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Essays on the Housing Market and Home Prices

by

Calvin Shuo Zhang

A dissertation submitted in partial satisfaction of the
requirements for the degree of
Doctor of Philosophy

in

Business Administration

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Professor Nancy Wallace, Chair
Professor Amir Kermani
Professor Christopher Palmer
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Professor Jesse Rothstein

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Abstract

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This dissertation consists of three chapters that concern the housing market and home prices. The first chapter analyzes why foreclosures were more prevalent than short sales despite the advantages that short sales offered. The Great Recession led to widespread mortgage defaults, with borrowers resorting to both foreclosures and short sales to resolve their defaults. I first quantify the economic impact of foreclosures relative to short sales by comparing the home price implications of both. After accounting for omitted variable bias, I find that homes selling as a short sale transact at 8.5% higher prices on average than those that sell after foreclosure. Short sales also exert smaller negative externalities than foreclosures, with one short sale decreasing nearby property values by one percentage point less than a foreclosure. So why weren't short sales more prevalent? These home-price benefits did not increase the prevalence of short sales because free rents during foreclosures caused more borrowers to select foreclosures, even though higher advances led servicers to prefer more short sales. In states with longer foreclosure timelines, the benefits from foreclosures increased for borrowers, so short sales were less utilized. I find that one standard deviation increase in the average length of the foreclosure process decreased the short sale share by 0.35-0.45 standard deviation. My results suggest that policies that increase the relative attractiveness of short sales could help stabilize distressed housing markets.

The second chapter analyzes how the housing market captures the efficiency of public goods. This chapter is co-authored with David Schönholzer. In the U.S., 36 million people live in unincorporated communities without separate municipal government, instead receiving limited local public goods by counties and special districts. This paper formalizes and empirically quantifies the extent of sorting induced by this arrangement of local governance. Based on predictions of a Tiebout model with heterogeneous income and preferences, we document the effect of municipal governance on housing supply, house prices, land prices, and public goods. We use a boundary discontinuity design and an event study design with administrative data from all boundary changes of 189 Californian cities, combined with the universe of individual property sales over the years 1988-2013. We find considerable sorting

induced by municipal boundaries and their changes: sales prices are around \$6,000 higher in municipalities and land values are 20% higher. Both housing supply and land values increase substantially after annexation. Changes in per capita expenditures and increases in the quality of police services provide suggestive evidence for public goods as the key mechanism for sorting.

The third chapter analyzes the effects of real estate investments by foreign Chinese on local economies in the United States. This chapter is co-authored with Zhimin Li and Leslie Sheng Shen. Starting in 2007, the U.S. witnessed an unprecedented surge in housing purchases by foreign Chinese. We exploit cross-local-area variation in the concentration of Chinese population stemming from pre-sample period differences in Chinese population settlement to identify the economic effects of these investments. Using detailed transaction-level housing purchase data, we find housing investment by foreigners induces higher local area housing net wealth, leading to higher local employment in the non-tradable sectors. Our results suggest the improvement in household balance sheet resulting from capital inflow for housing investment in the U.S. played a mitigating role for the domestic economy during the Great Recession. Based on our empirical findings, we develop a framework that incorporates the housing net worth channel for interpreting the empirical estimates. Our evidence highlights the role of capital inflow and foreign investments on the domestic output and employment, especially in times of economic downturns.

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Chapter 1

A Shortage of Short Sales: Explaining the Under-Utilization of a Foreclosure Alternative

1.1 Introduction

The recent housing market crash led to high foreclosure rates throughout the country. As borrowers became delinquent and home price declines led to negative equity, many borrowers lost their homes to foreclosure. Statistics from RealtyTrac indicate that between 2007-2011, there were over 4 million completed foreclosures. The flood of foreclosures also led to high rates of foreclosed homes being sold, with 29% of all homes sold in 2009 being foreclosure sales, and over 60% in the hardest hit states.^{1,2} Besides facing foreclosure, delinquent borrowers could also resolve their default via a short sale. Figure 1.1 plots data from DataQuick in 10 large MSAs across the country showing the total number of short sales and foreclosure sales per quarter. While foreclosures increased dramatically during the housing crash, short sales were also utilized, especially later on in the crisis. Despite the rise in both types of distress sales, the causes and economic impacts — both positive and negative — of short sales are less understood.³

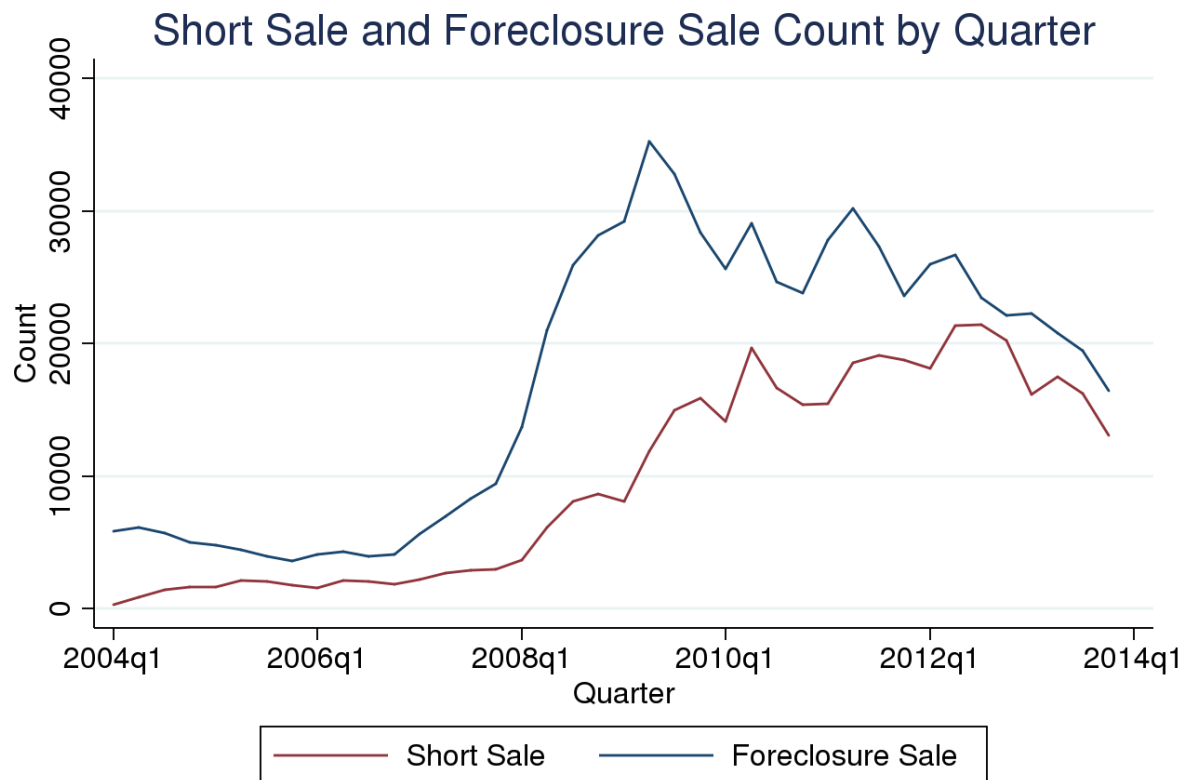
The economic importance of short sales is highlighted by multiple government programs, including the Home Affordable Foreclosure Alternatives (HAFA) program, that aimed to promote more short sales by offering financial incentives to the agents in charge of making

¹Foreclosure statistics come from <http://www.realtytrac.com/content/news-and-opinion/slideshow-2012-foreclosure-market-outlook-7021> and <http://www.realtytrac.com/news/realtytrac-reports/2010-year-end-and-q4-foreclosure-sales-report/>

²For the rest of this paper, I define a foreclosure sale as a sale of a home that had just been foreclosed on to a third party. The foreclosure sale could have taken place as a foreclosure auction or as a sale on a real estate owned (REO) property, which is a property owned by the lender.

³I use the term distress sale to refer to either a short sale or a foreclosure sale for the rest of this paper.

Figure 1.1: Foreclosure Sales and Short Sales Over Time



Data from DataQuick 10 MSA Sample

Notes: This figure shows the number of foreclosure sales and short sales in each quarter from 2004 quarter 1 to 2013 quarter 4 for the 10 MSAs in the DataQuick sample.

the short sale decision.⁴ The offering of incentives to encourage more short sales suggests that there might be efficiency gains from short sales over foreclosures. However, these efficiency gains have not been well quantified due to the non-random assignment of short sales. There is endogenous selection into short sales for delinquent borrowers based on unobservable characteristics such as home quality at the time of initial delinquency. In addition, when testing for factors that drive short sale behavior such as the foreclosure timeline, endogeneity is also a problem. Challenges arise due to reverse causality between the factors driving short sales and short sales themselves, and omitted variable bias resulting from unobservable conditions driving both short sales and these factors.

This is the first paper that combines multiple nationally-representative data sets with identification strategies to address these problems of endogeneity. I begin by using transacti-

⁴The money used to fund HAFA came from the Troubled Asset Relief Program (TARP). As of June 30, 2014, \$804 million of TARP money was spent on HAFA.

ons data from 10 large MSAs to examine how the transaction price differs when a home is sold as a short sale compared to being sold after a foreclosure. I find that although short sales were less common than foreclosures, they were actually more beneficial for home prices and the housing market. However, omitted variable bias could be present due to unobserved factors such as home quality at time of delinquency, which impacts both selection into short sale and transaction prices. Lower quality homes were more likely to be foreclosed on and to sell at lower prices.

I merge home transactions data with listings data to address the problem of omitted home quality in two ways. First, I distinguish if a foreclosed home was a result of a failed short sale if there was a listing on that home prior to the completion of the foreclosure. I assume that the listing of a home helps control for home quality since homeowners who list their homes with an intent to sell are more likely to maintain their home in order to maximize the likelihood of a successful sale and to obtain a higher selling price. By comparing only these pre-listed foreclosed homes with short sales, I am able to compare homes with similar quality. My results suggest that pre-listed foreclosed homes sell at 3% higher prices than non-pre-listed ones, but still sell at 9% lower prices than short sales.

Listing is not a perfect control for home quality, so I exploit plausibly exogenous variation in the time of loan origination and home listing for borrowers who sell distressed homes in the same census tract and time as an instrument for the success of a short sale. For each home, I calculate the percentage of loan balance outstanding at the time of listing by assuming constant amortization on a 30-year fixed rate mortgage, so older loans will have smaller balances. Mortgage lenders are then more likely to approve of a short sale for loans with a smaller outstanding balance because they face smaller losses. My results show that foreclosure sales still transact at 8.5% lower prices than short sales. One concern about the instrument is that borrowers who took out loans later in the housing boom might be lower quality and more likely to be foreclosed on and to neglect maintaining their homes. However, Palmer (2016) showed that home price changes explain more of the variation in default rates among different cohorts of borrowers than borrower quality due to looser lending conditions, which suggests that borrower quality may be exogenous to the success of a short sale. As an additional check, I focus only on loans originating after 2007 when lending conditions tightened up and find similar results.

Since short sales and foreclosures have different impacts on the sale price of a home, I would also expect them to have different externalities on the price of nearby homes. I employ the same spatial difference-in-difference method used by Campbell et al. (2011) and Anenberg and Kung (2014) in studying the foreclosure externality to show that homes near foreclosure sales sell at lower prices relative to homes near short sales, with home prices being up to one percentage point lower for each nearby foreclosure sale relative to a nearby short sale.⁵ Using listing data again to compare pre-listed foreclosures with short sales allows me

⁵While this spatial difference-in-difference specification has been used to study foreclosure externalities, it was based on the method used by Linden and Rockoff (2008) to show the impact of sex offenders on home prices.

to address omitted home quality and show that results are robust to differences in home quality.

If short sales were more beneficial for the recovery of the housing market, why weren't they more prevalent? I provide evidence that tension between the agents who make the short sale decision and those who enjoy the benefits of higher home prices is one factor that can explain this discrepancy. In particular, neither of the two agents directly involved in the short sale decision making — the delinquent borrower and the servicer of the loan — benefit from higher home prices.⁶ Instead, during the foreclosure process, borrowers can live for free in their homes and servicers can continue collecting servicing fees, but foreclosures can also delay the recovery of servicing advances — payments made to investors by the servicer to cover for missed payments by the borrower. Longer foreclosure timelines make foreclosures even more attractive to borrowers because they can enjoy more free housing, but the effect on servicers is not obvious since there is an increase in both the servicing fees and waiting time to recover advances.

To test for the impact of foreclosure timelines on short sale activity, I need to tackle endogeneity resulting from reverse causality between short sales and foreclosure timelines and omitted variable bias from unobserved local macroeconomic factors driving both short sale activity and foreclosure timelines. Therefore, I use a state's judicial foreclosure law as an instrument for foreclosure timeline similar to Mian et al. (2015). Pence (2006) first showed that state laws requiring judicial foreclosures increased the foreclosure timeline. The advantage of using these laws as an instrument is that their historical origins were not affected by different economic situations across states (Ghent (2013)). I find that a one standard deviation increase in the foreclosure timeline causes a 0.35-0.45 standard deviation decrease in a state's short sale share of distressed sales. These results are driven primarily by subprime borrowers.

Because borrowers and servicers respond differently to longer foreclosure timelines due to the differences in rents, servicing fees, and advances, it is important to see if one side contributed more to the decrease in short sales. To do so, I interact proxies for rent and advances with foreclosure timelines separately to test for the borrower and servicer channels. I find that both parties are responsive to foreclosure timelines, but in opposite directions. Higher rents decrease a borrower's preference for short sales while higher advances increase a servicer's preference.⁷

This paper has important implications for policies to help mitigate future negative home price shocks and stabilize the housing market. Based on my estimates of the difference in the discount and externalities between short sales and foreclosures, increasing short sales by

⁶I focus on the servicer of the mortgaged backed security (MBS) as the agent who must approve of short sales since the sample of mortgages I use to test for the short sale unpopularity consists of only private-label securitized loans. I go more into depth about the parties that approves short sales when discussing the institutional details.

⁷Because I do not have data on servicing fees, my results only show that higher advances cause longer foreclosure timelines to increase a servicer's preference for short sales, but the net impact of longer foreclosure timelines may actually decrease a servicer's preference for short sales if the fees they can collect are higher.

just 5% between 2007 and 2011 would have saved the housing market up to \$5.8 billion. While HAFA was a move in the right direction in encouraging short sales, my research suggests that reducing foreclosure timelines is another possible method to increase short sales. If policy makers can quantify the additional benefits that foreclosures offer borrowers over short sales, they can offer similar benefits to incentivize more short sales. Also, since a successful short sale requires servicer approval, additional incentives could be offered to financial institutions to encourage them to approve more short sales, including changes in accounting rules. Higher short sale rates can help protect against the price-default spiral modeled by Guren and McQuade (2015), which would help dampen initial housing market shocks in future recessions.

The rest of the paper proceeds as follows. The rest of this section reviews the related literature. Section 2 examines the institutional details of short sales and compares the trade-off between foreclosures and short sales for both borrowers and servicers. Section 3 details the different data sources I use and presents summary statistics. Section 4 highlights the benefits of short sales by showing how these homes sell at higher prices and have a smaller negative impact on the prices of nearby homes. Section 5 explains why short sales were less prevalent by empirically testing for the impact of foreclosure timelines on the probability of a short sale. Section 6 concludes the paper.

Related Literature

The research on short sales so far have been sparse compared to the work on foreclosures. Clauretie and Daneshvary (2011) and Daneshvary and Clauretie (2012) are the only two papers to study the differential home price impacts of short sales, while there is a plethora of work that focuses on foreclosures.⁸ They find that short sales lead to higher transaction prices and lower negative externalities, but they do not address the endogenous selection problem arising from omitted variables. Also, their results are restricted only to the city of Las Vegas. My paper improves upon their work because my higher quality data allows me to use identification strategies to deal with omitted home quality, and my results are nationally representative.

