Spillover Effects in Residential House Prices*

Marcel Fischer† Roland Füss‡ Simon Stehle§

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†Copenhagen Business School, Department of Finance, Solbjerg Plads 3, DK-2000 Frederiksberg, Denmark and University of Konstanz, Department of Economics, Postbox 147, D-78457 Konstanz, Germany, phone: +49 7531 882645, e-mail: marcel.fischer@uni.kn.

‡Swiss Institute of Banking and Finance (s/bf), University of St.Gallen, Unterer Graben 21, 9000 St. Gallen, Switzerland and Research Associate at the Centre for European Economic Research (ZEW), Mannheim, Germany; Phone: +41 (0)71 224 7055, Fax: +41 (0)71 224 7088; Email: roland.fuess@unisg.ch

§University of Konstanz, Department of Economics, Postbox 147, D-78457 Konstanz, Germany, phone: +49 7531 3620, e-mail: simon.stehle@uni.kn.

Abstract

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We study the micro-level evolution of residential house prices using data on all repeat sales on Manhattan Island from 2004 to 2015. We document that excess returns spill over to other trades in the neighborhood — even after controlling for general house price movements on the zip-code level. This effect quickly vanishes as the distance between trades increases. In booming states of the housing market cycle, spillover effects are weaker in the nearest neighborhood and stronger from more distant neighborhoods.

JEL Classification Codes: R30, R32
Key Words: housing market, spillover effects, urban economics, real estate, repeat sales

1 Introduction

The recent boom and bust in house prices dramatically illustrates the need for a better understanding of what drives prices of residential homes. Since the pioneering work of Case and Shiller (1989), it is a well-established fact that returns on national and city-wide house price indices are subject to strong autocorrelation. Beyond macroeconomic factors, such as gradually changing credit conditions (e.g., Chambers, Garriga, and Schlagenhauf, 2009; Landvoigt, Piazzesi, and Schneider, 2015; Amromin, Huang, Silam, and Zhong, 2018), on the local level, this autocorrelation can be partly explained by comovements in house prices caused by events, such as gentrification (Guerrieri, Hartley, and Hurst, 2013), urban revitalization (Rossi-Hansberg, Sarte, and Owens III, 2010) and foreclosures (Campbell, Giglio, and Pathak, 2011). Yet, little is known about comovements in house prices in regular sales, that we also refer to as spillovers throughout.

We contribute to this line of literature by investigating the micro-level price dynamics of homes in urban areas using repeat sales on Manhattan Island between 2000 and 2015 to show that the price at which a given home trades is significantly affected by recently observed trading prices of homes in the neighborhood. Manhattan Island seems ideal for investigating spillover effects in many regards. First, Manhattan Island is geographically separated from other parts of New York through water, implying that real estate transactions outside Manhattan Island are less likely to significantly affect local price dynamics on Manhattan Island. Second, Manhattan Island is a fairly liquid market for real estate. Third, Manhattan Island is densely populated implying that new constructions are scarce and unlikely to have major price impacts. Finally, the exact trading prices for all homes are publicly available from the New York City Department of Finance,[1] implying that information is easily available for all market participants.

We document that spillover effects do not only exist in the presence of event-specific externalities, but also in regular sales. Consistent with the evidence in Campbell, Giglio, and Pathak (2011), Guerrieri, Hartley, and Hurst (2013), and Rossi-Hansberg, Sarte, and Owens III (2010), spillover effects are strongest in the nearest neighborhood — particularly within the same building — and disappear quickly with increasing distance between traded homes. Our results are robust to controlling for the evolution of house prices on the borough level on monthly basis as well as zip-code-year fixed effects. That is, even after controlling for local house price dynamics, sales prices in the nearest neighborhood still matter. In extensive robustness checks, we further document that our results withstand various other assumptions and parameter choices.

[1]http://www1.nyc.gov/site/finance/taxes/property-rolling-sales-data.page

The existence of spillover effects in regular sales is largely driven through two main channels. First, homes in the same neighborhood share common amenities, such as access to schools, recreational areas, shopping facilities, etc. Hence, ceteris paribus, homes in the same neighborhood should be better substitutes than more distant ones. The more prices in a given neighborhood increase, the stronger the incentives for potential buyers to search for cheaper homes in the nearest surrounding. This substitution effect should cause price increases in the preferred neighborhood to spill over to close-by neighborhoods.

Second, spillover effects can be caused through the information channel. Available information is likely to affect both buyers' and sellers' behavior. For buyers, the market for residential real estate is characterized by an information disadvantage (Coval and Moskowitz, 1999; Garmaise and Moskowitz, 2004; Kurlat and Stroebel, 2015). Information about locally realized sales prices that is not (yet) publicly available is typically easier to access for sellers via private channels, such as mouth-to-mouth propaganda. Thus, buyers have an incentive to use previous sales prices in the neighborhood to reduce the information gap. Simultaneously, sellers should incorporate past sales prices in their offer prices and during price negotiations – for instance, because they do not want to sell at a worse price than their neighbors. Hence, past price changes in the neighborhood should spill over to present trading prices via both buyers' and sellers' incentives to use past sales prices as easily available anchors. Furthermore, the particularly strong within-building spillover effects are likely to be affected by a second anchoring effect: if a real estate agent has successfully sold a flat in a given building, other households wishing to sell may want to hire the same real estate agent, who would likely use his past realized sales price as an anchor for the new ask price. Ask prices, in turn, are known to affect the level of transaction prices of properties in the near neighborhood (Horowitz, 1992; Anenberg, 2016).

Liquidity and the volatility of house prices vary with the state of the housing market cycle, suggesting that the state of the market cycle should affect how spillover effects manifest in house price changes through two counteracting channels. On the one hand, the generally higher liquidity in booming housing markets suggests that the number of trades in the nearest neighborhood should be higher. From this liquidity channel, it may be sufficient to consider prices in this neighborhood to understand local price dynamics. Hence, spillover effects in neighborhoods should be more pronounced, whereas spillover effects from more distant neighborhoods should be weaker. On the other hand, volatility in booming states of the housing market cycle is generally lower, suggesting a higher quality of the price signal from more distant trades. This volatility channel suggests that spillover effects from the nearest neighborhood should be weaker and that those from more distant trades should be stronger. Our empirical results show that the volatility channel outweighs the liquidity channel: in booming states of the housing market cycle, spillover effects from more distant neighborhoods are stronger and spillover effects are weaker from the nearest neighborhoods.

In contrast to the work of Rossi-Hansberg, Sarte, and Owens III (2010), Campbell, Giglio, and Pathak (2011), or Guerrieri, Hartley, and Hurst (2013) that focuses on spillovers from specific events, our goal is to quantify spillover effects in regular trades where prices are not affected by specific shocks. It is therefore important to account for events that are likely to systematically affect house prices in a given area. In our empirical analysis, we focus on excess returns relative to the evolution of house prices on the zip-code level, which should remove any event that affects an entire zip-code, such as changes in air quality (Chay and Greenstone, 2005), for instance. Our results show that spillover effects only exist in the nearest neighborhood and die out quickly with increasing distance. Hence, our results are unlikely to be driven by events that affect larger neighborhoods within a zip-code.

Our work contributes to a growing strand of literature investigating micro-level dynamics of house prices. This literature demonstrates that local events, such as gentrification (Guerrieri, Hartley, and Hurst, 2013), urban revitalization (Rossi-Hansberg, Sarte, and Owens III, 2010), air pollution (Chay and Greenstone, 2005), legislative amendment (Autor, Palmer, and Pathak, 2014), unnatural deaths (Bhattacharya, Huang, and Nielsen, 2017), and foreclosures (Harding, Rosenblatt, and Yao, 2009; Campbell, Giglio, and Pathak, 2011; Anenberg and Kung, 2014; Gerardi, Rosenblatt, Willen, and Yao, 2015) are important drivers of micro-level house price dynamics. Yet, none of these papers focuses on explaining local house price dynamics in regular sales in the absence of specific events, which is the focus of our work.

