2.1 Summary Statistics

Table 1 presents summary statistics for the entire sample. We report summary statistics by MSA in Appendix Tables 1-4. The median sales price of REOs is significantly below the price for non-REOs. When we control for observable house characteristics and census tract by quarter fixed effects, the foreclosure discount narrows, but is still economically and statistically significant at 16 percent.

Table 2 shows the count of REO and non-REO transactions by MSA during our sample period. 36 percent of sales in our sample are REO sales; Phoenix has the highest REO share and Chicago the lowest. In total our sample includes over 240,000 home sales.

Since the timing of REO listings will be a central part of our identification strategy, we also investigate whether there are any notable patterns in when REO listings come onto the market. We report the distribution of the calendar weeks when REOs enter the market in Appendix Figure 1. There is some seasonality to the distribution, which we will control for in our empirical work, but in general the distribution is fairly uniform and is similar to the distribution for non-REO listings.

The four MSAs in our sample are diverse and representative of the different types of housing markets in the U.S. For example, Chicago and DC had more moderate price declines over our sample period (25 percent and 26 percent, respectively), while Phoenix and San Francisco had higher than average price declines (50 percent and 36 percent, respectively).[12] In addition, our sample contains markets that are affected by both judicial (where the time to foreclosure tends to be longer) and non-judicial foreclosure requirements.[13] If the disamenity effect occurs before foreclosure and if the disamenity effect is convex in the number of neighborhood homes in the pre-foreclosure stage,

[12]Source: Case Shiller house price index for 2007 to 2009. The 20 city composite declined by 28 percent.

[13]In non-judicial states, foreclosures are handled out of court. Illinois and Maryland (which overlaps with part of the DC MSA) are judicial states; the other states in our sample are non-judicial. Source: http://www.realtytrac.com/foreclosure-laws/foreclosure-laws-comparison.asp.

then the disamenity effect could be a more important source of price decline in judicial states. Finally, although we do not expect supply elasticity to be relevant for the short-run and highly localized effects we investigate in this paper, given the emphasis in the housing literature on supply elasticity as a determinant of longer term price dynamics[14], we note that our sample includes variation along this dimension as well. At one extreme is San Francisco, where supply is generally restricted by topography and regulation, and at the other is Phoenix, where housing supply can more readily respond to price changes.

3 Testing for a Causal Effect: Evidence from List Prices

3.1 Identification Challenges

We cannot identify foreclosure externalities by simply comparing sales prices of homes nearby foreclosures with prices of homes further away. Households that do not have enough wealth to absorb negative income shocks are more likely to default, and these very households are more likely to live in lower-amenity neighborhoods where the homes are of lower quality. Table 1 illustrates the importance of controlling for differences in homes nearby foreclosures. Houses that sell within 0.1 miles of an REO listing tend to be smaller and of significantly lower value. We can control for some of the differences in attributes, but we should be concerned that these homes differ along unobserved attributes as well.

One way to control for this is to compare sales prices before and after foreclosure. This is the approach used in Campbell et al. [2011]. Due to the thinness of sales volume in local areas, the before and after periods need to be long – a year each in Campbell et al. [2011] – in order to have enough precision. However, this introduces an additional potential source of endogeneity. Since price decline is a necessary condition for foreclosure, a foreclosure will tend to

[14] See, for example, Paciorek [2012] and Gyourko et al. [2006].

occur in a neighborhood that is declining in price at a faster than average rate. Endogeneity and the causal effect both create a correlation between the presence of a foreclosure and neighborhood price declines, and thus this approach cannot be definitive on whether foreclosures causally affect neighboring prices.

3.2 Econometric Specification

To control for these concerns, we look at the propensity for home sellers to adjust their list price in the few weeks surrounding a new, nearby REO listing. If the exact week that the REO becomes listed is not correlated with a local shock that causes nearby sellers to adjust their list prices, then any movement in list price is strong evidence that existing listings are responding to the entry of the REO. In general, our identification assumption is reasonable because the specific timing of a listing is largely influenced by exogenous factors, such as when work to get the house "ready to show" is completed and the timing of various stages of the foreclosure process.[15]

Furthermore, if we do see a price response around the listing date, we can draw conclusions about the source of this particular price decline. Since the date when the REO enters the MLS is the date when the REO begins competing with nearby listings for buyers, any price effect around the listing date should be related to competition. It is unlikely that disamenities would emerge over the course of a single week, and even if they do, there is no reason to expect that they would be correlated with the week that the house is first marketed for sale. If the effect around the listing date is indeed due to competition, then we should also see an effect around the listing date of a new non-REO listing.

The discussion above motivates the estimation of the following linear probability model:

[15] In addition, banking supervisory policy typically encourages banks to sell REOs as quickly as possible, which limits the scope for strategic timing of listings (FRB staff paper 2012).

$$y_{i,t} = \delta_1 N^{REO}_{i,t-4} + ... + \delta_9 N^{REO}_{i,t+4} + \delta_{10} N^{NonREO}_{i,t-4} + ... + \delta_{18} N^{NonREO}_{i,t+4} + \delta_{19} N^{REO}_{i,t} * Dist^{nonREO}_{it} + \delta_{20} N^{nonREO}_{i,t} * Dist^{REO}_{it} + w_t + \alpha_{j,t} + \beta X_{i,t} + \epsilon_{i,t} \tag{1}$$

where y_{it} is an indicator variable equal to 1 if house i in week t changes its list price. $N^{REO}_{i,t}$ ($N^{nonREO}_{i,t}$) is the count of new REO (non-REO) listings that are within 0.5 miles of listing i in week t. We are therefore estimating the propensity of nearby sellers to change their list prices in the 4 weeks before, on the week of, and in the 4 weeks after a new REO or non-REO listing. $Dist^{REO}_{it}$ ($Dist^{nonREO}_{it}$) measures how "distant", in physical and characteristic space, listing i is to the nearby REOs (non-REOs). We define it as

$$dist^{\tau}_{it} = \left(\frac{\sum_{j=1}^{N^{\tau}_{it}} |sqft_i - sqft_j|/N^{\tau}_{it}}{K_{sqft}}\right)^2 + \left(\frac{\sum_{j=1}^{N^{\tau}_{it}} |age_i - age_j|/N^{\tau}_{it}}{K_{age}}\right)^2 + \left(\frac{\sum_{j=1}^{N^{\tau}_{it}} |miles_{ij}|/N^{\tau}_{it}}{K_{miles_{ij}}}\right)^2 \tag{2}$$

for $\tau = REO, nonREO$ where $miles$ denotes physical distance between properties i and j. The K's are normalizing constants to adjust for differences in the scale of the three components of the index.[16] A listing with a low value of $Dist$ is more substitutable with the new entrants and, if there is a competitive effect, should be more likely to adjust the list price in response.

w_t is a set of week fixed effects and α_{jt} is a set of quarter-by-census tract fixed effects, where j indexes the particular census tract of the listing. X_{it} is a vector of controls, which in our baseline specification, includes an indicator for whether the listing is an REO, the number of weeks that the home has been on the market, the age of the house, the square footage of the house, and a dummy variable for whether the house is single-family.

[16] Specifically, $K_m = std(\sum_{j=1}^{N^{nonREO}_{it}} |m_i - m_j|/N^{nonREO}_{it})$ where the standard deviation is taken over all listings for which $N^{nonREO}_{it} > 0$. We divide $dist$ by 1000 for presentation purposes.

3.3 Main Results

We estimate equation (1) separately for each of the four MSAs in our sample. Our preferred specification uses the log counts of nearby listings (though we estimate a more flexible specification below) because standard price competition models predict that the competitive effect should be concave in the number of competitors. We add one to the counts to avoid taking the log of zero. Standard errors are clustered at the quarter-by-census tract level.

The effects of an additional REO and non-REO listing (relative to zero listings) are plotted in Figure 1, where the housing attribute components of *Dist* are set at the 10th percentile of their respective distributions and the physical distance component is set to 1/10th of the radius (or 1/20th of a mile). The full regression detail is reported in Table 3. Sellers are generally no more likely to change their list prices in the 4 weeks before and the 4 weeks after a new REO listing.[17] However, during the exact week of an REO entry, the probability that a seller adjusts their list price jumps significantly. The pattern is the same across all four MSAs. Relative to the average propensity to adjust price in each of the cities (from Appendix Tables 1-4), the response to a single REO listing is a 6 percent increase in Chicago, Phoenix, and DC, and an 8 percent increase in San Francisco. Consistent with the effect being due to competition, we find that the propensity to adjust price is declining in distance for each MSA in the sample. A new listing at the 90th percentile of the *Dist* distribution is, averaged across MSAs, about 50 percent less likely to elicit a price change than a listing with $Dist$=0.[18]

The bottom panel of Figure 1 shows that sellers respond to new non-REO listings in the same way that they respond to new REO listings. The action

[17]The small decline in the week after listing likely reflects the fact that after adjusting the list price, sellers are less likely to adjust the price in the following week (perhaps due to menu costs). The small decline in the week before listing could indicate that some neighbors receive notice of a new listing a few days in advance.

[18]In Appendix Table 5, we report results where we break the distance variable into three parts and interact each individual component with $N_{i,t}$. All three components appear to contribute to the negative coefficient reported in the baseline specification. When multiplying each coefficient by the 75th percentile less the 25th percentile of the respective distance distribution, we find that physical distance has the strongest effect.

occurs in the exact week of listing, not in the weeks before or the weeks after listing. The magnitudes of the effects are almost identical for new REO and non-REO listings. Distance affects the propensity to adjust list price with the expected sign for all cities, and the attenuation due to distance is similar in magnitude for non-REOs as for REOs.

3.4 Further Results and Robustness

Our baseline specification restricts the "treatment group" to be within 0.5 miles of a REO. This choice of radius is consistent with the existing literature discussed above, which finds that foreclosure spillovers die out beyond this distance. However, the evidence in the existing literature that spillovers are highly local, while highly suggestive, is not conclusive. This is because their identification strategies require differencing out any neighorhood-wide level effects. For example, if the true spillover effect is strictly declining in distance for short distances, but then flat and positive at longer distances, then the differences-in-differences estimator used in the existing literature would reject the null of broader spillover effects.

Since our identification strategy does not rely as heavily on differencing out local trends, we can provide a more powerful test of the null hypothesis that the spillover effect is highly local. To this end, we run a less parametric version of equation (1) where we divide distance from a new listing into discrete bins and include separate regressors for the number of new listings in each bin. For each distance bin, we include three dummy variables that indicate whether there are one, two, or more than two new listings. We exclude the census tract by quarter fixed effect terms, α_{jt}, so that we are not partialing out any potential neighborhood wide effect. Appendix Figure 2 plots the results for the effect of one new REO listing for each distance bin, which are 0.2 miles wide. The results show that indeed, the list price response from a marginal increase in REO listings is highly local. Most of the effect comes from homes within 0.5 miles, and after about 1 mile, the effect is close to zero. The pattern for non-REO listings, shown in Appendix Figure 3, is similar.

We also run (1) with house and week fixed effects, shown in columns 1-4 of Appendix Table 6. There is still variation in the dependent variable because the same house is listed over many weeks with varying numbers of nearby REOs and non-REOs on the market. The results are not sensitive to this control for house quality.

In the right-most columns of Appendix Table 6, we change the dependent variable to the percentage change in list price conditional on a change in list price. The results show that sellers are indeed adjusting their list prices *downwards* when new REOs enter the market.

We also test whether new REO listings induce a change in the composition of homes on the market. To this end, we estimate equation (1), substituting age, square feet, and a dummy variable equal to 1 if the listing exits the MLS (via a sale or a withdrawal) as the dependent variable. The coefficients of interest are plotted in Figure 2. To put the effects in perspective, we set the upper and lower limits of the y-axis to be 5 percent above and below the average value of the dependent variable. As a whole, there is no evidence of a significant shift in the number or composition of homes on the market. We return to this point when interpreting the evidence presented in Section 4. The results for non-REO listings (not reported) are similar.

3.5 Discussion

The stark response of sellers to nearby REO and non-REO listings in the exact week of the listing is strong evidence of a causal effect from the new listing. Moreover, the price pattern shown in Figure 1 is consistent with a model of price competition. In Appendix A.1, we present a simple 2-seller model that shows that even if some sellers are informed about the REO listing date in advance, the price pattern shown in Figure 1 can emerge in equilibrium. When the elasticity of the probability of sale with respect to the list price is sufficiently low, the informed seller finds it optimal to price as if he has no information about the impending REO listing.

Although we have presented evidence that price movements around the listing date are not due to a disamenity effect, we emphasize that this evidence alone does not disprove a disamenity effect. We expect that the disamenity effect, if present, should arise well-before the listing date. However, since the degradation of house quality is most likely a continuous process, we cannot use the type of identification strategy used in this section to test for disamenity effects.

4 Estimating the Size of the Competitive and Disamenity Effects

Having identified a causal effect on seller listing behavior, we use the difference-in-differences framework of Campbell et al. [2011] to estimate the local effects of new foreclosures on sales prices over the specific periods in the foreclosure process. We interpret an effect that emerges while the foreclosure is listed for sale as a competitive effect because this is the time period when the foreclosure is competing with neighboring listings for buyers. Once the property is listed for sale, the seller (and potentially the listing agent) has more incentive to preserve the quality of the property as potential buyers may be visiting and inspecting the house. Any disincentives to maintaining the property should have begun to emerge either around the date of eviction or around the date in which the borrower realizes default is inevitable. Both of these dates are unobserved to us, but they are usually many months before the listing date. Any price effect prior to the listing date, then, should be due to a disamenity effect.

4.1 Competitive Effect

To test for the size of the competitive effect, we estimate the following regression

$$log(P_{ijt}) = \alpha_{jt} + \beta X_{it} + \sum_{g \in G^F} (\delta_{Close,g} F_{it}^{Close,g} + \delta_{Far,g} F_{it}^{Far,g}) + \sum_{g \in G^{NF}} (\gamma_{Close,g} NF_{it}^{Close,g} + \gamma_{Far,g} NF_{it}^{Far,g}) + \epsilon_{ijt} \quad (3)$$

where $log(P)$ is the log sales prices and

- $F^{Close,g}$ ($F^{Far,g}$): the number of REOs at stage g in the listing process (e.g. prior to listing) that are close to (further away from) property i.
- $NF^{Close,g}$ ($NF^{Far,g}$): the number of non-REO listings at stage g of the listing process that are close to (further away from) property i.

In a majority of cases, the number of *Close* REO listings is zero; the average number of *Close* REO listings conditional on at least one *Close* REO listing is 2.2. We take logarithms of the counts, as we did in Section 3, to allow for a concave competitive effect.[19][20] α_{jt} is a set of quarter-by-census tract fixed effects, where j indexes the census tract. X_{it} are controls for square feet, age, their squares, TOM, and dummies for whether the house is itself an REO and for whether it is single family.

We use price trends across the various listing stages in the *Far* group as a control, and interpret any additional price effects in the *Close* group as foreclosure spillovers.[21] In our baseline specification, *Close* equals 0.1 miles; *Far* equals 0.33 miles. The two key assumptions are that 1) home prices within 0.1 miles of a foreclosure would not be trending differently from home prices within 0.33 miles of a foreclosure in the absence of the foreclosure and

[19]Listings that are temporarily off-market (i.e. they are de-listed without sale and then re-listed less than 180 days later) are treated as active listings when calculating the values of the regressors in equation (3).

[20]We also ran a specification where instead of imposing concavity, we bin the number of foreclosures into groups > 0, > 1, and > 2. The results are similar. This is not surprising since most homes have 0 or 1 foreclosures nearby.