Meanwhile, research on the causes of short sales is even more scant. Zhu and Pace (2015) is the only paper to document the factors that influence the probability of a short sale but they cannot identify the channel driving this effect.⁹ Also, their data is restricted to only mortgages in cross-state MSAs, which is problematic and produces results that cannot be

⁸Studies have looked into how foreclosures cause a discount in the transaction price (Clauretie and Daneshvary (2009), Campbell et al. (2011) and Harding et al. (2012)) and how they exert negative externalities by decreasing nearby home prices (Harding et al. (2009), Campbell et al. (2011), Anenberg and Kung (2014), Fisher et al. (2015), Hartley (2014), Gerardi et al. (2015), Mian et al. (2015)) and by increasing crime (Ellen et al. (2013)). The externalities are smaller when a single lender holds a large share of the outstanding mortgages in a neighborhood (Favara and Giannetti (2017)).

⁹In comparison to the lack of work on short sales, the causes of high foreclosure rates have been well documented both theoretically (Campbell and Cocco (2015) and Corbae and Quintin (2015)) and empirically (Foote et al. (2008), Bajari et al. (2008), Ghent and Kudlyak (2011), and Palmer (2016)).

generalized.¹⁰ Again, I am able to improve upon the past research on short sales by using better data to show that the borrower channel is more responsible for the decrease in short sales than the servicer channel and to generate results at the national level.

This paper highlights another consequence of longer foreclosure timelines — fewer short sales. Research has already found that longer foreclosure timelines increase foreclosures (Zhu and Pace (2011) and Chatterjee and Eyigungor (2015)), although Mian et al. (2015) show that judicial states, where foreclosure timelines are longer, had lower foreclosure rates. As borrowers save more on rent when timelines are longer, they can afford to pay off more of their nonmortgage debts (Calem et al. (2014)), but they also can afford to spend additional time searching for high-paying jobs so employment decreases (Herkenhoff and Ohanian (2015)). Lastly, longer foreclosure timelines increase costs for lenders because they may have to cover missed property tax, hazard insurance, and homeowner association payments, and they recover less at liquidation due to excess depreciation on homes (Cordell et al. (2015) and Cordell and Lambie-Hanson (2016)).

1.2 Short Sale Details and Comparison with Foreclosure

Overview of a Short Sale

When homeowners became underwater on their mortgages and delinquent on their mortgage payments as a result of the housing crash and poor economic conditions, many turned to foreclosures. However, there exists an alternative to foreclosures for borrowers who are behind on their mortgage. Instead of letting the lender foreclose on their homes, borrowers also have the option to seek a short sale. In a short sale, the borrower sells his home for less than what he owes on his mortgage and the lender releases the lien on that property. To begin, the borrower first contacts the lender to initiate the short sale procedure.¹¹ The borrower then works with a real estate agent to list the short sale. After an offer is received, the borrower must submit a short sale package containing a hardship letter showing why the borrower is seeking a short sale, other personal financial documents, and a signed purchase contract with the offer price to the lender, who then ultimately needs to approve of the selling price in order for the sale to take place.

Beginning in 2009, in an effort to help promote short sales, the US Treasury introduced HAFA while the government sponsored enterprises (GSEs) issued their own version of HAFA.

¹⁰Usually, the main urban center is located entirely in one state, while the surrounding states only contain the peripheries of the city and the suburbs. For example, the majority of the Chicago MSA is located in Illinois, including the entire city of Chicago. The parts that extend into Indiana and Wisconsin are more rural and less densely populated. Also, cross-state MSAs exclude states with large real estate markets such as California and Florida.

¹¹Lender is just a generic term here for the agent approving the short sale decision. My focus in this paper will be on the servicer.

These programs offered incentives for both the borrower and the servicer to do increase sales. Borrowers could receive money for relocation assistance after a short sale, while servicers received financial compensation to approve a short sale. Borrowers were also freed from any form of recourse, regardless of the state foreclosure recourse laws.

Comparison from a Borrower's Perspective

Borrowers face a trade off between the long term benefits from a short sale and the short term benefits from a foreclosure. Contrary to popular belief, borrowers' credit scores fall by the same amount when doing a short sale or a foreclosure.¹² However, they are locked out of the mortgage market for less time, so they can buy a new home sooner. Borrowers are allowed to obtain a new mortgage only 2 years after a short sale, while they must wait 3-7 years after a foreclosure. Not having to face a deficiency judgment saves them money in the longer term as well.

On the other hand, the biggest benefit of doing a foreclosure over a short sale is that borrowers have the right to live for free in the home during the entire foreclosure process. They cannot be evicted until ownership of the home changes after the foreclosure process is completed. For many borrowers who are going through financial distress, this immediate benefit will outweigh the long term benefits from doing a short sale, particularly if it is hard for them to imagine buying a home again after having trouble making mortgage payments. As foreclosure timelines increase and it takes longer to finish the foreclosure process, this foreclosure benefit increases for the borrower.

Comparison from the Servicer's Perspective

The agent who makes the decision to approve a short sale varies depending on what happened to the loan after it was originated. Table 1.1 presents a comparison of the type of loans, who makes the short sale decision, and what factors influence their decision. Traditionally, the lending institution would keep the loan on their balance sheet so they are responsible for deciding whether to approve a short sale for these loans. However, during the housing boom, the majority of the loans made were securitized into MBS. For mortgages securitized by private-labels, the servicer of the loans is the deciding party. For loans that were securitized by the government sponsored agencies, the GSEs are the ones who ultimately decide whether to approve a short sale.

The primary objective of the originating lenders and GSEs is to maximize the recovery value of the delinquent mortgages because they take the losses on the mortgages. They need to decide what option allows them to receive the highest selling price on the home. As I will show, since short sales sell on average for more than foreclosures, these agents had an incentive to approve more short sales. They would only opt for a foreclosure if the losses

¹²A study done by FICO actually shows an equal decline in credit scores for short sales and foreclosures. See <http://www.fico.com/en/blogs/risk-compliance/research-looks-at-how-mortgage-delinquencies-affect-scores/>

Table 1.1: Foreclosure and Short Sale Differences

| Loan Type | Decision Maker | Goal |
|---------------------------|-----------------------|--|
| On balance sheet | Originating lender | Maximize recovery value of mortgage |
| GSE securitized | GSE | Maximize recovery value of mortgage |
| Private-label securitized | Servicer of loan | Maximize revenue from servicing fees while minimizing advances |

Notes: This table presents information on the different 3 type of loans, based on what happened to the loan after origination.

from a short sale were so large that they believe they would be more likely to get a higher selling price in the future when it came time to sell the foreclosed home.

Servicers of private-label securitized mortgages do not directly gain from higher selling prices — instead, they generate income by collecting servicing fees. As foreclosure timelines increase, servicers may be able to collect more fees. At the same time, servicers have to make advances to cover the payments missed by the borrowers so the investors are paid still. While they recoup these advances when the home is liquidated, the advances still are costly if the servicer has to finance them by borrowing. Thus, servicers have to balance between maximizing their fees and minimizing their advances, especially when timelines are longer, since both increase. For this study, I focus my analysis on private-label servicers because the sample of loans used to study the impact of foreclosure timelines on short sales is all private-label securitized mortgages.

When there are multiple loans associated with one home, the servicer for each loan must approve of the short sale in order for it to go through. In these situations, the servicer on the second lien loan may be more reluctant to approve, as they cannot recover their advances until the first lien is completely paid due to their junior position. Given how much prices fell, there was the risk that the selling price was not high enough to compensate these servicers. In order to entice servicers of second liens to approve a short sale, all parties involved in the short sale need to negotiate a deal so that the servicers on the second liens can recover some money even if the proceeds from the short sale is not enough. HAFAs and their GSE counterpart programs also provided financial compensation to servicers on junior liens to encourage them to approve more short sales.¹³

¹³While I do not directly analyze the role that second liens play, I do find that foreclosure sales and short sales have similar shares of loans with second liens — 57% compared to 64%.

1.3 Data

Home Transaction Data

The data used to test the effects of short sales and foreclosure on home prices comes from DataQuick, which has transaction level data on every home sold. The data has flags for whether a transaction is a short sale or a foreclosure sale. Foreclosure sales may either be the sale of the home to a third party at a foreclosure auction or the sale of the home to a third party after it has become REO. However, DataQuick does not use the transaction records to determine when a short sale took place. Instead, they use a proprietary model to identify short sales. Using an approach of their own where they indicate a home as being a short sale if the sale price is less than 90% of the outstanding loan balance, Ferreira and Gyourko (2015) were able to match DataQuick's indicator 90% of the time. Thus, the DataQuick short sale flag appears to be reliable. Unfortunately, DataQuick only began reporting short sales beginning in 2004, so I use data from 2004 to 2013, which is when the data ends.

Another shortcoming of DataQuick is that I am unable to observe when a home started the foreclosure process, but I can see when it became REO and when the REO was liquidated, which I label as the foreclosure sale in this paper. Since I will be analyzing the effects of short sales and foreclosure sales on home prices, I only need to observe when the homes are sold. Because of the vast amount of data, I limit myself to a nationally-representative sample of transactions from 10 large MSAs across the country.¹⁴

Counts and summary statistics for the transactions of single family residential homes are presented in table 1.2.¹⁵ Panel A shows the number of short sales, foreclosure sales, and all sales in each MSA. While different MSAs had different ratios of short sales to foreclosure sales, all MSAs did have more foreclosure sales than short sales. Panel B shows that on average, there was approximately one short sale for every two foreclosures. Panel B also compares property level characteristics data for the two types of sales. Short sale homes were statistically different from foreclosure homes in that they sold for higher prices and were bigger and newer.

Merged Listing and Transaction Data

Listing data comes from Multiple Listing Services (MLS) provided by Altos Research. Every week, Altos Research takes a snapshot of the homes listed for sale on MLS and records the information. They provide listing data for the same 10 MSAs in my transaction data, but the listing data does not begin until October 2007. From these weekly snapshots, I can identify when the home owner is attempting to sell the home. For homes that went into foreclosure, it is possible to see if the borrower attempted to sell the home first by checking if a listing existed prior to the home becoming REO or selling as a foreclosure auction, which will be

¹⁴See the data appendix for the entire data cleaning procedure.

¹⁵Single family residential homes do include duplexes, triplexes, and quadplexes. I run robustness checks using transactions from all home types in the appendix. The mean effects are similar.

Table 1.2: DataQuick Summary Statistics

| Panel A | Sale Counts by MSA | | |
|--------------|--------------------|-------------|---------|
| | Foreclosures | Short Sales | All |
| Atlanta | 92,137 | 21,503 | 454,642 |
| Boston | 20,657 | 18,451 | 336,774 |
| Chicago | 68,974 | 45,370 | 675,392 |
| DC | 40,436 | 30,693 | 452,009 |
| Detroit | 100,909 | 24,906 | 385,072 |
| Los Angeles | 101,451 | 78,104 | 788,979 |
| Miami | 61,069 | 51,704 | 507,505 |
| Philadelphia | 26,835 | 19,765 | 516,584 |
| Phoenix | 141,383 | 70,709 | 784,283 |
| Seattle | 35,537 | 27,529 | 411,837 |

| Panel B | Transaction Level Variables | | |
|----------------|-----------------------------|--------------------------|---------------|
| | Foreclosures | Short Sale | Difference |
| Count | 689,388 | 388,734 | -300,654 |
| Sale Price | \$175,074 (\$150,565) | \$265,159 (\$201,423) | -\$150,565*** |
| Square Footage | 1,757 (782) | 1,920 (856) | -163*** |
| Age | 38.5 (28.2) | 37.5 (28.2) | 1*** |

Significantly different from 0 at * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents summary statistics on the DataQuick transaction data. Panel A contains counts of short sales, foreclosure sales, and all sales by MSA. Panel B presents means and standard deviations (in parenthesis) on different home characteristics and a difference of means test for foreclosure vs short sale homes.

the basis of the instrument I use to address omitted variable bias. I define a foreclosure home as "pre-listed" if there was a listing up to two years before the foreclosure auction or REO date.

The listing data has the full address of each home, which allows me to merge it with the transactions data. I do the merge for single family homes only because the apartment or unit numbers for multi-family buildings and condos are not consistently defined. The detailed merging procedures are documented in the data appendix. Because the listing data does not begin until October 2007, the merged listing and transaction data I have will be smaller in size. Also, listing a home on MLS is not the only way for homeowners to sell their home, so a listing cannot be found for all transactions.

Table 1.3 presents counts and summary statistics for the merged data set. Panel A shows that pre-listing varied across MSAs while Panel B shows that on average, approximately 20% of all foreclosure sales had previously been listed before the foreclosure was completed. Property characteristics-wise, there is a statistically significant difference between foreclosed homes that were pre-listed and those that were not. Homes that were pre-listed were bigger and sold for higher prices after they were foreclosed on. The fact that these two types of homes have observable differences may imply that they have different impacts on home prices.

Loan Performance, Borrower, and Geography Level Data

The loan level data that I use to test whether a delinquent mortgage ends in a foreclosure or short sale comes from ABSNet. It contains loan and borrower characteristics at origination and monthly performance data on private-label securitized mortgages. For each loan, I can observe the monthly status — whether it is current, delinquent, or in distress. There are also dates for when a loan entered foreclosure, became an REO, or was liquidated. The data has a flag for short sales, and I use the foreclosure start date, REO date, and liquidation date to generate a flag for foreclosures.

I define the foreclosure timeline as the length of time between when a foreclosure starts and when the home becomes REO or is sold at a foreclosure auction. Since the housing market crash began in 2007, I calculate the foreclosure timeline in 2007 by using only loans that began the foreclosure process in 2007. I first calculate the foreclosure timeline for each individual loan in ABSNet and then average across all loans in each state to obtain a state level measure.¹⁶ As a comparison, I also use 2007 foreclosure timelines calculated by RealtyTrac.¹⁷ However, the RealtyTrac data has less coverage, with only 36 states covered in 2007. Table 1.4 presents the average foreclosure timeline for each state using both measures

¹⁶There is too much idiosyncratic noise at the individual loan level so a state level average will be a more reliable measure. Also, I calculate foreclosure timelines at the state level because judicial foreclosure laws are the same within a state and these laws shape foreclosure timelines.

¹⁷RealtyTrac foreclosure timeline data comes from <http://www.baltimoresun.com/news/data/bal-average-length-of-foreclosure-by-state-by-number-of-days-20140924-htmlstory.html>.