Our results indicate that in addition to the well-documented autocorrelation in residential house prices, spillover effects constitute an additional systematic risk factor. In particular, spillover effects increase the correlation between house price changes on the local level, thus increasing the risk of clustered mortgage defaults – particularly for smaller (regional) banks. Spillover effects in the very local neighborhood are weaker during booming states of the housing market cycle and stronger in others, leading to particularly high mortgage default risk. Spillover effects should thus have important implications for ratings of banks as well as banking regulation.

This paper proceeds as follows: Section 2 explains the existence of spillover effects in a model and derives our main hypotheses. In section 3, we introduce our data. Section 4 presents our evidence on spillover effects in residential house prices. Section 5 documents the robustness of our results. Finally, section 6 concludes.

2 Model

In this section, we motivate spillover effects in residential house prices in a simple stylized model and derive two hypotheses, which we test empirically in section 4.

We consider a set of homes, $H_1, H_2, \ldots, H_n$. For simplicity, we assume that these homes only differ by their location. We denote the physical distance between two homes H_i and H_j by $D_{i,j}$. Households typically have a preference for a certain location of their homes. This preference could both reflect the neighborhood's facilities, such as good schools, restaurants, and shops, as well as social ties, such as other family members or friends living in the neighborhood.

A home outside the preferred location is a substitute for the home at the preferred location, because both homes provide households with the same housing services. The prices P_i and P_j of the two homes H_i and H_j should therefore be positively correlated:

$$P_i = f\left(P_j\right) \text{ with } \frac{\partial f}{\partial P_j} > 0. \tag{1}$$

That is, spillover effects in residential house prices reflect that households react to price increases for homes in a given neighborhood by purchasing substituting homes in close-by neighborhoods, thus causing price increases in these neighborhoods.

The distance $D_{i,j}$ between two homes can be interpreted as a proxy for how good two homes H_i and H_j can be substituted with each other. The distance is, among others, a good proxy for the commuting costs and time it takes to get from H_i to the amenities at H_j. The smaller the distance $D_{i,j}$ between two homes H_i and H_j, the better they proxy for each other. That is, we can refine our model to

$$P_i = g\left(P_j, D_{i,j}\right) \text{ with } \frac{\partial g}{\partial P_j} > 0 \text{ and } \frac{\partial g^2}{\partial P_j \partial D_{i,j}} < 0. \tag{2}$$

In other words, the evolution of two homes' prices in the same neighborhood is positively correlated. Such spillover effects in residential house prices are the stronger, the smaller the distance between two homes.

Liquidity in housing markets varies substantially with the stage of the housing market cycle (Stein, 1995). In markets with falling prices, volatility in returns is higher and the number of trades is lower affecting the strength of spillover effects through both a volatility and a liquidity channel. From the latter channel, th higher number of trades in booming markets increases the number of substitutes in the nearest neighborhood and thus decreases the need to consider more distant homes, suggesting that spillover effects from more distant homes should be weaker, i.e.:

$$\left.\frac{\partial g^2}{\partial D_{i,j}\partial P_j}\right|_{Boom} < \left.\frac{\partial g^2}{\partial D_{i,j}\partial P_j}\right|_{Non-boom}. \tag{3}$$

The volatility channel should have at least two counteracting effects on the predictive quality of price movements in more distant homes. On the one hand, the higher number of trades implies that more distant homes should have a poorer predictive power, because more distant homes are less needed to attain a reasonable number of recent house price changes. In line with the conclusion from the liquidity channel, more distant homes should thus have weaker predictive power in boom periods:

$$\left.\frac{\partial g^2}{\partial D_{i,j}\partial P_j}\right|_{Boom} < \left.\frac{\partial g^2}{\partial D_{i,j}\partial P_j}\right|_{Non-boom}. \tag{4}$$

On the other hand, volatility in returns in booming markets is generally lower. The inverse of this volatility can be interpreted as a measure for the precision of an informative signal (Veronesi, 2000; Epstein and Schneider, 2008). The higher informative quality of the signals from more distant homes in booming markets should cause stronger spillover effects from such trades:

$$\left.\frac{\partial g^2}{\partial D_{i,j}\partial P_j}\right|_{Boom} > \left.\frac{\partial g^2}{\partial D_{i,j}\partial P_j}\right|_{Non-boom}. \tag{5}$$

In section 4.2, we investigate whether the counteracting effects caused through the liquidity and volatility channel mainly offset each other or lead to systematic differences in the strength of spillovers from more distant homes.

Before we test our model's predictions in section 4, we first turn to introducing our data in section 3.

3 Data

Our data is from the CoreLogic database, which covers 99.9% of all residential transactions in the U.S.[2] We focus on repeat sales in urban areas using data from Manhattan Island, New York City. Manhattan Island seems ideal to study spillover effects in several regards. First, Manhattan Island is separated from other parts of New York through the Hudson River in the west, the Harlem River in the northeast, and the East River in the east and south, implying that real estate transactions outside Manhattan Island are outside the local

[2] http://www.corelogic.com/industry/real-estate-solutions.aspx

neighborhood and should therefore not affect local price dynamics on Manhattan Island. Second, given that Manhattan Island is generally perceived as a very attractive place to live at, the market for real estate is fairly liquid and foreclosures are rare.[3] Third, compared to more rural areas, Manhattan Island is densely populated and space for new buildings is therefore extremely scarce. This severely limits the number of new construction and the price impact of new buildings on existing places. Fourth, the exact prices of all trades are publicly available at the New York City Department of Finance's homepage. That is, information about actual trading prices of adjacent homes is easily available for all market participants and our results are less affected by information asymmetries. Our data spans the time period from January 2004 to December 2015 including sales prices on the most recent previous transactions ranging back to 2000.

3.1 Data cleaning

Initially, our data consists of 168,419 transactions. We focus on repeat sales of condominiums and apartments and remove trades of other real estate units, such as parking lots, hotels, office buildings, or warehouses, and observations that are not classified as resales or for which information about the date of the transaction, the current or most recent preceding sales price (prior sales price) is not available, leaving us with 42,301 observations. The removal of duplicates with identical sales price, prior sales price, transaction dates, and geographic coordinates leaves our sample with 41,905 observations. Following Landvoigt, Piazzesi, and Schneider (2015), we remove speculative trades with holding periods of less than 180 days,[4] leaving us with 41,283 observations. Finally, similar to Campbell, Giglio, and Pathak (2011), for every year we remove outliers with current or prior sales prices in the first and 99th percentile, respectively, leading to a cleaned data set of 39,771 observations. To account for data errors or physical changes in a property, we follow S&P (S&P Dow Jones Indices, 2017) in removing outliers. More specifically, we remove observations in the third and 97th percentile of the annualized return distribution.[5] Our final data set then consists of 37,385 observations.

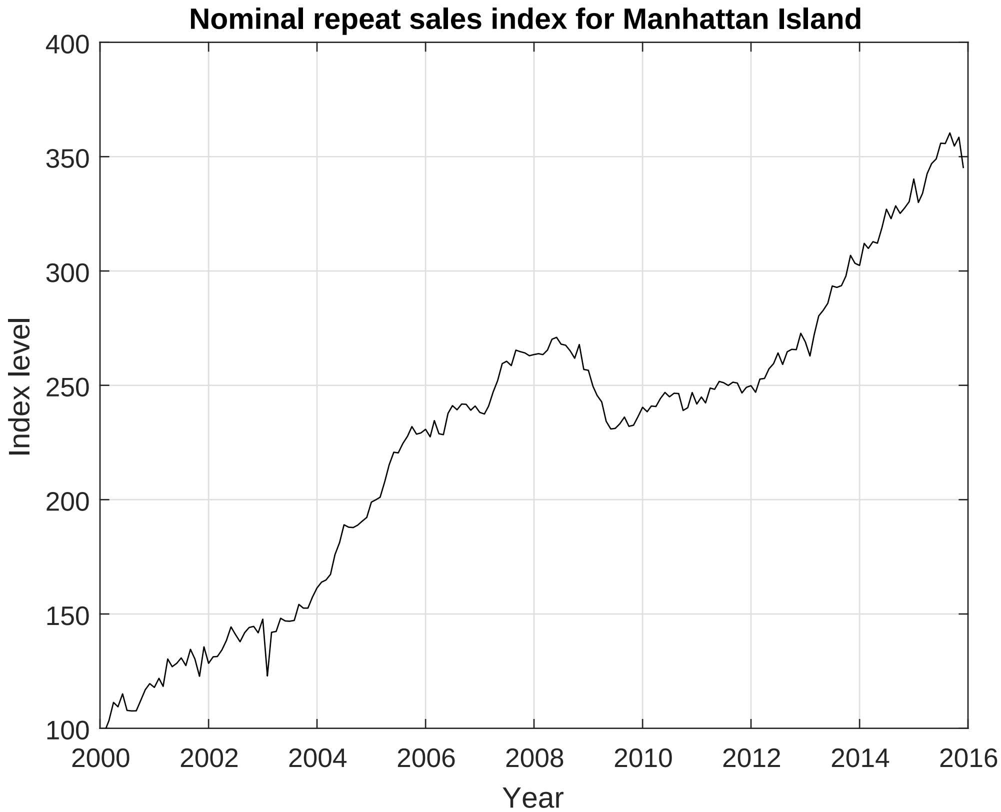
Figure 1 summarizes the evolution of residential house prices in our cleaned data set using a repeat sales index (Case and Shiller, 1989) constructed on a monthly basis for the time period from January 2000 to December 2015. Similar to house prices on the national