[21]It may also be reasonable to interpret the effect of the foreclosure on the *Far* group as a real spillover effect, rather than a pre-existing price trend. We offer evidence for this interpretation in Section 4.3.

2) foreclosure spillovers should be stronger for homes within 0.1 miles of a foreclosure relative to homes within 0.33 miles.

In our baseline specification, $G^F = G^{NF}$, and has four elements defined as:

- *Pre-Listing* (P): The 45 day interval immediately prior to listing.

- *During Listing* (D): The interval during the listing period (i.e. after initial listing, but before sale or withdrawal).

- *Soon After Listing* (SA): The 90 day interval immediately after sale or withdrawal.

- *After Listing* (A): The 90 day interval 3 to 6 months after sale or withdrawal.

The abbreviations for each interval are in parenthesis. Thus, $(\delta_{Close,D} - \delta_{Close,P})$ measures the *additional* percent change in sales price from a percent increase in REO listings located within 0.1 miles of a listing, relative to listings within 0.1-0.33 miles of the listing. $(\delta_{Far,D} - \delta_{Far,P})$ measures the the percent change in sales price from a percent increase in REO listings located within 0.1-0.33 miles of a listing

4.1.1 Results

We first present results where we pool the data from all the MSAs into a single regression. The qualitative results do not change when we run the regressions separately for each city, but the results are less precise. We return to the MSA level results below.

Figure 3 shows that home prices in the Far group decline by 0.8% after an REO listing, and this effect is statistically significant. The detailed regression output is reported in Table 4. The size of the additional competitive effect in the $Close$ group relative to the Far group is 0.6%. If we interpret the entire effect in the Far group as a spillover effect, then our estimated effect for one $Close$ REO listing is -1.5%. After the REOs and non-REOs are sold or withdrawn from the market, prices appear to gradually turn upward, consistent with the fact that the listing is no longer competing for buyers.[22] The upward movement is statistically significant; that is, we cannot reject the null that $\delta_{Close,A} = \delta_{Close,P}$, but we can reject the null that $\delta_{Close,A} = \delta_{Close,D}$.

The competitive effects in the *Close* group from non-REO listings, also shown in Figure 3, are statistically significant and almost identical in magnitude to the competitive effects from REO listings. This evidence is consistent with our interpretation that the effect during the listing period is a competitive effect. Non-REOs appear to have a smaller effect than REOs on the *Far* group. This suggests that the competitive effect of an REO is at least as strong (if the *Far* group measures pre-existing trends) or stronger (if the *Far* group measures a spillover effect) than the competitive effect of a non-REO listing. After presenting all the evidence, we will conclude that the effect from REOs is actually stronger. One likely explanation for this result is that banks tend to price REOs aggressively for the reasons discussed in Section 1.

Our findings do not depend on our choice of a radius equal to 0.33 miles for the *Far* group. In Appendix Table 7, we present estimates of equation (3) where we vary the outer radius. The results are essentially unchanged when we use an outer radius of 0.25 as in Campbell et al. [2011]. We also find that beyond 0.5 miles, the estimated effect in the *Far* group is close to zero, consistent with our results in Section 3 and Appendix Figure 2. Our results do depend on the type of fixed effects included. Appendix Table 7 shows that when we use quarter-by-city fixed effects – which is a much more aggregated level of geographic control than in our baseline specification, then the effect of one *Close* REO listing is -2.3% if we interpret the effect of the *Far* group as a spillover effect, which is about 50 percent larger than in our baseline estimate. However, with these broader geographic controls, it is more likely that some of the estimate in the *Far* group reflects pre-existing trends rather than a spillover effect and so it is not surprising that the estimate is larger.

Appendix Figure 4 presents results separately by MSA. The listing effect of REOs is comparable across MSAs. The case where there is the most het-

[22]The price recovery need not be immediate because the decrease in supply may be offset by the absorption of demand from the REO that sells.

erogeneity across MSAs is in the effect of an additional Far non-REO. In Phoenix, for example, there is actually a small increase in prices during the non-REO listing period.

We also test whether the listing effect depends on how substitutable the listings are with the nearby home sales. If the spillover effect identified here is a competitive effect, then the magnitude of the price decline should be stronger when the listing is a closer substitute to the home sale. We define substitutability as in equation (2), except we zero out the component related to physical distance since the home sales of interest in this specification are already restricted to be within 0.1 miles of the REO. We categorize each home sale near REO (non-REO) listings as "Similar" if $dist_{it}^{REO} < median(dist_{it}^{nonREO})$ ($dist_{it}^{nonREO} < median(dist_{it}^{nonREO})$), where the median is taken over all sales with at least one REO (non-REO) listed within 0.33 miles. Then, we interact a "Similar" dummy with $F^{Close,D}$ and $NF^{Close,D}$.[23] The results are shown in Table 4. For non-REO listings, the competitive effect is 1.2 percent stronger and statistically significant when the listing is more similar in observables to the home sale. For REO listings, the competitive effect is 0.5 percent stronger and statistically significant when the listing is more similar. We cannot reject the null that the similarity effect of REOs is different from the similarity effect of non-REOs. In Appendix Table 7, we test the robustness to our similarity cutoff by defining "Similar" as $dist_{it}^{\tau} < pct25(dist_{it}^{nonREO})$ for $\tau = REO, nonREO$. The results are comparable.

Finally, we test whether competition also lengthens the number of weeks it takes for a listing to sell or withdraw. This could be the case if, for example, there are a fixed number of potential buyers inspecting listed homes in a local area each period, and there is a cost to inspecting an additional home. The right-most columns of Table 4 present results of equation (3) when we change the dependent variable to log(weeks on market). REO listings do have

[23] We also include regressors to control for the possibility that homogeneity of a home is correlated with unobserved house quality, and/or the possibility that areas within a tract with more homogenous homes have differential price trends. To this end, we include "Similar" interacted with $F^{Far,D}$; "Similar" interacted with $NF^{Far,D}$; "Similar" interacted with $F^{Far,P}$; and "Similar" interacted with $NF^{Far,P}$.

a statistically significant, positive effect on TOM, but the magnitude is small. The effect of an additional listing (relative to zero listings) is 2 percent. New non-REO listings have the same effect on TOM. As with sales prices, TOM recovers to its pre-listing levels once the REOs leave the market.

4.2 Disamenity Effect

To test for the disamenity effect, we expand G^F in equation (3) to include time intervals well before the listing period. Specifically, we add

- *F-360 to F-270*: The 90 day interval 9 to 12 months before foreclosure.
- *F-270 to F-180*: The 90 day interval 6 to 9 months before foreclosure.
- *F-180 to F-90*: The 90 day interval 3 to 6 months before foreclosure.
- *F-90 to F*: The 90 day interval 3 months before foreclosure.
- *F to L*: The window after foreclosure but before listing.[24]

We let the count of nearby foreclosures enter the regression linearly in this specification.[25]

4.2.1 Results

There is some evidence of a pre-listing price decline, particularly during the period right before foreclosure, but prices bounce back up after foreclosure and before the REO is listed. The results are shown in Figure 4. In the *Far* group, shown in the top-right panel of Figure 4, the price trend is flat. This suggests that any foreclosure spillovers that operate in the pre-listing period die out beyond 0.1 miles of the foreclosure.

[24] We exclude *Pre-Listing* from G^F because it overlaps with *F to L*

[25] We tried other functional forms as well. When we bin the number of foreclosures into groups > 0, > 1, and > 2, we get approximately a linear relationship. However, we do not have enough sales nearby multiple foreclosures to make any strong conclusions on whether the disamenity effect is nonlinear in the number of nearby foreclosures.

To further explore the results in Figure 4, we test whether the disamenity effect is heterogeneous across foreclosures. We should expect a zero disamenity effect for many foreclosures because 1) not all homeowners will neglect their homes once foreclosure becomes imminent, and even if they do, not all forms of neglect will affect neighboring house prices and 2) banks can and do hire companies that provide property preservation services for foreclosures.[26] Ideally, we would like to observe the property condition of each foreclosure in our sample, and then test whether the pre-listing decline is stronger for poorly maintained foreclosures. Since property condition is unobserved in our dataset, we estimate foreclosure spillover effects separately for census tracts with high housing density and low housing value and census tracts with low housing density or high housing value. The separation of census tracts along these dimensions is motivated by two considerations: 1) the bank's return to property maintenance may be lower in low-demand areas and 2) a dense urban environment is more likely to attract crime or vandalism. Furthermore, data from Dataquick and from the Campbell/Inside Mortgage Finance survey indicate that REOs in such neighborhoods are indeed more likely to be in poor condition.[27]

We interact a dummy variable for "High Density, Low Value" with $F^{Close,g}$ $\forall g$ to test whether foreclosures in these types of census tracts are more likely to give rise to a disamenity effect.[28] We continue to include our census tract-by-quarter fixed effects so that differential trends in these types of tracts are not driving the results. A clearer pre-listing pattern emerges when we divide census tracts in this way. The bottom panel of Figure 4 shows that in high density, low value areas, there is a steady pre-listing price decline for homes nearby foreclosures, and the decline is present in all four MSAs. On average, prices drop by 1.5 percent for each additional foreclosure over the entire pre-

[26] For an example, see Safeguard Properties (http://www.safeguardproperties.com/).

[27] Appendix A.4 explains the detail behind these calculations.

[28] We define a census tract in MSA m as "High Density, Low Value" if 1) its density is below the median density of all census tracts in our four MSAs and 2) its median sales price is below the median sales price in MSA m. About 1/4 of sales in our sample are in "High Density, Low Value" tracts. San Francisco has the highest share at 33 percent; DC the lowest at 22 percent.

listing phase. However, in all other census tracts, there is no evidence of a pre-listing price decline. In these neighborhoods, the disamenity effect does not appear to be a source of price decline, on average.[29]

That foreclosures only cause pre-listing price declines in neighborhoods where damage to foreclosures is especially prevalent is strong evidence that the source of these declines is the disamenity effect rather than anticipation of a competitive effect. As an additional check on this conclusion, we test for pre-listing price declines for non-REO listings. If the pre-listing price declines identified above are related to competition, then they should appear prior to non-REO listings as well. If they are due to a disamenity effect, then they should not appear prior to non-REO listings, assuming that non-REOs do not depress prices through a disamenity effect. The results are plotted in Figure 5. Indeed, the pre-listing trend is flat in "High Density, Low Value" tracts, and all others as well.

We present the results separately by MSA in Appendix Figure 3. The patterns described for the pooled sample generally hold for each MSA individually. The pre-listing price decline in high density, low value census tracts is largest in Chicago, but are statistically significant in the other cities as well.

4.3 Discussion

Our initial motivation for dividing foreclosures into Far and $Close$ groups was to use price trends in the Far group as a control for any pre-existing trends in the $Close$ group. However, our empirical results allow us to interpret the price decline in the Far group during the listing period as a spillover effect caused by competition. We make this interpretation because prices are flat for the Far group during the pre-listing period (see Figure 4). If the price decline during the listing period for homes in the Far group were due to an exogenous downward trend, then the trend should appear in the pre-listing period as well. Moreover, prices in the Far group should not turn up after the

[29] Appendix Table 7 shows that this result is robust to alternative choices for the radius in the Far group. In unreported results, we find that the listing effect for both REOs and non-REOs does not depend on whether the tract is high density, low value.

REO sale as they do in the data. Thus, our estimate of the competitive effect of one REO in the *Close* group is 1.6%. In high density, low value census tracts, this is comparable to the estimated disamenity effect at 1.5%. In the majority of census tracts, we find that the disamenity effect is close to zero and thus the competitive effect dominates.

Our results as a whole also allow us to further interpret the nature of the competitive effect. First, we do not believe that this effect operates through the use of foreclosures as comparables because the estimated spillover effect disappears within 3 to 6 months of the REO exiting the market. This finding is consistent with Campbell et al. [2011], who find that at the zip code level, REO prices have little predictive power for the prices of non-REO sales. Second, the competitive effect does not operate through the composition of houses that remain on the market in response to a new REO listing. We show that sellers are not especially more likely to exit the market in response to a new REO listing, but instead respond by lowering their asking prices. Therefore, the price effect of a new foreclosure is not attributable to a reduction in the quality of houses for sale, but rather to a change in seller behavior induced by a new competitor.

Finally, we believe that our results offer a sensible unification of the literature regarding disamenity versus competition effects. Campbell et al. [2011] find evidence that the impact of foreclosures, as measured by the flow of foreclosure completions, is stronger on properties which sell after the foreclosure than on properties which sell before the foreclosure. To the extent that the flow of nearby foreclosures before a property sale proxies for the stock of REO listings during the property sale, and to the extent that the flow of nearby foreclosures after a property sale proxies for the stock of foreclosures in the pre-listing period during the property sale, then their results suggest that the competitive effect dominates. We believe this is a reasonable interpretation of their results in light of our new evidence, especially given the similarity in magnitudes of our measured effects. Our findings that disamenity effects are relevant in areas where REOs are more likely to be damaged is consistent with the evidence in Gerardi et al. [2012] that REOs in poor condition exert a larger spillover effect. We differ from Gerardi et al. [2012] in that we also find evidence of an additional local spillover effect due to competition. One possible explanation for this difference is that Gerardi et al. [2012] have a noisier measure of when the competitive effect arises (because they do not observe the exact dates that the REO is on the MLS and competing for buyers), which may bias them against finding a competitive effect.

5 Conclusion

Our results combine to show that, first, there is a direct effect of foreclosures on neighborhood house prices, and second, that the competitive pressure a foreclosed property exerts on nearby sellers is an important source of the spillover effect. We also find that disamenity effects can be an important source of the spillover, but we only find evidence for them in neighborhoods with high housing density and low property values.

Our new findings on the nature of foreclosure spillover effects have practical policy implications. First, our results suggest that decreasing the number of REOs for sale – such as by redirecting some of these properties to the rental market – should alleviate some price pressure for neighborhoods hit hard by foreclosures. Second, the existence of foreclosure spillovers is often used to justify foreclosure prevention policies and the size of the spillover effect is often cited when discussing the potential social benefits of foreclosure prevention policies. While our results on the presence of disamenity effects show that some of this spillover effect creates economic inefficiency that may justify policy intervention, our results also suggest that a bulk of the spillover costs associated with foreclosure are due to simple price competition, which we do not typically think of as an externality justifying policy intervention. That said, we cannot say whether estimates of the local spillover effect understate or overstate the social benefits of foreclosure prevention policies, and thus we leave a proper evaluation of the welfare consequences of various foreclosure policies to future research.

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A For Online Publication Only: Appendix

A.1 Model of Price Setting in Response to New REO Listing

Here we present a stylized model to understand how the pricing pattern in Figure 1 emerges in equilibrium. Suppose there are two players $i = 1, 2$ and two time periods $t = 1, 2$. Each player has a single house of identical quality to sell. The demand for house i can be summarized by the function

$$\gamma(p_{it}^L, p_{-it}^L, R_t) \tag{4}$$

where $\gamma()$ denotes the probability that player $i's$ house sells given each players' list price, p^L, and R, which is a dummy variable equal to one if there is an REO listing, exogenous to the model, to compete with. We assume that

1. $\frac{\partial \gamma}{\partial p_i^L} < 0$
2. $\frac{\partial \gamma}{\partial p_{-i}^L} > 0$
3. $\gamma(p_i^L, p_{-i}^L, 1) < \gamma(p_i^L, p_{-i}^L, 0)\ \forall p_i^L, p_{-i}^L$

We assume that $R_1 = 0$ and $R_2 = 1$. R_t is observable to both players at time t. We impose the following information asymmetry at $t = 1$: one of the players knows that $R_2 = 1$ whereas the other player does not know R_2, but believes that R_2 is Bernoulli. Otherwise, the two players are identical.

