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Neighboring Effects of Concentrated Mortgage Foreclosures

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Abstract

As the national mortgage crisis has worsened, an increasing number of communities are facing declining housing prices and high rates of foreclosure. Central to the call for government intervention in this crisis is the claim that foreclosures not only hurt those who are losing their homes to foreclosure, but also harm neighbors by reducing the value of nearby properties and in turn, reducing local governments' tax bases. The extent to which foreclosures do in fact drive down neighboring property values has become a crucial question for policy-makers. In this paper, we use a unique dataset on property sales and foreclosure filings in New York City from 2000 to 2005 to identify the effects of foreclosure starts on housing prices in the surrounding neighborhood. Regression results suggest that above some threshold, proximity to properties in foreclosure is associated with lower sales prices. The magnitude of the price discount increases with the number of properties in foreclosure, but not in a linear relationship.

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Section 1: Introduction

As the national mortgage crisis has worsened, an increasing number of communities are experiencing declining housing prices and high rates of foreclosure. Central to the call for government intervention in this crisis is the claim that foreclosures not only hurt those who are losing their homes to foreclosure, but also harm neighbors by reducing the value of nearby properties and in turn, reducing local governments' tax bases. The extent to which foreclosures do in fact drive down neighboring property values, and how those impacts vary according to neighborhood characteristics and local housing markets, are thus critical questions for policy-makers as they struggle to address the rising tide of foreclosures throughout the country.

In part due to the difficulty of obtaining the necessary data, few empirical studies have examined how foreclosures affect surrounding property values. Moreover, those that have considered such impacts have typically relied on cross-sectional sales data and thus have been unable to control for pre-existing differences between neighborhoods with different prevalence of foreclosures. Without such baseline controls, any estimated association between foreclosures and property values may simply reflect the fact that foreclosures tend to occur in neighborhoods with lower-valued homes, rather than demonstrating that they actually drive down property values. In this paper, we bring unique, longitudinal data to bear that can shed new light on the impact that the filing of a foreclosure notice (a "*lis pendens*", or LP) has on the sales prices of nearby properties.[1] With six years of foreclosure starts and sales prices, we are able to control for differences between the prices of properties that are near to foreclosures and the prices of other nearby properties *before* the bulk of the foreclosures occur. Our work also differs from

[1] There is some inconsistency in the literature about whether the term "foreclosure" refers to completed foreclosures (i.e. properties sold at auction) or any stage in the foreclosure process. In this paper, because we only have data on the beginning of the process – the filing of the *lis pendens* – we will use the terms "foreclosure" and "foreclosure start" interchangeably to refer to a property on which a *lis pendens* has been filed. Most other empirical papers also use data on foreclosure starts rather than completed foreclosures.

prior studies in that we examine the impacts of foreclosures in a rapidly appreciating housing market: New York City between 2000 and 2006. We expect that the impact of foreclosure starts may be lower in areas that have enjoyed such rapidly increasing property values.

Results of our regression analysis show that properties in close proximity to foreclosures sell at a discount. There may be a threshold effect, however: being near to just a few LPs does not appear to consistently depress property values. Results also suggest that housing prices are significantly lower in neighborhoods in which foreclosures will occur, even before the foreclosures, suggesting that estimates of foreclosure effects that fail to control for pre-existing differences across neighborhoods will be biased. Because our analysis examines the effects of foreclosures in a rapidly appreciating housing market, our results should provide a conservative estimate of the impacts of foreclosure in other, softer markets.

The remainder of this paper is organized as follows. Section 2 briefly reviews other empirical studies on the spillover effects of foreclosures; Section 3 describes the foreclosure process in New York State; Section 4 lays out specific research hypotheses. In Section 5, we describe our data sources and empirical strategy. Section 6 presents regression results and Section 7 concludes.

Section 2: Previous empirical research on neighborhood effects

Until quite recently, few empirical studies had attempted to quantify the effect of foreclosures on surrounding neighborhoods. Several studies have been released in the past two years, all of which use hedonic regression models combined with data on the number of foreclosures within specified time and distance intervals from recent sales to estimate the impact of foreclosures. Each of these studies examines a different housing market within the U.S. and

varies somewhat in the details of the methodology (for instance, in the choice of distance and time intervals); the methodological differences reflect differences in both the availability of data and the characteristics of the underlying housing markets. Below we review in some detail the most relevant studies.

The earliest and most frequently cited study, by Immergluck and Smith (2006), attempts to estimate the effects of foreclosures of one- to four-family homes on the property values of surrounding one- to four-family homes in Chicago and finds that each additional foreclosure within one-eighth of a mile is associated with roughly a one-percent decline in property value. Their study is the first to use hedonic regression models to estimate the impacts of foreclosure on surrounding property values, but their data only include sales prices for one year (1999) shortly after their period of recorded foreclosures (1997-98). Therefore they are unable to control for pre-existing price levels in the micro-neighborhoods where foreclosures occur, raising the concern of endogeneity. Specifically, foreclosures are likely to be more common in neighborhoods where property values are lower. They also cannot observe any longer-term effects. Their use of a relatively short time interval after foreclosures could lead either to over- or under-estimates of the effects; it may be that foreclosures cause a sudden dip in property values, but that the neighborhood reverts to the mean over the next several years, or it may be that foreclosures cause a decline which then leads to further neighborhood degeneration. Immergluck and Smith also do not consider non-linear relationships between the number of foreclosures and property values, although it seems quite likely that such threshold points might exist.

Leonard and Murdoch (2007) use hedonic models to estimate the effects of foreclosures on single family home sales in Dallas County in 2006.[2] They find that in neighborhoods with homeownership rates below 80%, each additional foreclosure within 250 feet of a sale is associated with approximately 1% decrease in sales price. Lin, Rosenblatt and Yao (2008) focus on the impacts foreclosures have on neighboring property values through the appraisal mechanism; assuming that foreclosed properties sell at a discount, and that the discounted sales prices are used as comparables, foreclosures will lead to reduced appraised values of nearby houses. They test this hypothesis using cross-sectional, hedonic regression models on home sales in the Chicago PMSA in 2003 and 2006, including indicators of the number of foreclosures in 425 different time and distance intervals from the sale. Their regression results suggest that foreclosures have a significant negative effect up to 0.9 kilometers away from the sale, and up to five years after the foreclosure.

A key limitation of all these studies is that they estimate cross-sectional regressions on just one year of sales prices.[3] Therefore they are unable to control for pre-existing price levels in the micro-neighborhoods where foreclosures occur, raising the concern of reverse causality. Our key contribution is that we use longitudinal data and are able to control more effectively for these pre-existing price differences.

Another notable difference with our work relates to housing markets. The characteristics of the housing markets examined in each of these studies – the City of Chicago, the Chicago metropolitan area and Dallas County – differ from one another and from the area we examine, New York City, in several ways that are likely to matter for the impact of foreclosures. For instance, the higher the housing density surrounding foreclosed properties, the smaller the

[2] Although they estimate a cross-sectional model, they add controls for recent trends in prices in the near proximity of the sale.
[3] While Lin et al use data from both 2003 and 2006, they estimate two separate cross-sectional regressions.

geographic area that is likely to be affected. New York City has approximately 17 housing units per acre, compared to about 8 units per acre in the City of Chicago and 1-2 units per acre in Dallas County and the Chicago PMSA, according to the 2000 census. In addition, housing prices in New York City are considerably higher and appreciated more rapidly than in any of the other markets examined during the time periods studied, which should affect the likelihood that foreclosed units remain vacant or sell at a discount.