[3]According to RealtyTrac.com (http://www.realtytrac.com/statsandtrends/foreclosuretrends/ny/new-york-county/new-york/ as of November 2017) only one in 24,721 trades in New York City relates to a foreclosure.

[4]Similarly, in the construction of the S&P 500 Case-Shiller house price index, observations with holding periods of less than six months are removed (S&P Dow Jones Indices, 2017).

[5]The results are qualitatively robust to removal of one or two percent of each tail, but point estimates are likely to be outlier driven.

Figure 1
Evolution of house prices on Manhattan Island



Nominal repeat sales index of Manhattan Island's condominium/apartment market based on our final data set. The index level is normalized to 100 in January 2000.

level, from Figure 1, Manhattan Island experienced a significant boom during the 2000s with prices more than doubling from 2000 to 2006. Thereafter, house prices did not show a clear trend until house prices declined sharply in late 2008 – later than on the national level.[6] This relatively late decline may reflect that layoffs in the financial industry and their implications for house prices on Manhattan Island did not occur instantly when house prices on national level started declining, but with a certain delay.

3.2 Excess returns

The repeat sales in our data differ along two important dimensions that make a direct comparison of returns difficult. First, the lengths of the time intervals between two trades may

[6]In section 4.2, we exploit these differences in the general evolution of house prices to investigate whether spillover effects vary with the state of the housing market cycle.

differ substantially. Second, returns depend crucially on the state of the housing market cycle. To control for these two effects, we compute annualized market-adjusted excess returns, $r_{t,t-}$, for properties traded at month t and previously traded at month $t-$ as follows:

$$r_{t,t-} = \left(\frac{P_t}{P_{t-}}\right)^{\frac{1}{y(t,t-)}} - \left(\frac{C_t}{C_{t-}}\right)^{\frac{1}{y(t,t-)}} \tag{6}$$

in which P_t and P_{t-} denote the present and prior trading prices of the property in months t and $t-$, respectively, $y\,(t,t-)$ is the time distance in years between the two trades, and C_t and C_{t-} denote the index levels of the Manhattan Island repeat-sales price index constructed as in Case and Shiller (1989) from our cleaned data in months t and $t-$, respectively. By subtracting the index return, we remove aggregate effects that should have a systematic effect on house prices, such as inflation or seasonal effects.

3.3 Control variables

Our control variables can be broadly allocated into three different categories: (1) transaction-specific , (2) locational , and (3) macro-financial control variables.

3.3.1 Transaction-specific variables

In our data cleaning procedure, we remove transactions with holding periods of less than 180 days, which are likely to be speculative trades. Yet, shorter holding periods may be targeted at larger renovations during that period aiming at substantially increasing the property's value. Simultaneously, with short holding periods, sellers may use their purchasing price rather than neighboring house prices as a reference point. To account for these possible effects, we include mutually exclusive dummy variables for holding periods of less than one or less than two years, respectively. The results in Landvoigt, Piazzesi, and Schneider (2015) document that during the recent housing market boom, housing returns varied substantially between homes in different price segments in a nonlinear fashion. To account for this effect, we control for the log of the inflation-adjusted prior sales price (in January 2015 dollars) as well as its square. Whereas private investors profit from both their home as a durable consumption good and from house price appreciations, corporations should place higher emphasis on earning higher returns on their investments. To control for these effects, we include two dummies for whether a property is sold or bought by a corporation and a dummy for whether a home is bought to become an owner-occupied home. Transactions in which the buyer is a corporation or the home is bought to serve as an owner-occupied home are already marked in our database. We define a seller as a corporation if the seller name contains key words such as LLC, Bank, and Fund.[7]

3.3.2 Locational variables

The location of a residential home is one of the key factors determining its price (e.g., Can, 1990; Case and Mayer, 1996). To control for possible changes in the pricing of location-specific factors, we control for the view on the Central Park and the waterfront as well as the walking-distance to these two. We further control for distance to Times Square, New York Stock Exchange, and the nearest entry to the subway.[8] More specifically, we include mutually exclusive dummies for a view and a walking-distance to the Central Park if the beeline does not exceed 100 feet and the city block walking-distance does not exceed 500 feet, respectively. In a similar fashion, we include a waterfront-view-dummy if a home has a direct view on the water surrounding Manhattan Island; i.e., if the home is separated from water only by a road, a park, or both, but not any other building. We further include a walking-distance dummy if the city block distance to the waterfront does not exceed 500 feet. To account for easy access to the subway system, we include two dummies: a dummy for very close distances to the nearest entry for city block walking-distances of less than 100 feet and a dummy for close distances of 100 to less than 500 feet. For the Times Square and the New York Stock Exchange we include two dummies for short walking-distance and medium walking distance if the city-block distance is less than 1,000 feet or 1,000 to less than 2,000 feet, respectively.

Guerrieri, Hartley, and Hurst (2013) document substantial differences in house price growth across neighborhoods. To account for these differences, we proceed similar to Campbell, Giglio, and Pathak (2011), who use census-tract-year dummies, and control for zip-code-year fixed effects in the year of the current and the prior trade of the home. To attain a reasonable number of observations per zip-code (at least 1,000 observations), we have to cluster a few adjacent ones. A detailed overview over the clustered zip-codes can be found in Appendix A. To control for the impact of liquidity in the local housing market on transaction prices (Caplin and Leahy, 2011), we control for the log of one plus the number of trades in the past 90 days on zip-code level.

3.3.3 Macro-financial variables

To control for changes in the macroeconomic environment, we include the seasonally adjusted real growth rate of the GDP relative to the previous quarter with a lag of one period from

[7] A complete list of key words can be found in Appendix B.

[8] The geographic coordinates of the New York subway entries are from NYC Open Data (`https://opendata.cityofnewyork.us`)

the US Bureau of Economic Analysis, the seasonally adjusted monthly growth rate of the unemployment rate in New York City from the Bureau of Labor Statistics, and the percentage change average fixed mortgage lending rate from the Federal Housing Finance Board.

Since the pioneering work of Case and Shiller (1989), it is known that residential house prices exhibit a significant degree of autocorrelation. To explain price movements, it is therefore important to control for this persistence. Our analysis focuses on explaining excess returns rather than raw returns thus removing this systematic component.

Table 1 summarizes key properties of our data. As to be expected, the annualized excess return is not significantly different from zero.[9] The average holding period is only about 5.5 years, indicating that Manhattan Island is a fairly liquid market for residential homes. About 9% of properties are even resold within up to 2 years, which may, among others, reflect institutional investors' activities that account for 15% of sales. Yet, the majority of trades (53%) still represents sales of owner-occupied places. With an average prior trading price of 1.279 million dollars (inflation-adjusted to 2015 prices), prices on Manhattan Island are among the most expensive ones in the U.S. This high average trading price suggests that prices should be largely determined by location. In contrast, renovations or a new kitchen should have a lower impact on the trading price, advocating the repeat sales approach. Likewise, the short average holding period provides additional support for the repeat sales approach.