We assume that if a home sells, it sells at its list price. For simplicity we assume that the discount factor equals one. We write player $i's$ expected profit function in $t = 1$ as

$$\Pi_i^1 = \gamma(p_{i1}^L, p_{-i1}^L, 0) * p_{i1}^L + (1 - \gamma(p_{i1}^L, p_{-i1}^L, 0)) * \Pi_i^2. \tag{5}$$

Π_i^2 takes a similar form, except if the home does not sell, the seller receives some exogenous terminal utility x. Consider the informed player's optimal choice of period 1 price in a pure-strategy Bayesian Nash equilibrium. He can pretend he is not informed about R_2, and price according to the equilibrium that would arise if both players are symmetrically uninformed about R_2. Alternatively, he could lower his price to increase his chances of selling in $t = 1$ since he knows demand in $t = 2$ will be low. It is straightforward to show that this is exactly what he would do if he were a monopolist. However, by lowering his price, the informed player signals to the uninformed player that demand will be low, which would cause the uninformed player to lower his period 1 price in equilibrium. Thus, some of the gains that the informed seller would get from lowering his price are competed away.

Whether the informed player prices low or high depends on the elasticity of $\gamma()$ with respect to price. For $\gamma()$ sufficiently inelastic, the informed player will not adjust his period 1 price for the impending REO listing. In period 2, both players will lower their prices once $R_2 = 1$ becomes common knowledge. Under this parametrization, the equilibrium price pattern is just as it appears in Figure 1.

A.2 Data Selection

We began the project by investigating San Francisco only. We chose San Francisco because we had already purchased and cleaned the data for a different project. We subsequently decided to expand our sample to additional cities. Our budget allowed us to purchase data for three additional cities. To be a viable candidate for our analysis, the city must have transaction data in Dataquick and listing data in Altos Research. The latter criteria is more restrictive, since Altos Research does not have listing data for every MSA. We went down the list of most populous MSAs according to the Census in order until we obtained three cities that met our criteria. Chicago is the 3rd most populous MSA, Washington the 7th, and Phoenix the 14th. San Francisco ranks 11th. Los Angeles (2) and Riverside (12) were both viable candidates, but we chose not to consider them because they are both California markets and share similarities with San Francisco. Our estimation samples begin in 2007 because that is the earliest date that the listing data is available (the variation in start dates by MSA reflects variation in when Altos Research started collecting data for different cities). Our sample ends in 2009 because we do not have Dataquick data beyond that time period.

A.3 Data Appendix

A.3.1 San Francisco

We first describe how we merge the listing data from Altos Research with the transaction data from Dataquick. The listing data contains separate variables for the street address, city, and zip code of each listing. The address variable contains the house number, the street name, and the street suffix in that order as a single string. We alter the street suffixes to make them consistent with the street suffixes in the transaction data (e.g. change "road" to "rd", "avenue" to "ave", etc). In some cases, the same house is listed under 2 slightly different addresses (e.g. "123 Main" and "123 Main St") with the same MLSIDs. We combine listings where the address is different, but the city and zip are the same, the MLSids are the same, the difference in dates between the two listings is less than 3 weeks, and at least one of the following conditions applies:

1. The listings have the same year built and the ratio of the list prices is greater than 0.9 and less than 1.1.

2. The listings have the same square feet and the ratio of the list prices is greater than 0.9 and less than 1.1.

3. The listings have the same lotsize and the ratio of the list prices is greater than 0.9 and less than 1.1.

4. The first five characters of the address are the same.

The address variables in the transaction data are clean and standardized because they come from county assessor files. We merge the listing data and the transaction data together using the address. We classify a listing as a sale if there is a match and the difference in closing date (the date in the transaction data) and the agreement date (the date the property is deslisted from the MLS) is greater than zero and less than 365 days. If a listing merges with an observation in the transaction data that does not satisfy this timing criteria, we record the latitude and longitude coordinates of the property but do not treat the listing as a sale. We drop all listings that do not match to at least 1 record in the transaction data because we do not have the latitude and longitude for these listings.[30] Listings do not match to a sales record for one of two reasons: a listing last sold prior to 1988 or there is a quirk in the way the address is recorded in the transaction or listing data. Before we do the merge, we flag properties that sold more than once during a 1.5 year span during our sample period. To avoid confusion during the merge that can arise from multiple sales occurring close together, we drop any listings that merge to one of these flagged properties ($<$ 1 percent of listings). We also drop listings where the ratio of the minimum list price to the maximum list price is less than the first percentile.

For the list price specifications, we do not treat listings where the initial listing date is the first week in our dataset as a new listing. We do this because we do not know whether these listings truly began in the initial week of the sample, or whether they had been on the market previously. For the specifications that use sales prices and TOM as the dependent variable, we make the following restrictions to the estimation sample:

1. Drop sales with prices that are below the 1st and 99th percentiles, respectively. Drop sales with square feet equal to zero or greater than 5000.

2. Drop sales where the TOM is greater than 2 years ($<$ 10 sales).

Furthermore, because we want to use the agreement date of a home sale to more precisely categorize homes sales into pre nearby foreclosure, post nearby foreclosure, etc. in Section 4, our estimation sample only uses home sales that match to a listing.

[30]This eliminates about 15 percent of listings. These dropped listings do not include REO listings because an observation appears in the transaction data at the foreclosure sale date.

We spent a great deal of time familiarizing ourselves with the data to develop the following algorithm that we believe to be highly accurate in identifying REO listings. We classify a listing as an REO if it merges with an arms length sales record where the following conditions hold:

1. The buyer's name does not have a comma, which always separates a last name and a first name in our dataset. This suggests that the buyer is not an individual and perhaps is a bank.

2. The buyer's name does not contain the strings "ESTATE", "FAMILY", "LIVING", "RELOC".

3. The buyer's name contains strings that suggest it is a bank, mortgage servicing company, or GSE (e.g. "BANK", "MTG", "FANNIE").

These arms length transactions are the transfer of ownership when a foreclosure occurs. In most cases, a non-arms length transaction occurs within a couple years of this transfer where the seller is a non-individual. This subsequent sale is the REO. We use the transfer rather than the REO sale to identify REO listings because our transaction data is right-censored. We do, however, use the seller names for the REO sales that we observe to help generate a list of strings that we search for in the buyer's name in the algorithm described above.

A.3.2 Chicago, Phoenix, Washington DC

As mention in Appendix A.2, we purchased the San Francisco data from Altos Research prior to purchasing the three other MSAs we consider in this paper. In between purchases, Altos Research made some improvements to their listing data[31], which allowed us to circumvent a number of the steps described above to arrive at our final, merged sample for Chicago, Phoenix, and DC. In particular, the raw address for each listing is broken out into separate street number, street name, and zip code variables. This circumvented the need to clean the

[31] The Dataquick data is formatted identically and contains the same information across all four MSAs.

address variables in the ways described for San Francisco. Each listing also had a property id, which circumvented the need to manually combine listings with slightly different addresses, as described above. The final improvement to the data is that the latitude and longitude coordinates are reported for each listing in the listing data. Thus, for Chicago, Phoenix, and DC, we do not need to do a preliminary merge with Dataquick as we did for San Francisco, where we merged the listing with *any* sales record for that property in Dataquick to obtain the latitude and longitude coordinates.

A.4 Details on Analysis of Campbell/Inside Mortgage Finance Survey

This section describes how we use data from the Campbell survey to establish the following relationship: in census tracts where home prices are low and housing density is high, a larger share of REOs are likely to be damaged.

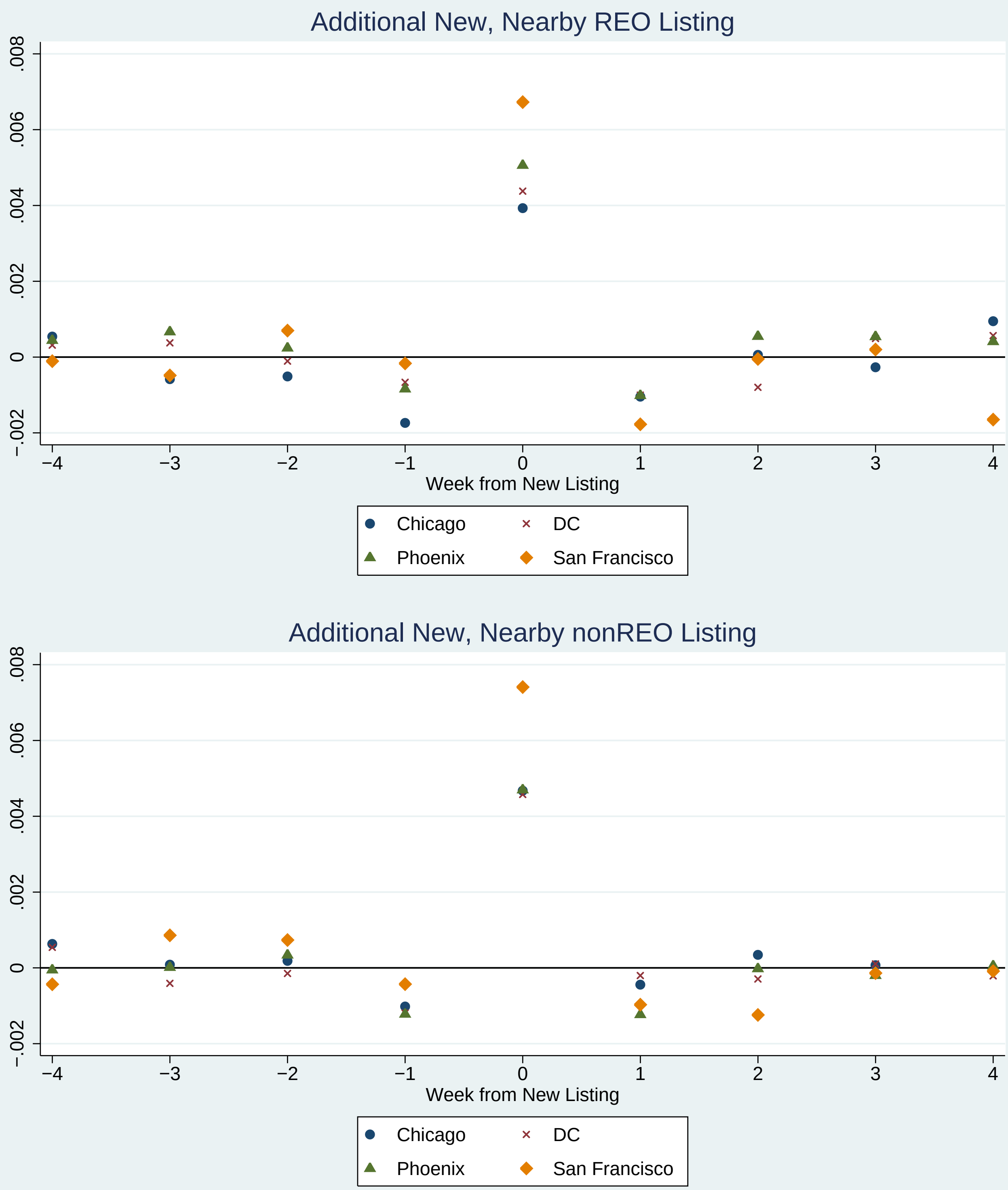
The Campbell Survey is a sample of over 150,000 home sales throughout the U.S. from July 2009-October 2012. The information for each sales record comes from the individual real estate agent involved in the transaction. The realtor reports several variables about the transaction, including whether the sale is REO, the condition of the home if the sale is REO (specifically, "damaged" or "Move-in-Ready" REO), the list price, the sales price, and the financing method of the buyer. We observe the state that the home is in, but not the exact address or even the city. In the data, we observe that a strong predictor of whether an REO is damaged or not is whether or not the buyer pays cash for the property. For example, 63 percent of damaged REOs are paid for in cash versus 27 percent for Move-in-Ready REOs. The unconditional cash average is 30 percent. The most likely reason for this empirical relationship is that investors, who are more likely to pay in cash, are more likely to buy damaged REOs.

We next look at the types of census tracts in our Dataquick data that have a large share of transactions where the buyer pays cash. We think that it is reasonable to expect that foreclosures in these census tracts are more likely to be damaged. We define a cash transaction in our dataset as a sale where the first, second, and third loan amounts equal zero. 37 percent of sales in our sample are cash sales. For each census tract, we calculate the share of all sales over our sample period that are cash sales. Then, we regress this share on the tract density[32] and the log of the median tract sales price relative to the log of the median MSA sales price, with MSA fixed effects.[33] A one standard deviation increase in tract density (price) increases (decreases) the cash share by .02 (-.03). The effects are statistically significant.

[32]The source for the housing density data is the 2000 census.

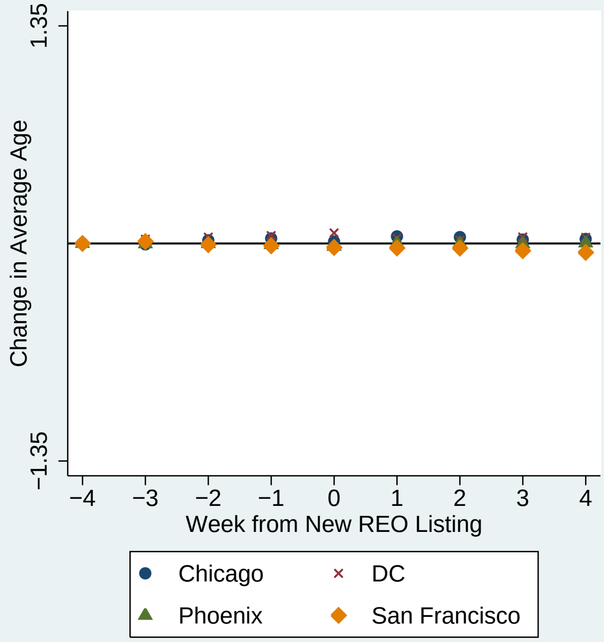
[33]We exclude tracts with less than 100 observations during our sample period.

Figure 1: Change in Probability of Adjusting List Price

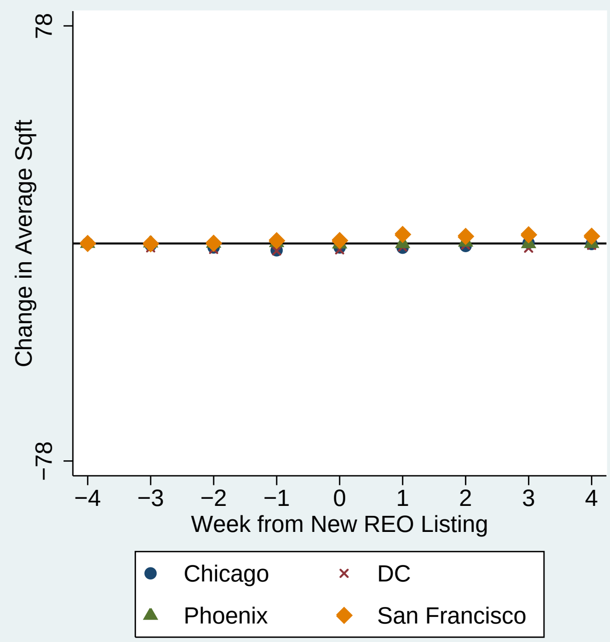


These figures show the change in the probability of adjusting list price in the 4 weeks before, the week of, and the 4 weeks after 1 nearby REO and nonREO are first listed for sale. The probability of adjusting list price is allowed to vary linearly with distance from the listing. The coefficients reported here are for the tenth percentile of distance. All changes are relative to a baseline probability of adjusting list price of around .07. The detailed regression output is reported in Table 3.