Table 1.3: Merged MLS-DataQuick Summary Statistics

| Panel A | Sale Counts by MSA | | | |
|--------------|-----------------------------|-------------------------|-------------|---------|
| | Non Pre-Listed Foreclosures | Pre-Listed Foreclosures | Short Sales | All |
| Atlanta | 58,798 | 6,921 | 15,163 | 202,497 |
| Boston | 7,198 | 1,463 | 7,348 | 87,562 |
| Chicago | 34,471 | 10,611 | 31,937 | 222,949 |
| DC | 24,340 | 10,092 | 26,516 | 192,186 |
| Detroit | 59,153 | 8,413 | 17,018 | 170,663 |
| Los Angeles | 67,296 | 19,197 | 65,086 | 368,529 |
| Miami | 37,102 | 13,174 | 39,389 | 192,077 |
| Philadelphia | 7,239 | 2,466 | 8,013 | 119,246 |
| Phoenix | 100,703 | 23,635 | 58,655 | 339,711 |
| Seattle | 22,532 | 7,059 | 21,590 | 170,917 |

| Panel B | Foreclosure Property Level Variables | | |
|----------------|--------------------------------------|--------------------------|--------------|
| | Not Pre-Listed | Pre-Listed | Difference |
| Count | 418,832 | 103,031 | 315,801 |
| Sale Price | \$169,972 (\$145,106) | \$203,411 (\$164,441) | -\$33,439*** |
| Square Footage | 1,751 (761) | 1,833 (838) | -82*** |
| Age | 36.6 (26.7) | 36.5 (26.8) | 0.1 |
| Bedrooms | 3.37 (0.82) | 3.45 (0.87) | -0.08*** |
| Bathrooms | 2.15 (0.85) | 2.26 (0.90) | -0.11*** |

Significantly different from 0 at * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents summary statistics on the DataQuick transaction data. Panel A contains counts of short sales, foreclosure sales, and all sales by MSA. Panel B presents means and standard deviations (in parenthesis) on different home characteristics and a difference of means test for non-pre-listed foreclosure vs pre-listed foreclosure homes. Square footage and age comes from transaction data while bedrooms and bathrooms data comes from listings data.

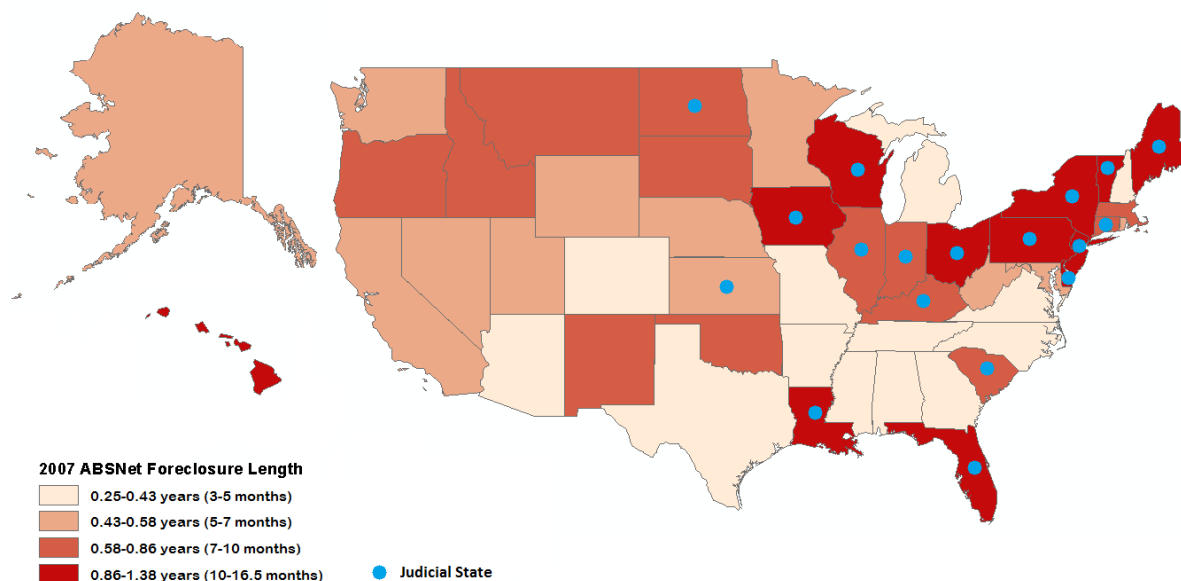
Table 1.4: State Foreclosure Timelines and Judicial Foreclosure Classification

| State | ABSNet Foreclosure Length | RealtyTrac Foreclosure Length | Judicial Foreclosure |
|-------|---------------------------|-------------------------------|----------------------|
| AK | 0.57 | | NJ |
| AL | 0.35 | 0.26 | NJ |
| AR | 0.40 | 0.30 | NJ |
| AZ | 0.41 | 0.35 | NJ |
| CA | 0.45 | 0.50 | NJ |
| CO | 0.39 | 0.48 | NJ |
| CT | 0.79 | 0.57 | J |
| DC | 0.49 | | NJ |
| DE | 1.08 | | J |
| FL | 1.12 | 0.61 | J |
| GA | 0.33 | 0.30 | NJ |
| HI | 1.02 | | NJ |
| IA | 0.91 | 0.46 | J |
| ID | 0.59 | | NJ |
| IL | 0.86 | 0.87 | J |
| IN | 0.77 | 0.82 | J |
| KS | 0.51 | 0.42 | J |
| KY | 0.84 | 0.60 | J |
| LA | 0.87 | 0.35 | J |
| MA | 0.59 | 0.70 | NJ |
| MD | 0.51 | 0.46 | NJ |
| ME | 1.16 | | J |
| MI | 0.33 | 0.19 | NJ |
| MN | 0.44 | 0.56 | NJ |
| MO | 0.25 | 0.16 | NJ |
| MS | 0.43 | | NJ |
| MT | 0.74 | | NJ |
| NC | 0.40 | 0.50 | NJ |
| ND | 0.84 | | J |
| NE | 0.49 | | NJ |
| NH | 0.42 | 0.30 | NJ |
| NJ | 1.29 | 0.93 | J |
| NM | 0.75 | 0.69 | NJ |
| NV | 0.49 | 0.46 | NJ |
| NY | 1.38 | 0.99 | J |
| OH | 0.89 | 0.65 | J |
| OK | 0.71 | 0.81 | NJ |
| OR | 0.59 | 0.49 | NJ |
| PA | 0.91 | 0.95 | J |
| RI | 0.47 | 0.33 | NJ |
| SC | 0.66 | | J |
| SD | 0.70 | | NJ |
| TN | 0.29 | 0.24 | NJ |
| TX | 0.37 | 0.17 | NJ |
| UT | 0.58 | 0.59 | NJ |
| VA | 0.31 | 0.25 | NJ |
| VT | 1.34 | | J |
| WA | 0.57 | 0.39 | NJ |
| WI | 0.92 | 0.94 | J |
| WV | 0.51 | | NJ |
| WY | 0.52 | | NJ |

Notes: This table presents both the 2007 ABSNet and RealtyTrac foreclosure timeline measures for each state and the state's judicial foreclosure classification. The judicial foreclosure classification comes from Gerardi et al. (2013)

and an indicator for whether the state requires judicial foreclosures.¹⁸ Figure 1.2 presents the same data in a map for easier visualization. It is clear to see that judicial states had longer timelines, with some judicial states having a timeline over 1 year, and that the majority of judicial states are in the Northeast and Midwest.

Figure 1.2: Foreclosure Timelines and Judicial Foreclosures Map



Notes: This figure shows a map of the US with each state’s foreclosure timeline grouped into one of four quartiles, with a circle marker for if the state allows judicial foreclosures.

Lastly, I supplement the individual loan level data with zip code data on home prices, rents, unemployment rates, and income. I get my home price index and housing market turnover rates from Zillow. For rents, I use the 2000 Census zip code level rent-to-income ratio. I get employment data from the Bureau of Labor Statistics Local Area Unemployment Statistics and income comes from the IRS.

Table 1.5 presents summary statistics for the ABSNet and supplemental data. Panel A presents loan level counts and variable means. There is a smaller share of short sales to foreclosures compared to the DataQuick transaction data. This difference may be due to the fact that ABSNet only has private-label securitized loans, which could have been more restrictive of short sales, while DataQuick contains transactions for all loan types. Loan characteristics are significantly different between these types of transacted homes. Panel B presents summary statistics on both state level and zip code level variables. The mean 2007 ABSNet foreclosure timeline measure is 0.58 years (7 months) with a 0.29 year standard

¹⁸State judicial foreclosure law classification comes from Gerardi et al. (2013).

deviation, while the both the mean and the standard deviation for the 2009 measure is longer at 0.71 years (9 months) and 0.37 years, respectively.

Table 1.5: ABSNet Summary Statistics

| Panel A | Loan Level Variables | | |
|--|----------------------|-------------|-------------|
| | Foreclosure | Short Sale | Difference |
| Count | 865,222 | 90,331 | 774,891 |
| Original Interest Rate | 6.99% | 7.53% | -0.54%*** |
| | (2.36%) | (2.62%) | |
| LTV at Origination | 81.0% | 81.8% | 0.8%*** |
| | (9.0%) | (13.6%) | |
| Original Loan Balance | \$266,057 | \$235,753 | \$30,304*** |
| | (\$180,380) | (\$199,351) | |
| FICO Score | 662 | 664 | -2*** |
| | (63) | (67) | |
| Owner Occupied | 78.9% | 79.8% | -0.9%*** |
| | (40.8%) | (40.2%) | |
| ARM | 73.0% | 59.1% | 13.9%*** |
| | (44.4%) | (49.2%) | |
| Home Price Change (Origination to Delinquency) | -20.3% | -25.6% | 5.3%*** |
| | (18.8%) | (19.0%) | |

| Panel B | Geographical Level Variables | | | | | |
|---|------------------------------|-------|-------|------------------|------------------|------------------|
| | N | Mean | SD | 10 th | 50 th | 90 th |
| 2007 ABSNet Foreclosure Timeline in Years (State Level) | 51 | 0.66 | 0.29 | 0.35 | 0.58 | 1.08 |
| 2009 ABSNet Foreclosure Timeline in Years (State Level) | 51 | 0.86 | 0.37 | 0.44 | 0.71 | 1.41 |
| 2007 RealtyTrac Foreclosure Timeline in Years (State Level) | 36 | 0.52 | 0.24 | 0.24 | 0.49 | 0.93 |
| Short Sale Share of All Distressed Sale (State Level) | 51 | 0.086 | 0.035 | 0.055 | 0.077 | 0.121 |
| Log Employment (Zip Code Level) | 21,163 | 7.32 | 1.87 | 4.73 | 7.49 | 9.64 |
| Log Income (Zip Code Level) | 21,163 | 22.02 | 2.36 | 17.93 | 22.61 | 24.45 |
| 2000 Rent to Income Ratio (Zip Code Level) | 21,163 | 0.013 | 0.003 | 0.010 | 0.013 | 0.017 |
| Housing Market Turnover (Zip Code Level) | 13,096 | 4.27% | 1.99% | 2.24% | 3.97% | 6.47% |

Significantly different from 0 at * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents summary statistics on the ABSNet loan performance data. Panel A presents means and standard deviations (in parenthesis) on different loan level variables. Panel B presents more detailed statistics on geographical level, both state and zip code level, variables. 10th, 50th, and 90th represent the corresponding percentile.

1.4 Benefits of Short Sales Over Foreclosures

Benefit for Home Prices

Empirical Setup

Since foreclosures and short sales are two different ways to deal with the same problem of delinquency, it is important to understand how they may impact the selling price of a home differently. As shown by previous research, selling a home that has been foreclosed on leads to a discount on the transaction price (Campbell et al. (2011) and Clauretie and Daneshvary (2009)). One reason may be due to the fact that foreclosed homes tend to be in worse condition, especially since the previous owners have no incentive to maintain them if they know that they will lose their homes and lenders lack the ability to properly maintain them. A desire by banks to sell the home faster in a fire sale may also play a role in lowering the selling price. However, Harding et al. (2009) find this discount to not be the result of fire sales.

Because short sales transact differently from foreclosure sales, they should have a different discount. Homeowners who wish to do a short sale must have the lender approve of their selling price, so they have an incentive to properly maintain their homes in order to achieve a high enough selling price that will be approved.¹⁹ A lack of maintenance may lower the price too much to be accepted for a short sale by the lender. However, a price discount may still exist for short sales because of the urgency to sell. Short sales also take less time to sell than a foreclosure and are lower risk for the potential buyer, since the seller will be more knowledgeable about the home so the buyer can be more informed about what he is buying.

To test for the foreclosure discount versus the short sale discount, I run a hedonic home price regression with indicator variables for foreclosure sales or short sales. The equation I estimate for measuring the foreclosure and short sale discount is:

$$\ln P_{ict} = \alpha_{ct} + \beta X_i + \lambda_f * foreclosure_{it} + \lambda_s * shortsale_{it} + \epsilon_{ict} \quad (1.1)$$

where $\ln P_{ict}$ is the log selling price of home i in census tract c and half year t ; X_i include a set of house characteristics; $foreclosure_{it}$ and $shortsale_{it}$ are dummies indicating if home i sold as a foreclosure or a short sale at time t ; α_{ct} are census tract by half year fixed effects; and ϵ_{ict} are the error terms.²⁰ I also include month dummies to control for seasonality effects in the housing market.

A naive OLS estimate of equation 1.1 will produced biased results due to omitted variable bias. I can only include controls for observable home characteristics, and any unobserved

¹⁹The DataQuick sample is not restricted to only private-label securitized loans. Thus, the agent approving of short sales is not restricted to just the loan servicer, so I use the term lender to refer to any agent that makes the short sale approval decision. As a result, the recovery value on the mortgage can influence the success of a short sale as detailed in table 1.1.

²⁰I use half-year time intervals because later on, I will be measuring nearby transaction counts in six month windows.

characteristics influencing both home prices and foreclosures or short sales will bias my estimate. Most notably, home quality is a factor that I cannot observe and is correlated both with selection into short sale and the transaction price. Lambie-Hanson (2015) showed that although home conditions deteriorate the most after a foreclosure when a home is bank owned, borrowers do begin to neglect maintaining their homes when they first become delinquent. Variation in home quality at first delinquency causes bias by affecting both the likelihood of a short sale and the transaction price. However, variation in home quality after foreclosure due to bank negligence is exactly the variation I want to capture in the difference between the foreclosure and short sale discount.

Addressing Omitted Home Quality with the Intent to Sell

One way to try to control for initial differences in home quality is to condition on the intent to sell by using home listings.²¹ Homeowners who list their homes for sale have incentives to keep it well maintained in order to achieve the highest possible price. A higher selling price will increase the likelihood that a short sale is approved so delinquent borrowers who intend to do a short sale will have homes in better condition compared to delinquent borrowers who don't attempt a short sale before foreclosure. Merging the listing data with the transaction data allows me to observe when a home was listed prior to a transaction. This merged data set includes all homes that ever had a listing so I can observe listings for homes that were foreclosed on and never sold.

For a home that went through the foreclosure process and later transacted either in the foreclosure auction or as an REO property, I classify it as pre-listed if I observe a listing any time in the two years prior to completion of the foreclosure.²² I do not need to observe if a short sale had a listing because every short sale must be listed in order to sell. I can then compute the foreclosure discount separately for non-pre-listed and pre-listed foreclosures and compare it to the short sale discount.

Table 1.6 shows the results of splitting foreclosures into pre-listed and non-pre-listed. First, I estimate equation 1.1 without separating the two different types of foreclosures using both the entire transactions only sample and the smaller merged transaction-listing sample to see if using just the smaller merged sample generates any bias. Column (1) reports the estimate from the larger transactions-only sample while column (2) uses the smaller merged sample. The estimates are the same for both, suggesting that foreclosures sell at 11% lower prices than short sales, so there are no sample bias concerns when using the merged data set.

I then estimate the discount difference between pre-listed foreclosures and non-pre-listed foreclosures in two different ways. In column (3), I first estimate equation 1.1 after excluding

²¹I define initial home quality as quality at first delinquency.

²²Since foreclosure timelines can be well over a year in some states, the homeowner may well have already been delinquent on his mortgage and looking to do a short sale up to 2 years prior to the completion of the foreclosure. I also estimated everything using a 1.5 year window to classify pre-listed foreclosures instead and get similar results everywhere.