3.4 Methodology

The goal of our work is to investigate how excess returns in residential house prices spill over to other homes in the neighborhood. For that purpose, we define K mutually exclusive neighborhoods for each observed trade. We refer to trades with coinciding geographic coordinates, i.e., trades in the same building, as the first-order neighborhood throughout. Additionally, we draw $K-1$ circles around each observed trade. We want to end up with roughly comparable numbers of observations inside all circles to make sure that all of them bear a comparable informational content. We therefore draw the circles such that the area inside each of them is essentially identical.

In our base-case setting, the first circle, also referred to as the second-order neighborhood throughout, is characterized by a maximum distance of 500 feet, roughly corresponding to two blocks. The borders of the third-, fourth-, fifth-, and sixth-order neighborhoods are then 707, 866, 1,000, and 1,118 feet, respectively, leaving us with roughly three historical trades in the second- to sixth-order neighborhood for every current trade. Figure 2 visualizes our

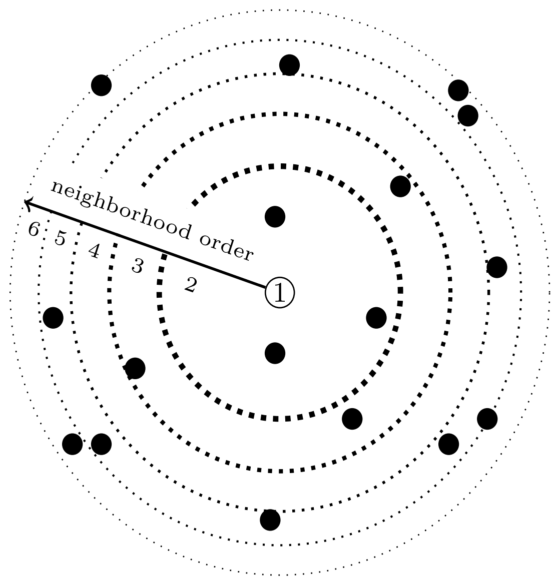
[9]The small positive value reflects that the market return constructed using the Case-Shiller methodology weighs observations unequally.

Table 1
Summary statistics

Variable name	Mean	Std.
Annualized excess return	0.007	0.056
Holding period (in years)	5.401	2.666
Liquidity	87.639	52.871
Central Park view	0.029	0.167
Central Park walking	0.042	0.200
Very close subway	0.024	0.154
Close subway	0.185	0.389
Short distance Times Square	0.003	0.056
Medium distance Times Square	0.007	0.085
Short distance NYSE	0.013	0.114
Medium distance NYSE	0.019	0.135
Waterfront view	0.027	0.162
Waterfront walking distance	0.050	0.218
Dummy one year	0.018	0.134
Dummy two years	0.074	0.261
Price (in mio USD)	1.279	1.301
Seller corporation	0.103	0.304
Buyer corporation	0.156	0.363
Owner-occupied	0.531	0.499
Lagged GDP growth	0.005	0.005
Lagged unemployment growth	-0.007	0.018
Lagged interest change * 10,000	-1.935	300.379

This table provides descriptive statistics of the variables used in our work. *Annualized excess returns* are defined in Equation (6). *Holding period (in years)* is the number of years between two trades of a given residential home. *Liquidity* is the number of sales during the past 90 days in the respective zip-code. *Central Park view* and *Central Park walking* are two dummies indicating whether a home has a view on the Central Park (distance of less than 100 feet beeline) and the city-block distance to the nearest entrance is less than 500 feet, respectively. *Very close subway* and *Close subway* are two mutually exclusive dummies indicating whether the city-block distance to the nearest subway entrance is less than 100 feet or 100 to less than 500 feet, respectively. *Short distance Times Square / NYSE* and *Medium distance Times Square / NYSE* are mutually exclusive dummies for whether the city-block distance to the Times Squares / NYSE is less than 1,000 feet or 1,000 to less than 2,000 feet, respectively. *Water front view* is a dummy indicating whether a home has direct view on the water surrounding Manhattan Island. *Waterfront walking distance* is a dummy indicating whether the city-block distance to the waterfront does not exceed 500 feet. *Dummy one year* and *Dummy two years* are indicators for holding periods of one and two years, respectively. *Price (in mio USD)* is the most recent available prior trading price of the home CPI-adjusted to January 2015 dollars. *Seller/Buyer* corporation is a dummy indicating whether the seller/buyer is a corporation. *Owner-occupied* is a dummy indicating whether the buyer is the new inhabitant. *Lagged GDP growth* is the previous quarter's U.S. GDP growth. *Lagged unemployment growth* is the previous month's New York City wide unemployment growth rate. *Lagged interest change* is the percentage change of the average fixed mortgage lending rate in the month prior to the sale.

Figure 2
Construction of neighborhoods



This figure visualizes our construction of neighborhoods. The center symbolizes a trade for a given home. Other trades in the same building are defined as trades in the first-order neighborhood. The dotted circles surrounding the center depict edges of mutually exclusive neighborhoods of orders two to six.

construction of $K = 6$ neighborhoods for a specific property. For every neighborhood k, we define a neighborhood-specific excess return, $\bar{r}^e_{i,k}$ as the average of the observed excess returns in the T days prior to our trade. We employ the following regression setup:[10]

$$r^e_{i,t,t-,z} = \alpha_z + \sum_{k=1}^{K} \rho_k \bar{r}^e_{i,k} + \delta_{y(t),z} - \delta_{y(t-),z} + X_{i,t}\beta + \epsilon_{i,t,t-,z} \tag{7}$$

in which $r^e_{i,t,t-,z}$ is the annualized excess return on property i in zip-code z realized between time $t-$ and t, $\delta_{y(t),z}$ and $\delta_{y(t-),z}$ are the zip-code-z specific deviations of the annualized excess returns from the Manhattan Island wide index in years $y(t)$ and $y(t-)$, respectively. $X_{i,t}$ is a vector of control variables. $\epsilon_{i,t,t-,z}$ is a normally-distributed error term. The precision of our estimate for the annualized excess return is generally increasing with the length of the time interval between the two trades. Intuitively, when the two trades occur within a relatively short time period, small deviations in observed trading prices of individual properties and short-term fluctuations in the local house price index lead to significant amplifications when

[10]Equation (7) can be easily rewritten in spatial econometrics notation because $\bar{r}^e_{i,k}$ reflects the kth spatial lag. Nevertheless, under the assumption of homoskedastic error terms, OLS is applicable since we account for the time-directionality in constructing the spatial weights.

being annualized. Hence, annualized excess returns tend to be subject to higher variation when two trades occur within a relatively short time period. To account for this phenomenon in our analysis, we allow the variance of $\epsilon_{i,t,t-,z}$ to depend on the difference D between t and $t-$: $\mathrm{Var}\left(\epsilon_{i,t,t-,z}\right) = \exp\left(\gamma_1 + \gamma_2 D\right)$ where γ_1 and γ_2 are regression-endogenously determined coefficients.[11]

Our goal is to explore whether price changes in the neighborhood spill over to other trades, i.e., whether the ρ_ks are different from zero and, if so, whether such spillover effects decay with increasing distance, i.e., whether $|\rho_1| > \cdots > |\rho_K|$.

4 Empirical Results

For our empirical analysis, we need to determine a few parameters for our model introduced in section 3.4. Specifically, we need to choose the number of distinct neighborhoods that we want to consider. In particular, we want to understand whether neighborhood effects are strongest in the first-order neighborhood and whether they are dying out in more distant neighborhoods. We therefore set the number of neighborhoods to $K = 6$.[12]

We further need to choose the maximum number of days prior to our trade, T, such that trades on other properties should reasonably have the potential of affecting a home's price. The choice of T is driven by a tradeoff between two opposing objectives. On the one hand, we want to estimate spillover effects as precisely as possible, suggesting that we should use as much past data as possible. On the other hand, the precision can be reduced by using outdated observations, that have little informational content for present prices, among others, because it is already incorporated in more recent prices. We set $T = 90$ for three main reasons. First, gathering information in the housing market costs more time than for example gathering information about the stock market. Second, finding a buyer for a given home typically takes time. Third, our choice of about a quarter of a year provides us with a reasonable number of observations to estimate effects at a good precision.