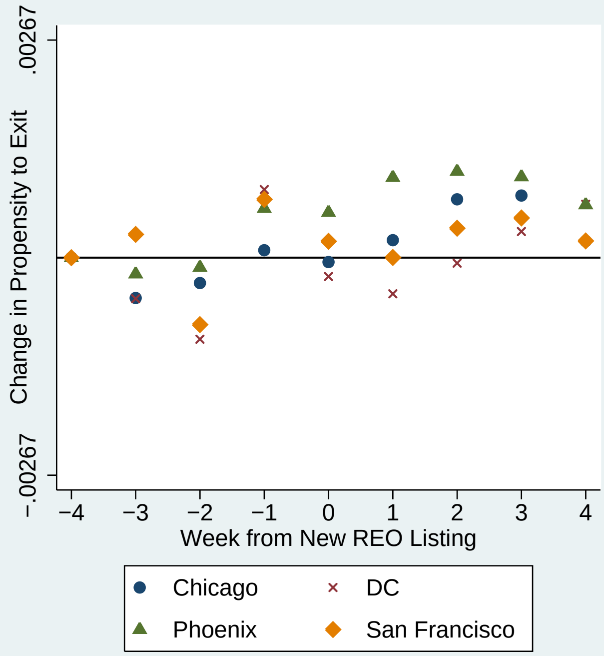
Figure 2: Change in Listings Composition



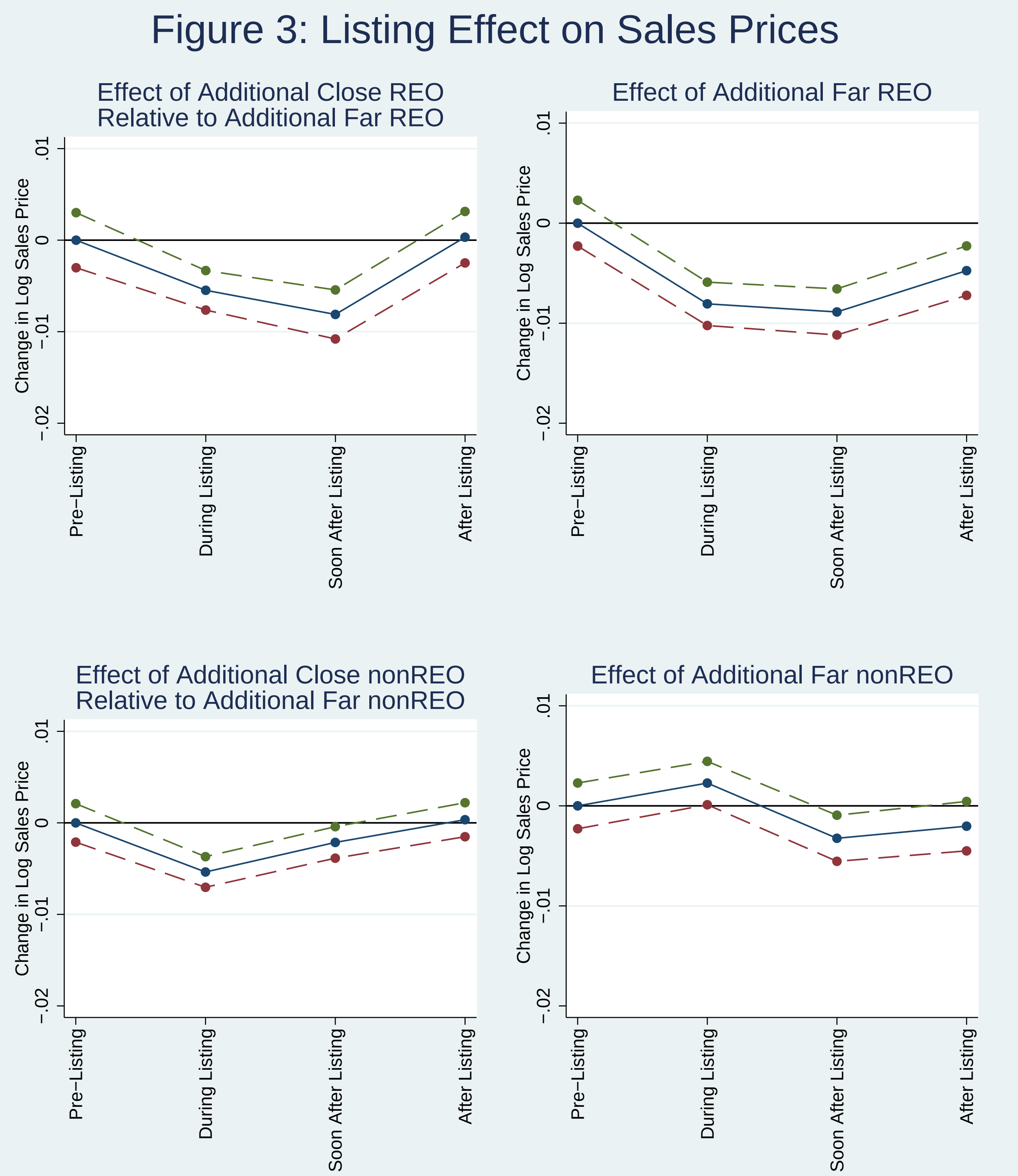
Note: This figure shows the average age of listings within 0.5 miles of a new REO listing in the 4 weeks before, the week of, and the 4 weeks after the listing is first listed for sale. The coefficients are changes relative to average age in week = −4. The upper (lower) limit on the y−axis is 5 (−5) percent of the average age in the sample.



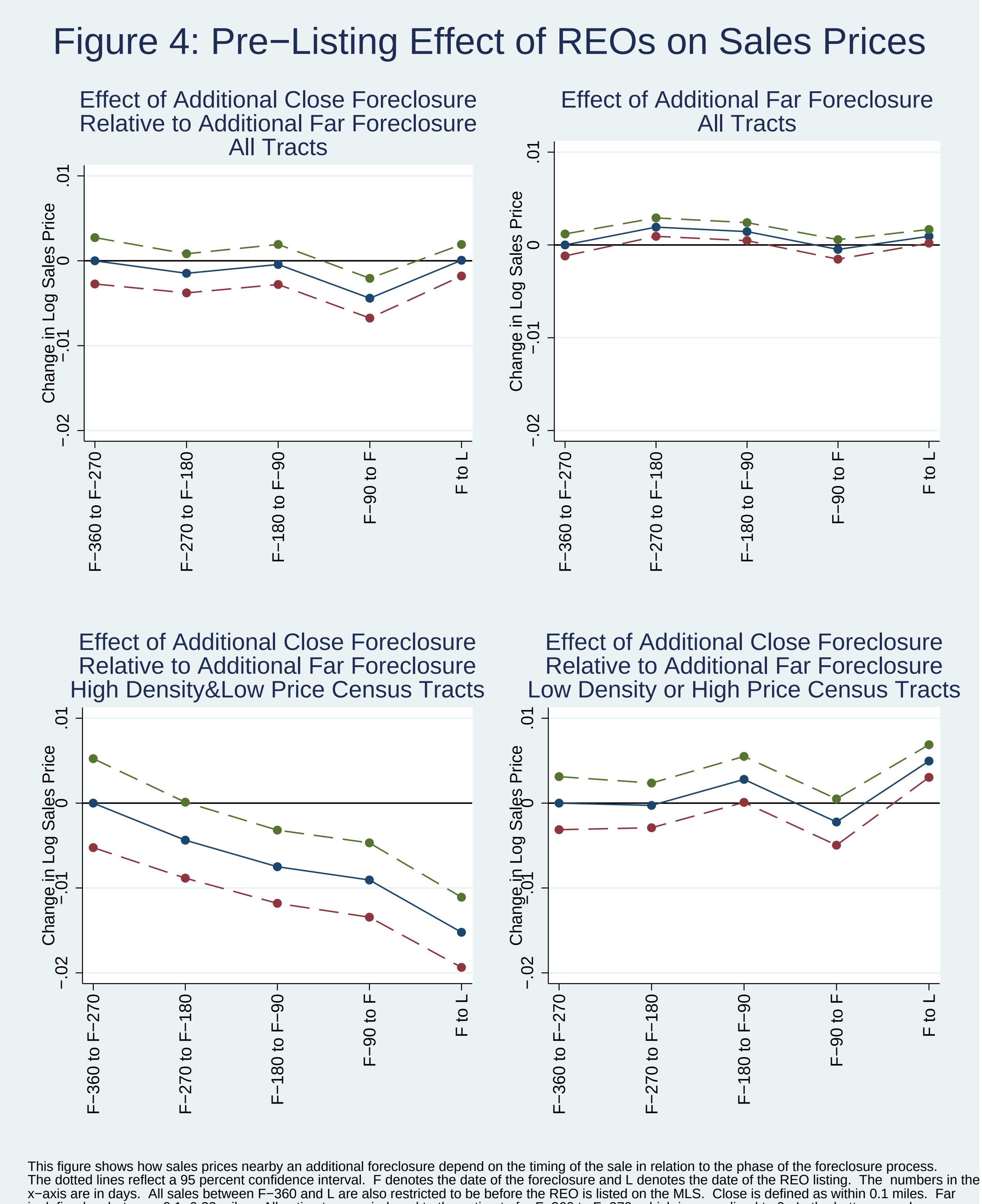
Note: This figure shows the average sqft of listings within 0.5 miles of a new REO listing in the 4 weeks before, the week of, and the 4 weeks after the listing is first listed for sale. The coefficients are changes relative to average sqft in week = −4. The upper (lower) limit on the y−axis is 5 (−5) percent of the average sqft in the sample.



Note: This figure shows the probability of exiting the market of listings within 0.5 miles of a new REO listing in the 4 weeks before, the week of, and the 4 weeks after the listing is first listed for sale. The coefficients are changes relative to the average exit probability in week = −4. The upper (lower) limit on the y−axis is 5 (−5) percent of the average weekly exit probability in the sample.

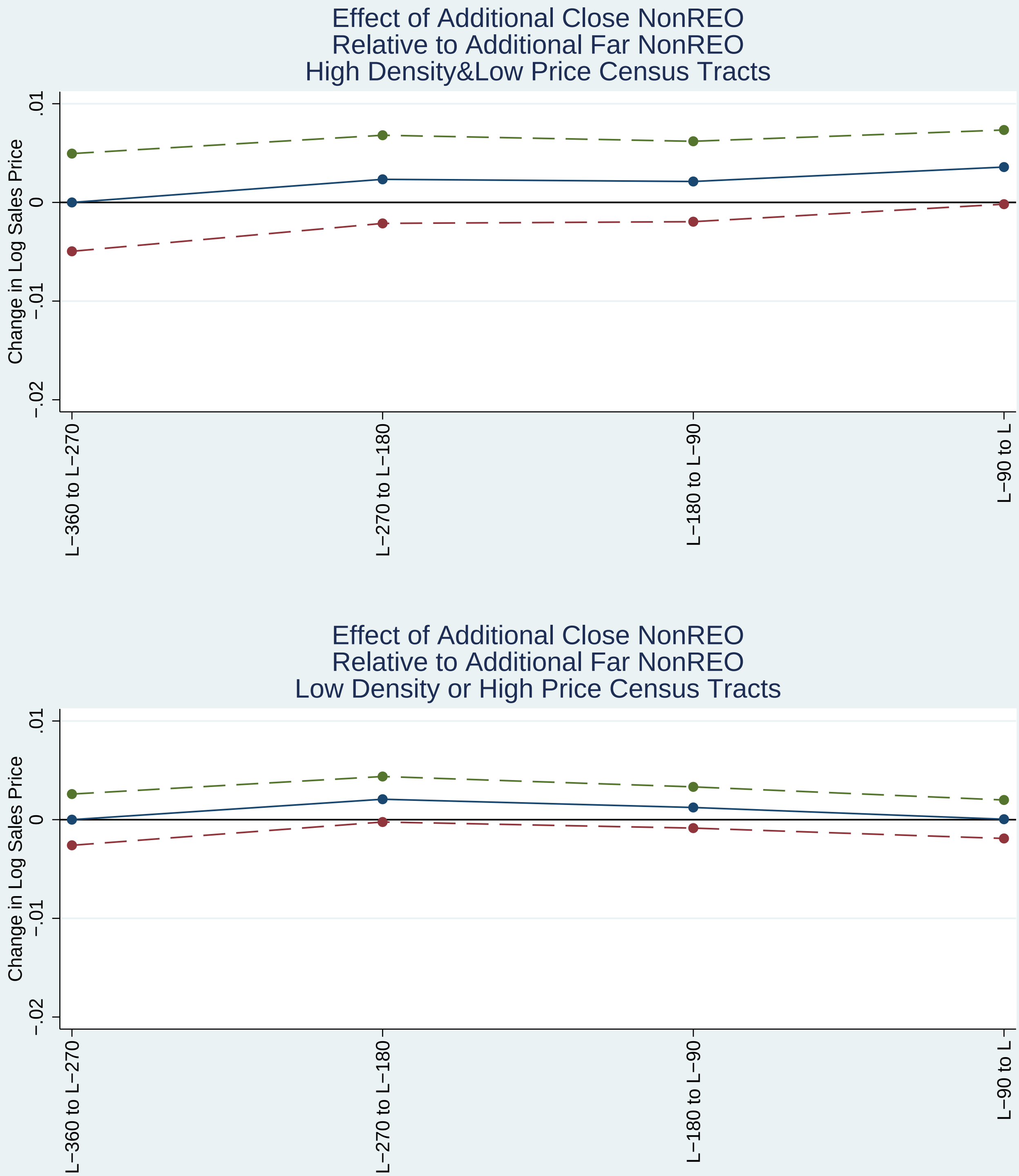


This figure shows how sales prices nearby a single REO/nonREO listing depend on the timing of the sale in relation to the listing date. The dotted lines reflect a 95 percent confidence interval. Pre−listing is the 45 day interval immediately prior to listing. Soon After Listing is the 90 day interval immediately after sale or withdrawal. After Listing is the 90 day interval 3 to 6 months after sale or withdrawal. Close is defined as within 0.1 miles. Far is defined as between 0.1−0.33 miles. All estimates are indexed to the estimate for Pre−Listing, which is normalized to 0. The change in sales price is relative to a sale with zero nearby REOs. Estimates are also summarized in Table 4.