Table 1.6: Pre-Listed Foreclosure Discounts

| | (1) | (2) | (3) | (4) |
|--------------------------|----------------------|----------------------|----------------------|----------------------|
| Foreclosure | -0.258*** (0.001) | -0.263*** (0.001) | -0.235*** (0.001) | -0.269*** (0.001) |
| Short Sale | -0.146*** (0.001) | -0.147*** (0.001) | -0.141*** (0.001) | -0.147*** (0.001) |
| Pre-Listed Foreclosure | | | | 0.029*** (0.001) |
| Tract by Half Year FE | X | X | X | X |
| Month FE | X | X | X | X |
| Property Characteristics | X | X | X | X |
| Foreclosure Sample | All | All | Pre-Listed Only | All |
| N | 4,996,050 | 1,958,106 | 1,554,552 | 1,958,106 |
| R ² | 0.87 | 0.89 | 0.89 | 0.89 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the estimates and standard errors (in parenthesis) from a regression of log sale price on a foreclosure sale indicator and a short sale indicator to tests for the difference in the foreclosure sale discount after controlling for pre-listing. Column (1) first presents the estimate without controlling for pre-listing using the entire transaction data set while column (2) uses only the merged transaction and listing sample. Column (3) then restricts foreclosure sales to only the pre-listed ones while column (4) uses all foreclosure sales but adds an additional indicator variable for pre-listed foreclosure sales. All regressions include tract by half year and month fixed effects and property characteristics. Property characteristics include square footage and age and their squared terms in column (1). Bathrooms and bedrooms are added from the listing data in columns (2) - (4). Standard errors are clustered at the census tract by half year level.

all non-pre-listed foreclosures. The results show that pre-listed foreclosures sell at slightly lower discounts compared to all foreclosures — a 23.5% discount versus a 26.3% discount. I then use the entire merged sample again, but include an additional indicator variable for if a home sold as a pre-listed foreclosure. The estimates reported in column (4) again show that pre-listed foreclosures have a 3% smaller discount. However, in comparison with the short sale discount, the foreclosure discount is still over 9% higher even just for pre-listed foreclosures, which suggests that initial home quality alone cannot explain the difference in the discounts.

Addressing Omitted Home Quality with Instrumental Variables

An additional way to address for omitted home quality is to instrument for the probability of a successful short sale. When estimating equation 1.1, I estimate how much selling a home as a foreclosure or a short sale lowers the transaction price relative to selling the home as a normal sale. To be able to instrument for the success of a short sale, I now modify my empirical setup by focusing only on the sample of pre-listed foreclosures and short sales, and estimate the discount of a foreclosure sale relative to a short sale, which I call the relative foreclosure discount. In estimating this equation, I will only have one indicator variable — for a foreclosure sale — which I can instrument for.

The instrument I use is the imputed percentage of the mortgage outstanding at the time of listing — defined as the outstanding loan balance divided by the original loan amount.²³ This percentage is imputed because I do not observe the actual balance at listing. The calculation of this percentage is based on the future value formula for a 30-year fixed rate mortgage with monthly payments. For each home i with a mortgage interest rate r_{t_1} originating at time t_1 and listed at time t_2 , I calculate the imputed percentage outstanding as:

$$outstanding\%_{i,t_1,t_2} = \frac{(1 + r_{t_1})^{360} - (1 + r_{t_1})^{(t_2-t_1)}}{(1 + r_{t_1})^{360} - 1} \quad (1.2)$$

In the transaction data, I can find the origination date t_1 from the previous first lien mortgage taken out on a home that ended in either foreclosure or short sale.²⁴ I am able to use the entire DataQuick transaction history dating to back 1988 to look up the loan record because I no longer need short sale flags. I obtain weekly mortgage rates from the Freddie Mac Primary Mortgage Market Survey. I also discard homes that had a loan originated less than six months before listing, since it's not plausible that a borrower becomes delinquent right after obtaining a new loan, and loans originating before 2002, since older loans had more equity and were less likely to default.

²³A similar instrument has used by others. Bernstein (2016) uses the percentage of mortgage paid instead of outstanding to instrument for the probability of negative home equity. Guren (2016) uses the log of the ratio of home price, instead of loan value, at listing and the previous transaction to instrument for the seller's listing price markup.

²⁴The previous mortgage could either be a purchase loan or a refinance. In the case of a refinanced loan, I need to distinguish it from an equity extraction or secondary mortgage. I classify a loan as a refinance if it is at least 2/3 the value of the original first lien mortgage.

In order for the percentage of the mortgage outstanding to be a good instrument, it must have a strong first stage and satisfy the exclusion restriction. I claim that the percentage of the loan outstanding significantly impacts the probability of a listed home failing the short sale and becoming a foreclosure because banks may be more weary of accepting a short sale if the losses are higher. By including home characteristics and having census-tract by half year fixed effects in my regression, I can control for the market value of the home so the losses on the mortgage will only be driven by the unpaid balance. Column (1) of table 1.7 reports the first stage results. I find that loans with higher balances are more likely to end in a foreclosure with strong statistical significance, which provides evidence of a strong instrument.

Table 1.7: IV Estimate of the Difference Between Discounts

| | (1) | (2) | (3) | (4) |
|-----------------------------|---------------------|----------------------|-----------------------|--------------------|
| | <i>Foreclosure</i> | | <i>Log Sale Price</i> | |
| Percent Balance Outstanding | 0.035*** (0.000) | | | |
| Foreclosure | | -0.098*** (0.001) | -0.085*** (0.008) | -0.105* (0.061) |
| Tract by Half Year FE | X | X | X | X |
| Month FE | X | X | X | X |
| Property Characteristics | X | X | X | X |
| Loan Origination Years | Post-2002 | Post-2002 | Post-2002 | Post-2007 |
| Regression Type | OLS | OLS | IV | IV |
| N | 274,063 | 274,063 | 274,063 | 21,587 |
| R ² | 0.31 | 0.91 | | |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents results from the IV regression testing for the foreclosure discount relative to the short sale discount. Column (1) reports estimates from the 1st stage OLS regression of a foreclosure sale indicator on the percentage of loan balance outstanding at listing. Column (2) reports the estimates of a an OLS regression of log sale price on a foreclosure sale indicator variable using the IV sample. Columns (3) and (4) report the estimates from an IV regression of log sale price on a foreclosure sale indicator variable where the instrument is the percentage of loan balance outstanding at listing. All regressions include tract by half year and month fixed effects and property characteristics. Property characteristics include square footage and age and their squared terms, bathrooms, and bedrooms. Standard errors are clustered at the census tract level by half year level.

The exclusion restriction is satisfied if the instrument does not impact home prices except

through the probability of a short sale. Since I'm assuming the same interest rate for every origination week and constant payments from origination to listing, variation in the percentage of the mortgage outstanding only comes from the time when the loan was made and the length of time between origination and listing, which can be thought of the age of the loan at listing. One may argue that the exclusion restriction does not hold because borrowers who obtained a loan later on during the housing boom may be lower quality borrowers because of looser credit standards. These lower quality borrowers may have defaulted more and may also have been more careless about maintaining their homes. However, Palmer (2016) showed that home price declines and not different borrower characteristics related to credit expansion can explain the majority of the difference in default rates among cohorts. Since differences in borrower characteristics were not primarily responsible for the higher default rates, I also assume that it was less likely that they were linked to lower quality homes.

To further address the problem of borrower quality varying over time due to looser credit standards, I can focus my analysis only on mortgages that originated after 2007. When the housing market collapsed and banks suffered big losses, mortgage lending tightened up. It became much more difficult for low quality borrowers such as those with insufficient income to obtain mortgages. Thus, it is less likely for origination year to influence home prices through borrower quality.

Columns (2) and (3) present the results of estimating the relative foreclosure discount using IV. Column (2) first reports the OLS estimate of the relative foreclosure discount using the new sample. I obtain an estimate of a 9.8%, which is consistent with the difference in previous estimates of the foreclosure and short sale discount for pre-listed foreclosures from table 1.6. When I implement the IV regression in column (3), I find a smaller but still statistically significant relative foreclosure discount of 8.5%. Column (4) reports the estimate using the restrict sample of loans that were originated in 2008 or later. I still find evidence that foreclosures sell for lower prices than short sales. Thus, the use of an IV provides further evidence that omitted variable bias is not causing the difference in the transaction discounts between homes selling after foreclosures and homes selling via short sales.

Benefits for Local Housing Market

While short sales and foreclosure sales deflate the selling price of the home itself compared to a non-distress sale, their negative price impacts may also extend to surrounding homes. And just as they have different discounts, they should have different externalities. There has been overwhelming evidence of negative price externalities associated with foreclosures, but less is known about the externalities from short sales.

To test how short sales affect the selling price of neighboring homes, I run a similar difference-in-difference regression as employed by Campbell et al. (2011) and Anenberg and Kung (2014). I use counts of the number of foreclosure sales and short sales that occurred around each home to estimate the externalities. I obtain counts at both a close distance (0.10 miles) and a far distance (0.25 miles) in each six month period within a three year

window around the transaction date for each home — both one and a half years before and after. Counts at the far distance serve as a control for preexisting local neighborhood level economic shocks that may be affecting both prices and the number of distress sales, because these shocks should not have differential effects for the close distance versus the far distance. After estimating the coefficient for the close counts for each of these six periods, I then normalize the coefficient in the earliest period to 0 and index all subsequent coefficients to it.²⁵ The indexed coefficients on the close counts represents the externality effect.

Like previous work, I find that foreclosure sale and short sale counts are extremely right skewed. To adjust for the skewness, I employ the same method as Anenberg and Kung (2014) and take the log of 1 plus the counts. Then I run the following regression with lags and leads up to one and half years around each sale:

$$\begin{aligned} \ln P_{igt} = & \alpha_{gt} + \beta X_i + \lambda Y_{it} + \sum_{k \in \{-1.5, 1.5\}} (\gamma_{f,t-k}^c \text{foreclosurecount}_{i,t-k}^c + \\ & \gamma_{f,t-k}^f \text{foreclosurecount}_{i,t-k}^f + \gamma_{s,t-k}^c \text{shortsalecount}_{i,t-k}^c + \\ & \gamma_{s,t-k}^f \text{shortsalecount}_{i,t-k}^f) + \epsilon_{igt} \end{aligned} \quad (1.3)$$

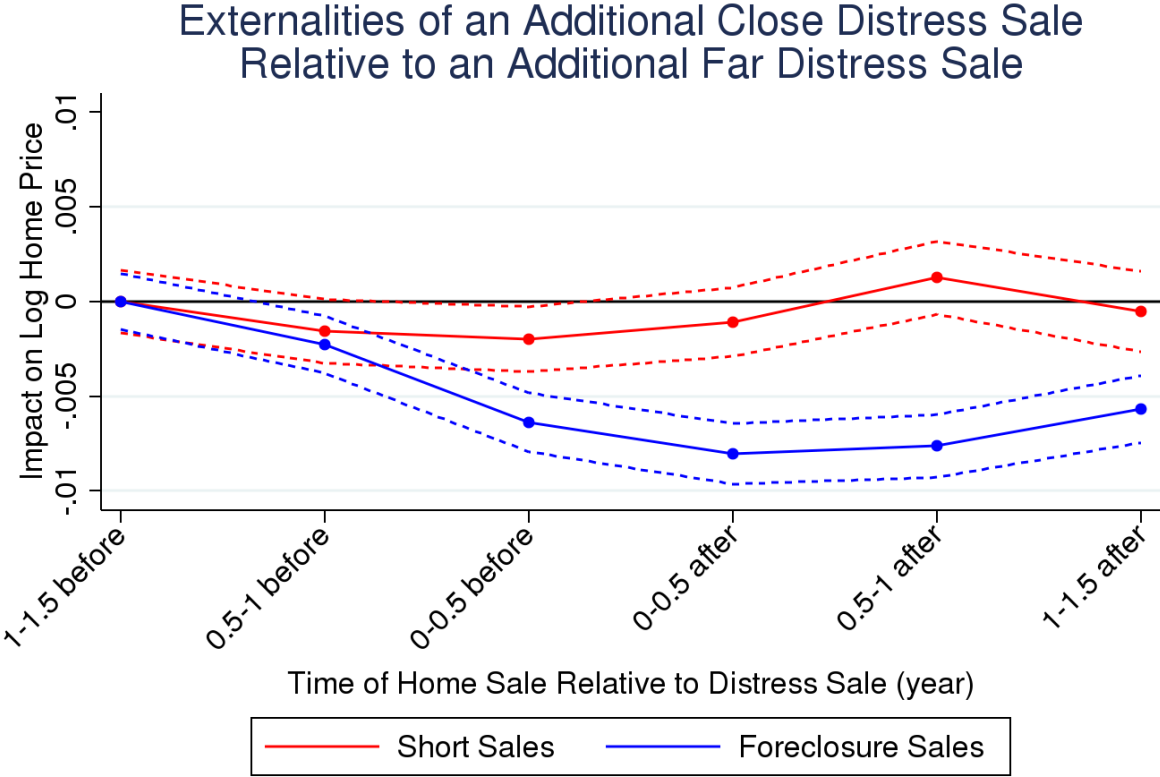
where $\text{foreclosurecount}_{i,t-k}^c$ and $\text{shortsalecount}_{i,t-k}^c$ are foreclosure sale and short sale counts within a close distance of home i measured k periods from time t ; $\text{foreclosurecount}_{i,t-k}^f$ and $\text{shortsalecount}_{i,t-k}^f$ are foreclosure sale and short sale counts within a far distance; and Y_{it} include indicators for if the transaction of home i at time t is a short sale or foreclosure sale and indicators for if home i had 0 short sales or foreclosure sales from $t - 1.5$ to $t + 1.5$ within a close distance. I use sales from July 2005 to June 2012 since I have one and a half years of lags and leads.

Figure 1.3 shows the plots of the indexed $\gamma_{f,t-k}^c$ and $\gamma_{s,t-k}^c$ for the different values of k after estimating equation 1.3. The solid lines are the estimates themselves and the dashed lines are 95% confidence intervals. The plots can be interpreted as the impact of one additional close foreclosure sale or short sale relative to one additional far sale. We can see evidence of strongly different externalities associated with each type of sale. Each foreclosure sale decreases nearby home prices by up to 0.6% after the foreclosure sale itself, and this negative foreclosure externality does not disappear even one and a half years after the foreclosure sale itself. On the other hand, the short sale externality is almost non-existent.

While I find evidence of a foreclosure externality, my estimates of the magnitude or duration of the externality differ from previous research. In their study of four different MSAs between 2007 to 2009, Anenberg and Kung (2014) find that each foreclosure sale decreases the price of nearby homes by 0.6%, which the same as my estimate of 0.6%. However, they showed this externality price effect is gone six months after the foreclosure sale, while I find that the externality still exists one and a half years after the foreclosure sale. Using a sample of sales in the state of Massachusetts starting in 1988, Campbell et al. (2011)

²⁵Campbell et al. (2011) only run this regression for counts a year before and a year after so they just take the difference between the past and future coefficient.

Figure 1.3: Price Externalities of Distress Sales



Data from DataQuick; Single family residential home transactions from July 2005 - June 2012

Notes: This figure presents the price externality of a foreclosure sale or a short sale by plotting the estimates and 95% confidence intervals from a regression of log home prices on close and far foreclosure sale and short sale counts that occurred within a three year window around the sale of each home. Close is within 0.10 miles and far is within 0.25 miles. The estimates represent how sale prices are affected by a close foreclosure sale relative to a close short sale that occurred in each six month interval relative to the sale date. All regressions include tract by half year and month fixed effects and property characteristics. Property characteristics include square footage and age and their squared terms. Standard errors are clustered at the census tract by half year level.

also find evidence of foreclosure externalities lasting more than a year, but they estimate the impact of each foreclosure sale to be 2%, which is much higher than my estimate. The samples used in these studies were either limited by time or location, so it may be difficult to generalize these results. The benefit of my study is that I use data with wider geographical coverage during the entire housing crisis, so my estimates are more nationally representative of what happened during the housing crash.