4.1 Spillover Effects

In this section, we provide empirical evidence on the existence and strength of spillover effects in residential house prices. Table 2 summarizes the results of five Maximum Likelihood regressions explaining the annualized excess returns of repeat sales relative to trades on

[11] Our estimates are qualitatively robust to the homoskedastic case.

[12] Empirically, it turns out that a larger number of neighborhoods does not further contribute significantly to explaining house prices while a smaller number does not allow us to fully capture the decay in spillovers with increasing distance.

Manhattan Island. The first-order neighborhood relates to trades in the same building. Second-, third-, fourth-, fifth-, and sixth-order neighborhoods are less than 500, 500 to 707, 707 to less than 866, 866 to less than 1,000, and 1,000 to less than 1,118 feet. Our choice of distances from the traded homes is motivated by the goal to build neighborhoods of identical sizes to end up with similar numbers of traded homes in every neighborhood. Locational controls are our measure for liquidity, dummies indicating Central Park view, Central Park walking distance, a very close subway station, a close subway station, short distance to the Times Square, medium distance to the Times Square, short distance to the NYSE, medium distance to the NYSE, waterfront view, and waterfront walking distance. Transaction-specific controls two dummy variables indicating a resale took place within one year, or between one and two years, respectively, log inflation-adjusted prior sale price and its square, two dummies indicating whether seller or buyer of the property is a corporation, and a dummy indicating whether the property is owner-occupied. Marco-financial controls are lagged GDP growth, lagged unemployment growth, and lagged percentage interest rate change. Fixed effects are on the zip-code level (zip) or the zip-code-year level (zip-year).

From section 2, spillover effects should exist in residential house prices and decay as the distance between homes increases. From Table 2, the coefficients for neighborhoods one to five are all positive. That is, Table 2 confirms the existence of spillover effects. From the first- to the sixth-order neighborhood the coefficients generally decrease, indicating that spillover effects are the weaker, the larger the distance between two traded homes. Coefficients are monotonically decreasing, except for the transition from the second- to the fourth-order neighborhood, for which the strength of spillover effects is of roughly the same order of magnitude.

For all specifications in Table 2, the sharpest decline in spillover effects is observed for the transition from the first- to the second-order-neighborhood, where coefficients drop by more than 80%. This result should be mainly driven through two channels. First, trades within the same building should be among the closest substitutes, thus exhibiting very strong spillover effects. Second, within the first-order neighborhood both the transmission of information via informal channels, such as chats among neighbors, but also active search for information should be most intense.

Local price movements should generally be driven by location-specific events. It is therefore important to control for them. A comparison of columns (1) and (2) reveals that after including our locational controls and controlling for zip-code fixed effects, the coefficients generally decrease, but remain highly significant for the first five neighborhoods. That is, even after controlling for location-specific events, there is still a strong informational content in house price movements in the closest neighborhoods. However, our results also reveal that

Table 2
Estimation results, base case

Variable Name	(1)	(2)	(3)	(4)	(5)
First-order neighborhood	0.246***	0.224***	0.202***	0.199***	0.178***
	(0.016)	(0.016)	(0.014)	(0.015)	(0.013)
Second-order neighborhood	0.086***	0.050***	0.048***	0.045***	0.029**
	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)
Third-order neighborhood	0.079***	0.047***	0.047***	0.045***	0.032***
	(0.009)	(0.009)	(0.008)	(0.008)	(0.008)
Fourth-order neighborhood	0.088***	0.053***	0.049***	0.046***	0.037***
	(0.009)	(0.009)	(0.008)	(0.008)	(0.008)
Fifth-order neighborhood	0.060***	0.028**	0.023*	0.021*	0.013
	(0.011)	(0.009)	(0.009)	(0.009)	(0.009)
Sixth-order neighborhood	0.036***	0.002	0.000	-0.003	-0.017*
	(0.009)	(0.009)	(0.008)	(0.008)	(0.009)
ln(1+Liquidity)		-0.005***	-0.002***	-0.002*	0.000
		(0.001)	(0.001)	(0.001)	(0.001)
Central Park view		0.008**	0.007**	0.007**	0.008**
		(0.002)	(0.003)	(0.003)	(0.003)
Central Park walking distance		0.001	-0.001	-0.001	0.000
		(0.003)	(0.003)	(0.003)	(0.003)
Very close subway		0.004	0.004	0.004	0.004
		(0.002)	(0.002)	(0.002)	(0.002)
Close subway		0.003***	0.003***	0.003***	0.004***
		(0.001)	(0.001)	(0.001)	(0.001)
Short distance Times Square		-0.007**	-0.006**	-0.006**	-0.008**
		(0.002)	(0.002)	(0.002)	(0.003)
Medium distance Times Square		0.001	-0.001	-0.001	-0.002
		(0.002)	(0.002)	(0.002)	(0.002)
Short distance NYSE		-0.002	-0.005	-0.005	-0.013**
		(0.005)	(0.005)	(0.005)	(0.005)
Medium distance NYSE		-0.012***	-0.012***	-0.011***	-0.018***
		(0.002)	(0.002)	(0.002)	(0.003)
Waterfront view		0.007**	0.005	0.004	0.001
		(0.002)	(0.003)	(0.003)	(0.003)
Waterfront walking distance		-0.008***	-0.006***	-0.005***	-0.003
		(0.002)	(0.001)	(0.001)	(0.002)
Dummy one year			0.061***	0.061***	0.051***
			(0.006)	(0.006)	(0.006)
Dummy two years			0.034***	0.034***	0.025***
			(0.002)	(0.002)	(0.002)
ln(Price)			-0.140***	-0.140***	-0.138***
			(0.018)	(0.017)	(0.017)
$\ln(\text{Price})^2/100$			0.488***	0.488***	0.482***
			(0.064)	(0.061)	(0.061)
Seller corporation			0.010***	0.011***	0.011***
			(0.002)	(0.002)	(0.002)
Buyer corporation			0.013***	0.013***	0.013***
			(0.001)	(0.001)	(0.001)
Owner-occupied			-0.001	-0.001*	-0.001*
			(0.001)	(0.001)	(0.001)
Lagged GDP growth				-0.016	-0.070
				(0.063)	(0.060)
Lagged unemployment growth				0.106***	0.064**
				(0.021)	(0.024)
Lagged interest change				-0.006	-0.014
				(0.009)	(0.010)
Fixed effects	no	zip	zip	zip	zip-year
Akaike criterion	-117,162	-118,118	-120,307	-120,362	-121,404

This table summarizes the results of Maximum Likelihood regressions explaining the annualized excess return of repeat sales relative to trades on Manhattan Island. The first-order neighborhood relates to trades in the same building. Second-, third-, fourth-, fifth-, and sixth-order neighborhoods have distances to the traded home of less than 500 feet, 500 to less than 707 feet, 707 to less than 866 feet, 866 to less than 1,000 feet, 1,000 to less than 1,118 feet, respectively. For further variable descriptions see Table 1. Fixed effects are on the zip-code level (zip) or the zip-code-year level (zip-year). Heteroskedasticity-robust standard errors are clustered over the zip-code-year level and reported in parentheses. ***, **, and * denote significance at the 0.1%, 1%, and 5% level, respectively.

the coefficient for the most remote, the fifth-order, neighborhood becomes close to zero and insignificant. In other words, our local controls and zip-code fixed effects already capture local price trends quite well. Furthermore, the insignificance of the coefficient for the sixth-order neighborhood in column (2) points to two conclusions: First, beyond local price trends, the sixth-order neighborhood no longer contains price information. Second, the reduction in the coefficients for the first- to fifth-order neighborhood largely reflects the removal of the location and zip-code specific events. The locational controls and the zip-code fixed effects thus not only capture the general price movement in the sixth-, but also the first- to fifth-order neighborhoods very well.

Changes in our coefficients in the transition from column (2) to (3), where we include transaction-specific controls, are rather small. In the transition from column (3) to (4), where we include macro-financial controls, these changes are even smaller, indicating that the excess returns, our work builds on, already capture the effects of macroeconomic events very well.