Differences in state laws may shape the neighborhood impacts of foreclosures too. Differences in foreclosure laws can affect the length of time between initial foreclosure filing and the completed foreclosure; for example, both New York and Illinois are judicial process states in which foreclosure proceedings may last for a year or more, while in Texas most foreclosures are non-judicial and may be resolved in as little as three months (Bergman 1996; 735 ILL. COMP. STAT. ANN. 5/15/1405; Nelson and Whitman 2004). The differences in foreclosure process and local housing market conditions suggest that even studies using comparable data and methods may reach different conclusions when applied to different parts of the country. Moreover, most of these studies have obtained data on foreclosure filings from different sources, so it is unclear whether even the count of foreclosures is truly comparable across studies. This could lead to problems such as confounding the effects of mortgage-related foreclosures with those of tax liens, or simply an inaccurate count of the number of foreclosures within the time-distance intervals.

Besides the studies on housing prices reviewed above, several studies have examined the effects of foreclosures on other neighborhood outcomes. Immergluck and Smith (2005b) use a cross-sectional methodology to examine the effects of single-family foreclosures on crime rates in Chicago, and conclude that foreclosures increase violent crime but not property crime. In a set

of related studies, Baxter and Lauria (Lauria 1998; Baxter and Lauria 1999 and 2000) find that foreclosures accelerated racial transition in New Orleans by depressing housing prices and creating opportunities for lower-income black households to move into formerly white-occupied homes. They also find that higher foreclosure rates were associated with higher vacancy rates and lower proportions of owner-occupied housing. Apgar, Duda and Gorey (2005) estimate that in the City of Chicago, foreclosures impose substantial costs upon the municipal government, ranging from under $30 for properties that are never vacant and are sold at auction to upwards of $30,000 for a property that is vacant for a lengthy period, and thereby attracts criminal activity or squatters, requires physical maintenance and/or incurs structural damage from fire or abandonment.

Section 3: Foreclosure process in New York City

The mechanisms through which defaulted loans can generate negative spillovers into their neighborhoods, and the point in time when these spillovers occur, depend on the details of the foreclosure process. Because the foreclosure process differs considerably across states, this section reviews the process in New York, focusing particularly on points during the process when information becomes available to third parties.

The first stage of loan distress, mortgage default, occurs when the borrower fails to make mortgage payments on the schedule required by the mortgage contract. Once a borrower defaults on the mortgage, lenders then have several options, including restructuring the loan, forbearing enforcement of the contract terms for some time in the hope that the borrower will resume payments, or beginning the process to reclaim the property, described below. We are not able to observe when a borrower initially defaults or any actions taken by the lender prior to the

lis pendens filing, because no public notice or third party involvement occurs in the initial stage of default. Thus, for the purposes of our analysis, we treat the date of the *lis pendens* filing as the starting point for the foreclosure process.

After a mortgage has gone unpaid for a minimum of three consecutive months, the lender can file a *lis pendens*, essentially a notice of the intention to sue the property owner and reclaim the property if the loan is not repaid.[4] The *lis pendens* (LP) is filed with the county clerk's office and is therefore a public record. A number of private data vendors collect and sell information on LP filings, which prospective real estate investors use to identify properties for potential purchase.

After the *lis pendens* has been filed, the borrower may attempt to prevent the property from being foreclosed by restructuring the loan with the existing lender, refinancing the property with a different lender, or selling the property to a third party and satisfying the loan. The borrower may also turn over the deed to the property to the lender in lieu of paying off the loan.

In the third and final stage, if the borrower and lender do not reach an agreement to satisfy the outstanding loan after the filing of a *lis pendens* notice, then the lender may request that the court appoint a referee (an attorney who ultimately conducts the foreclosure sale) and schedule an auction. The judge then signs a Judgment of Foreclosure and Sale that directs a Notice of Sale to be published. According to New York State law, a notice of the property's pending sale – including the date, time, and location of the auction, the property address, and the names of the borrower and lender – must be published in newspapers or other media for four successive weeks prior to the auction.[5] The announcements of foreclosure auctions are thus

[4] *Lis pendens* can be filed for a number of reasons other than default on mortgage loan, including unpaid taxes, unpaid condominium fees, or mechanic's or contractor's liens. We include only *lis pendens* filings that result from mortgage default.
[5] N.Y. REAL PROP. ACTS. LAW § 231(2)(a)

available to any party that chooses to search the papers; several data vendors also collect and sell this information.

At the auction itself, the property will be sold to the highest bidder. The original lender will generally purchase the property if no private investor bids higher than the amount of the outstanding loan. The winning bidder must pay 10% of the purchase price immediately after the auction, and is required to pay the balance within 30 days.

If the lender takes ownership of the property, either through an agreement with the borrower during pre-foreclosure or at the foreclosure auction, the lender will typically re-sell the property to recover the unpaid loan amount in what is known as a "Real Estate Owned" (REO) sale. The transfer of property ownership back to the lender, as well as the subsequent REO sale price, is recorded as part of the public record.

The time a foreclosure takes varies dramatically among jurisdictions. In New York City, the time between the filing of a *lis pendens* and the auction of the property is typically about 18 months. Of course, some borrowers work out a payment plan with the lenders to take the property out of foreclosure, and others sell their properties to pay off the loan (a so-called "short sale"), both of which would avoid the auction, and return the property to productive use sooner than 18 months. On the other hand, some properties will be bought at auction by the lender, and become part of the lender's REO inventory, which might delay the return of the property to productive use for more than 18 months. Moreover, some properties may also be sold to investors who convert them to rental and/or fail to maintain them.

Section 4: Research Hypotheses

There are several channels through which foreclosures might have a negative impact on surrounding housing prices. First, property owners who receive foreclosure notices may be less likely than other homeowners (even others in financial distress but not in foreclosure) to maintain or upgrade their properties because they have less incentive to maintain property they may lose. Properties consequently may start to show visible signs of neglect, which may make the surrounding homes less desirable. Second, after the completion of foreclosure proceedings and the eviction of the delinquent borrower, the property may sit vacant, potentially attracting vandalism and crime, and more generally signaling that the neighborhood is not stable. Even if the vacant properties are well maintained and do not attract criminal or other undesirable activities, they add to the local supply of available units, and thus will depress property values. Third, distressed properties sold either at foreclosure auctions or pre-foreclosure sales may be more likely to be sold to investors and to become renter-occupied, which would lead to higher levels of turnover and perhaps lower levels of maintenance, even after the properties are re-occupied, which may depress surrounding values. Finally, properties with distressed loans are likely to sell at a discount – both at pre-foreclosure sales and at foreclosure auctions – thus affecting the price of "comparables" used to estimate neighboring property values (Lin at al. 2008).

Because there is a range of possible outcomes for any given foreclosure start – loan workout, pre-foreclosure sale, sale at auction – and because the time to reach those outcomes will likely vary across properties, it is difficult to forecast exactly the length of time that LPs may affect surrounding properties. However, based on the timeline discussed above and confirmed by conversations with local experts, it appears that the time period between *lis pendens* filing and

resolution of the foreclosure process is approximately 18 months during the period of our study.[6] Therefore we assume that any negative impacts of foreclosures in New York City will last for about 18 months after the filing of the LP then will diminish gradually as the property is returned to productive use. (Our estimation, however, also tests if impacts last for more than 18 months.)

Both because of differences in the state legal process and local housing market conditions, the timing and size of impacts will likely differ across markets. Foreclosed properties are less likely to remain vacant for long periods in appreciating markets than in stable or declining housing markets. In hot markets, investors who buy foreclosed properties are also more likely to resell the property rapidly through conventional channels, and less likely to sell at a discount.