Given the focus of extant research on the existence of the foreclosure externality, I use the foreclosure externality itself as a benchmark and reformulate equation 1.3 to instead focus on the *relative* externalities of foreclosure sales. That is, I estimate the externality of a foreclosure sale relative to the externality of a short sale to see how much better short sales are than foreclosures for the local housing market. I run the following regression to test for the relative externality of foreclosure sales:

$$\begin{aligned} \ln P_{igt} = & \alpha_{gt} + \beta X_i + \lambda Y_{it} + \sum_{k \in \{-1.5, 1.5\}} (\gamma_{f,t-k}^c \text{foreclosurecount}_{i,t-k}^c + \\ & \gamma_{f,t-k}^f \text{foreclosurecount}_{i,t-k}^f + \gamma_{d,t-k}^c \text{distresscount}_{i,t-k}^c + \\ & \gamma_{d,t-k}^f \text{distresscount}_{i,t-k}^f) + \epsilon_{igt} \end{aligned} \quad (1.4)$$

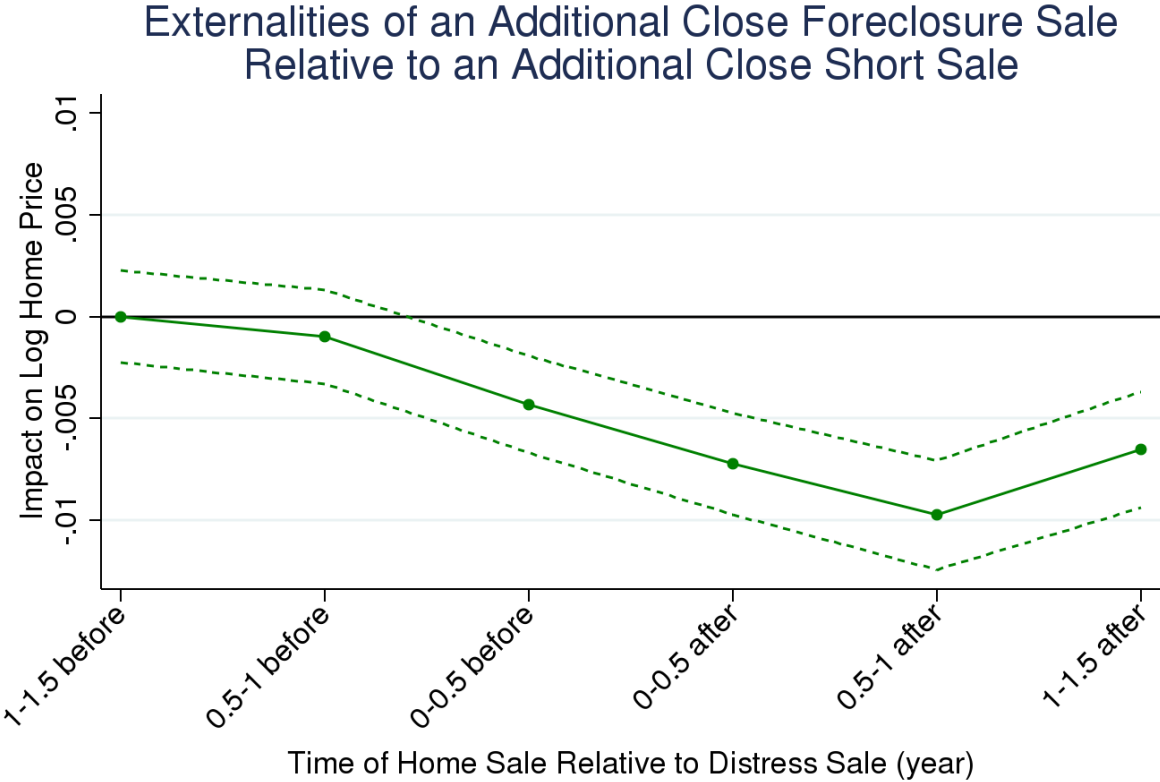
where $\text{distresscount}_{i,t-k}^c$ and $\text{distresscount}_{i,t-k}^f$, which are the sum of close and far short sale and foreclosure sale counts, replace $\text{shortsalecount}_{i,t-k}^c$ and $\text{shortsalecount}_{i,t-k}^f$ from equation 1.3. $\gamma_{f,t-k}^c$ now represents the externality of a close foreclosure sale relative to that of a close short sale. Again, I index the coefficient estimates by the initial period's estimate, which is normalized to 0.

Figure 1.4 plots $\gamma_{f,t-k}^c$ over k . The results here in effect represent the difference between the two lines from figure 1.3. The relative externality for foreclosure sales starts to become negative and statistically different from 0 for homes that sell less than half a year before a distress sale. This negative relative externality grows as the distress sale occurs later on relative to the date of a home sale. A year after a distress sale has occurred, home prices are about one percentage point lower for homes near a previous foreclosure sale than those near a previous short sale. These results show that short sales are better than foreclosures for the housing market because they don't lower the price of nearby homes as much as foreclosures do.

Again, I have to content with omitted variable bias because initial home quality could be dictating the success of a short sale and also be influencing nearby home prices. I separate out pre-listed foreclosures from non-pre-listed foreclosures to condition for home quality. Before estimating the foreclosure externality separately for non-pre-listed and pre-listed foreclosures, I first estimate equation 1.4 for all foreclosures using the smaller merged data set. The result in figure 1.5 shows that the relative externality is weaker in this new sample, but foreclosures still do have a larger negative externality relative to short sales.

Figure 1.6 plots coefficient estimates of $\gamma_{f,t-k}^c$ over k for each type of foreclosure separately. The results show that the relative externality for foreclosed properties that were pre-listed

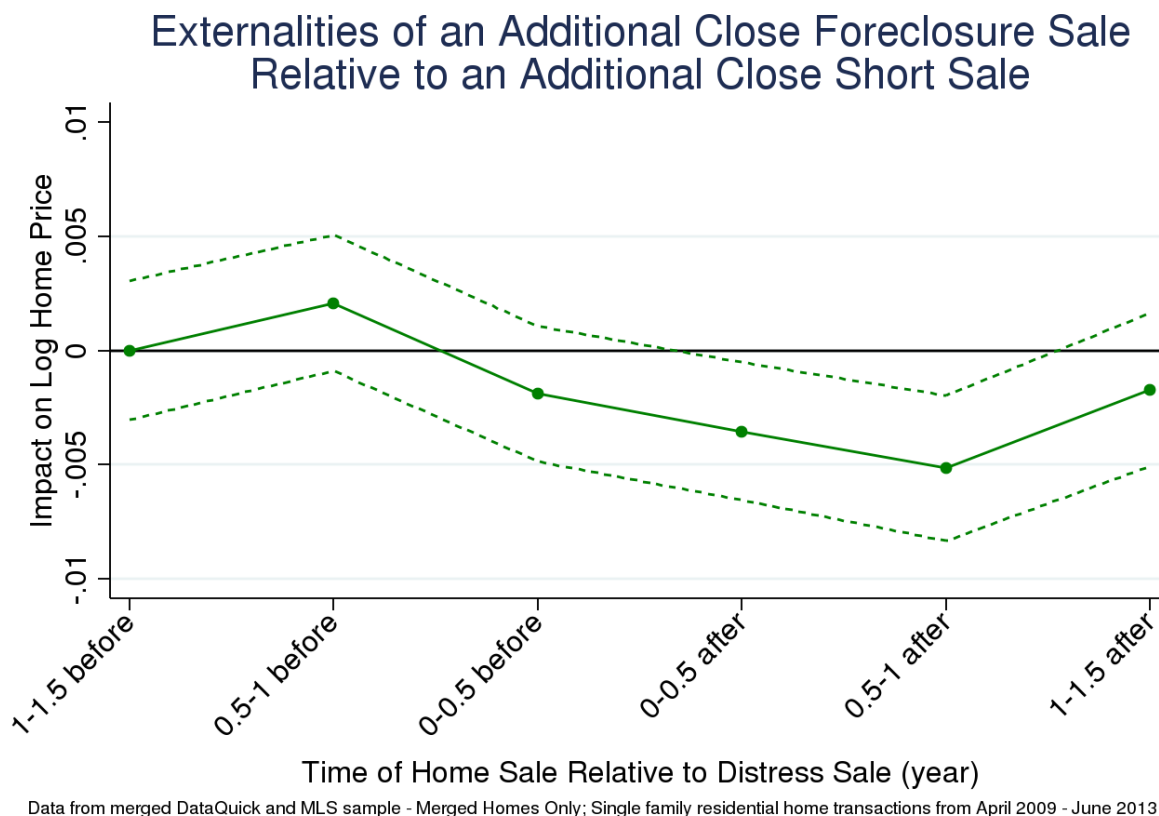
Figure 1.4: Relative Price Externalities of Foreclosures Sale to Short Sale



Data from DataQuick; Single family residential home transactions from July 2005 - June 2012

Notes: This figure presents the price externality of a foreclosure sale relative to that of a short sale by plotting the estimates and 95% confidence intervals from a regression of log home prices on close and far foreclosure sale and distress sale counts that occurred within a three year window around the sale of each home. Close is within 0.10 miles and far is within 0.25 miles. The estimates represent how sale prices are affected by a close foreclosure sale relative to a close short sale that occurred in each six month interval relative to the sale date. All regressions include tract by half year and month fixed effects and property characteristics. Property characteristics include square footage and age and their squared terms. Standard errors are clustered at the census tract by half year level.

Figure 1.5: Relative Price Externality using Merged Transaction-Listing Sample



Notes: This figure presents the price externality of a foreclosure sale relative to that of a short sale by plotting the estimates and 95% confidence intervals from a regression of log home prices on close and far foreclosure sale and distress sale counts that occurred within a three year window around the sale of each home using the sample of loans from the MLS-DataQuick merger. Close is within 0.10 miles and far is within 0.25 miles. The estimates represent how sale prices are affected by a close foreclosure sale relative to a close short sale that occurred in each six month interval relative to the sale date. All regressions include tract by half year and month fixed effects and property characteristics. Property characteristics include square footage and age and their squared terms. Standard errors are clustered at the census tract by half year level.

are not significantly different from those that were not pre-listed, suggesting that omitted home quality is not driving the relative foreclosure externality. Thus, since I find that the type of foreclosure does not influence the externality, I use my original transactions-only data set to run further robustness checks. The advantage of using the transactions-only data set is that it contains transactions going back to 2004, which allows me to use transactions during the entire housing crash in my regressions. These additional robustness checks are shown in the appendix.

Discussion

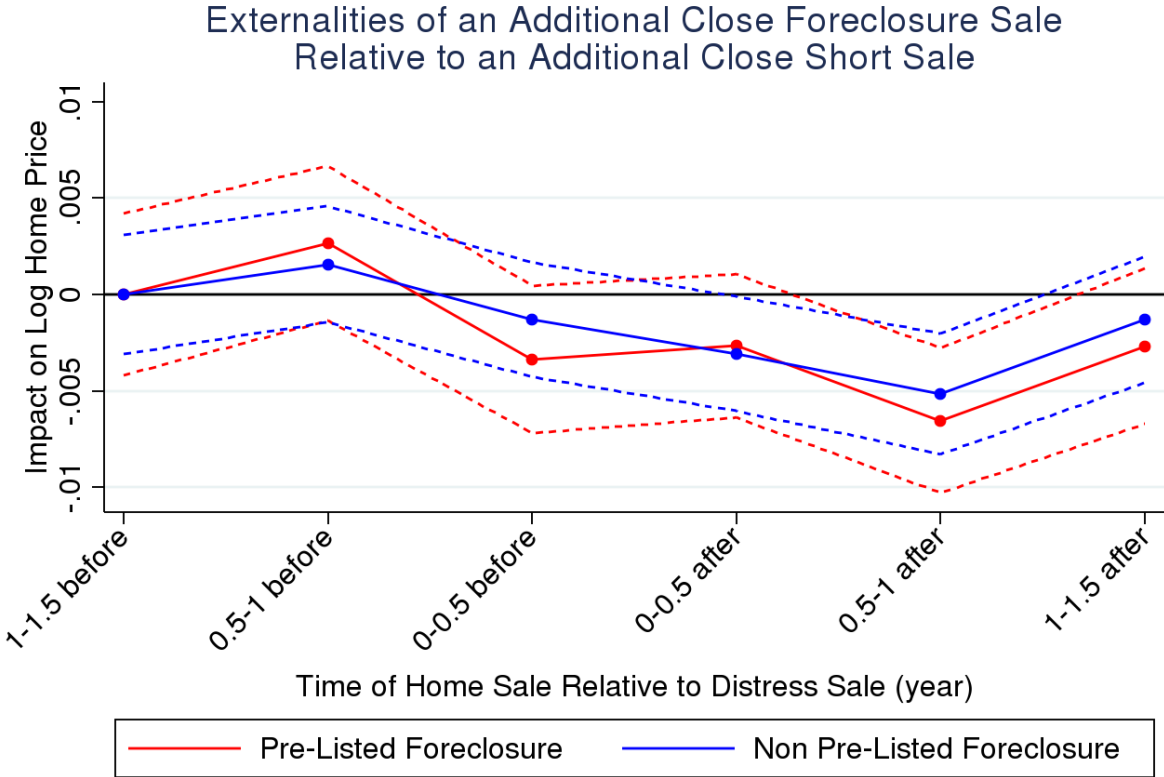
While I show that short sales do not lower home prices as much as foreclosures, it is also important to understand why. What differences between the two types of transactions cause foreclosures to sell at a lower discount and decrease nearby prices more? While I do not test for the different factors that cause the price differences, I speculate on a few reasons for this difference. Further research is needed to break out the individual channels.

The most obvious cause is differences in home quality. I do control for variation in initial home quality that may cause endogenous selection into short sale. However, home conditions continue to deteriorate even after the foreclosure is complete due to negligence by the banks (Lambie-Hanson (2015)) so there can still exist differences in home quality between short sales and foreclosure sales. Quality affects the transaction price simply because quality itself is priced, but also because a lower quality home will require a cash only transaction if the conditions are too poor to qualify the home for a loan, which further reduces the transaction price by decreasing the number of potential buyers.

Second, the two type of transactions convey different amounts of information for the potential buyer. With a short sale, the buyer is able to view the home and consult real estate agents with any questions that may arise. When buying a foreclosed home, the transaction may not be as transparent and bidders may not even get to view the home before buying. Also, banks looking to liquidate homes may know less about the home and may spend less time trying to answer all of the potential buyer's questions.