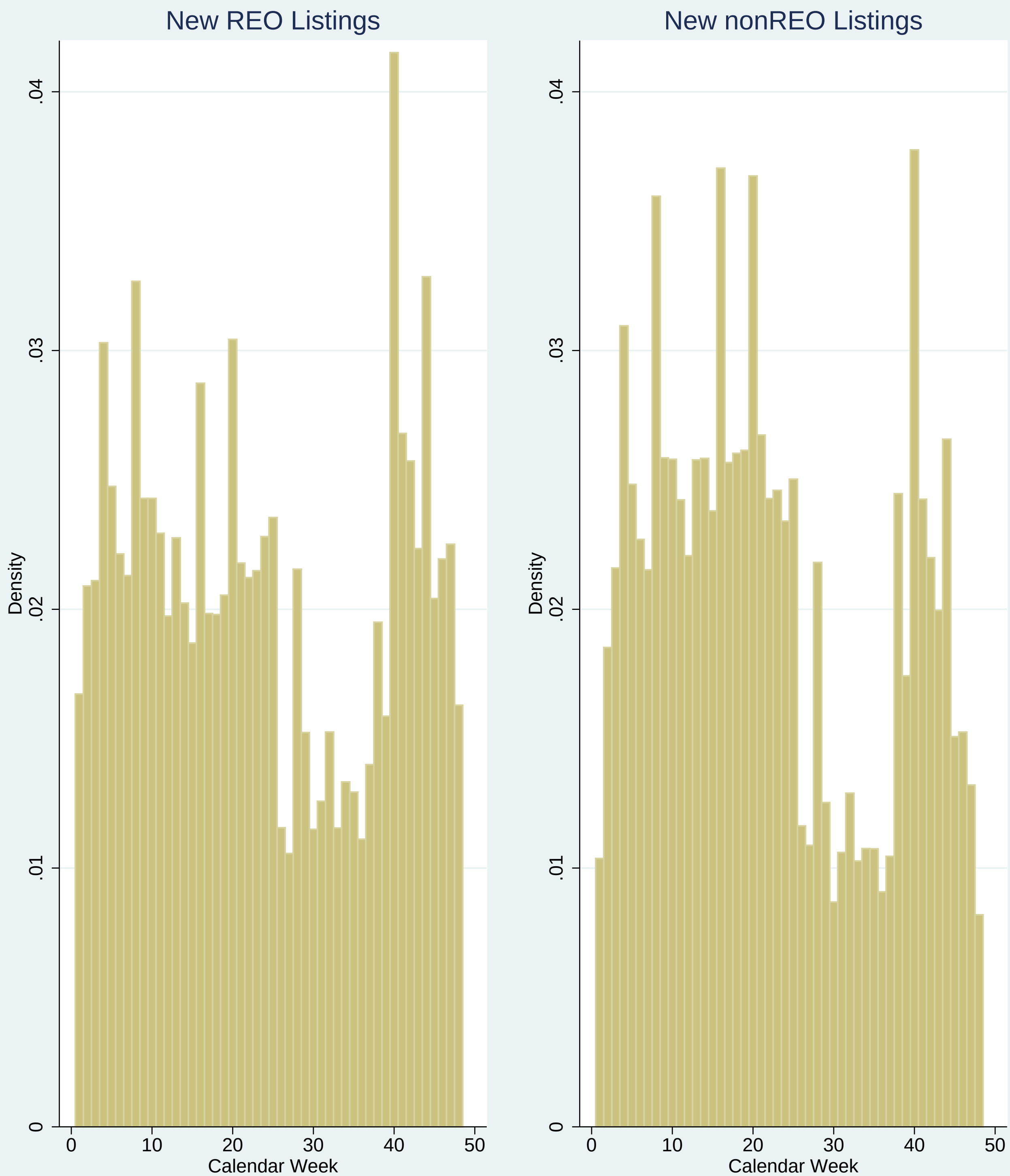
This figure shows how sales prices nearby an additional foreclosure depend on the timing of the sale in relation to the phase of the foreclosure process. The dotted lines reflect a 95 percent confidence interval. F denotes the date of the foreclosure and L denotes the date of the REO listing. The numbers in the x−axis are in days. All sales between F−360 and L are also restricted to be before the REO is listed on the MLS. Close is defined as within 0.1 miles. Far is defined as between 0.1−0.33 miles. All estimates are indexed to the estimate for F−360 to F−270, which is normalized to 0. In the bottom panel, we test for differential effects by census tract type, as defined in the main text.

Figure 5: Pre−Listing Effect of nonREOs on Sales Prices



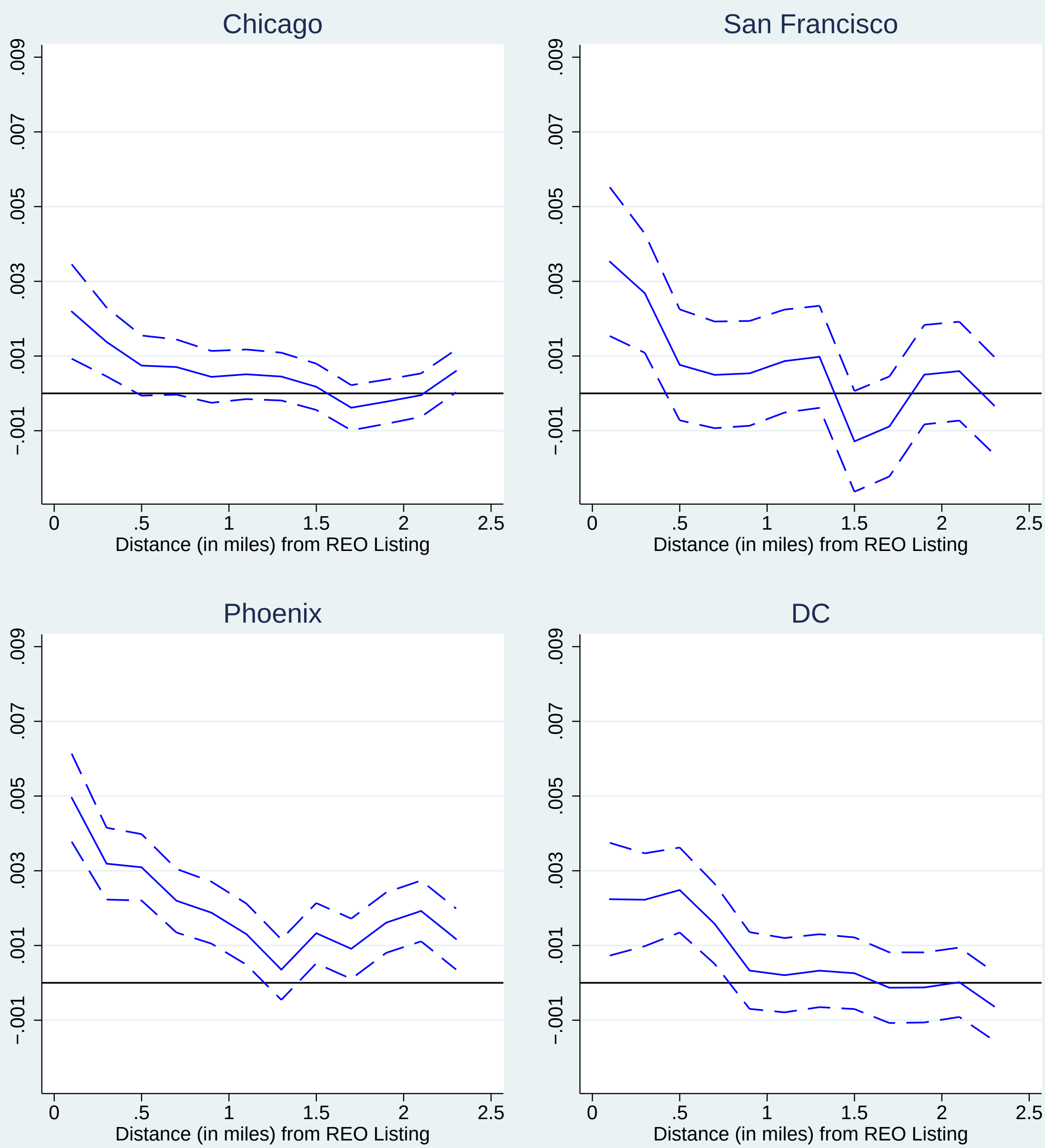
This figure shows how sales prices nearby an additional nonREO listing depend on the timing of the sale in relation to the nonREO listing date. The dotted lines reflect a 95 percent confidence interval. L denotes the date of the nonREO listing. The numbers in the x−axis are in days. Close is defined as within 0.1 miles. Far is defined as between 0.1−0.33 miles. All estimates are indexed to the estimate for L−360 to L−270, which is normalized to 0. Census tracts are grouped into low lensity and high price tracts based on the definitions in the main text.

Appendix Figure 1
Distribution of Calendar Weeks of Initial Listing



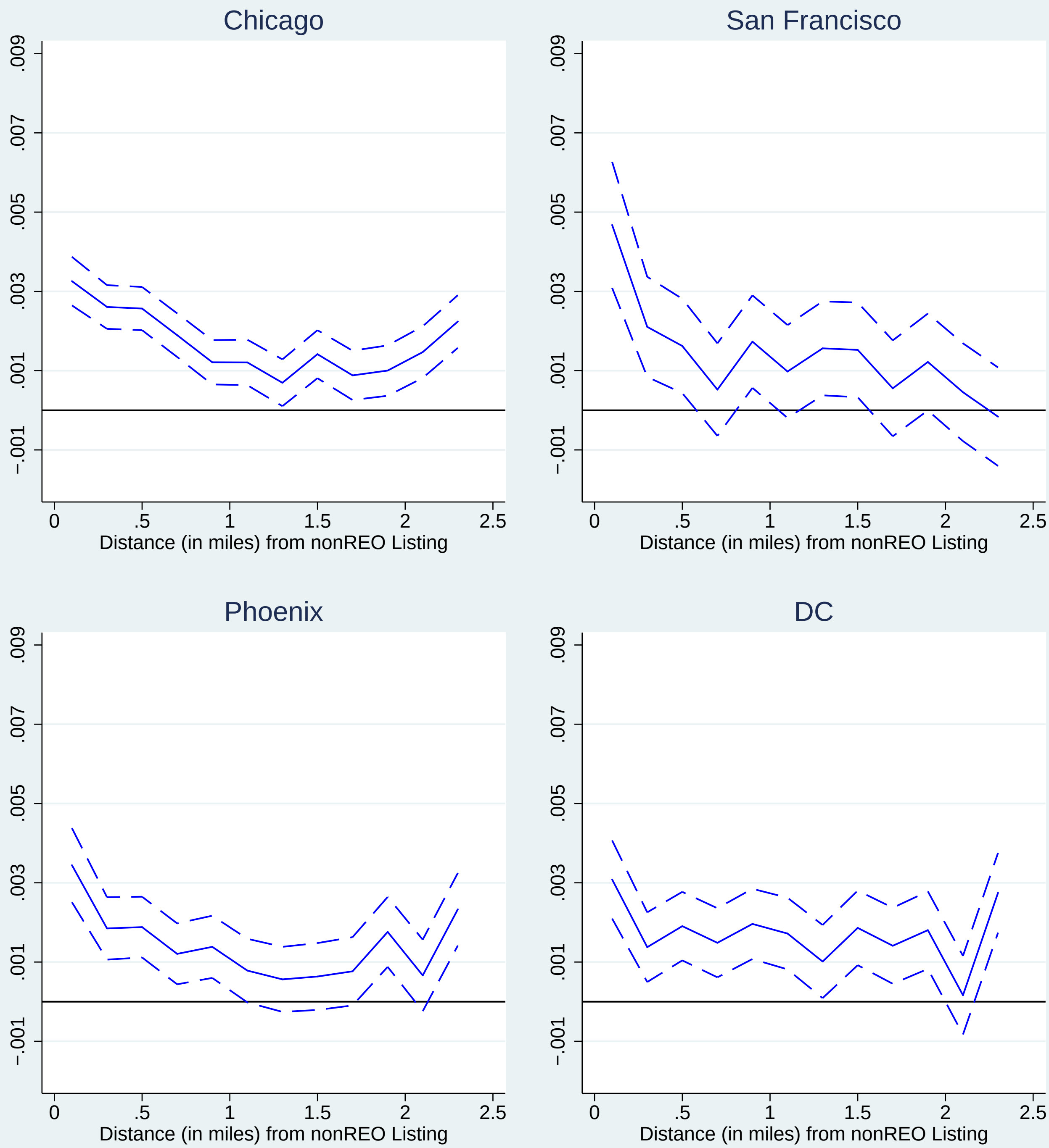
Note: Listing dates are divided into the 48 calendar weeks, where each month is assumed to have 4 weeks with 7 days per week, except for the 4th week of the month which includes all days in the month greater than 21.

Appendix Figure 2: Change in Probability of Adjusting List Price in Week of Additional New, Nearby REO Listing



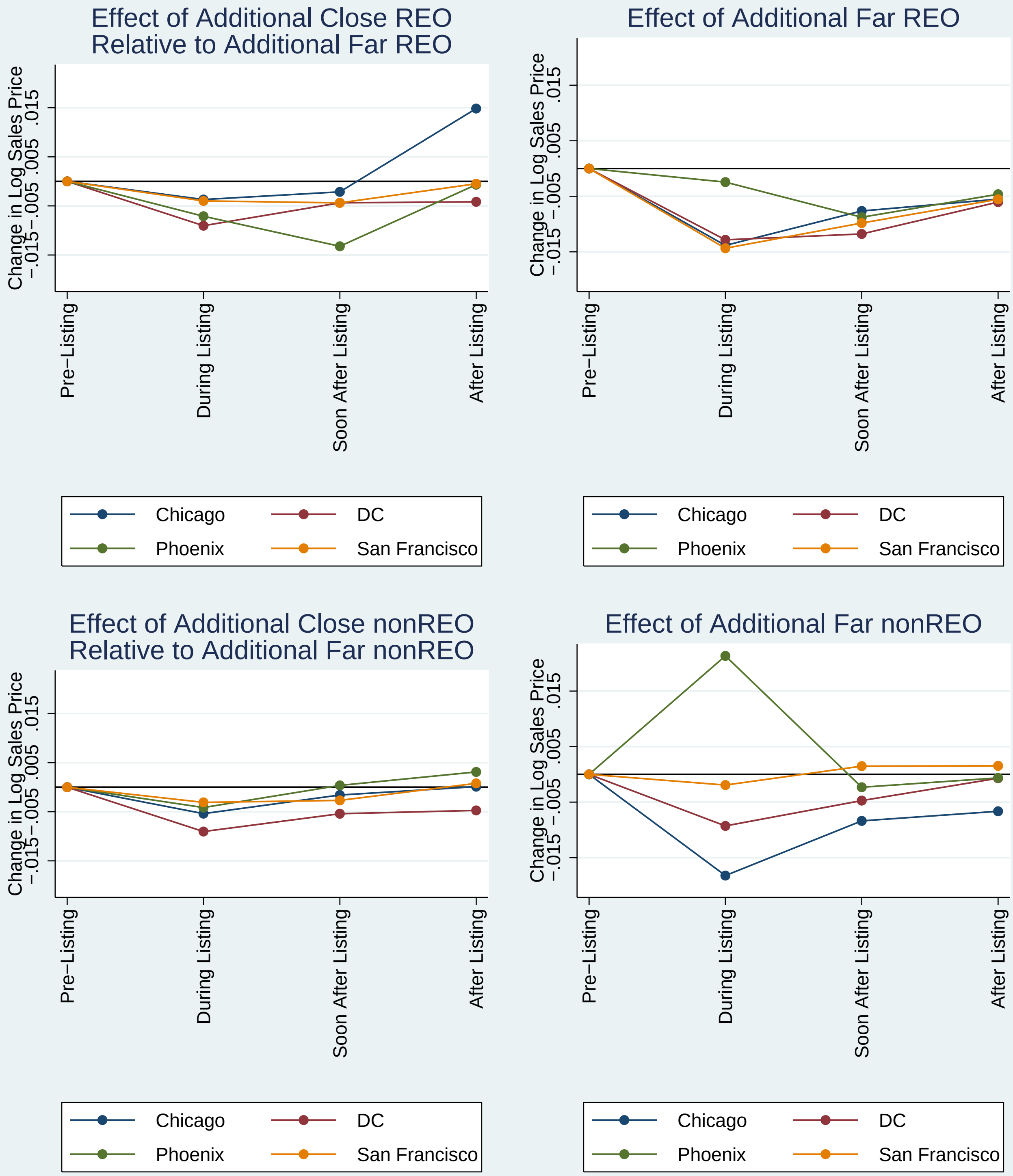
This figure shows results from a variant of the main specification where distance from the REO listing affects the probability of adjusting list price less parametrically. Each data point on the graph corresponds to a bin that is 0.2 miles wide.
For each distance bin, we include three dummy variables that indicate whether there are one, two, or more than two new listings.
The effects shown are for going from zero to one new listing. The dotted lines represent the 5th and 95th percentiles of the confidence interval.