Section 5: Data and Empirical Strategy

To identify the effect of foreclosure starts on neighboring property values, we use a variation of hedonic regression analysis, controlling for property and neighborhood characteristics. The general form of the regression is shown in Equation 1 below:

$$(1)\quad LPRICE_{ijt} = \beta_0 + \beta_1 LP_{ijt} + \beta_2 \Pr opChars_{ij} + ZIP_j + Boro * quarter * year$$

in which $LPRICE_{ijt}$ is the log per unit sales price of property i in ZIP code j in quarter t; LP_{ijt} is a vector of variables indicating the number of LP filings within a given time and distance interval of property i; $\Pr opChars_{ij}$ is a vector of characteristics describing property i, including square footage of the lot, building and unit; unit age; structure type and distance to the nearest subway stop; ZIP_j is a set of ZIP code area fixed effects that control for time-invariant amenities

[6] Although no systematic data on average time to resolution is available, this matches anecdotal evidence provided by staff at two local organizations: PropertyShark, a New York-based firm that collects and sells data on foreclosed properties, and Neighborhood Housing Services of New York City, a not-for-profit organization that conducts foreclosure prevention counseling.

and characteristics of the local neighborhood. As shown in Figure 1, ZIP codes in New York City are geographically quite small (averaging 1.4 square miles in area), and are fairly homogenous in demographic and economic characteristics, so are reasonable approximations of neighborhoods.[7] We use ZIP code fixed effects rather than census tract fixed effects because there are relatively few property sales that are near to LP filings but in the same census tract as property sales that are not near to LPs.[8] Finally, we also include a set of borough-quarter-year time fixed effects, $Boro * quarter * year$, to control for time-varying economic trends that may differ by borough. Data sources and brief descriptions of each variable are shown in Table 1; summary statistics are shown in Table 2.[9] Our sample size is roughly 90,000 property sales.

Our dependent variable is the actual per-unit sales price of residential properties in New York City, provided by the City's Department of Finance.[10] Because we assume that spillover effects may occur up to 18 months after the LP is filed, we restrict the sales to 2002-2005, the years for which we have data on LPs during the prior 18 months.

To identify whether each sale is likely to be affected by *lis pendens* filings, we draw upon property-level LP filings between 2000 and 2005 in four of the five NYC boroughs (counties) (Staten Island does not report LP data in a comparable fashion). Data on LPs from Bronx, Kings, New York and Queens Counties were purchased from Public Data Corporation, which collected these data from the county court registers. We exclude all LPs that are not related to mortgage foreclosures (such as tax liens, mechanics liens and housing code violations) and LPs

[7] A few ZIP codes are excluded because they have a very small number of sales or because other geographic information is missing; results are robust to including all ZIP codes.
[8] In addition to sample size concerns, the census tracts that have both "treatment" and "control" sales tend to be in neighborhoods with a much lower prevalence of LPs than the city as a whole. As a robustness check, we run some specifications including controls for various characteristics at the tract level, and using tract fixed effects rather than ZIP fixed effects. As shown in Appendix Table 3, the direction and significance of the LP measures are quite similar when using tract fixed effects, compared to ZIP fixed effects, although the magnitude is not comparable.
[9] Note we define and show summary statistics for our count variables of LPs; in the regressions, we also use alternative measures of LP prevalence.
[10] We exclude sales of coops, which are counted as shares in a corporation.

filed on non-residential properties.[11] It is fairly common for multiple LPs to be filed on the same property in a short period of time; we assume that the first filing indicates the beginning of the period of financial distress and drop any subsequent LPs filed on that property within 365 days by the same plaintiff. We exclude sales of condominiums because we expect that the mechanisms through which LPs create spillover effects would differ for condominium properties, and we do not have sufficient observations in our data set to test for such differences. Over 80 percent of the properties on which LPs were filed are 1-4 family buildings, with a small number of multifamily or mixed residential-commercial buildings.

We create a number of measures of proximity to foreclosures. The distance intervals of interest are 0-250 feet (approximately the length of a north-south block in Manhattan),[12] 250-500 feet, and 500-1000 feet. As noted earlier, the foreclosure process may last up to 18 months in New York State, although individual properties may be returned to productive use in less than or more than 18 months. We assume that the first 18 months after a LP is filed are the months that are most critical to the market's estimate of the undesirability of living near the property. We identify the number of LPs filed within each of those distance bands during the 18 months prior to the sale. We also identify the number of LPs filed within each of these distance bands more than 18 months prior to the sale, to test whether impacts last longer.

As shown in Figure 2, LPs are highly concentrated. In particular, they are concentrated in neighborhoods with low incomes, high shares of minority residents, and high incidence of subprime lending (Furman Center 2008). This raises concerns about the direction of causality (do foreclosures reduce property values, or do foreclosures occur more frequently in lower-income and minority neighborhoods that typically have lower property values?) and underscores

[11] Some LPs are dropped from our analysis because they are missing the geographic indicators needed to match with sales locations, but these are quite small numbers and are unlikely to affect our results.
[12] Across the city as a whole, the average block is 500 feet long.

the need to use longitudinal data and to control in some way for initial differences between properties that are located in the type of neighborhoods where foreclosures tend to occur and properties that are not.

The inclusion of ZIP code fixed effects helps partially to control for these neighborhood differences, but in addition, we include a set of variables indicating the number of foreclosure starts filed *after* the sale. Presumably, these future foreclosures would not yet be affecting the value of nearby properties, but they do help to proxy for differences in unobserved characteristics between those micro-neighborhoods where foreclosures tend to occur and those where they do not. While our inclusion of future foreclosures and ZIP code fixed effects should help to reduce the likelihood that our coefficients reflect the propensity of neighborhoods with lower property values to have high rates of foreclosure, rather than the effects foreclosures have on surrounding property values, they do not completely eliminate it.

As shown in Table 3, the average number of LPs near each sale varies considerably across the time-distance intervals of interest. Only about one-third of the sales in our dataset are within 250 feet of one or more LPs in the 18 months prior to the sale, and very few sales had more than two LPs within that narrowest time-distance interval. By contrast, approximately three-fourths of our sales had at least one LP within 500-1000 feet in the 18 months prior to the sale, and roughly 18 percent of sales had 10 or more LPs in that range. In some specifications we use a simple count of the number of LPs in each time-distance interval, but because the distribution of data within several intervals is highly skewed, and because we do not expect that impacts are linear, in most specifications we use one or more dummy variables indicating the number of LPs in the interval (i.e. 1-5 LPs, 6 or more). Because very few sales are within 250

feet of more than one or two foreclosure starts, we cannot estimate the marginal effects of additional foreclosures in the smallest distance band.

As shown in Table 4, annual foreclosure starts during our study period ranged between 5,735 and 6,779, with over 80 percent occurring in Brooklyn and Queens. The relatively low number of foreclosure starts in Manhattan and the Bronx likely reflects the composition of the housing stock – primarily large multifamily buildings, most of which are renter-occupied.

The geographic concentration of LPs in our data creates some empirical challenges to identifying the effect of a single foreclosure start. Figure 3 presents a stylized illustration of the typical sale in our database that is near to a property entering foreclosure. We ideally want to control for baseline differences between prices of properties in neighborhoods that are vulnerable to foreclosure and properties in neighborhoods that are not. However, of the 89,814 sales in our dataset, only 2,870 were not within 1,000 feet of at least one property entering foreclosure between 2000 and 2005. That is, we have very few sales in micro-neighborhoods that were completely unaffected by foreclosures. (Note that this is not such an issue within 250 and 500 feet of a sale. There are many sales that do not experience any foreclosure activity within 250 feet, and a reasonable number that do not experience any foreclosure activity within 500 feet.) Thus, rather than simply controlling for baseline differences in price between sales that are within 500 to 1,000 feet of homes that will enter foreclosures in the future and the few sales that are in neighborhoods which will have no foreclosures during our study period, we control for baseline price differences between sales that are within neighborhoods that will suffer substantial numbers of foreclosures (generally 6 or more) later in the study period and those that will not.