Lastly, there is a difference in the urgency to sell. Bank are more urgent to liquidate the home after foreclosure than when deciding to approve a short sale. They may only approve of a short sale if the price is high enough because they know they can always liquidate the home later via foreclosure, and the prospect of selling later may yield a higher price if the housing market rebounds. Prior to the home becoming REO, maintenance costs can also be charged to the borrower of the loan. Once the home has become REO, banks may be in a greater rush to sell the home, especially if maintenance costs are high. Shleifer and Vishny (1992) showed that a fire sale occurs when an asset is forced to be sold and the potential buyers are unable to buy the asset, leading to the asset selling at lower prices to parties who value the asset less. Both types of transactions are occurring in the same economic environment where home owners are limited in their ability to buy homes. However, foreclosure sales are more like fire sales because the greater urgency to sell makes them forced sales, which lowers

Figure 1.6: Relative Price Externality of Non-Pre-Listed vs Pre-Listed Foreclosure



Data from merged DataQuick and MLS sample - Merged Homes Only; Single family residential home transactions from April 2009 - June 2013

Notes: This figure presents the price externality of a non-pre-listed and a pre-listed foreclosure sale relative to that of a short sale by plotting the estimates and 95% confidence intervals from a regression of log home prices on close and far foreclosure sale and distress sale counts that occurred a three year window around the sale of each home using the sample of loans from the MLS-DataQuick merger. A foreclosure is classified as pre-listed if there was an active listing for that home two years prior to completion of the foreclosure process. Close is within 0.10 miles and far is within 0.25 miles. The estimates represent how sale prices are affected by a close foreclosure sale relative to a close short sale that occurred in each six month interval relative to the sale date. All regressions include tract by half year and month fixed effects and property characteristics. Property characteristics include square footage and age and their squared terms. Standard errors are clustered at the census tract by half year level.

the price.²⁶

The causes of the foreclosures externality have been well documented to be caused by either a supply channel and a disamenity channel. Anenberg and Kung (2014) and Hartley (2014) showed that foreclosures decrease nearby home prices by increasing the supply of homes, while Fisher et al. (2015) and Gerardi et al. (2015) showed that foreclosure externalities are the result of disamenities or poor conditions. Given that both a foreclosure and short sale increase the supply of homes on the market, the supply effect should lead to similar externalities for the two transactions, but I find evidence of different externalities for the two, which suggests that the supply channel does not explain the larger foreclosure externality.

Instead, the disamenity channel can explain the relative foreclosure externality due to the timing of the externality. My results shows that the externality differences begin shortly before the distress sale itself, which is the time when the home is bank owned, suggesting that the lack of maintenance during REO is causing a spillover. The growth of the negative externality after the distress sale could reflect a delay in the time that it took to clean up the disamenities that resulted from the foreclosure. The persistence of the externality could result from the use of the foreclosure sales as comparables for other homes on the market. Since short sales transact at higher prices than foreclosure sales, they can lead to a higher “reference” price for the neighborhood.

1.5 Explaining Why Short Sales Weren’t More Prevalent

Empirical Setup and Results

Because short sales were better for home prices than foreclosures, it is surprising that there were fewer short sales than foreclosures during the housing crash. However, because these home price benefits did not apply directly to the agents who make the short sale decision, short sales were not optimal for for them. Instead, foreclosures may have provided more benefits, and as foreclosures timelines increased, the benefits may also increase. For borrowers, the option to do a foreclosure provides them with free housing during the entire foreclosure process, which makes foreclosure a more attractive option, especially during times of financial distress for the borrower. If a borrower could not afford to make mortgage payments, he may also have trouble moving out and renting a home so a faster exit out of the home via short sale would not be preferred. When foreclosure timelines are longer, the borrower is able to capitalize on even more free rent when selecting foreclosure, so the decision to do a foreclosure will be even more attractive.

While the borrower is the one who initiates the short sale, he must find a buyer who submits an offer that the servicer of his loan will approve. Even if all borrowers wanted to

²⁶Pulvino (1998) has also shown that fire sales decrease prices by looking at the sale of commercial aircrafts by distressed airlines.

do short sales, servicers may still decline some of them. Servicers have more time to collect servicing fees if they foreclose on a home, but they also want to avoid waiting to recoup advances that have already been made. If servicers do not have enough cash on hand, they would have to finance the cost of their advances, which makes the recovery of advances more urgent, since servicers are borrowing to make what is essentially an interest free loan. A longer foreclosure timeline can increase the servicing fees they can collect but will also delay the recovery of these advances. Thus, the impact of longer foreclosure timelines on the servicer’s decision is more ambiguous.

To test for the impact of foreclosure timelines on the unpopularity of short sales, I estimate how differences in state level foreclosure timelines affected the probability that a delinquent loan will end in a short sale. In the data, I can only observe the outcome for the home — whether it was foreclosed on or sold as a short sale. If I see a foreclosure, I do not know if the borrower decided to allow the foreclosure or if the servicer declined the short sale. When I test for impact of foreclosure timelines on the probability of a short sale, I control for factors that affect how both the borrower and servicer respond to different foreclosure timelines. There is also the possibility that due to poor housing market conditions, a home listed as a short sale never receives an offer. The servicer will not wait forever for an offer to come along and will eventually have to foreclose on the home. Thus, I control for housing market conditions as well.

I test for the the impact of foreclosure timelines on the probability that a delinquent loan will end in a short sale after including controls for factors that influence both the borrower and servicer decision as well as loan characteristics and general zip code level economic controls. I use a linear probability model (LPM) to estimate:

$$\begin{aligned} \text{shortsale}_{i,z,s,t_1,t_2} = & \alpha + \beta_1 \text{foreclosureschedule}_s + \theta X_{i,z,s,t_1,t_2} \\ & + \eta_{t_1} + \eta_{t_2} + \eta_{servicer} + \epsilon_{ict} \end{aligned} \tag{1.5}$$

where $\text{shortsale}_{i,c,s,t_1,t_2}$ is an indicator for a delinquent loan i in zip code z and state s with an origination year t_1 that became 90-days delinquent in year t_2 ended in a short sale; $\text{foreclosureschedule}_s$ is the 2007 foreclosure timeline measured in years for state s ; X_{i,z,s,t_1,t_2} are controls that vary at any level which include loan characteristics and zip code level economic and housing market conditions; and η ’s are fixed effects for year of loan origination, year of distress, and loan servicer.

Estimates of equation 1.5 could be plagued by endogeneity between short sales and foreclosure timelines. Reverse causality exists if low short sale probabilities increased foreclosure counts, and this increase led to longer foreclosure timelines. I aim to get around reverse causality by measuring foreclosure timelines in 2007 while using a sample of loans that became delinquent between 2008 and 2013 to run my analysis. Loans that became delinquent later should not affect the 2007 foreclosure timeline measure. However, there may still be unobserved regional level variation arising from omitted variables that could be driving both foreclosure timelines and the probability of a short sale. Since my foreclosure timeline measure varies at the state level, I am unable to include any regional level fixed effects in my regression to help control for the omitted variables.

To deal with endogeneity, I rely on an instrumental variables approach similar to the one used by Mian et al. (2015). For each state, I know whether the law requires a judicial foreclosure or not. These judicial foreclosure laws serve as a good instrument because they are directly related to the foreclosure timeline as highlighted by Pence (2006), and their historical adaptations were exogenous to economic factors according to Ghent (2013).

Table 1.8 reports the results from both the first stage regression and the 2SLS IV regression. Columns (1) and (2) report the first stage estimates. The results show that states which allow judicial foreclosures have foreclosure timelines that are 0.63 years longer, regardless if servicer fixed effects are included or not. Columns (3) and (4) report the 2SLS IV regression. While it is plausible that some servicers may be more short sale friendly, the results do not change from column (3) to column (4) when I include servicer fixed effects to control for differences across servicers. The coefficient estimate of -4.2% implies that increasing the 2007 foreclosure timeline by one standard deviation decreases the probability that a delinquent loan will end in a short sale by about 1.2%. Applying this coefficient estimate to the 2009 ABSNet foreclosure timelines, I find that short sales decrease by 1.5%. Thus, a standard deviation increase in the foreclosure timeline can explain a 0.35-0.45 standard deviation decrease in the state level short sale share of distressed sales. When I use the RealtyTrac measurement of the 2007 foreclosure timeline in column (5), I obtain a larger estimate in magnitude, which may be explained by the RealtyTrac measure having less coverage and being shorter on average.

One caveat about the ABSNet data is that since it consists only of private-label securitized mortgages, there is a larger proportion of subprime loans, which may be driving the results. Subprime borrowers are higher risk so they are lower quality borrowers and tend to have lower credit scores and incomes. Thus, I would expect them to prefer foreclosures even more because they most likely value free housing even more than the future benefits from short sales. Furthermore, servicers may be less likely to approve their short sales. It is useful to analyze how heterogeneity across borrower quality affects the impact of foreclosure timelines on the probability of a short sale.

I estimate the IV regression of short sales on foreclosure timelines again, but I break out borrowers into subprime, Alt-A, and prime. Table 1.9 report the estimate results for each type. I find that foreclosure timelines have the largest impact for the riskiest borrowers as shown in column (1). The coefficient estimate of -5.0% is larger than the mean estimate for the whole sample of -4.2%. As the borrower quality improves when moving from column (1) to column (3), the impact of foreclosure timelines decreases. For prime borrowers, the foreclosure timeline does not seem to have any impact on short sales. These results suggest that the mean results are being driven by subprime borrowers.

Testing for Borrower Channel Versus Servicer Channel

The estimates so far have only shown that longer foreclosure timelines cause fewer short sales but do not distinguish if the cause is driven by the borrower or the servicer reacting to different foreclosure timelines. As mentioned before, borrowers like foreclosures because

Table 1.8: IV Estimate of the Impact of Foreclosure Timelines on Short Sales

| | (1) | (2) | (3) | (4) | (5) |
|------------------------------|-----------------------------|---------------------|----------------------|----------------------|----------------------|
| | <i>Foreclosure Timeline</i> | | | <i>Short Sale</i> | |
| Judicial | 0.633*** (0.004) | 0.632*** (0.004) | | | |
| Foreclosure Timeline | | | -0.043*** (0.008) | -0.042*** (0.008) | -0.071*** (0.013) |
| Controls | X | X | X | X | X |
| Year of Origin FE | X | X | X | X | X |
| Year of Distress FE | X | X | X | X | X |
| Servicer FE | | X | | X | X |
| Foreclosure Timeline Measure | ABSNet | ABSNet | ABSNet | ABSNet | RealtyTrac |
| Regression Type | OLS | OLS | IV | IV | IV |
| N | 807,155 | 807,150 | 807,155 | 807,150 | 797,759 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents results from the IV regression testing for how foreclosure timelines affect the probability of a short sale. Columns (1) and (2) report the results of the first stage estimate of the state level foreclosure timeline on the judicial foreclosure indicator plus controls and fixed effects. Columns (3) - (5) report estimates from the IV regression of an indicator for whether a delinquent loan ends in a short sale on the state level foreclosure timeline and controls and fixed effects where the instrument is the judicial foreclosure indicator. Foreclosure timeline is measured in years. Columns (1) - (4) use the 2007 ABSNet measure of foreclosure timelines while column (5) uses the 2007 RealtyTrac measure. Controls include original LTV, log original balance, original interest rate; indicators for adjustable rate mortgage, low FICO score (below 650), owner occupied, and state recourse law; zip code level rent, log employment, log income, home price change, and housing turnover rates. The standard errors are clustered at the zip code level.

Table 1.9: Impact of Foreclosure Timelines on Short Sales by Borrower Quality

| | (1) | (2) | (3) |
|----------------------|----------------------|----------------------|-------------------|
| Foreclosure Timeline | -0.050*** (0.009) | -0.021*** (0.004) | -0.014 (0.009) |
| Controls | X | X | X |
| Year of Origin FE | X | X | X |
| Year of Distress FE | X | X | X |
| Servicer FE | X | X | X |
| Borrower Type | Subprime | Alt-A | Prime |
| N | 410,728 | 215,555 | 45,127 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the estimates and standard errors (in parenthesis) from the IV regression of an indicator for whether a delinquent loan ends in a short sale on the state level foreclosure timeline and controls and fixed effects where the instrument is the judicial foreclosure indicator split by borrower type. Foreclosure timeline is measured in years. Controls include original LTV, log original balance, original interest rate; indicators for adjustable rate mortgage, low FICO score (below 650), owner occupied, and state recourse law; zip code level rent, log employment, log income, home price change, and housing turnover rates. The standard errors are clustered at the zip code level.

they get free rent while servicers like foreclosures because it allows them to collect more fees but at the expense of waiting longer to recoup advances. If servicers have already made significant advances, they may actually prefer short sales instead in order to recoup their advances sooner, especially if they had to start borrowing to finance them.

I first test to see how rents affect a borrower's response to different foreclosure timelines. Since the impact of rent affects primarily the borrower, I argue that the varying impact of foreclosure timelines due to differences in rent works through the borrower channel. The coefficient estimate on rent from the baseline specification is positive, which suggests that higher rents and short sales are correlated. A region with higher rents having more short sales could be due to a stronger housing market. To further investigate the importance of rents, I test how differences in rents affect the impact of foreclosure timelines on short sales by adding an interaction term between foreclosure timeline and rent to the baseline LPM regression.²⁷ The interaction term captures how rents affect short sales through foreclosure timelines. The rent value I use is the rent to price ratio from the 2000 census. Using a historical rent value can help eliminate some endogeneity between rent and short sales.

²⁷I have demeaned both foreclosure timeline and rent so that we can interpret either main effect terms when the other is set to 0. All terms that are interacted in any future regressions will be demeaned as well.

The results of estimating the impact of rents is reported in column (1) of table 1.10. The interaction term is negative and significant, which implies that longer foreclosures lead to even fewer short sales in zip codes where rents are higher. At the mean rent level, a one standard deviation increase in the 2007 foreclosure timeline decreases the probability of a short sale by 1.0%. Increasing rent by one standard deviation increases this probability up to 1.7%. Thus, I find that borrowers are responding to longer foreclosure timelines by doing more foreclosures to maximize the amount of free housing they receive.

Table 1.10: Testing for Borrower and Servicer Responses to Foreclosure Timelines

| | <i>LPM</i> | | | <i>LPM with IV</i> | | |
|----------------------------|----------------------|----------------------|----------------------|----------------------|----------------------|----------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Foreclosure Timeline | -0.031*** (0.007) | -0.036*** (0.007) | -0.033*** (0.007) | -0.042*** (0.008) | -0.047*** (0.008) | -0.046*** (0.007) |
| F Timeline X Rent | -8.045*** (1.497) | | -8.048*** (1.498) | -2.630* (1.451) | | -2.568* (1.450) |
| F Timeline X Orig Int Rate | | 0.503*** (0.115) | 0.504*** (0.114) | | 0.903*** (0.111) | 0.899*** (0.112) |
| Rent | 3.870*** (0.566) | 3.504*** (0.527) | 3.890*** (0.566) | 3.770*** (0.556) | 3.679*** (0.526) | 3.807*** (0.556) |
| Original Interest Rate | 0.834*** (0.022) | 0.839*** (0.022) | 0.838*** (0.022) | 0.834*** (0.022) | 0.841*** (0.022) | 0.840*** (0.022) |
| Controls | X | X | X | X | X | X |
| Year of Origin FE | X | X | X | X | X | X |
| Year of Distress FE | X | X | X | X | X | X |
| Servicer FE | X | X | X | X | X | X |
| N | 807,150 | 807,150 | 807,150 | 807,150 | 807,150 | 807,150 |
| R ² | 0.09 | 0.09 | 0.09 | | | |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table test presents estimates and standard errors (in parenthesis) from a linear probability model regression of an indicator for whether a delinquent loan ends in a short sale on state level foreclosure timeline, rent, original interest rate, and their interactions with foreclosure timeline, and controls and fixed effects. All variables used in the interaction terms are demeaned. Foreclosure timeline is measured in years. Rent is the 2000 Census zip code measure of rent to income. Original interest rate is the proxy for servicer advance since advances are a function of interest rates. Controls include original LTV and log original balance; indicators for adjustable rate mortgage, low FICO score (below 650), owner occupied, and state recourse law; zip code level log employment, log income, home price change, and housing turnover rates. The standard errors are clustered at the zip code level.

Next, I test how servicers respond to varying foreclosure timelines by interacting foreclosure timelines with the loan interest rate to see how sensitive servicers are to advances. While servicers are motivated by both fees and advances, my analysis only tests for the impact of advances because I do not have data on fees. Since advances are equal to the borrower's missed payments, they can be calculated using the loan amount and the loan interest rate. By controlling for loan origination amount, I can then use the loan interest rate as a proxy for advances. After I control for borrower credit score and year of loan origination, I assume that any other variation in the interest rate will be exogenous to short sales. The interaction term captures how advances affect short sales through foreclosure timelines.