Appendix Figure 3:
Change in Probability of Adjusting List Price
in Week of Additional New, Nearby nonREO Listing



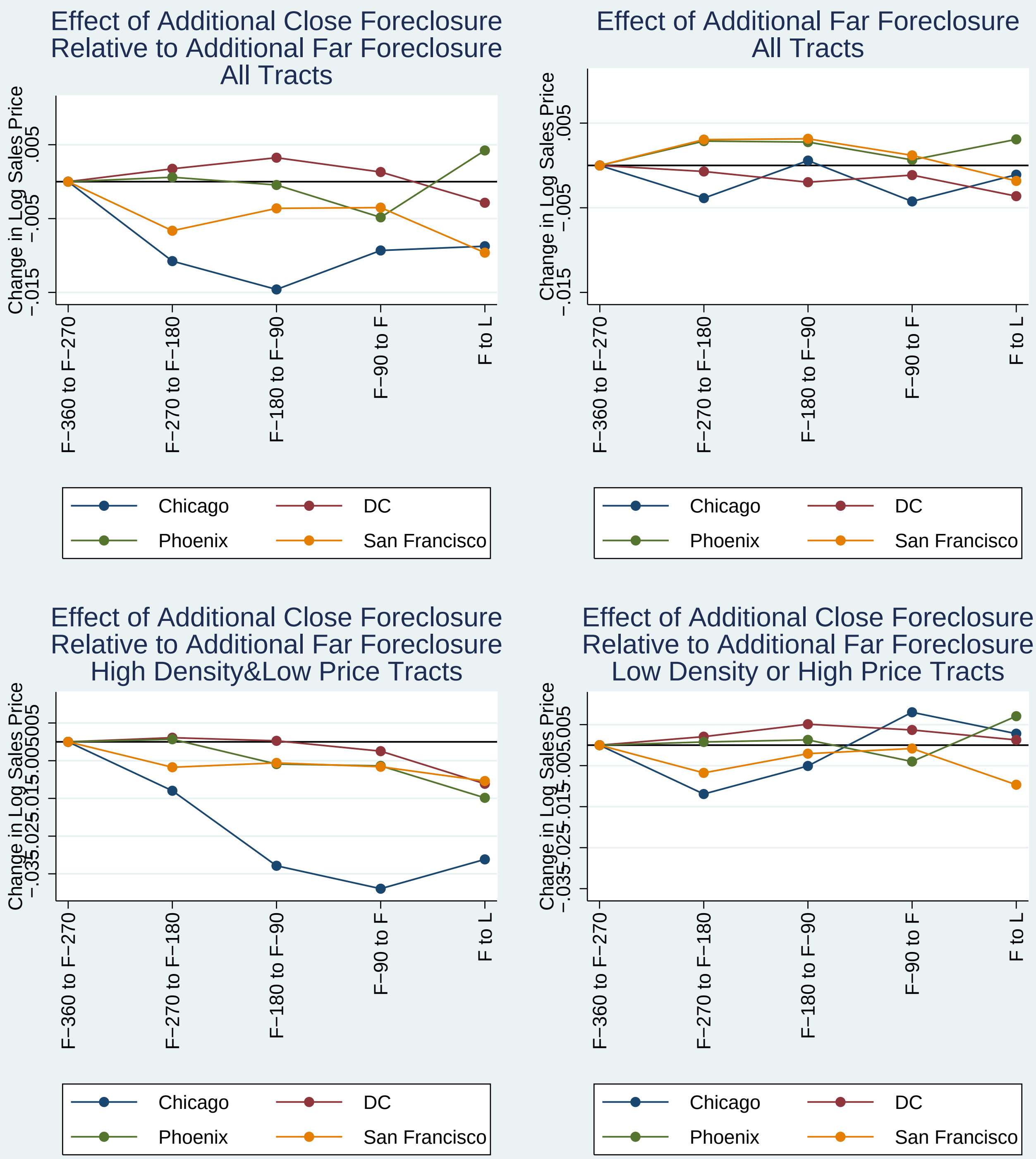
This figure shows results from a variant of the main specification where distance from the REO listing affects the probability of adjusting list price less parametrically. Each data point on the graph corresponds to a bin that is 0.2 miles wide.
For each distance bin, we include three dummy variables that indicate whether there are one, two, or more than two new listings.
The effects shown are for going from zero to one new listing. The dotted lines represent the 5th and 95th percentiles of the confidence interval.

Appendix Figure 4: Listing Effect on Sales Prices, by MSA



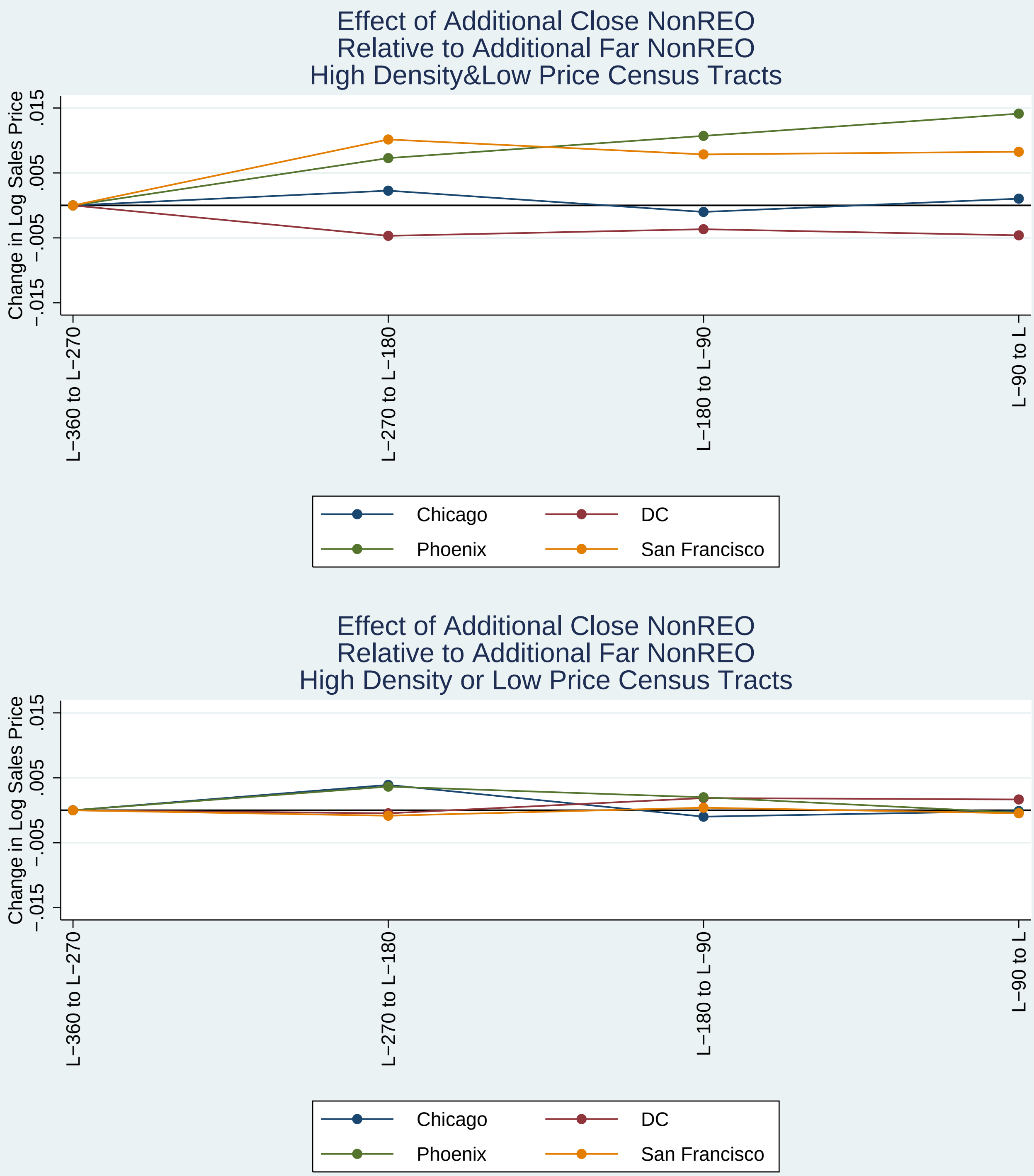
This figure shows how sales prices nearby a single REO/nonREO listing depend on the timing of the sale in relation to the listing date. Pre−listing is the 45 day interval immediately prior to listing. Soon After Listing is the 90 day interval immediately after sale or withdrawal. After Listing is the 90 day interval 3 to 6 months after sale or withdrawal. Close is defined as within 0.1 miles. Far is defined as between 0.1−0.33 miles. All estimates are indexed to the estimate for Pre−Listing, which is normalized to 0. The change in sales price is relative to a sale with zero nearby REOs. Estimates are also summarized in Table 4.

Appendix Figure 5: Pre−Listing Effect of REOs on Sales Prices, by MSA



This figure shows how sales prices nearby an additional foreclosure depend on the timing of the sale in relation to the phase of the foreclosure process. F denotes the date of the foreclosure and L denotes the date of the REO listing. The numbers in the x−axis are in days. All sales between F−360 and L are also restricted to be before the REO is listed on the MLS. Close is defined as within 0.1 miles. Far is defined as between 0.1−0.33 miles. All estimates are indexed to the estimate for F−360 to F−270, which is normalized to 0. In the bottom panel, we test for differential effects by census tract type, as defined in the main text.

Appendix Figure 6: Pre−Listing Effect of nonREOs on Sales Prices



This figure shows how sales prices nearby an additional nonREO listing depend on the timing of the sale in relation to the nonREO listing date.
L denotes the date of the nonREO listing. The numbers in the x−axis are in days. Close is defined as within 0.1 miles.
All estimates are indexed to the estimate for L−360 to L−270, which is normalized to 0. Census tracts are grouped into low lensity and high price tracts based on the definitions in the main text.

Table 1: Summary Statistics by Listing Category

		Sale Price ($)	Square Feet (1000's)	Age	Time on Market (Weeks)	Closing Gap[1] (Days)	Sale/List Price (Ratio)	$I[\text{List Price}_t \neq \text{List Price}_{t-1}]$[2] (Fraction)	Δ List Price[3] (%)
REO, No Sale (N=43,306)	Mean	.	1.735	37	20	.	.	0.07	-0.111
	p25	.	1.198	9	5	.	.	0	-0.154
	p50	.	1.534	33	14	.	.	0	-0.081
	p75	.	2.080	54	29	.	.	0	-0.038
REO, Sale (N=86,684)	Mean	227,812	1.820	27	21	48	0.98	0.08	-0.137
	p25	120,000	1.298	6	6	20	0.94	0	-0.187
	p50	185,000	1.650	22	15	35	0.99	0	-0.087
	p75	292,500	2.131	44	31	54	1.02	0	-0.047
Non-REO, No Sale (N=285,443)	Mean	.	1.827	34	21	.	.	0.04	-0.059
	p25	.	1.200	10	8	.	.	0	-0.078
	p50	.	1.629	28	17	.	.	0	-0.041
	p75	.	2.251	50	29	.	.	0	-0.020
Non-REO, Sale (N=155,384)	Mean	463,363	1.933	35	17	40	0.96	0.04	-0.058
	p25	226,357	1.320	12	5	12	0.93	0	-0.080
	p50	355,000	1.763	30	12	26	0.97	0	-0.046
	p75	610,000	2.371	51	24	42	1.00	0	-0.023
Nearby REO[4], Sale (N=87,562)	Mean	247,245	1.766	27	20	47	0.97	0.05	-0.095
	p25	133,900	1.262	6	6	18	0.93	0	-0.124
	p50	204,500	1.602	21	15	33	0.98	0	-0.058
	p75	315,000	2.125	43	29	50	1.00	0	-0.029
Total (N=570,817)	Mean	379,013	1.848	33	20	43	0.96	0.05	-0.079
	p25	175,000	1.252	10	7	14	0.93	0	-0.102
	p50	280,100	1.674	28	15	28	0.97	0	-0.051
	p75	480,000	2.250	50	28	47	1.00	0	-0.024

1. Defined as closing date - agreement date.
2. Takes on the value 1 if the list price does not equal the list price in the week before.
3. Conditional on a price change occurring.
4. The sale is within 0.1 miles of an active REO listing.

Table 2: Number of REO and non-REO Sales by MSA

MSA	REO	non-REO	Share REO
Chicago	9,100	39,221	19%
Phoenix	37,802	32,082	54%
San Francisco	18,744	44,085	30%
Washington DC	21,031	40,396	34%
Total	86,677	155,784	36%

Table 3: Effects of REO and NonREO Listings on List Prices

Dependent Variable: I[Change List]

	Baseline			
	Chicago	Phoenix	San Francisco	DC
Log(#REOs) coming on market in t+4 weeks	0.0004	0.0005*	0.0000	0.0002
	(0.0003)	(0.0002)	(0.0004)	(0.0003)
Log(#REOs) coming on market in t+3 weeks	-0.0004	0.0004*	-0.0003	0.0004
	(0.0003)	(0.0002)	(0.0004)	(0.0003)
Log(#REOs) coming on market in t+2 weeks	-0.0005	-0.0000	0.0003	-0.0001
	(0.0003)	(0.0003)	(0.0004)	(0.0003)
Log(#REOs) coming on market in t+1 weeks	-0.0016***	-0.0006**	-0.0002	-0.0005
	(0.0003)	(0.0002)	(0.0004)	(0.0003)
Log(#REOs) come on the market in week t	0.0057***	0.0074***	0.0098***	0.0064***
	(0.0009)	(0.0007)	(0.0014)	(0.0010)
(Log(#REOs) come on the market in week t)*Distance	-0.1327***	-0.1162***	-0.2195***	-0.0793
	(0.0420)	(0.0417)	(0.0730)	(0.0539)
Log(#REOs) came on the market in week t-1	-0.0009***	-0.0009***	-0.0013***	-0.0007**
	(0.0003)	(0.0002)	(0.0004)	(0.0003)
Log(#REOs) came on the market in week t-2	-0.0000	0.0004	-0.0000	-0.0006*
	(0.0003)	(0.0002)	(0.0004)	(0.0003)
Log(#REOs) came on the market in week t-3	-0.0002	0.0004	0.0000	0.0005*
	(0.0003)	(0.0002)	(0.0004)	(0.0003)
Log(#REOs) came on the market in week t-4	0.0008***	0.0003	-0.0012***	0.0004
	(0.0003)	(0.0002)	(0.0004)	(0.0003)
Log(#nonREOs) coming on market in t+4 weeks	0.0001***	-0.0002	-0.0004	0.0002
	(0.0000)	(0.0002)	(0.0003)	(0.0001)
Log(#nonREOs) coming on market in t+3 weeks	0.0000	-0.0001	0.0006*	-0.0001
	(0.0001)	(0.0002)	(0.0003)	(0.0001)
Log(#nonREOs) coming on market in t+2 weeks	0.0000	0.0004**	0.0007**	-0.0002*
	(0.0001)	(0.0002)	(0.0003)	(0.0001)
Log(#nonREOs) coming on market in t+1 weeks	-0.0002***	-0.0008***	-0.0004	-0.0006***
	(0.0001)	(0.0002)	(0.0003)	(0.0001)
Log(#nonREOs) come on the market in week t	0.0068***	0.0069***	0.0107***	0.0066***
	(0.0004)	(0.0006)	(0.0012)	(0.0006)
(Log(#nonREOs) come on the market in week t)*Distance	-0.0733***	-0.1863***	-0.1397**	-0.0716**
	(0.0160)	(0.0352)	(0.0601)	(0.0316)
Log(#nonREOs) came on the market in week t-1	-0.0002***	-0.0007***	-0.0011**	-0.0002
	(0.0001)	(0.0002)	(0.0005)	(0.0001)
Log(#nonREOs) came on the market in week t-2	0.0001	-0.0000	-0.0008***	-0.0003*
	(0.0001)	(0.0002)	(0.0003)	(0.0001)
Log(#nonREOs) came on the market in week t-3	-0.0000	-0.0001	-0.0004	0.0001
	(0.0001)	(0.0002)	(0.0004)	(0.0001)
Log(#nonREOs) came on the market in week t-4	-0.0001*	0.0000	-0.0002	-0.0001
	(0.0000)	(0.0002)	(0.0003)	(0.0001)
Weeks on Market	-0.0002***	-0.0000**	0.0005***	-0.0001***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
REO Dummy	0.0410***	0.0460***	0.0359***	0.0398***
	(0.0006)	(0.0006)	(0.0008)	(0.0007)
Single Family Dummy	0.0109***	0.0149***	0.0000	0.0085***
	(0.0005)	(0.0008)	(0.0000)	(0.0007)
Square Feet	-0.0018***	-0.0000	-0.0020***	-0.0020***
	(0.0002)	(0.0004)	(0.0005)	(0.0003)
Age	-0.0001***	-0.0000**	0.0000	-0.0000***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Constant	0.0607***	0.0719***	0.0731***	0.0821***
	(0.0007)	(0.0014)	(0.0019)	(0.0012)
Observations	5380609	3157740	1464888	2488385
Adjusted R-squared	0.009	0.012	0.011	0.009
Week + Tract x Quarter x Year Fixed Effects	x	x	x	x

Standard errors clustered at the census tract-by-quarter level are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table complements Figure 1 and presents results on the likelihood that homes on the market that are nearby new REO and nonREO listings adjust their list price during a short window around the week that the REO/nonREO is first listed for sale. I[Change List] is a dummy variable equal to one if the list price in week t is not equal to the list price in week t-1. Distance measures how distant in physical and characteristic space each listing is to the nearby REO and nonREO listings.