This concentration also means that many ZIP codes are lacking either treatment or control sales. (This problem is significantly more pronounced when we use census tract fixed

effects, which is a key reason why we choose to show regressions with ZIP code fixed effects.) To obtain a more precise identification of the effects of LPs, in some specifications we limit the sample to ZIP codes in which at least one sale is near zero LPs within the specified time-distance intervals and at least one sale is near one or more LPs – essentially selecting ZIP codes with both treatment and control sales. The regressions using only ZIP codes with treatment and control sales may allow a cleaner identification of the effects of LPs within ZIP codes, but these areas have a somewhat lower prevalence of foreclosures than the city as a whole (and exclude several of the hardest-hit neighborhoods that are of greatest concern to policymakers).

In addition to the econometric difficulties described above, identifying the effects of LPs is complicated by limited information about the intermediate and final outcomes of the distressed property and the length of time needed to resolve each LP. As described in Section 3, the magnitude and duration of spillover effects depend on the extent and timing of visible signs of deterioration, when and to whom the property is sold, when and by whom it is occupied, etc. Unfortunately we do not have data that allow us to determine the outcomes of individual LPs and so cannot examine differential effects by outcomes, but this is an area that we hope to pursue in future research.

Section 6: Regression results

In general, our regression results provide fairly consistent evidence that properties in close proximity to foreclosures sell at a discount. Column 1 of Table 5 presents the results of a somewhat naïve specification, including the number of LPs that occurred in the 18 months prior to the sale within three distance intervals.[13] In Column 2, we add counts of the number of LPs filed more than 18 months prior to the sale in each distance band, to test whether effects linger

[13] Coefficients on the hedonic variables are shown in Appendix Table 1.

beyond the minimum period. All three coefficients on the number of LPs in the 0-18 month window are negative, although only two are statistically significant, and the relative magnitude of the shorter and longer times varies by distance. Our estimated impacts are similar, though smaller in magnitude, to those estimated by Immergluck and Smith (2006), who use a very similar model.

As noted above however, these coefficient estimates may be biased, picking up the effects of underlying neighborhood conditions that are associated with foreclosures. Thus, in the final column of Table 5, we add counts of LPs that will occur at some point after the sale but within our study period, as an indicator of whether pre-existing neighborhood conditions that may increase the likelihood of foreclosures could also be affecting current property values. The coefficients on all three "post-sale" variables are negative and strongly significant, suggesting that the occurrence of future foreclosure starts is indeed correlated with current conditions and property values. The magnitude of the coefficient on the "post-sale" variables decreases as the distance from the sale increases, suggesting that properties that will be near to more proximate foreclosures sell at a greater discount than properties that will experience foreclosure activity further away. As expected, once the post-sale measures are added, the coefficients on the counts of foreclosures in the 18 months prior to the sale shrink considerably in size and no longer provide statistically significant evidence that LPs within 500 feet in the 18 months prior to the sale decrease property values; indeed, the coefficient on LPs in the most immediate time-distance interval (0-250 feet, 0-18 months) becomes positive and significant. However, this may reflect

the problems of the linear functional form, which can be biased by the small number of sales near a large number of LPs in that interval.[14]

The models including counts of foreclosure have the advantage that coefficients can be interpreted as the marginal effect of an additional LP in each interval. However, it seems highly unlikely that each additional foreclosure would have the same impact on property values, regardless of whether it is the first in the neighborhood or the 30th. Moreover, our data is highly skewed, with a small number of sales being in close proximity to a large number of LPs.[15] Thus, we strongly prefer a model that uses a set of dummy variables to capture foreclosure activity, rather than a simple linear count of LPs.

As shown in Table 6, using dummy variables to indicate different numbers of LPs in each time-distance interval yields more robust and intuitively straightforward estimates, and provides some evidence of non-linear marginal effects. The simplest specification, shown in Column 1, suggests that properties that experienced one or more LPs in the 18 months prior to sale sold at a discount, for all three distance bands. As we might expect, the size of the impact decreases as distance between LP and sale increases, ranging from -0.02 for LPs within 250 feet to -0.012 for LPs within 500-1000 feet. Adding indicators for LPs more than 18 months prior to sale (Column 2) decreases the magnitude on the 0-18 month indicators, and all the coefficients on the 18+ month variables are negative and statistically significant, suggesting that impacts linger beyond the initial sale of a foreclosed property. Counterintuitively, the coefficients on the "18+ month"

[14] Comparing this coefficient to the first one in Table 7, Column 3, suggests that the positive sign is a result of functional form. A robustness check in which the small number of sales with 4 or more LPs within 250 feet and 18 months were excluded also eliminates the positive coefficient.

[15] An alternate specification using the log of LPs in each interval is shown in Appendix Table 2, column 1. Even taking the natural logarithm does not fully correct the skewness of the distribution, and the interpretation of the coefficients is less intuitive.

variables are larger than those on the "0-18 months" variables in the smallest and largest distance band, but this may reflect the presence of more LPs in the longer time intervals.

In Column 3 we add dummies for LPs post-sale, to help control for pre-existing neighborhood conditions. As in the linear models shown in Table 5, the coefficients on the post-sale dummies are all negative and at least weakly significant. Moreover, and more importantly, their inclusion reduces the magnitude of the "0-18 month" coefficients, suggesting that estimates obtained from cross-sectional rather than longitudinal data will suffer from selection bias.[16]

To test for the possibility of non-linear marginal effects and of threshold effects, in Column 4 we move from a single dummy indicating any LPs to a set of dummies representing the number of LPs within the two larger distance bands.[17] In the 250-500 foot ring, we find no significant effect of 1-2 LPs in the 18 months prior to sale, but we do find that three or more LPs during this time period and within this distance of a sale are associated with significantly lower property values. Prices appear still less sensitive to small numbers of LPs within the 500-1000 foot band. Properties within that range of 1-5 LPs, in the 18 months prior to sale, do not show a significant price discount, but proximity to 6 or more LPs is associated with 2.8% lower sales prices. Various additional specifications not shown here revealed similar thresholds for LPs more than 18 months prior to sale or post-sale; significant negative coefficients are robust only for 6 or more LPs. The geographic patterns in coefficients shown in Table 6 seem quite reasonable: prices are sensitive to small numbers of foreclosures in close proximity, while at greater distances, one or two foreclosures have no discernable impact.

[16] Longitudinal analyses that fail control for baseline price differences between properties that will be near to significant foreclosure activity and those that will not will also produce biased estimates.

[17] In the 0-250 foot band, there is not enough variation in the number of LPs in the prior 18 months to test for different marginal effects. In general we have tried to match the thresholds for dummies between 0-18 months prior to sale and the other time periods, where sample size permits us to do so.

As a robustness check, the final column in Table 6 presents results of the same specification as that shown in Column 4, but using only sales in ZIP codes that have both treatment and control sales, to reduce the possibility of selection bias. Approximately one-third of the 163 ZIP codes in New York City are excluded from this model. Although some ZIP codes are excluded because no sales in the ZIP are within 1,000 feet of an LP, most are dropped because all sales in the ZIP are within 1000 feet of at least one LP, so that on average, the sales in the remaining ZIP codes have lower exposure to LPs than in the full sample. The magnitude and significance of most coefficients are fairly robust to excluding these ZIP codes, suggesting that the estimates in model 4 do not suffer from much bias by including ZIP codes that contribute less to the identification strategy.

Additional robustness checks are included in the Appendix. In Appendix Table 2, we show that our basic results are unchanged when we measure proximity to foreclosure activity using the natural logarithm of the number of recent foreclosures. Our results are also similar when we use two rather than three distance bands (measuring impacts within 500 feet and then between 500-1000 feet).

Perhaps more significantly, Appendix Table 3 shows that our results are also qualitatively the same when we go below the ZIP code and include either characteristics of the surrounding census tract (as of 2000) or census tract fixed effects. As explained above, however, the point estimates obtained when including census tract fixed effects should be treated with caution because our effective sample size falls considerably in this model, as so many census tracts lack either sales near to foreclosure activity or sales far from such activity.