The estimates of the impact of advances is reported in column (2) of table 1.10. The base term and interaction term is positive and significant, which implies that servicers want to do more short sales when more advances have been made, especially in states with longer foreclosure timelines because they have to wait even longer to recoup fees if they foreclose on homes in those states. At the mean interest rate, a standard deviation increase in the 2007 foreclosure timeline decreases the probability of a short sale by 1.1%. Increasing the loan interest rate by one standard deviation decreases this probability down to 0.7%. Thus, I find that servicers are also responding to longer foreclosure timelines by minimizing their costs, but this response actually leads to higher short sale rates.²⁸

After having found that both the borrower and servicer respond to changes in foreclosure timelines when testing for each individually, I then test to see how they interact with each other and if one effect dominates the other. Column (3) of table 1.10 report the estimates when I include both interaction terms. The estimate on foreclosure timelines base term and both interaction terms are similar to the estimates in columns (1) and (2), which indicates that both borrowers and lenders are responding to variations in the foreclosure timeline at the same time. Variation in the foreclosure timeline and rent prices drive borrower behavior while variation in the foreclosure timeline and mortgage interest rates drive servicer behavior.

Columns (4) to (6) repeat the same estimates as columns (1) to (3), but with an IV LPM regression instead of an OLS LPM. The estimates on the foreclosure timeline base term are consistent with the IV estimates in table 1.8. The coefficient estimate on the interaction of foreclosure timeline and rent is much smaller in magnitude and not as statistically strong but still shows the same relationship, which suggests that rent is still important in explaining why foreclosure timelines cause fewer short sales. The coefficient estimates on the interaction between foreclosure timeline and interest rate is now larger, suggesting that servicers are increasing short sales even more in response to longer foreclosure timelines and higher advance payments.

²⁸I only show that advances are one factor that affects the servicer's decision and how servicers prefer more short sales when advances are higher. In reality, servicers almost must take into consideration fees and their overall response to longer foreclosure timelines may be different.

Economic Significance

While these coefficient estimates of the impact of foreclosure timelines on short sales may be small in magnitude, their economic impact is not given the size of the housing market. Increasing short sales by just 5% would have caused 200,000 out of the 4 million completed foreclosures between 2007 to 2011 to be short sales instead. The primary benefit of these additional short sales would be an increase in housing wealth due to higher transaction prices. Given my results showing that foreclosures have roughly a 8.5% larger discount than short sales and using an average transaction value of \$200,000 for a distressed home sale from my data, a back-of-the-envelope calculation shows having 5% more short sales would have saved the housing market from a loss of around \$3.4 billion during 2007-2011.

Furthermore, the secondary benefit of these these extra short sales would be a smaller negative externality on the prices of nearby homes, which would have led to even larger savings. For the sample of homes in my data, I find that there are on average approximately four transactions within a 0.1 mile radius around each distress sale up to a year after the distress sale. Based on the estimated relative foreclosure externality of one percentage point, having 5% more short sales would have saved up to an additional \$2.4 billion for the housing market when using \$300,000 as the average transaction value for all homes.²⁹ Thus, there are tremendous social welfare gains to increasing the percentage of short sales, even if only by a few percent, which can be done through shorter foreclosure timelines.

1.6 Conclusion

Because of high rates of foreclosures during the housing crash, much research has been done studying the causes and consequences of foreclosures. In addition to undergoing foreclosure, delinquent borrowers also had the option of doing a short sale. A careful study is needed to understand the different economic consequences between short sales and foreclosures. However, the research on short sales is plagued by various endogeneity challenges such as omitted variable bias and reverse causality that need to be resolved in order to establish causal results.

I contribute to the literature by using multiple nationally-representative data sets to quantify the benefits of short sales and explain why they weren't more prevalent. Merging the multiple data sets allows me to achieve stronger identification and to address the endogeneity challenges. I find that short sales lead to transaction prices that are 8.5% higher than foreclosure sales. Short sales also have smaller negative externalities on the prices of nearby homes by up to one percentage point per short sale. Despite all these benefits, short sales were still not as utilized as foreclosures because longer foreclosure timelines made foreclosure more attractive for delinquent borrowers. I show that a one standard deviation longer foreclosure timelines decreases a state's share of short sales by approximately 0.4 standard deviations.

²⁹The average transaction value for all homes regardless of distress is higher than the average transaction value for distressed homes in my data.

While these individual results seem small in magnitude, the total economic impact is big because of how large the real estate market is. A back-of-the-envelope calculation suggests that having 5% more short sales than foreclosures would have saved up to \$5.8 billion in housing wealth between 2007 and 2011. Thus, there needs to be more incentives for short sales to be done. The government and GSEs already began encouraging short sales by offering programs like HAFA starting in 2009 to increase the benefits of short sales for both the borrower and the servicer, but more could be done such as decreasing foreclosure timelines. If we can continue to increase the incentives to do short sales so that they become more popular than foreclosures, future housing downturns may not be as extreme or last as long.

Chapter 2

Municipal Governance and Annexations in Tiebout Equilibrium

2.1 Introduction

Local governments provide basic services like education, public safety, street maintenance, water and sanitation, public transit, garbage collection, and others. There is broad agreement among economists that, in principle, many of these services are important for economic activity and local development. However, there is a long-standing and unresolved debate about the appropriate institutional structure of local governments for efficient service provision. This debate includes questions about the optimal number and size of local governments, the degree of decentralization of services, and what the “right” level of government is to be charged with providing a particular service.

As there is no consensus about the optimal arrangement of local governments, there is also little agreement on the extent of service inefficiency and the ultimate causes of local government inefficiency. Models of underprovision (Samuelson, 1954), efficient provision (Tiebout, 1956), and overprovision (Brennan and Buchanan, 1980) are all part of the canon of public finance theory; recent empirical assessments of efficiency cover the same spectrum from underprovision (e.g. Cellini et al., 2010) to overprovision (e.g. Calabrese et al., 2012).

In the U.S., the institutional structure of local governments gives rise to two classes of local governments: county governments and municipal governments.¹ County governments are extensions of their respective state government charged with the execution of state mandated services. They provide local services only in unincorporated places where municipal governments are absent, or where municipalities contract with the county for service provision. In contrast, municipal governments exist for the purpose of pro-actively choosing their

¹The Census distinguishes between county and sub-county general-purpose governments, with the latter including municipalities and townships (Hogue, 2013). Since townships are a weaker form of municipal government in many states, I refer to all sub-county governments as municipalities. The Census also distinguishes between general-purpose and special-purpose governments, the latter including special districts and school districts. Here, the focus is on general-purpose local government.

own policies for local services in accordance with the preferences of voters from within the municipality.

To study the relative efficiency of county versus municipal government, we exploit that municipal boundaries expand over time through annexation, replacing county rule with municipal rule. Annexations occur both in scarcely developed land as well as built up neighborhoods, allowing us to study both the increase in housing supply and the market response in existing neighborhoods.

To this end, we combine fine-grained administrative boundary change data from 189 Californian cities showing the complete history of boundaries since incorporation with the universe of residential real estate transactions for 1988-2013. In California, more than 1,300 square miles were annexed since 1991, making it the state with the fifth largest area to be annexed in the last 25 years (U.S. Census Bureau, 2015). The frequency of annexations, the existence of fine-grained administrative boundary data, the availability of high-quality real estate sales data, and the fact that Proposition 13 essentially fixes property tax rates irrespective of local government make California an ideal setting to study the effects of expanding municipal government.

To motivate the relevance of municipal boundaries, we first show that properties on either side of the boundary look substantially different on a number of characteristics as well as the sales price per acre of lot size: homes just outside municipal boundaries are smaller and older but built on substantially larger lots. While the sales price is similar on either side of the boundary, the implied land price is substantially higher just on the inside of the municipality, which is only partially driven by differences in house characteristics.

Annexations are typically initiated by municipalities. To deal with the endogeneity of municipal annexations, we estimate the extensive margin market response to annexations using an event study design. This allows us to study to what degree the presence of municipal governance is a cause or a consequence of local development. We find strong evidence that municipal government precedes local development: growth rates of residential real estate in an area in the years leading up to an annexation are indistinguishable from average growth rates in other areas; but in the years right after annexation, growth accelerates rapidly, reaching more than 50% after three years and continuing to surpass average growth for at least fifteen years. The growth induced by municipal annexations translates into rapid and sustained appreciation in the average value per acre of built-up land: average sales prices per acre of lot size grow at the same rate before annexation and rise by more than 20% in three years. Together, these results suggest a comparative advantage in municipal governments providing local services for residential development.

We argue that these changes in the real estate market are induced by changes in public goods provided by municipalities. To this end, we provide suggestive evidence that the quality of public goods changes when switching to a municipal service provider: first, per capita expenditure is around \$500 higher after annexation; and second, the adjusted crime clearance rate of the police agency responsible for service provision in an area is about 16% higher after annexation.

This paper speaks to a number of related literatures in local public finance and political

economy. It adds to the literature on the determination of local public goods initiated by Tiebout (1956) that residential sorting may not only take place from one municipality to another but also between counties and municipalities, adding another degree of freedom in the determination of equilibrium; county governments play the role of the default service provider for people who do not find a municipal government that matches their type. This view is also consistent with Bewley’s (1981) critique of Tiebout in terms of the stringent requirements imposed for an equilibrium to exist.

The empirical assessment of residential sorting in equilibrium models has typically used only cross-sectional data from a small number of jurisdictions (Epple and Sieg, 1999; Epple et al., 2001; Calabrese et al., 2006). Our reduced form results using rich microdata provide a useful complement to these structural approaches.

We also contribute to the literature assessing efficiency of local public goods and place based policies originating in Samuelson (1954). While many studies find inefficiently low spending (Bradbury et al., 2001; Cellini et al., 2010; Busso et al., 2013), others point towards overspending (Barrow and Rouse, 2004) or excessive regulation (Turner et al., 2014). Also closely related are papers on the real estate valuation of public goods such as education (Black, 1999), neighborhoods (Bayer et al., 2007) and high income municipalities (Boustan, 2013). We add to this literature a dynamic assessment of valuation, showing that there is surprisingly little capitalization in anticipation of jurisdictional changes, speaking for substantial frictions in the realization of gains from higher valued local public goods.

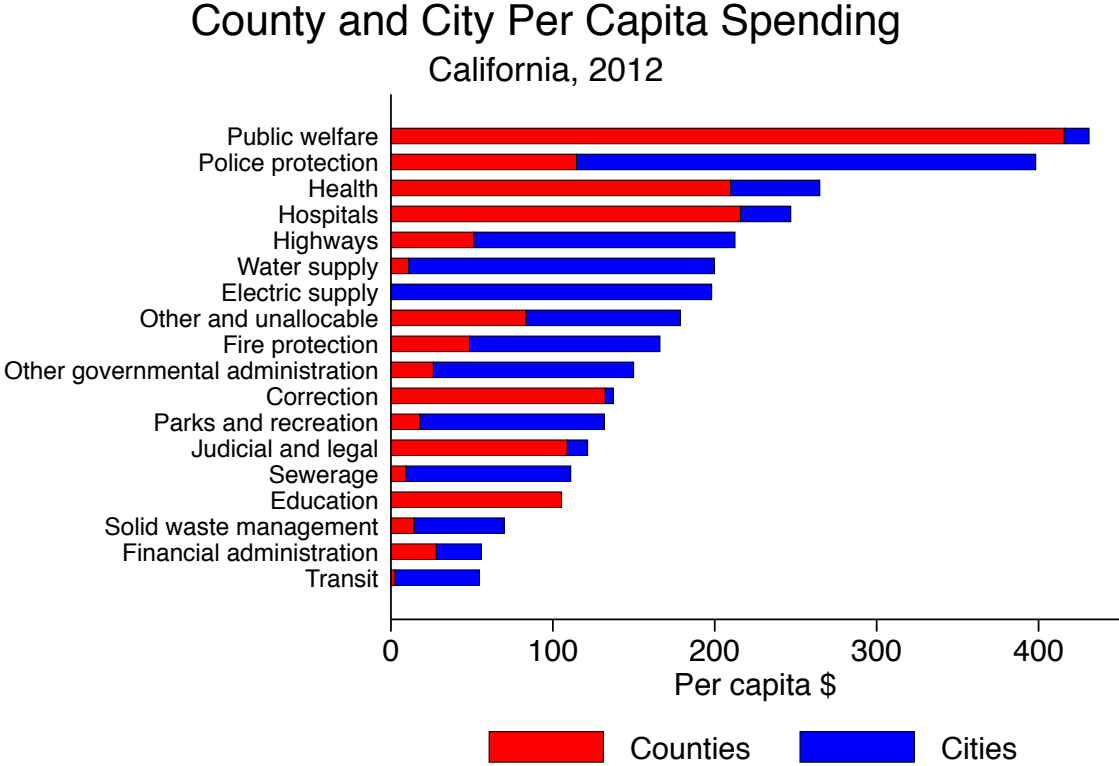
We proceed as follows: section 2.2 describes the institutional setting of U.S. local governments in more detail. We then sketch a Tiebout model adapted to county-municipality sorting and derive predictions in section 2.3. Section 2.4 describes the boundary, property, and public goods data. Section 2.5 shows discontinuities at municipal boundaries, and section 2.6 shows event studies after annexations. We argue that these changes are mediated through public goods, as discussed in section 2.7. Finally, section 2.8 concludes.

2.2 Background: U.S. Local Governance Structure

Local governance in the U.S. is typically divided into two classes: general purpose governments and special purpose governments (special districts and school districts). The focus in this study is on the two main types of general purpose governments: counties and municipalities.² While school districts typically take up the bulk of attention in local public finance, they do not exhibit the phenomena of interest here, particularly the lack of coverage of some areas and frequent boundary changes. These phenomena are present for municipalities and form the basis of this study.

²The Census of Governments also defines a second class of sub-county governments – towns or townships – which “provide services to an area without regard necessarily to population” (U.S. Census Bureau Government Division, 2013, p. viii). Since this type of local government does not exist in our setting, there is no need to adjudicate whether they are more similar to counties or municipalities, our main comparison.

Figure 2.1: General Purpose Local Government Expenditures



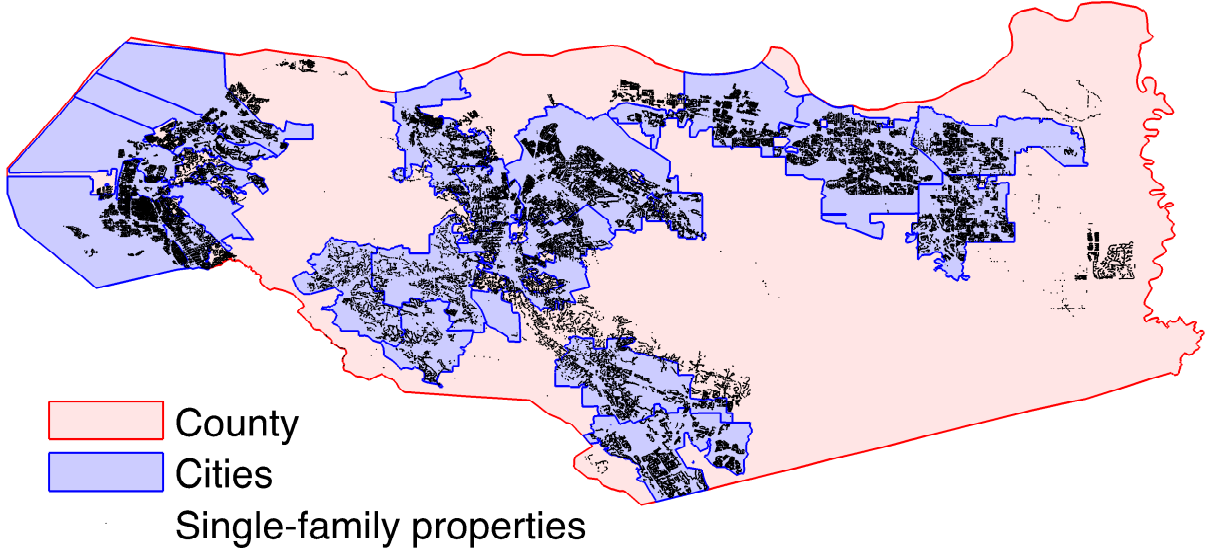
Source: Census of Governments

Notes: This figure shows per capita expenditures by counties and cities in California. Each bar shows the total per capita expenditures for a category, and the colors indicate the amount of the total coming either from counties or municipalities.