Column (5) reports the estimates for our full specification. Compared to the model presented in column (4), we include zip-code-year fixed effects as opposed to zip-code fixed effects. From column (5), 18% of the increase in the annualized excess return in the first-neighborhood spill over to future home prices. For example, a one standard deviation increase in the annualized excess return in the first-order neighborhood, i.e., an increase in the annualized excess return by about 6%, leads to an increase in the expected annualized excess return of future home prices of about 1.1%. With an average holding period of about 5 years, the realized excess return is about 5.5%. For the second- to fourth-order neighborhoods these effects are around 80% weaker than for the first-order neighborhood. That is, a one standard deviation increase in the second- to fourth-order neighborhood's excess return leads to an increase in the expected future excess return after the typical holding period of 5 years of around 1%. With excess returns of identical signs in the first- to third-order neighborhoods, spillover effects are accumulating and expected future excess returns can be even higher. For example, a one standard deviation increase in all four neighborhoods leads to an increase in the expected future excess return of around 8.5%.

4.2 Spillover Effects over the Housing Market Cycle

Having demonstrated the existence of spillover effects in residential house prices, we next ask whether the strength of these effect varies with the state of the housing market cycle, i.e., whether the strength of spillover effects and the distance over which they are measurable is different in periods where the housing market is booming compared to other periods.

For our empirical analysis, we define boom and non-boom periods using our price index for Manhattan Island from Figure 1. From that index, the boom in the early 2000s ends in October 2005, and house prices start booming again in March 2013. We therefore define the period from November 2005 to February 2013 as the non-boom period and the remaining months as the boom period.[13] Accordingly, we extend the base case from equation (7) to:

$$r^e_{i,t,t-,z} = \alpha_z + \sum_{s=1}^{2}\sum_{k=1}^{K} \rho_{k,s} \times \bar{r}^e_{i,k} \times \mathbb{1}_{t\cap s} + \delta_{y(t),z} - \delta_{y(t-),z} + X_{i,t}\beta + \epsilon_{i,t,t-,z}, \tag{8}$$

where the two states of the cycle are defined by s, and $\mathbb{1}_{t\cap s}$ is an indicator function that equals one if the housing market is in state s at time t.

Table 3 summarizes regression results explaining the annualized excess return of repeat sales relative to trades on Manhattan Island in boom and non-boom periods. Consistent with our results from Table 2 and our model prediction from section 2, we find evidence for spillover effects decaying with increasing distance between traded homes through different stages of the housing market cycle.

The state of the housing market cycle should affect how spillover effects drive house prices through two counteracting channels. On the one hand, booming housing markets are generally characterized by a high degree of liquidity. Hence, the number of trades in closer neighborhoods is typically higher in boom periods. This suggests that households do not need to rely on price signals from more distant trades. Consequently, from this liquidity channel, the strength of spillover effects should be particularly pronounced in the nearest neighborhood and die out quickly. On the other hand, the lower level of volatility in booming states of the housing market cycle suggests that the informational content of more distant trades is higher. Hence, more distant homes should have a stronger predictive power in boom periods. Consequently, from this volatility channel, spillover effects should be weaker in the nearest neighborhood and fade out less quickly with increasing distance. Overall, the liquidity channel suggests that in booming states of the housing market cycle spillover effects should be more pronounced in the nearest neighborhood, whereas the volatility channel implies that in booming states of the housing market cycle spillover effects should be more pronounced for more distant trades.

[13] Using the publicly available S&P CoreLogic Case-Shiller New York City condominium index (download link `https://us.spindices.com/documents/additionalinfo/20170926-589149/589149_cs-condoindices-0926.xls?force_download=true`), we identify a non-boom period between February 2006 and April 2012. Using the S&P Case-Shiller National home price index, we identify a period from March 2006 to March 2012. Similarly, we characterize our non-boom period using a purely liquidity-based approach building on the number of observed trades. In section 5.2, we document that our results are robust to all these alternative specifications.

Table 3
Estimation results, boom versus non-boom periods

Variable Name	Boom	Non-boom
First-order neighborhood	0.164***	0.197***
	(0.019)	(0.017)
Second-order neighborhood	0.025	0.033**
	(0.014)	(0.012)
Third-order neighborhood	0.044***	0.020
	(0.012)	(0.011)
Fourth-order neighborhood	0.050***	0.022
	(0.010)	(0.012)
Fifth-order neighborhood	0.002	0.025*
	(0.013)	(0.012)
Sixth-order neighborhood	-0.024	-0.010
	(0.013)	(0.011)
Locational controls	yes	
Transaction-specific controls	yes	
Macro-financial controls	yes	
Fixed effects	zip-year	
LR test (p-value)	0.032	
Akaike criterion	-121,402	

This table summarizes Maximum Likelihood regression results explaining the annualized excess return of repeat sales relative to trades on Manhattan Island in boom (January 2004 to October 2005, and March 2015 to December 2015) and non-boom (November 2005 to February 2013) periods. The first-order neighborhood relates to trades in the same building. Second-, third-, fourth-, fifth-, and sixth-order neighborhoods have distances to the traded home of less than 500 feet, 500 to less than 707 feet, 707 to less than 866 feet, 866 to less than 1,000 feet, 1,000 to less than 1,118 feet, respectively. The locational, transaction-specific and macro-financial control variables are as defined in section 3.3. Fixed effects are on the zip-code-year level (zip-year). The Likelihood Ratio test (LR test) is a test of joint equality of neighborhood coefficiets, i.e. under the H_0: $\rho_{1,b} = \rho_{1,nb}, \ldots, \rho_{6,b} = \rho_{6,nb}$. Heteroskedasticity-robust standard errors are clustered over the zip-code-year level and reported in parentheses. ***, **, and * denote significance at the 0.1%, 1%, and 5% level, respectively.

From Table 3, in the first- and second-order neighborhood, spillover effects are weaker in booming markets. During boom periods, 16.4% of price changes in the first-order neighborhood, i.e., in the same building, spill over, whereas 20% of price changes in other periods do. For the second-order neighborhood, these values decrease to 2.5% and 3.3%, respectively. Simultaneously, spillover effects are stronger from the third- and the fourth-order neighborhood. During boom periods, about 5% of price changes spill over, whereas in other periods only about 2% do. We conducted a likelihood ratio test to assess whether the coefficients are significantly different between boom and bust periods. With a p-value of 0.032 we can reject the null hypothesis that $\rho_{1,b} = \rho_{1,nb}, \rho_{2,b} = \rho_{2,nb}, \ldots, \rho_{6,b} = \rho_{6,nb}$ at the 5% level. Our results thus reveal that the volatility channel generally outweighs the liquidity channel.

5 Robustness Analysis

This section documents the robustness of our key findings with respect to various assumptions made throughout our manuscript. Section 5.1 provides evidence for our base case parameter setting, in which we do not distinguish between boom and non-boom periods. Section 5.2 provides results for different definitions of the boom and non-boom periods.

5.1 Robustness of base case results

From our results in Table 2, spillover effects are important drivers of house prices on the micro-level. In this section, we demonstrate the robustness of these effects to various assumptions underlying the results in Table 2. More specifically, we demonstrate the robustness of our results with regard to four key dimensions and report these results in Table 4. To simplify the comparison with our base-case results, we repeat the results from Table 2 in Panel A of Table 4.