Finally, we also explored whether the impacts of foreclosures differed across different types of New York City neighborhoods (results available upon request). Specifically, by

stratifying our sample into high- and low-density communities, we examined whether estimated impacts vary depending on the density of the neighborhood.[18] We find little difference in impacts, which suggests that our impacts may be generalizable to other, lower-density cities. Of course, we are also studying the impact of foreclosures in a rapidly appreciating market, so results may still be idiosyncratic.

Section 7: Conclusions and policy implications

Our regression results provide some evidence that properties in close proximity to foreclosures sell at a discount. There is some evidence of a threshold effect; being near a very small number of LPs does not appear to consistently depress property values. The magnitude of the price discount increases with the number of nearby LPs, although not in a direct linear relationship, suggesting some diminishing marginal impacts. Our results also suggest that housing prices are significantly lower in neighborhoods in which foreclosures will occur, even before foreclosures. Therefore estimates of foreclosure impacts that fail to control for pre-existing price differences across neighborhoods will suffer from selection bias.

These results offer several useful implications for policymakers attempting to cope with the rising tide of foreclosures. First, our results provide some evidence that the effects of foreclosures extend to neighboring property owners as well as the distressed borrowers themselves, which may offer a stronger justification for government intervention, despite concerns over moral hazard problems resulting from such intervention. Second, the evidence of the threshold effect may help guide the targeting of resources. We find little evidence that being near a small number of foreclosures has a significant negative impact on prices, but beyond the

[18] The four boroughs of New York City included in our analysis are divided into 56 community districts. We divide these districts into those with above and below average housing density.

threshold, we do see a price discount. Therefore rather than treat all foreclosures equally, it may be more efficient to target prevention effects in neighborhoods that have not yet had large numbers of foreclosures and prevent them from reaching the threshold for harm.

The evidence on significant differences in prices even before foreclosures occur is interesting for policy reasons as well as methodological ones. This strongly suggests that neighborhoods with lower housing values are more vulnerable to higher concentrations of foreclosures in the future. It may be that residents living in these neighborhoods are more likely to experience events that result in foreclosure, such as job loss, health problems or marital dissolution or that they simply are less able to weather such problems due to lower assets and income. It may also be that residents in these neighborhoods were more likely to take out subprime mortgages with loans terms that increase the probability of default. But whatever the reason, our results provide strong evidence for pre-existing differences in property values and the importance of controlling for them.

The New York City housing market is idiosyncratic in a number of ways that may alter the impacts of foreclosures on property values, relative to other parts of the county. First and most importantly, the housing market was very strong in New York City during the time of our research, with prices appreciating much more rapidly than in any market in which foreclosures have previously been examined. We expect that in hot housing markets, foreclosed properties may sell at a smaller discount and may be less likely to remain vacant for long periods of time, thus mitigating the negative spillover effects on neighboring properties. Similarly, in many newer areas hard hit by foreclosures, especially Florida and the West Coast, it appears that a period of overbuilding preceded the current wave of foreclosures, suggesting that there may be considerable excess stock and that prices in those areas may decline precipitously before they

realign with market fundamentals. Compared to these areas, New York City is a supply-constrained market (due both to scarce land and to strict zoning and building code regulations) and thus saw less of a building boom. The housing stock in New York also differs from that in other large cities. New York City's housing is built at higher densities, includes a greater share of multi-family buildings and contains a greater share of rental units. The net effect of all these differences on spillover effects of foreclosures is not clear, but it seems likely that they would shape the nature and magnitude of spillovers.

Despite these differences between New York City and other housing markets, however, it should be noted that the overwhelming majority of foreclosures in our sample occurred not in Manhattan, which is undeniably an unusual submarket, but in the outer boroughs, particularly in Brooklyn and Queens. Thus the parts of the city which contribute the most to our identification of the effects of foreclosures are neighborhoods that are fairly modest in scale, with housing and population characteristics that bear close resemblance to many older neighborhoods in other Northeastern and Midwestern central cities and some inner-ring suburbs, and so should provide useful comparisons for policy makers outside of New York City.