Table 4: Effects of REO and NonREO Listings on Sales Prices and Time-on-Market

	Dependent Variable: Log Sales Price										Dependent Variable: Log TOM				
	Baseline					Controls for Similarity									
Diff-in-Diffs of Interest	All MSAs	Chicago	Phoenix	San Fran	DC	All MSAs	Chicago	Phoenix	San Fran	DC	All MSAs	Chicago	Phoenix	San Fran	DC
During REO Listing Relative to Before Listing, Close	-0.006	-0.004	-0.007	-0.004	-0.009	-0.003	-0.002	-0.001	-0.005	-0.008	0.021	-0.012	0.019	0.041	0.012
	0.003	*0.688*	*0.005*	*0.287*	*0.036*	*0.135*	*0.863*	*0.706*	*0.225*	*0.045*	*0.008*	*0.548*	*0.106*	*0.041*	*0.407*
During nonREO Listing Relative to Before Listing, Close	-0.005	-0.005	-0.002	-0.003	-0.009	0.000	0.000	-0.001	0.000	-0.005	0.023	0.007	0.036	0.011	0.019
	0.000	*0.167*	*0.079*	*0.209*	*0.000*	*0.776*	*0.872*	*0.717*	*0.950*	*0.094*	*0.000*	*0.444*	*0.000*	*0.422*	*0.048*
Additional Effect when Similar to REO, Close						-0.005	-0.012	-0.011	0.001	-0.003					
						0.030	*0.293*	*0.001*	*0.862*	*0.550*					
Additional Effect when Similar to nonREO, Close						-0.012	-0.016	-0.006	-0.009	-0.010					
						0.000	*0.000*	*0.019*	*0.017*	*0.000*					
During REO Listing Relative to Before Listing, Far	-0.008	-0.014	-0.004	-0.014	-0.013	-0.010	-0.017	0.000	-0.015	-0.014	0.011	0.003	0.005	0.023	0.008
	0.000	*0.000*	*0.001*	*0.000*	*0.000*	*0.000*	*0.010*	*0.902*	*0.000*	*0.000*	*0.046*	*0.785*	*0.606*	*0.089*	*0.457*
During nonREO Listing Relative to Before Listing, Far	0.002	-0.018	0.021	-0.002	-0.009	0.003	-0.023	0.023	0.003	-0.009	-0.013	-0.013	-0.021	-0.028	-0.006
	0.160	*0.000*	*0.001*	*0.368*	*0.000*	*0.059*	*0.000*	*0.000*	*0.203*	*0.000*	*0.006*	*0.105*	*0.029*	*0.005*	*0.513*
N	242356	48276	69820	62850	61410	233652	42793	68656	62689	59514	515239	146324	141640	110160	117115

p-values in italics.

Notes: This table complements Figure 3 and presents results on the effects of REO and nonREO listings on nearby sales prices. "During listing relative to before listing, Close" is the additional effect (relative to the Far group) of one listing (relative to 0 listings) Close to the home sale. "During listing relative to before listing, Far" is the effect of one listing (relative to 0 listings) further from the home sale. Close is within 0.1 miles, Far is 0.1-0.33 miles. Since the regression is a log-log specification, we multiply the coefficient of interest by ln(2) to get the estimates presented here (and the ones summarized in the figures). Similar is a dummy variable equal to one when the sale is similar in observables to the listing. See text for exact definition. All specifications also include controls for property characteristics and quarter by census tract fixed effects. Standard errors are clustered at the quarter by census tract level.

Appendix Table 1: Chicago Summary Statistics by Listing Category

		Sale Price ($)	Square Feet (1000's)	Age	Time on Market (Weeks)	Closing Gap[1] (Days)	Sale/List Price (Ratio)	$I[\text{List Price}_t \neq \text{List Price}_{t-1}]$[2] (Fraction)	Δ List Price[3] (%)
REO, No Sale	Mean	.	1.754	55	23	.	.	0.09	-0.073
	p25	.	1.188	33	8	.	.	0	-0.105
	p50	.	1.508	52	17	.	.	0	-0.054
	p75	.	2.080	82	32	.	.	0	-0.028
REO, Sale	Mean	183,884	1.860	47	23	54	0.98	0.10	-0.077
	p25	102,000	1.288	22	8	14	0.90	0	-0.105
	p50	154,888	1.672	47	18	35	0.96	0	-0.055
	p75	219,275	2.183	65	33	67	1.00	0	-0.032
Non-REO, No Sale	Mean	.	1.790	39	24	.	.	0.06	-0.041
	p25	.	1.125	12	9	.	.	0	-0.056
	p50	.	1.533	34	19	.	.	0	-0.031
	p75	.	2.240	55	33	.	.	0	-0.015
Non-REO, Sale	Mean	312,108	1.958	40	22	36	0.94	0.08	-0.038
	p25	188,000	1.280	14	9	11	0.93	0	-0.051
	p50	260,000	1.764	36	17	19	0.95	0	-0.031
	p75	374,000	2.420	54	31	40	0.97	0	-0.017
Nearby REO[4], Sale	Mean	241,339	1.733	47	21	46	0.96	0.08	-0.055
	p25	135,000	1.240	22	9	12	0.92	0	-0.073
	p50	195,000	1.568	44	17	26	0.96	0	-0.041
	p75	285,000	2.044	63	30	54	0.98	0	-0.022
Total	Mean	287,960	1.855	41	23	39	0.95	0.06	-0.045
	p25	167,000	1.204	14	9	11	0.92	0	-0.060
	p50	240,000	1.635	36	18	21	0.95	0	-0.033
	p75	350,000	2.292	56	33	46	0.98	0	-0.016

1. Defined as closing date - agreement date.
2. Takes on the value 1 if the list price does not equal the list price in the week before.
3. Conditional on a price change occurring.
4. The sale is within 0.1 miles of an active REO listing.

Appendix Table 2: DC Summary Statistics by Listing Category

		Sale Price ($)	Square Feet (1000's)	Age	Time on Market (Weeks)	Closing Gap[1] (Days)	Sale/List Price (Ratio)	$I[\text{List Price}_t \neq \text{List Price}_{t-1}]$[2] (Fraction)	Δ List Price[3] (%)
REO, No Sale	Mean	.	1.754	35	20	.	.	0.09	-0.073
	p25	.	1.188	16	6	.	.	0	-0.105
	p50	.	1.508	31	14	.	.	0	-0.057
	p75	.	2.080	49	28	.	.	0	-0.033
REO, Sale	Mean	260,324	1.860	27	21	47	0.98	0.10	-0.084
	p25	170,000	1.288	11	7	13	0.95	0	-0.108
	p50	236,000	1.672	24	16	32	1.00	0	-0.057
	p75	320,000	2.183	38	31	54	1.02	0	-0.032
Non-REO, No Sale	Mean	.	1.790	33	19	.	.	0.06	-0.061
	p25	.	1.125	14	7	.	.	0	-0.083
	p50	.	1.533	28	14	.	.	0	-0.047
	p75	.	2.240	47	26	.	.	0	-0.025
Non-REO, Sale	Mean	407,292	1.958	32	17	42	0.96	0.09	-0.055
	p25	267,000	1.280	13	6	12	0.94	0	-0.072
	p50	360,000	1.764	26	12	27	0.97	0	-0.042
	p75	500,000	2.420	45	24	47	1.00	0	-0.025
Nearby REO[4], Sale	Mean	270,340	1.733	28	19	44	0.98	0.10	-0.082
	p25	176,000	1.240	11	7	11	0.95	0	-0.105
	p50	248,997	1.568	24	15	27	0.99	0	-0.055
	p75	335,000	2.044	39	28	48	1.01	0	-0.031
Total	Mean	356,965	1.855	32	19	44	0.97	0.08	-0.065
	p25	225,000	1.204	13	7	12	0.95	0	-0.087
	p50	315,000	1.635	27	14	28	0.98	0	-0.049
	p75	434,500	2.292	45	26	49	1.00	0	-0.027

1. Defined as closing date - agreement date.
2. Takes on the value 1 if the list price does not equal the list price in the week before.
3. Conditional on a price change occurring.
4. The sale is within 0.1 miles of an active REO listing.

Appendix Table 3: Phoenix Summary Statistics by Listing Category

		Sale Price ($)	Square Feet (1000's)	Age	Time on Market (Weeks)	Closing Gap[1] (Days)	Sale/List Price (Ratio)	$I[\text{List Price}_t \neq \text{List Price}_{t-1}]$[2] (Fraction)	Δ List Price[3] (%)
REO, No Sale	Mean	.	1.754	19	20	.	.	0.09	-0.089
	p25	.	1.188	4	5	.	.	0	-0.118
	p50	.	1.508	10	14	.	.	0	-0.066
	p75	.	2.080	33	27	.	.	0	-0.037
REO, Sale	Mean	158,262	1.860	16	19	48	0.96	0.12	-0.098
	p25	90,000	1.288	4	5	22	0.93	0	-0.124
	p50	135,000	1.672	8	14	35	0.99	0	-0.067
	p75	192,000	2.183	27	28	50	1.01	0	-0.041
Non-REO, No Sale	Mean	.	1.790	19	21	.	.	0.07	-0.067
	p25	.	1.125	6	8	.	.	0	-0.087
	p50	.	1.533	13	17	.	.	0	-0.051
	p75	.	2.240	29	28	.	.	0	-0.028
Non-REO, Sale	Mean	261,925	1.958	17	18	46	0.94	0.10	-0.062
	p25	163,000	1.280	5	6	19	0.92	0	-0.080
	p50	222,000	1.764	12	14	29	0.96	0	-0.048
	p75	315,000	2.420	26	26	43	0.99	0	-0.027
Nearby REO[4], Sale	Mean	173,123	1.733	15	18	47	0.96	0.12	-0.091
	p25	100,000	1.240	3	5	21	0.93	0	-0.114
	p50	146,900	1.568	7	14	34	0.98	0	-0.062
	p75	210,000	2.044	23	27	49	1.00	0	-0.037
Total	Mean	205,850	1.855	18	20	47	0.95	0.08	-0.077
	p25	116,000	1.204	4	6	21	0.92	0	-0.099
	p50	171,000	1.635	11	15	33	0.97	0	-0.055
	p75	250,000	2.292	28	27	48	1.00	0	-0.031

1. Defined as closing date - agreement date.
2. Takes on the value 1 if the list price does not equal the list price in the week before.
3. Conditional on a price change occurring.
4. The sale is within 0.1 miles of an active REO listing.

Appendix Table 4: San Francisco Summary Statistics by Listing Category

		Sale Price ($)	Square Feet (1000's)	Age	Time on Market (Weeks)	Closing Gap[1] (Days)	Sale/List Price (Ratio)	$I[\text{List Price}_t \neq \text{List Price}_{t-1}]$[2] (Fraction)	Δ List Price[3] (%)
REO, No Sale	Mean	.	1.754	45	19	.	.	0.09	-0.088
	p25	.	1.188	23	4	.	.	0	-0.111
	p50	.	1.508	48	12	.	.	0	-0.058
	p75	.	2.080	61	28	.	.	0	-0.032
REO, Sale	Mean	352,855	1.860	41	23	46	0.99	0.11	-0.094
	p25	215,500	1.288	18	4	24	0.95	0	-0.114
	p50	319,000	1.672	42	15	38	1.00	0	-0.059
	p75	440,000	2.183	57	37	54	1.03	0	-0.034
Non-REO, No Sale	Mean	.	1.790	42	18	.	.	0.07	-0.057
	p25	.	1.125	19	6	.	.	0	-0.074
	p50	.	1.533	43	13	.	.	0	-0.042
	p75	.	2.240	57	24	.	.	0	-0.023
Non-REO, Sale	Mean	798,734	1.958	45	12	38	0.97	0.09	-0.055
	p25	538,000	1.280	28	3	12	0.95	0	-0.069
	p50	720,000	1.764	47	7	26	0.98	0	-0.041
	p75	951,000	2.420	58	16	38	1.00	0	-0.024
Nearby REO[4], Sale	Mean	374,722	1.733	41	21	52	0.97	0.10	-0.087
	p25	225,000	1.240	16	6	20	0.94	0	-0.106
	p50	330,000	1.568	43	15	35	0.99	0	-0.056
	p75	470,000	2.044	57	30	55	1.02	0	-0.031
Total	Mean	664,850	1.855	43	17	40	0.98	0.09	-0.070
	p25	370,000	1.204	22	4	14	0.95	0	-0.087
	p50	600,000	1.635	45	10	28	0.98	0	-0.049
	p75	844,500	2.292	58	23	42	1.01	0	-0.027

1. Defined as closing date - agreement date.
2. Takes on the value 1 if the list price does not equal the list price in the week before.
3. Conditional on a price change occurring.
4. The sale is within 0.1 miles of an active REO listing.