In Panel B of Table 4, we allow for a different number of past days used to compute average excess returns in the neighborhoods. In our base case parameter setting, we used the past $T = 90$ days, which we consider a good tradeoff between the two opposing goals of having a reasonably larger number of observations and very recent up-to-date observations; in Panel B, we explore the cases in which we set $T = 60$ or $T = 120$ days. Our results for these two cases demonstrate the robustness of our key findings that effects are strongest in the same building, i.e., the first-order neighborhood, remain significant in the second- to fourth-order neighborhood, and fade out for higher-order neighborhoods. Similarly, the point estimates for the strength of spillover effects in the various neighborhoods are of a very similar order of magnitude. A notable exemption is the estimate for the second-order

Table 4
Robustness, base case

Neighborhood order	First	Second	Third	Fourth	Fifth	Sixth
Panel A: Base case	0.178***	0.029**	0.032***	0.037***	0.013	-0.017*
	(0.013)	(0.009)	(0.008)	(0.008)	(0.0011)	(0.009)
Panel B: Varying computation of excess returns in neighborhoods						
$T = 60$	0.158***	0.009	0.037***	0.029***	0.010	-0.009
	(0.013)	(0.008)	(0.008)	(0.007)	(0.009)	(0.009)
$T = 120$	0.186***	0.031***	0.032***	0.043***	0.017	-0.018
	(0.014)	(0.010)	(0.010)	(0.008)	(0.010)	(0.009)
Panel C: Varying neighborhood definitions						
333 feet	0.178***	0.033***	0.000	0.039***	0.030**	0.032***
	(0.013)	(0.010)	(0.010)	(0.009)	(0.008)	(0.009)
0.1, 0.25 miles	0.178***	0.035***	0.051***			
	(0.013)	(0.010)	(0.014)			
Panel D: Varying maximum holding period						
Seven years	0.165***	0.023*	0.026**	0.029***	0.007	-0.006
	(0.015)	(0.009)	(0.009)	(0.009)	(0.009)	(0.008)
Ten years	0.172***	0.023**	0.028**	0.030***	0.015	-0.011
	(0.013)	(0.009)	(0.007)	(0.007)	(0.009)	(0.008)
Panel E: City block and waterfront						
City block metric	0.178***	0.027**	0.028**	0.022**	0.025***	0.016
	(0.013)	(0.009)	(0.009)	(0.008)	(0.008)	(0.010)
Exclude waterfront obs.	0.180***	0.029**	0.028***	0.033***	0.016	-0.025**
	(0.015)	(0.010)	(0.009)	(0.008)	(0.010)	(0.009)

This table documents the robustness of our key results with respect to various assumptions. Panel B presents results when varying the definition of T, the maximum number of past days used to compute average excess returns in the neighborhood. Panel C presents results for different neighborhood definitions. In the row "333 feet", the second-order neighborhood is defined by a maximum distance of 333 feet. The subsequent neighborhoods are defined, such that the area within each neighborhood is the same as in the second-order, yielding borders of 470, 576, 666, and 744 feet. In the row marked "0.1, 0.25 miles", the second- and third-order neighborhoods are defined by maximum distances of 0.1 and 0.25 miles from the traded home, i.e., 528 and 1,320 feet, respectively. In Panel D, observations with a holding period of more than seven or ten years, respectively, are excluded. Panel E shows results for a change in the distance measure to the city block metric, and when excluding observations for which the waterfront lies within at least the sixth-order neighborhood (i.e., 1,118 feet). All regressions include the entire set of controls: locational, transaction-specific, and macro-financial. Fixed effects are on the zip-code-year level. Heteroskedasticity-robust standard errors are clustered over the zip-code-year level and reported in parentheses. ***, **, and * denote significance at the 0.1%, 1%, and 5% level, respectively.

neighborhood for the case with only $T = 60$ days of past trades used to compute average excess returns in the neighborhood. In that case, the coefficient is an order of magnitude smaller and not significantly different from zero, suggesting that setting $T = 60$ results in a too small number of observations used in computing the average excess return in the neighborhoods to attain a reasonably precise estimate. For instance, for $T = 60$, the share of observations with one single observation in the second-order neighborhood is about 50% higher than for $T = 90$.

In Panel C, we vary the definitions of the neighborhoods. In our base case parameter setting, the second-order neighborhood was characterized by a maximum distance from the traded home of not more than 500 feet, roughly corresponding to two blocks. Here, we report results when shrinking this distance measure by two thirds, i.e., to 333 feet. Again, the borders of the higher-order neighborhoods are defined such that the area is the same as in the second-order neighborhood. We also depict results for the case, in which the neighborhoods are defined as in Campbell, Giglio, and Pathak (2011), i.e., a maximum distance of 0.1 miles, corresponding to 528 feet, for the second-order and 0.25 miles, corresponding to 1,320 feet, for the third-order neighborhood. As in Campbell, Giglio, and Pathak (2011), we do not account for neighborhoods of higher order. Our results in Panel C again document the robustness of our key finding that spillover effects are strongest in the first-order neighborhood. With smaller second- to sixth-order neighborhoods for the former case, results remain significant even in the sixth-order neighborhood, reflecting that the maximum distance of a trade in this neighborhood is 744 feet, corresponding to a trade in the fourth neighborhood in our base-case parameter setting. Similar to the case with a shorter maximum number of past days used to compute average excess returns in the neighborhood from Panel B, a more narrow definition of neighborhoods again suffers from the problem of relatively small numbers of historical trades in each of the neighborhoods, which, among others, leads to the coefficient for the third-order neighborhood to be insignificant. For instance, the average number of historical trades in this neighborhood decreases by 65% compared to our base-case parameter setting with wider neighborhoods.

In Panel D, we restrict the maximum holding period to seven and ten years, respectively.[14] Further restricting the maximum holding period to less than seven years leads to such a strong decline in the number of observations that it no longer provides a representative picture of market movements and – due to the lack of this information – predicts largely insignificant effects. Specifically, reducing the maximum holding period to six years removes more than a third of all trades and the information contained in these trades.

[14]For shorter holding periods, larger reconstructions and major changes in the neighborhood should be less likely. That is, the repeat-sales approach should yield particularly precise estimates.

In Panel E, we change the distance measure used in the definition of our neighborhoods from the Euclidean to the city-block metric and ask whether excluding observations for which the waterfront lies within at least the sixth-order neighborhood, affects our results. Intuitively, for such observations, the area covered by higher-order neighborhoods may be smaller than that of smaller-order neighborhoods giving rise to potentially significantly different numbers of past trades in the different neighborhoods. Our results for both cases confirm our key findings that spillover effects are strongest in the first-order neighborhood and fade out for the most distant neighborhoods. Under the city block metric, results are still significant for the fifth-order neighborhood, reflecting that several observations that, under the Euclidean norm, fall into the fourth-order neighborhood end up in the fifth-order neighborhood, once the city block metric is used, which leads to higher values for the distance measure for homes that are not in the same street.

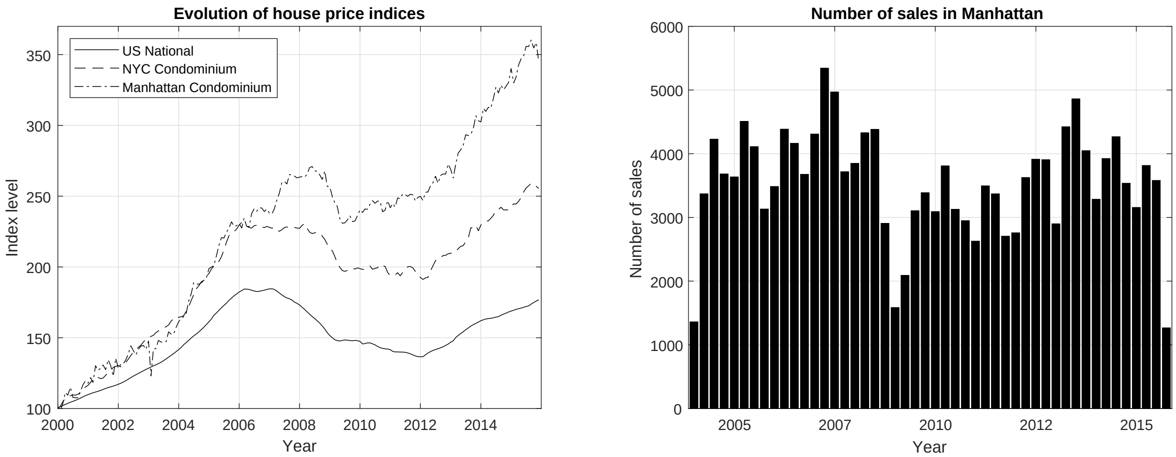
Overall, our results in this section demonstrate the robustness of our key findings on spillover effects to various assumptions in our base case parameter setting, in which we do not split the sample into boom and non-boom periods. We next proceed to demonstrate that our key results on spillover effects in boom and non-boom periods remain robust when using different criteria to determine these two subpriods.