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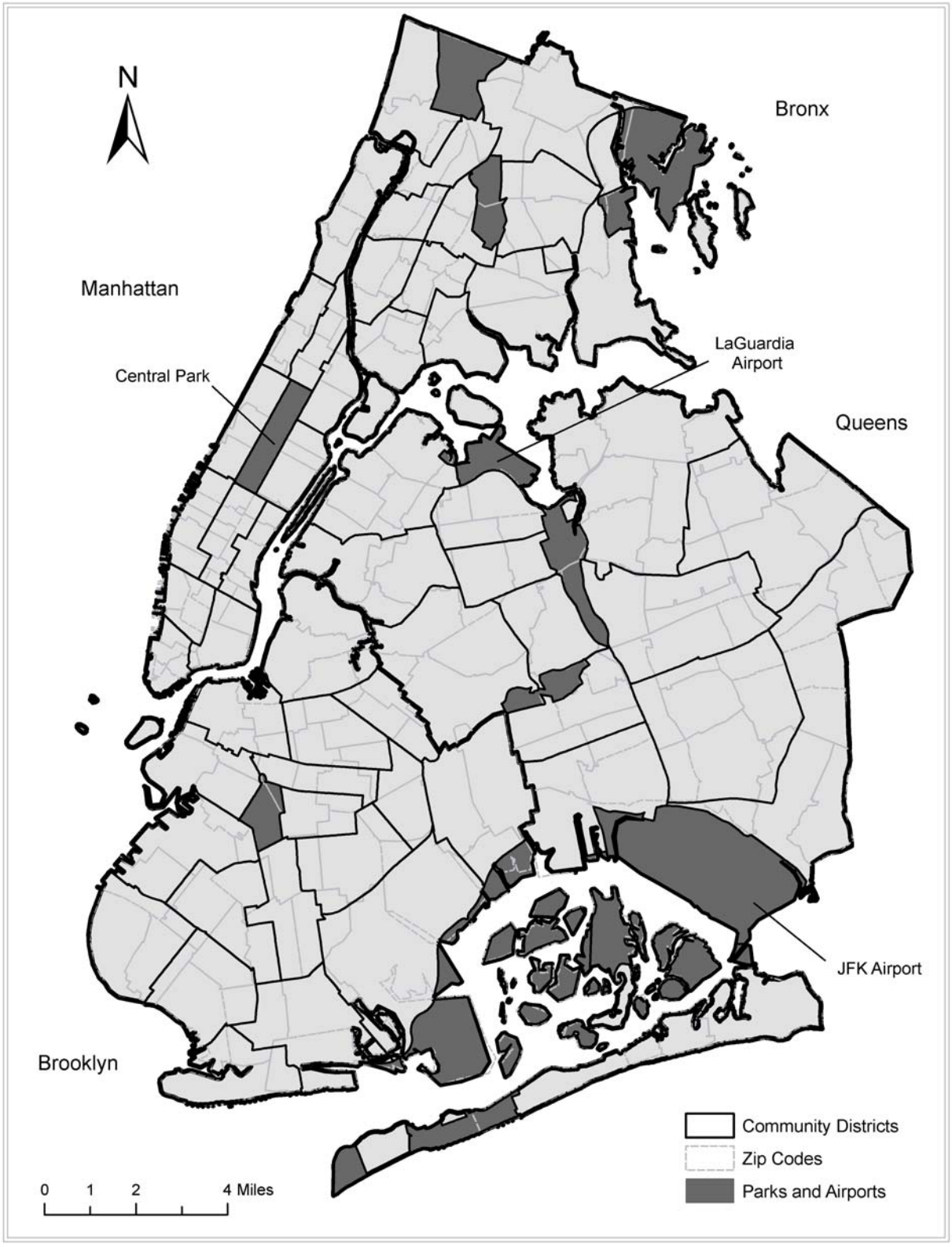
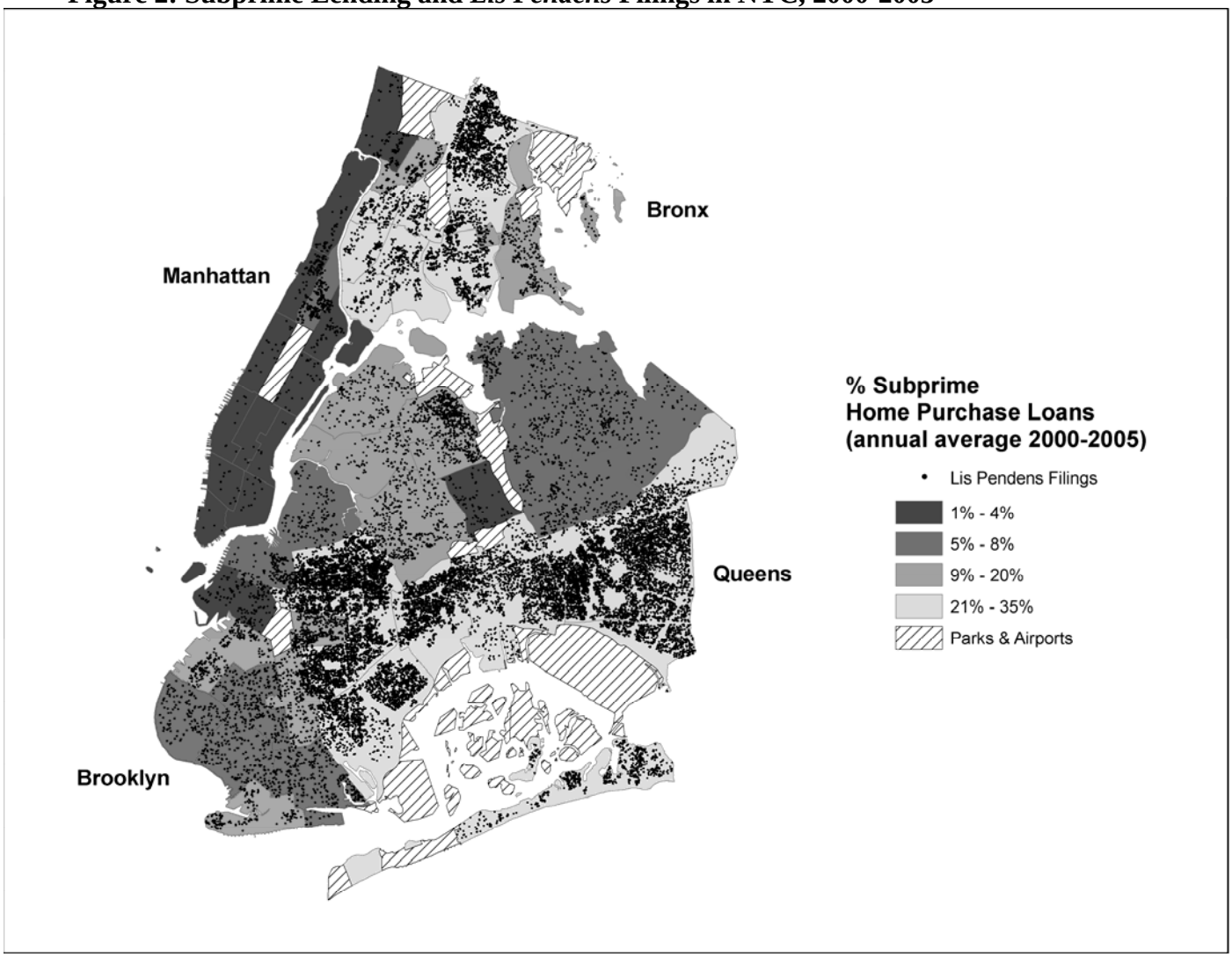
Figure 1: Geographic and political jurisdictions within New York City



Community districts are official "neighborhoods" designated for local government purposes, similar to political "wards" in some other cities.