Appendix Table 5: Effects of REO and NonREO Listings on List Prices -- Distance Detail

Dependent Variable: I[Change List]

	Baseline			
	Chicago	Phoenix	San Francisco	DC
Log(#REOs) coming on market in t+4 weeks	0.0008*	0.0006	-0.0002	0.0004
	(0.0005)	(0.0004)	(0.0007)	(0.0006)
Log(#REOs) coming on market in t+3 weeks	-0.0008*	0.0010**	-0.0007	0.0005
	(0.0005)	(0.0004)	(0.0007)	(0.0006)
Log(#REOs) coming on market in t+2 weeks	-0.0007	0.0003	0.0010	-0.0002
	(0.0005)	(0.0004)	(0.0007)	(0.0006)
Log(#REOs) coming on market in t+1 weeks	-0.0025***	-0.0012***	-0.0002	-0.0010*
	(0.0005)	(0.0004)	(0.0007)	(0.0006)
Log(#REOs) come on the market in week t	0.0059***	0.0073***	0.0107***	0.0056***
	(0.0009)	(0.0007)	(0.0012)	(0.0009)
(Log(#REOs) come on the market in week t)*SqftDistance	-0.1200	-0.0099	0.1129	-0.1375
	(0.0833)	(0.0829)	(0.1638)	(0.1019)
(Log(#REOs) come on the market in week t)*AgeDistance	-0.0807	-0.1768***	-0.1393	-0.1799*
	(0.0738)	(0.0685)	(0.1397)	(0.0961)
(Log(#REOs) come on the market in week t)*MilesDistance	-0.1676***	-0.1241*	-0.4086***	0.0337
	(0.0591)	(0.0649)	(0.1015)	(0.0754)
Log(#REOs) came on the market in week t-1	-0.0015***	-0.0015***	-0.0025***	-0.0015***
	(0.0005)	(0.0004)	(0.0007)	(0.0006)
Log(#REOs) came on the market in week t-2	0.0001	0.0008*	-0.0001	-0.0012**
	(0.0005)	(0.0004)	(0.0007)	(0.0006)
Log(#REOs) came on the market in week t-3	-0.0004	0.0008*	0.0003	0.0007
	(0.0005)	(0.0004)	(0.0007)	(0.0006)
Log(#REOs) came on the market in week t-4	0.0014***	0.0006	-0.0024***	0.0008
	(0.0005)	(0.0004)	(0.0007)	(0.0006)
Log(#nonREOs) coming on market in t+4 weeks	0.0009***	-0.0001	-0.0006	0.0008**
	(0.0002)	(0.0004)	(0.0006)	(0.0004)
Log(#nonREOs) coming on market in t+3 weeks	0.0001	0.0000	0.0012**	-0.0006
	(0.0002)	(0.0004)	(0.0006)	(0.0004)
Log(#nonREOs) coming on market in t+2 weeks	0.0003	0.0005	0.0010*	-0.0002
	(0.0002)	(0.0004)	(0.0006)	(0.0004)
Log(#nonREOs) coming on market in t+1 weeks	-0.0015***	-0.0018***	-0.0006	-0.0017***
	(0.0002)	(0.0003)	(0.0006)	(0.0004)
Log(#nonREOs) come on the market in week t	0.0075***	0.0071***	0.0118***	0.0074***
	(0.0003)	(0.0006)	(0.0010)	(0.0006)
(Log(#nonREOs) come on the market in week t)*SqftDistance	-0.1316***	-0.1246*	0.0715	-0.1246*
	(0.0310)	(0.0674)	(0.1257)	(0.0672)
(Log(#nonREOs) come on the market in week t)*AgeDistance	-0.0369	-0.1953***	-0.1945	-0.0286
	(0.0290)	(0.0721)	(0.1337)	(0.0577)
(Log(#nonREOs) come on the market in week t)*MilesDistance	-0.1362***	-0.2372***	-0.3064***	-0.1513***
	(0.0234)	(0.0512)	(0.0833)	(0.0467)
Log(#nonREOs) came on the market in week t-1	-0.0006***	-0.0018***	-0.0014**	-0.0003
	(0.0002)	(0.0003)	(0.0006)	(0.0004)
Log(#nonREOs) came on the market in week t-2	0.0005**	-0.0000	-0.0018***	-0.0004
	(0.0002)	(0.0004)	(0.0006)	(0.0004)
Log(#nonREOs) came on the market in week t-3	0.0001	-0.0003	-0.0002	0.0002
	(0.0002)	(0.0004)	(0.0006)	(0.0004)
Log(#nonREOs) came on the market in week t-4	-0.0001	0.0001	-0.0001	-0.0003
	(0.0002)	(0.0004)	(0.0006)	(0.0004)
Weeks on Market	-0.0002***	-0.0000***	0.0005***	-0.0001***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
REO Dummy	0.0410***	0.0460***	0.0359***	0.0398***
	(0.0004)	(0.0004)	(0.0006)	(0.0005)
Single Family Dummy	0.0109***	0.0150***	0.0000	0.0086***
	(0.0004)	(0.0006)	(0.0000)	(0.0005)
Square Feet	-0.0016***	-0.0001	-0.0022***	-0.0019***
	(0.0002)	(0.0003)	(0.0005)	(0.0003)
Age	-0.0001***	-0.0000**	0.0000*	-0.0000***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Constant	0.0607***	0.0719***	0.0731***	0.0821***
	(0.0007)	(0.0014)	(0.0019)	(0.0012)
Observations	5380609	3157740	1464888	2488385
Adjusted R-squared	0.009	0.012	0.011	0.009
Week + Tract x Quarter x Year Fixed Effects	x	x	x	x

Standard errors clustered at the census tract-by-quarter level are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Notes: This is a slight variation of the regression presented in Table 3. SqftDistance, AgeDistance, MilesDistance measures the distance between the new listing and the nearby listing in square footage, age, and miles using the formula described in the main text.

Appendix Table 6: Effects of REO and NonREO Listings on List Prices -- Robustness

	Dependent Variable: I[Change List]				Dependent Variable: $ListPrice_t$ - $ListPrice_{t-1}$			
	Chicago	Phoenix	San Francisco	DC	Chicago	Phoenix	San Francisco	DC
Log(#REOs) coming on market in t+4 weeks	0.0006**	0.0010***	0.0002	0.0004	0.0002	-0.0002	-0.0003	-0.0006
	(0.0003)	(0.0002)	(0.0004)	(0.0003)	(0.0004)	(0.0002)	(0.0005)	(0.0004)
Log(#REOs) coming on market in t+3 weeks	-0.0003	0.0010***	-0.0001	0.0006*	-0.0003	-0.0002	-0.0008*	-0.0007*
	(0.0003)	(0.0002)	(0.0004)	(0.0003)	(0.0004)	(0.0002)	(0.0004)	(0.0004)
Log(#REOs) coming on market in t+2 weeks	-0.0003	0.0005**	0.0005	0.0001	-0.0003	-0.0007***	-0.0005	-0.0011***
	(0.0003)	(0.0003)	(0.0004)	(0.0003)	(0.0004)	(0.0002)	(0.0004)	(0.0004)
Log(#REOs) coming on market in t+1 weeks	-0.0014***	-0.0001	-0.0000	-0.0004	-0.0002	-0.0004*	-0.0010**	-0.0003
	(0.0003)	(0.0002)	(0.0004)	(0.0003)	(0.0004)	(0.0003)	(0.0004)	(0.0004)
Log(#REOs) come on the market in week t	0.0066***	0.0100***	0.0111***	0.0077***	-0.0019*	-0.0026***	-0.0052***	-0.0062***
	(0.0009)	(0.0007)	(0.0014)	(0.0010)	(0.0011)	(0.0008)	(0.0013)	(0.0011)
(Log(#REOs) come on the market in week t)*Distance	-0.1732***	-0.2261***	-0.2948***	-0.1559***	0.0609	0.0931*	0.1879**	0.2414***
	(0.0421)	(0.0425)	(0.0729)	(0.0542)	(0.0521)	(0.0503)	(0.0772)	(0.0608)
Log(#REOs) came on the market in week t-1	-0.0008**	-0.0004	-0.0012***	-0.0005*	-0.0004	-0.0005*	-0.0007*	-0.0009**
	(0.0003)	(0.0002)	(0.0004)	(0.0003)	(0.0004)	(0.0003)	(0.0004)	(0.0004)
Log(#REOs) came on the market in week t-2	0.0001	0.0009***	0.0001	-0.0004	-0.0009**	-0.0006**	-0.0001	-0.0011***
	(0.0003)	(0.0002)	(0.0004)	(0.0003)	(0.0004)	(0.0003)	(0.0004)	(0.0004)
Log(#REOs) came on the market in week t-3	-0.0001	0.0009***	0.0001	0.0007**	-0.0003	-0.0007***	-0.0003	-0.0009***
	(0.0003)	(0.0002)	(0.0004)	(0.0003)	(0.0004)	(0.0002)	(0.0004)	(0.0004)
Log(#REOs) came on the market in week t-4	0.0009***	0.0008***	-0.0012***	0.0005*	-0.0005	-0.0002	-0.0005	-0.0006
	(0.0003)	(0.0002)	(0.0004)	(0.0003)	(0.0004)	(0.0002)	(0.0004)	(0.0004)
Log(#nonREOs) coming on market in t+4 weeks	0.0001**	-0.0004**	-0.0003	0.0002	0.0000	0.0001	-0.0001	0.0001
	(0.0000)	(0.0002)	(0.0003)	(0.0001)	(0.0001)	(0.0002)	(0.0003)	(0.0002)
Log(#nonREOs) coming on market in t+3 weeks	0.0000	-0.0004**	0.0008**	-0.0001	0.0001**	0.0002	0.0001	-0.0001
	(0.0001)	(0.0002)	(0.0003)	(0.0001)	(0.0001)	(0.0002)	(0.0003)	(0.0002)
Log(#nonREOs) coming on market in t+2 weeks	0.0000	0.0001	0.0008***	-0.0003*	0.0001	-0.0000	0.0001	0.0001
	(0.0001)	(0.0002)	(0.0003)	(0.0001)	(0.0001)	(0.0002)	(0.0003)	(0.0002)
Log(#nonREOs) coming on market in t+1 weeks	-0.0003***	-0.0010***	-0.0003	-0.0007***	0.0001	0.0003*	0.0003	-0.0000
	(0.0001)	(0.0002)	(0.0003)	(0.0001)	(0.0001)	(0.0002)	(0.0003)	(0.0002)
Log(#nonREOs) come on the market in week t	0.0062***	0.0055***	0.0115***	0.0063***	0.0014***	0.0017***	-0.0001	0.0006
	(0.0004)	(0.0006)	(0.0012)	(0.0006)	(0.0004)	(0.0006)	(0.0009)	(0.0006)
(Log(#nonREOs) come on the market in week t)*Distance	-0.0464***	-0.1119***	-0.1768***	-0.0560*	-0.0423***	-0.0891**	0.0113	-0.0354
	(0.0161)	(0.0352)	(0.0597)	(0.0316)	(0.0163)	(0.0375)	(0.0529)	(0.0332)
Log(#nonREOs) came on the market in week t-1	-0.0002***	-0.0010***	-0.0010**	-0.0003*	0.0000	0.0004***	0.0006**	0.0000
	(0.0001)	(0.0002)	(0.0005)	(0.0001)	(0.0001)	(0.0002)	(0.0003)	(0.0002)
Log(#nonREOs) came on the market in week t-2	0.0001	-0.0003*	-0.0006**	-0.0003**	0.0000	0.0005***	0.0003	0.0001
	(0.0001)	(0.0002)	(0.0003)	(0.0001)	(0.0000)	(0.0002)	(0.0003)	(0.0002)
Log(#nonREOs) came on the market in week t-3	-0.0000	-0.0004**	-0.0002	0.0001	0.0001	0.0004**	0.0005**	0.0003*
	(0.0001)	(0.0002)	(0.0004)	(0.0001)	(0.0000)	(0.0002)	(0.0003)	(0.0002)
Log(#nonREOs) came on the market in week t-4	-0.0001*	-0.0002	-0.0000	-0.0002	0.0000	0.0003*	0.0002	-0.0001
	(0.0000)	(0.0002)	(0.0004)	(0.0001)	(0.0001)	(0.0002)	(0.0003)	(0.0002)
Weeks on Market	-0.0002***	-0.0001***	0.0006***	-0.0001***	-0.0000*	-0.0005***	-0.0005***	-0.0002***
	(0.0000)	(0.0000)	(0.0000)		(0.0000)	(0.0000)	(0.0000)	(0.0000)
REO Dummy					-0.0227***	-0.0154***	-0.0075***	-0.0084***
					(0.0007)	(0.0005)	(0.0007)	(0.0006)
Single Family					0.0003	0.0115***	0.0000	0.0039***
					(0.0004)	(0.0009)	(0.0000)	(0.0008)
Square Feet					0.0012***	0.0011***	0.0027***	0.0028***
					(0.0002)	(0.0004)	(0.0005)	(0.0003)
Age					-0.0001***	-0.0005***	-0.0002***	-0.0003***
					(0.0000)	(0.0000)	(0.0000)	(0.0000)
Constant	0.0662***	0.0963***	0.0793***	0.0892***	-0.0414***	-0.0615***	-0.0501***	-0.0539***
	(0.0005)	(0.0010)	(0.0013)	(0.0009)	(0.0008)	(0.0015)	(0.0019)	(0.0014)
Observations	5380609	3157740	1464888	2488385	383925	304062	151947	216056
Adjusted R-squared	0.007	0.008	0.009	0.007	0.107	0.089	0.116	0.070
Week + House Fixed Effects	x	x	x	x				
Week + Tract x Quarter x Year Fixed Effects					x	x	x	x

Clustered standard errors are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Notes: Distance measures how distant in physical and characteristic space each listing is to the nearby REO and nonREO listings. In the right panel, the change in list price, which is the dependent variable, is conditional on a change actually occuring.

Appendix Table 7: Effects of REO and NonREO Listings on Sales Prices -- Robustness

	Baseline (see Table 4)				
	Far Group = 0.25 miles	Far Group = 0.33 miles	Far Group = 0.75 miles	City Fixed Effects	Alternative Defn. of Similar
Diff-in-Diffs of Interest (p-values in italics)	All MSAs	All MSAs	All MSAs	All MSAs	All MSAs
During REO Listing Relative to Before Listing, Close	-0.005	-0.006	-0.006	-0.010	-0.003
	0.006	*0.003*	*0.001*	*0.000*	*0.157*
During nonREO Listing Relative to Before Listing, Close	-0.005	-0.005	-0.006	-0.008	-0.002
	0.001	*0.000*	*0.000*	*0.007*	*0.256*
Additional Effect when Similar to REO, Close					-0.005
					0.002
Additional Effect when Similar to nonREO, Close					-0.011
					0.001
During REO Listing Relative to Before Listing, Far	-0.007	-0.008	-0.003	-0.013	-0.009
	0.000	*0.000*	*0.223*	*0.002*	*0.000*
During nonREO Listing Relative to Before Listing, Far	0.000	0.002	0.010	0.003	0.003
	0.779	*0.160*	*0.003*	*0.597*	*0.121*
During REO Listing Relative to Before Listing, Medium			-0.007		
			0.000		
During nonREO Listing Relative to Before Listing, Medium			0.002		
			0.293		
(F to L) - (F-360 to F-270) , Close, Low Density or High Value	*0.006*	*0.005*	*0.006*		
	0.006	*0.009*	*0.003*		
(F to L) - (F-360 to F-270) , Close, High Density and Low Value	*-0.015*	*-0.015*	*-0.014*		
	0.000	*0.000*	*0.000*		

p-values in italics.

Notes: "During listing relative to before listing, Close" is the additional effect (relative to the Far group) of one listing (relative to 0 listings) Close to the home sale. "During listing relative to before listing, Far" is the effect of one listing (relative to 0 listings) further from the home sale relative to a home sale with zero listings. Close is within 0.1 miles, Far varies by specification. In the third column, we also include a medium distance group, which is 0.1-0.5 miles. In the third column, we estimate the baseline specification with city by quarter fixed effects instead of city by census tract fixed effects. Similar is a dummy variable equal to one when the sale is similar in observables to the listing. We are more stringent in our similarity criteria relative to the baseline specification in Table 4. The bottom two rows present estimates of interest when we test for a pre REO listing effect. The estimates capture the change in sales price in the interval between foreclosure and listing relative to the 90 day interval 9-12 months prior to the foreclosure. Low density and low value refer to the categorization of census tracts, as described in the main text.