5.2 Robustness of boom versus non-boom

In section 4.2, we defined boom and non-boom periods based on our Manhattan Condominium index that we constructed using the Case-Shiller methodology (Case and Shiller, 1989). Using this database, our non-boom period lasted from November 2005 to February 2013. We further documented that spillover effects in the first- and second-order neighborhood are stronger during non-boom periods and weaker during boom periods. In this section, we use alternative definitions for the boom and non-boom periods using different house price indices and a liquidity measure.

The left panel of Figure 3 depicts the evolution of real house price indices for Manhattan (dotted line), New York (dashed line), and the entire United States (solid line). Similar to our proceeds from section 4.2, we define the beginning of a non-boom period as the month in which a previously sharp incline in house prices ends. Likewise, the end of a non-boom period is the month in which a new sharp incline in house prices begins. That is, for the NYC Condominium Index, the non-boom period is March 2006 to April 2012 and for the US National House Price Index, this period is March 2006 to February 2012. The right panel in Figure 3 depicts the number of sold apartments and condominiums on Manhattan Island after removing observations with missing values in sales prices, sales dates, and duplicates.

Figure 3
Identification of non-booming periods



The left panel of this figure depicts the evolution of the S&P US National House Price Index (solid line), the S&P Case-Shiller Condominium Index for New York City (dashed line) and the Manhattan Condominium Index constructed using the methodology of Case and Shiller (1989). Index levels are normalized to 100 in January 2000. The right panel depicts the absolute number of sales of apartments and condominiums on Manhattan Island from the first quarter 2004 to the fourth quarter 2015 after removing observations with missing values in sales prices, sales dates, and duplicates.

From this panel, the number of sales declined from 4,381 trades 2,906 trades in October 2008 and did not recover systematically before March 2012. As an additional definition for our non-boom period, we therefore use the time period October 2008 to March 2012 as a liquidity-based definition of our non-boom period.

Table 5 summarizes in a similar fashion as Table 3 our results for the different definitions of the non-boom period. For ease of comparison with our results in Table 3, these are repeated in Panel A of Table 5. Consistent with our key findings from section 4.2, our robustness results with different definitions of the non-boom period confirm that during non-boom periods spillover effects are stronger from the first- and second-order neighborhood. Similarly, spillover effects are stronger from the third- and fourth-order neighborhoods during boom periods. Irrespective of the exact definition of our non-boom period, point estimates for our coefficients are very similar. In other words, our results in Table 5 confirm the finding from section 4.2 that the increased uncertainty during non-boom periods leads investors to rely more on the more homogeneous signals from the nearest neighborhoods – even in times of low liquidity.

Table 5
Robustness, boom versus non-boom

	Boom						Non-boom					
Neighborhood order	First	Second	Third	Fourth	Fifth	Sixth	First	Second	Third	Fourth	Fifth	Sixth
Panel A: Base case:	0.164***	0.025	0.044***	0.050***	0.002	-0.024	0.197***	0.033**	0.020	0.022	0.025*	-0.010
	(0.019)	(0.014)	(0.012)	(0.010)	(0.013)	(0.013)	(0.017)	(0.012)	(0.011)	(0.012)	(0.012)	(0.011)
Panel B: Varying non-boom periods												
March 2006 to April 2012	0.166***	0.027*	0.044***	0.041***	0.009	-0.016	0.201***	0.031*	0.014	0.030*	0.018	-0.019
	(0.017)	(0.013)	(0.011)	(0.010)	(0.011)	(0.012)	(0.019)	(0.014)	(0.012)	(0.013)	(0.014)	(0.011)
March 2006 to February 2012	0.166***	0.026*	0.044***	0.038***	0.013	-0.017	0.202***	0.033*	0.013	0.035**	0.011	-0.017
	(0.016)	(0.012)	(0.011)	(0.010)	(0.012)	(0.012)	(0.020)	(0.014)	(0.013)	(0.013)	(0.013)	(0.011)
October 2008 to March 2012	0.176***	0.024*	0.035***	0.044***	0.011	-0.020*	0.196***	0.049*	0.028	0.019	0.025	0.001
	(0.015)	(0.011)	(0.009)	(0.010)	(0.010)	(0.010)	(0.025)	(0.020)	(0.016)	(0.014)	(0.020)	(0.014)

This table documents the robustness of our key findings with respect to various ways to define boom and non-boom periods in Panel B. Panel A repeats our results for our base-case parameter setting in which boom and non-boom periods (November 2005 to February 2013) are defined using our Manhattan Condominium index that we constructed using the Case and Shiller (1989) methodology. Row "March 2006 to April 2012" depicts results for the case in which the non-boom period is set to its counterpart in the S&P/CS NYC Condominium index, i.e., the time period March 2006 to April 2012; the row "March 2006 to February 2012" for the case in which the non-boom period is set to its counterpart in the S&P/CS US National Home Price index, i.e., the time period from March 2006 to February 2012. The row "October 2008 to March 2012" reports results when defining the non-boom period using the liquidity-dry-up-period from Figure 3, i.e., October 2008 to March 2012. All regressions include the entire set of controls: locational, transaction-specific, and macro-financial. Fixed effects are on the zip-code-year level. Heteroskedasticity-robust standard errors are clustered over the zip-code-year level and reported in parentheses. ***, **, and * denote significance at the 0.1%, 1%, and 5% level, respectively.

6 Conclusion

The housing market boom and bust of the early 2000s highlights the importance for a better understanding of the evolution of residential house prices. We contribute to this challenging endeavor by exploring the micro-level evolution of residential house prices using data from trades on Manhattan Island between 2000 and 2015. Our work makes two main predictions.

First, sales prices of homes are significantly affected by sales prices in the nearest neighborhood — even after controlling for the evolution of house price movements on the zip-code level, monthly borough-wide house price movements, and various other controls. Second, these spillover effects are strongest in the nearest neighborhood and die out quickly as the distance between traded homes increases.

Liquidity and the volatility of house prices vary with the state of the housing market cycle, suggesting that the state of the market cycle should affect how spillover effects manifest in house price changes through two counteracting channels. On the one hand, the generally higher liquidity in booming housing markets suggests that the number of trades in the nearest neighborhood should be higher. From this liquidity channel, the number of substitutes in the nearest neighborhood is higher. It may thus be sufficient to consider these prices. Hence, spillover effects there should be more pronounced whereas spillover effects from more distant neighborhoods should be weaker. On the other hand, volatility in booming states of the housing market cycle is generally lower, suggesting a higher quality of the price signal from more distant trades. This volatility channel suggests that spillover effects from the nearest neighborhood should be weaker and that those from more distant trades should be stronger. Our empirical results show that the volatility channel outweighs the liquidity channel: in booming states of the housing market cycle, spillover effects from more distant neighborhoods are stronger and spillover effects are weaker from the nearest neighborhoods.

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A Clustering of zip-codes

The zip-codes have been clustered the following way:

- 10001 & 10011 (Chelsea and Clinton)
- 10002 & 10003 & 10009 (Lower East Side)
- 10004 & 10005 & 10006 & 10007 & 10038 & 10280 & 10282 (Lower Manhattan)
- 10012 & 10013 (Greenwich Village/Lower Manhattan)
- 10017 & 10163 (Gramercy Park and Murray Hill)
- 10018 & & 10019 & 10036 & 10129 (Chelsea and Clinton)
- 10023 & 10069 (Upper Westside)

- 10026 & 10027 & 10030 & 10037 & 10039 (Central Harlem)
- 10029 & 10035 & 10128 (East Harlem, 10128 is Upper East)
- 10031 & 10032 & 10033 & 10034 & 10040 (Inwood and Washington Heights)

B Key words identifying a seller as corporation

A seller is identified as a corporation if the name includes one of the following key words.

ACQUI, ASSOC, AVENUE, BANK, BOARD, CORP, CREDITOR, EQUIT, ESTATE, FUND, HDFC, HLDGS, HOLDING, HOUSING, HSNG, INC, INVEST, L*L*C, LLC, LP, LTD, OWNER, PARTNER, PLC, PORTFOLIO, PROP, QUATAR, REALTY, STREET, TRUST, *LP, where * signifies blank spaces. A manual comparison of more than 3,000 observations did not indicate any missing words.