Figure 2: Subprime Lending and *Lis Pendens* Filings in NYC, 2000-2005



Source: Public Data Corporation and HMDA

Figure 3: Geographic concentration of LPs complicates empirical strategy

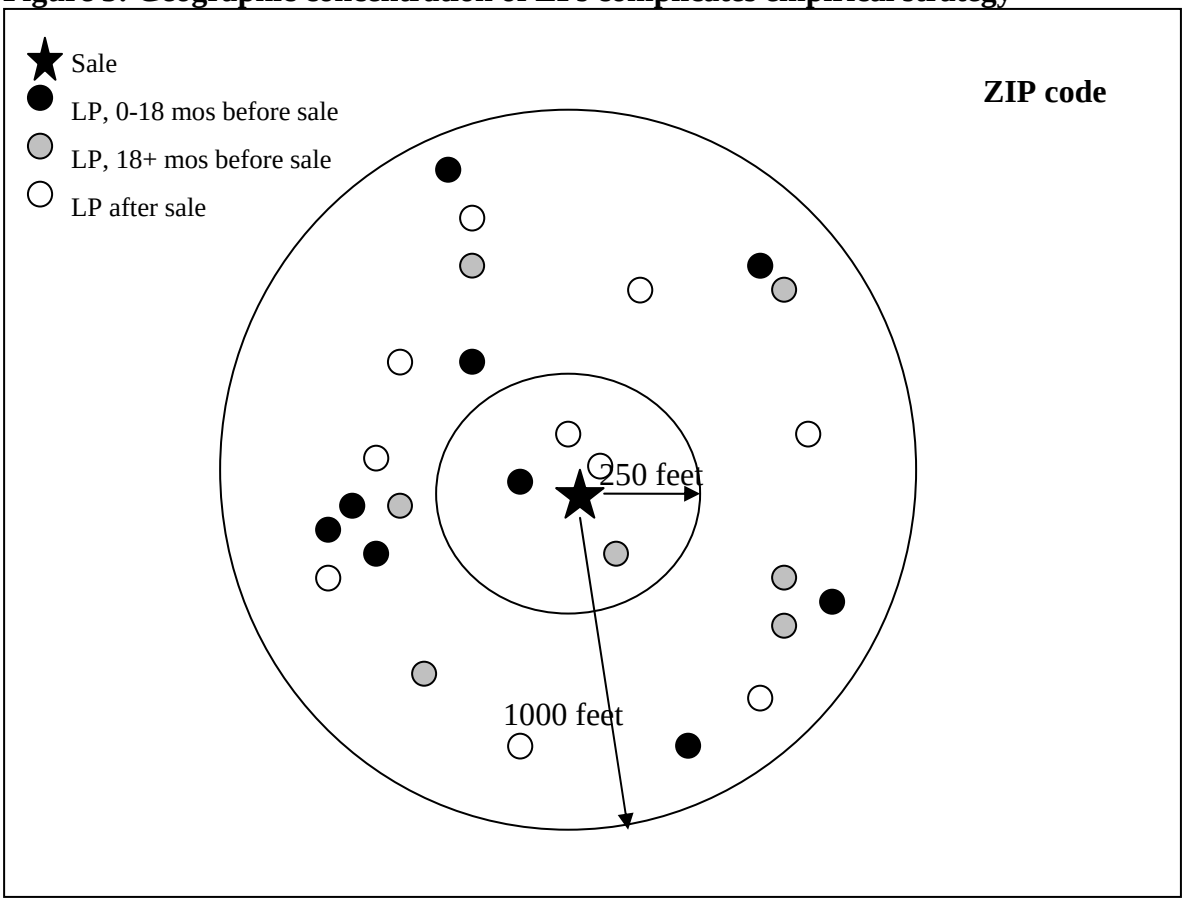


Table 1: Variable definitions and data sources

Variable	Description and source
Dependent variable	Source: NYC DOF
Log(Price/unit)	Log(price per unit), constant 2005 $.
Proximity to LP measures	*Source: PDC, DOF*
# LPs, 0-18 mos, 0-250 ft	Number of LPs within 250 feet of sale, 0-18 months before sale
# LPs, 18+ mos, 0-250 ft	Number of LPs within 250 feet of sale, >18 months before sale
# LPs, post-sale, 0-250 ft	Number of LPs within 250 feet of sale, after the sale
# LPs, 0-18 mos, 250-500 ft	Number of LPs within 250-500 feet of sale, 0-18 months before sale
# LPs, 18+ mos, 250-500 ft	Number of LPs within 250-500 feet of sale, >18 months before sale
# LPs, post-sale, 250-500 ft	Number of LPs within 250-500 feet of sale, after the sale
# LPs, 0-18 mos, 500-1000 ft	Number of LPs within 500-1000 feet of sale, 0-18 months before sale
# LPs, 18+ mos, 500-1000 ft	Number of LPs within 500-1000 feet of sale, >18 months before sale
# LPs, post-sale, 500-1000 ft	Number of LPs within 500-1000 feet of sale, after the sale
Hedonic characteristics	*Source: RPAD*
unitage	Unit age, years
noyrblt	= 1 if unit age missing
sqftunt	Square feet of unit
lnd_area	Square feet of lot
gr_sqft	Square feet of building
Bldgs	Number of buildings on lot
numunits	Number of units in building
sf_det	= 1 if SF detached (multifamily is omitted category)
sf_att	= 1 if SF attached
twofam	=1 if two-family
Dist_subway	Distance to nearest subway stop (feet)
express	= 1 if nearest subway stop on express line
twostory	= 1 if building has 2+ stories
Fixed effects	
Boro-year-qrtr	Dummy variables for each borough-quarter-year of sale
ZIP	Dummy variables for each ZIP code

Table 2: Variable descriptive statistics

Variable	Mean	Std. Dev.	Min	Max
Dependent variable				
Price/hsg unit	351,678	456,507	133	31,000,000
Counts of LPs				
# LPs, 0-18 mos, 0-250 ft	0.60	1.14	0	20
# LPs, 18+ mos, 0-250 ft	0.34	0.97	0	21
# LPs, post-sale, 0-250 ft	2.10	3.08	0	42
# LPs, 0-18 mos, 250-500 ft	1.46	2.24	0	36
# LPs, 18+ mos, 250-500 ft	0.84	2.01	0	32
# LPs, post-sale, 250-500 ft	5.02	6.55	0	60
# LPs, 0-18 mos, 500-1000 ft	5.25	6.75	0	69
# LPs, 18+ mos, 500-1000 ft	3.03	6.32	0	78
# LPs, post-sale, 500-1000 ft	18.00	21.40	0	165
Dummy indicators of LPs				
Any LPs, 0-18 mos, 0-250 ft	0.321	0.467	0	1
Any LPs, 18+ mos, 0-250 ft	0.178	0.382	0	1
Any LPs, post-sale, 0-250 ft	0.568	0.495	0	1
Any LPs, 0-18 mos, 250-500 ft	0.505	0.500	0	1
Any LPs, 18+ mos, 250-500 ft	0.281	0.449	0	1
Any LPs, post-sale, 250-500 ft	0.737	0.440	0	1
Any LPs, 0-18 mos, 500-1000 ft	0.754	0.431	0	1
Any LPs, 18+ mos, 500-1000 ft	0.436	0.496	0	1
Any LPs, post-sale, 500-1000 ft	0.915	0.279	0	1
Hedonic characteristics				
lnd_area	5,012	25,480	0	1,510,315
gr_sqft	2,526	8,793	0	1,970,736
lsqftunt	7.04	0.53	0.00	12.01
unitage	61.68	32.61	0.00	205.00
noyrblt	0.11	0.31	0	1
dist_subway	5,063	5,724	11	31,798
express	0.252	0.434	0	1
bldgs	0.998	0.207	0	15
numunits	2.301	10.041	1	2002
sf_det	0.193	0.394	0	1
sf_att	0.125	0.331	0	1
twofam	0.335	0.472	0	1
twostory	0.898	0.303	0	1
n =				89,814

Table 3: Number of Sales with Given Number of LPS by Distance/Time Categories

	LP 0-18 months before sale		**LP > 18 mos before sale**		**LP post sale (up to 2005)**	
	Number	%	Number	%	Number	%
0-250 ft						
0 LPs	60841	67.7%	73789	82.2%	38,851	43.2%
1-2 LPs	22,702	25.3%	12,423	13.8%	24,560	27.3%
3-5 LPs	5,568	6.2%	3,026	3.4%	15,799	17.6%
6+ LPs	703	0.8%	576	0.6%	10,733	11.9%
250-500 ft						
0 LPs	44,223	49.2%	64,464	71.8%	23,642	26.3%
1-2 LPs	26,617	29.6%	15,125	16.8%	21,843	24.3%
3-5 LPs	13,155	14.6%	6,681	7.4%	15,788	17.6%
6-10 LPs	5,084	5.7%	2,830	3.2%	14,317	15.9%
11+ LPs	735	0.8%	714	0.8%	14,353	16.0%
500-1000 ft						
0 LPs	21,755	24.2%	50,493	56.2%	7,624	8.5%
1-2 LPs	23,234	25.9%	14,744	16.4%	13,323	14.8%
3-5 LPs	15,726	17.5%	9,208	10.3%	14,317	15.9%
6-10 LPs	13,180	14.7%	6,907	7.7%	12,717	14.1%
11-19 LPs	11,498	12.8%	5,363	6.0%	12,095	13.4%
20+ LPs	4,421	4.9%	3,099	3.5%	29,867	33.2%

Table 4: Number of LPs by borough and year

	2000	2001	2002	2003	2004	2005	Total
Bronx	755	967	1,052	979	871	775	5,399
Brooklyn	2,742	2,466	2,944	2,861	2,455	2,504	15,972
Manhattan	268	155	146	123	95	84	871
Queens	2,553	2,556	2,637	2,482	2,330	2,372	14,930
Total	6,318	6,144	6,779	6,445	5,751	5,735	37,172

Table 5: Estimated linear impact of LPs on nearby sales prices

Dependent variable:	**Log(price)**		
Variable	(1)	(2)	(3)
# LPs, 0-18 mos, 0-250 ft	-0.00217*	-0.000869	0.00228*
	(0.0013)	(0.0013)	(0.0014)
# LPs, 18+ mos, 0-250 ft		-0.00486**	-0.00478**
		(0.0020)	(0.0020)
# LPs, post-sale, 0-250 ft			-0.00434***
			(0.0006)
# LPs, 0-18 mos, 250-500 ft	-0.00376***	-0.00277***	-0.000834
	(0.0008)	(0.0008)	(0.0009)
# LPs, 18+ mos, 250-500 ft		-0.00172	-0.00235*
		(0.0012)	(0.0012)
# LPs, post-sale, 250-500 ft			-0.00106***
			(0.0003)
# LPs, 0-18 mos, 500-1000 ft	-0.00401***	-0.00329***	-0.00224***
	(0.0003)	(0.0003)	(0.0004)
# LPs, 18+ mos, 500-1000 ft		-0.000900**	-0.00187***
		(0.0004)	(0.0005)
# LPs, post-sale, 500-1000 ft			-0.000388***
			(0.0001)
Observations	89814	89814	89814
R-squared	0.685	0.685	0.686

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
All models include a variety of property characteristics, ZIP code fixed effects, and borough-quarter-year time fixed effects.

Table 6: Non-linear impacts of LPs on nearby sales prices

Dependent variable:	**Log(price)**				
hVariable	(1)	(2)	(3)	(4)	(5)
Any LPs, 0-18 mos, 0-250 ft	-0.0222***	-0.0172***	-0.0124***	-0.00719**	-0.00829*
	(0.0031)	(0.0031)	(0.0031)	(0.0031)	-0.0046
Any LPs, 18+ mos, 0-250 ft		-0.0237***	-0.0198***	-0.0144***	-0.0201***
		(0.0042)	(0.0042)	(0.0043)	-0.00651
Any LPs, post-sale, 0-250 ft			-0.0289***	-0.0231***	-0.0209***
			(0.0035)	(0.0035)	-0.00409
Any LPs, 0-18 mos, 250-500 ft	-0.0184***	-0.0144***	-0.0101***		
	(0.0032)	(0.0033)	(0.0033)		
1-2 LPs, 0-18 mos, 250-500 ft				0.000302	0.00431
				(0.0034)	-0.00421
3+ LPs, 0-18 mos, 250-500 ft				-0.0135***	-0.00191
				(0.0045)	-0.00731
Any LPs, 18+ mos, 250-500 ft		-0.0129***	-0.00939**	-0.00371	-0.00447
		(0.0041)	(0.0041)	(0.0042)	-0.00563
Any LPs, post-sale, 250-500 ft			-0.0150***		
			(0.0042)		
1-2 LPs, post-sale, 250-500 ft				-0.00263	-0.00361
				(0.0045)	-0.00469
3+ LPs, post-sale, 250-500 ft				-0.0331***	-0.0421***
				(0.0050)	-0.0056
Any LPs, 0-18 mos, 500-1000 ft	-0.0120***	-0.0108**	-0.00694*		
	(0.0042)	(0.0042)	(0.0042)		
1-5 LPs, 0-18 mos, 500-1000 ft				-0.00278	-0.00112
				(0.0042)	-0.00456
6+ LPs, 0-18 mos, 500-1000 ft				-0.0284***	-0.0237***
				(0.0057)	-0.00784
Any LPs, 18+ mos, 500-1000 ft		-0.0310***	-0.0294***		
		(0.0049)	(0.0049)		
6+ LPs, 18+ mos, 500-1000 ft				-0.0115**	-0.00542
				(0.0050)	-0.00818
3+ LPs, post-sale, 500-1000 ft			-0.00853*		
			(0.0048)		
6+ LPs, post-sale, 500-1000 ft				-0.0319***	-0.0303***
				(0.0043)	-0.00458
Observations	89814	89814	89814	89814	56590
R-squared	0.685	0.685	0.685	0.686	0.662

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Model 5 includes only ZIP codes with both treatment and control sales. All models include property characteristics, ZIP code fixed effects, and borough-quarter-year time fixed effects.

Appendix Table 1: Coefficients on hedonic variables

Dependent variable:	**Log(price)**
lnLND_AREA	0.00617***
	-0.00121
lnBLDNG_AREA	-2.429***
	-0.4
unitage	-0.00329***
	-0.000224
unitage2	2.18e-05***
	-2.06E-06
twostory	0.0152***
	-0.00551
ldistsub	0.00184
	-0.00293
express	-0.0217***
	-0.00472
noyrblt	-0.120***
	-0.0101
lsqftunt	2.733***
	-0.404
bldgs	0.0436***
	-0.0107
lunits	2.027***
	-0.403
sf_det	0.368***
	-0.00842
sf_att	0.244***
	-0.0077
twofam	0.174***
	-0.00431
Observations	89814
R-squared	0.686

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Coefficients taken from Table 6, model 4. The hedonic coefficients do not change substantially across the models shown in Tables 5 and 6. Model also includes ZIP and borough-quarter-year fixed effects.

Appendix Table 2: Robustness checks on functional form

Dependent variable:	**Log(price)**		
Variable	(1)	(2)	(3)
Log(# LPs, 0-250 ft, 0-18 mos)	0.00271		
	-0.00304		
Log(# LPs, 0-250 ft, 18+ mos)	-0.0122***		
	-0.00441		
Log(# LPs, 0-250 ft, post-sale)	-0.0223***		
	-0.00251		
Log(# LPs, 250-500 ft, 0-18 mos)	-0.00173		
	-0.00272		
Log(# LPs, 250-500 ft, 18+ mos)	-0.00653*		
	-0.00361		
Log(# LPs, 250-500 ft, post-sale)	-0.0152***		
	-0.00252		
Log(# LPs, 500-1000 ft, 0-18 mos)	-0.0153***		
	-0.00272		
Log(# LPs, 500-1000 ft, 18+ mos)	-0.0152***		
	-0.00298		
Log(# LPs, 500-1000 ft, post-sale)	-0.0103***		
	-0.00275		
# LPs, 0-500 feet, 0-18 mos		9.14E-05	
		-0.000692	
# LPs, 0-500 feet, 18+ mos		-0.00308***	
		-0.000952	
# LPs, 0-500 feet, post-sale		-0.00205***	
		-0.000272	
# LPs, 500-1000 ft, 0-18 mos		-0.00222***	
		-0.000392	
# LPs, 500-1000 ft, 18+ mos		-0.00184***	
		-0.000457	
# LPs, 500-1000 ft, post-sale		-0.000357***	
		-0.000134	
Any LPs, 0-18 mos, 0-500 feet			-0.00786**
			-0.00344
Any LPs, 18+ mos, 0-500 feet			-0.0101**
			-0.00422
Any LPs, post-sale, 0-500 feet			-0.0501***
			-0.00407
Any LPs, 0-18 mos, 500-1000 feet			-0.00548
			-0.0042
Any LPs, 18+ mos, 500-1000 feet			-0.0320***
			-0.00491
3+ LPs, post-sale, 500-1000 feet			-0.00890*
			-0.00481
Observations	89814	89814	89814
R-squared	0.686	0.686	0.685

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
All models include property characteristics, ZIP fixed effects and borough-quarter-year fixed effects.

Appendix Table 3: Models including census tract characteristics and fixed effects

Dependent variable:	**Log(price)**	
Variable	(1)	(2)
Any LPs, 0-18 mos, 0-250 ft	-0.00502	-0.0209***
	-0.00418	-0.00323
Any LPs, 18+ mos, 0-250 ft	-0.0153***	-0.0131***
	-0.00524	-0.00449
Any LPs, post-sale, 0-250 ft	-0.0192***	-0.0599***
	-0.00674	-0.00373
3+ LPs, 0-18 mos, 250-500 ft	-0.0141***	-0.0251***
	-0.00514	-0.00382
Any LPs, 18+ mos, 250-500 ft	-0.00331	-0.0207***
	-0.00647	-0.00436
3+ LPs, post-sale, 250-500 ft	-0.0254***	-0.0816***
	-0.00705	-0.00405
3+ LPs, 0-18 mos, 500-1000 ft	-0.0349***	-0.0802***
	-0.00947	-0.00403
6+ LPs, 18+ mos, 500-1000 ft	-0.0132**	-0.0292***
	-0.00635	-0.00483
6+ LPs, post-sale, 500-1000 ft	-0.0195*	-0.110***
	-0.0101	-0.00478
Housing density (2000)	-0.00242***	
	-0.000841	
% subprime (2000)	-0.225***	
	-0.0405	
% owner-occupancy (2000)	0.00179***	
	-0.000398	
Log(pop 2000)	0.00629	
	-0.0156	
Fixed effects	ZIP	Tract
Observations	87759	89814
R-squared	0.692	0.68

Robust standard errors, clustered by census tract in Model 1, in parentheses
*** p<0.01, ** p<0.05, * p<0.1