General purpose governments provide a wide variety of services (see figure 2.1). Counties provide three types of services: state-mandated services, such as public welfare and health services; county-wide services such as courts, property assessment, election administration, and correction (jails); and finally, they provide local services in places without municipal government or where municipalities outsource services to them. In contrast, municipalities provide only local services. Their largest expenditure item and their most important responsibility is public safety, including police and fire protection. On average, municipalities spend about \$300 per capita on police protection and \$100 per capita on fire protection. Local services include a number of other items too, such as street maintenance, utilities, parks and recreation, sewerage maintenance, and solid waste management.

County governments play an important role in local service provision. To see this, it is useful to examine local governance across “places”. The U.S. Census defines a place

Figure 2.2: Territorial Division of Local Services into County and Municipality



Notes: This figure shows a map of Contra Costa County as an example of the territorial division of local services into municipalities (in blue) and unincorporated county land (in red). Black dots are the locations of observed property sales. It can be seen that a substantial number of properties lie outside municipal boundaries, falling under the jurisdiction of the county (or special districts).

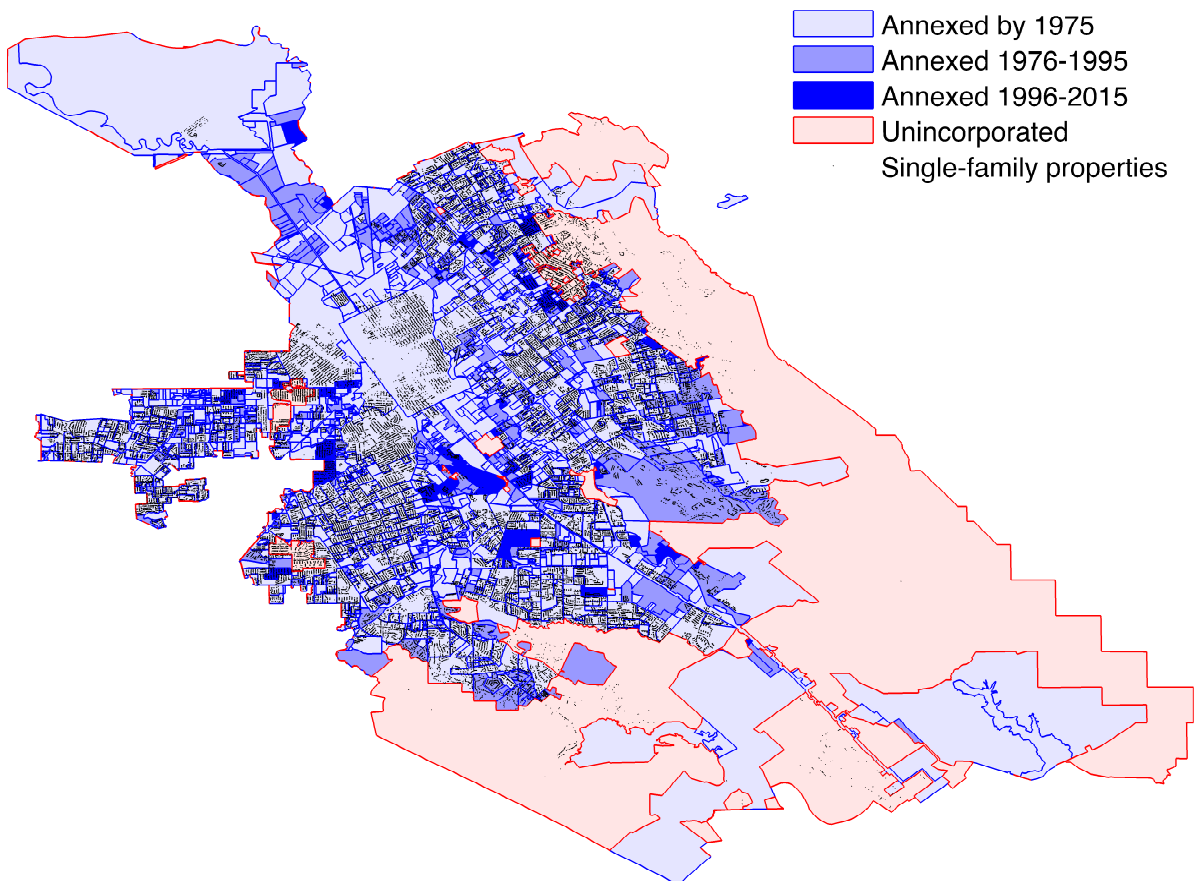
as a concentration of people, irrespective of its local government structure. According to the American Community Survey (U.S. Census Bureau, 2016a), 9,691 places (32%) in the U.S. are unincorporated, with a total population of more than 36 million (16% of total place population). County governments and special districts are in charge of local service provision in unincorporated places. They also provide local services to the 85 million people living in rural areas outside of population concentrations. Service levels and code enforcement are typically lower in counties than in municipalities, especially in unincorporated neighborhoods interspersed between collections of municipal governments in metropolitan areas (Anderson, 2008).

In contrast to the limited powers of county governments, municipalities enjoy “home rule”. Municipal home rule grants local governments substantial autonomy from state governments, allowing them to regulate matters of local interest without interference from higher levels of government. These municipal powers have a long tradition of support in court on the basis of federal and state constitutions alluding to “an inherent right of local self-government” (McBain, 1916).

Municipal boundaries change frequently through annexation of unincorporated county territory. The Boundary and Annexation Survey (U.S. Census Bureau, 2015) of the U.S. Census recorded almost 200,000 annexation events covering almost 25,000 square miles since 1990. This process is regulated by state law and typically involves agreement from the muni-

city, the land or property owner, or both; elections may be required in both the annexing and the annexed territory; and commissions may also adjudicate the process according to particular goals set by the state government (Facer, 2006; Edwards, 2008).

Figure 2.3: Example of Municipal Annexations: San Jose



Notes: This figure shows a map of San Jose with areas (neighborhoods) and their respective annexation date. Shades of blue denote the era in which an area was annexed: from light blue – before 1975 – to dark blue – after 1996; red are areas that continue to be unincorporated. Black dots are the locations of observed property sales.

Figure 2.3 shows an example of municipal annexations. Looking at areas annexed by 1975, it can be seen that local governance is organized in a haphazard way, with the municipality and the county dividing service provision responsibility in a complex assignment of neighborhoods to jurisdictions. This pattern of incomplete municipal governance can be found across U.S. metropolitan areas. In California, it roots in the rapid expansion of municipal boundaries during the boom years after the Second World War, when municipalities often leapfrogged across areas to reach neighborhoods generating high tax revenue. The state

has since then put laws into place to combat this pattern of local governance. However, these laws had only partial success at integrating urban areas under a single jurisdiction: even by 2014, there are still a number of residential areas that are under county governance.

It is important to note that the municipal boundary change process is largely independent from school district boundary changes. School districts typically cover the entire sphere of a municipality, encompassing both incorporated and unincorporated areas.

California exhibits a number of peculiarities that make it a particularly suitable setting to study questions of municipal governance versus county governance. First, and most importantly, due to proposition 13, property taxes are essentially fixed across all locations at 1%. Excess property taxes are mostly due to school bonds, which apply across municipal boundaries. Second, the state mandates that every municipality has a “sphere of influence” in which it has the exclusive right to annex territory. This mandate was put in place to avoid what was perceived as harmful competition over territory.(Caballero, 2009).

2.3 Model

In this section, we outline a Tiebout model based on Calabrese et al. (2012) for the purpose of (a) making predictions about the distribution of local public goods, residential density and prices between a municipality and unincorporated county land, and (b) to derive an expression for efficiency of municipal annexations. The model builds on a class of models developed in a series of papers (Epple et al., 1978, 1984; Epple and Platt, 1998; Calabrese et al., 2012). We proceed by first summarizing the model environment and then collecting key predictions of the equilibrium.

Environment

Households. A unit mass of households resides in a metropolitan area with county and municipal governance. Households are heterogeneous along two dimensions: income y and preference for public goods α , with density $f(y, \alpha)$ over the domain $S \equiv [y_l, y_h] \times [\alpha_l, \alpha_h] \subset \mathbf{R}_+^2$. They derive utility from a private composite good x , from consumption of housing units h , and from public goods g in the form $U(x, h, g, \alpha)$, where $U(\cdot)$ is twice differentiable and strictly increasing and strictly concave in its first three arguments.

We also assume a standard single-crossing condition: households’ willingness to pay for housing increases with higher public goods, *ceteris paribus*. Households choose consumption baskets and residential location to maximize utility, correctly anticipating equilibrium values realized in the housing market and the public goods market.

Jurisdictions. A metropolitan area consists of two overlapping jurisdictions: a municipality and a county.³ The county serves its whole territory L , and the municipality serves a

³For simplicity, we operate with a single municipality, although the key predictions concerning the differences between municipal and unincorporated areas are not affected by the dynamics introduced by

subset $L_M \leq L$, and the rest, $L_C = L - L_M$, is served only by the county.

All residents of the county pay a property tax τ_C to the county and receive a fully congested public good g_C in return. Residents within the municipality pay τ_M to the municipality, who provides g_M only to them. As is the case for most local and state governments, we require budget balance. Notice that municipal residents receive $g_M + g_C$ public goods. This means that, by construction, municipal residents receive more public goods than county residents.⁴

Housing Markets. We refer to prices, taxes and public goods in the municipal part of the county with subscripts $j = M$ and those in the unincorporated county part with $j = C$. The supplier price of a unit of housing in j is $p_j^s = (1 + \tau_j)p_j$, where p_j is the market price. Upward sloping housing supply is given by $H_j^s(p_j^s)$.

Property tax limitation. We simplify the determination of the property tax by using the existence of property tax limitations. These provisions exist in most states and essentially fix the property tax rates across jurisdictions.⁵ The implication for our model is that $\tau_M = \tau_C = \tau$. This means that residents pay the same tax rate no matter where they live, but in the municipality they get services from both governments, while they only get served by the county in unincorporated territory.

Equilibrium. A Tiebout equilibrium consists of prices and public goods (p_j, g_j) such that:

1. Households' choice of residence and consumption baskets maximizes utility.
2. Both jurisdictions have balanced budgets: This implies for counties:

$$g_C = \tau p_C^s H_C^s(p_C^s)$$

and for municipalities:

$$g_M \int_S f_M(y, \alpha) dy d\alpha = \tau p_M^s H_M^s(p_M^s)$$

where $f_j(y, \alpha)$ is the share of residents in j .

many municipalities.

⁴Here, municipal residents free-ride on the county public goods paid for by county residents. In a richer model, it is possible to determine the distribution of both municipal tax revenue and county public goods across the two areas using the political process in the county: county governments are elected by all residents in the county; municipal voters typically field the median voter, who would pick bundles of taxation and public goods provision that favor municipal residents at the expense of residents in the unincorporated area. We also provide empirical evidence that is consistent with this assumption.

⁵Exceptions are possible for voter approved debt obligations. However, the variation induced by these exceptions is small. For example, in California, property taxes before the introduction of Proposition 13, which fixed property taxes to 1%, were 2-3 times higher than after the introduction of the limitation, with substantial variation across jurisdictions (Rosen, 1982); The range of property taxes today is less than 20% (Taylor, 2012).

3. Housing markets clear:

$$\int_S h_M(y, \alpha) f_M(y, \alpha) dy d\alpha = H_j^s(p_j^s)$$

where $h_M(y, \alpha)$ is the amount of housing consumed in j by type (y, α) .

Predictions

Under endogenous property taxes, Tiebout models generally have many equilibria, and existence is not guaranteed (Calabrese et al., 2012). In our setting, thanks to our simplifying assumptions, we arrive at less ambiguous predictions for house prices and density. The following predictions derive in a straightforward manner from similar models in the literature; we refer to the literature for a formal discussion of the proofs.

House prices. Given that municipal residents receive more public goods than county residents, it has to be the case that $p_M > p_C$. In other words, housing in the county has to be cheaper to compensate for the worse public goods at the same tax price.

Income and taste sorting. Residents sort by income and taste: for a given income level, residents with a higher taste for public goods live in the municipality; and for a given taste level, higher income residents live in the municipality.

Existence of indifferent households. There are households (y, α) for whom $V(p_M, g_M, y, \alpha) = V(p_C, g_C, y, \alpha)$, where $V(p, g, y, \alpha) = U(x - ph, h, g, \alpha)$ is the household's indirect utility function. Everyone else strictly prefers their residential location.

Density. Demand for housing units is higher in the municipality than in the county. As a consequence, housing density H_j^s/L_j is higher in the municipality than in the county.

Annexations. If the municipality expands into the county, we expect (a) an increase in price per unit of housing; (b) an increase in density; (c) an increase in the units of housing consumed per household; (d) a temporary increase in sales activity due to resorting; (e) a shift towards households with a higher taste for public goods.

Further model predictions. The model offers the potential to evaluate the extent of externalities that arise as a combination of property taxation, property tax limitation and limited municipal governance. To this end, the solution to the social planner's problem would provide an avenue to quantify the extent of inefficiency. Another interesting avenue would be to use the social planner's problem to derive an expression for the change in welfare as a consequence of annexations. We leave these steps for future research.

Moving to Empirics

In our empirical evaluation of municipal governance, we focus on the predictions about changes in density, prices, and intensity of housing consumption. We treat each time period in our data as a readjustment to a Tiebout equilibrium according to the predictions about the effect of annexations. These boundary changes are small relative to the overall size of the municipality and the county, which is why we treat equilibrium effects on areas other than the annexed area as negligible.

While annexations are themselves the outcome of a bargaining process, we treat this process as a black box and restrict our interpretation to the average treatment effect of areas whose property owners (or land owners) consider annexation a worthwhile change in public goods. At this point, we do not attempt to answer what extent these effects carry over to other areas. It is also worth noting that explicitly integrating the annexation process into the model may be an interesting avenue for future research.

2.4 Data

We combine three data sources to estimate the relationship between municipal governance, prices, and public goods. First, we collect a novel dataset on the universe of boundary changes for 189 municipalities in California. Second, we combine these boundary changes with the universe of property sales in the state for 1988-2013. Finally, we include detailed public finance data from counties and cities in California. We describe each of these in turn.

To measure jurisdictional boundaries, we use administrative municipal boundary change data from individual counties and cities in California. These data precisely document the evolution of municipal boundaries, often all the way back to the original municipal incorporation. In addition to the boundaries, these data capture the year a particular area (neighborhood) was annexed to a given municipality. Unlike alternative data sources such as the TIGER/Line place database by the U.S. Census, these data are collected for administrative purposes, often in the context of property assessment. In contrast, TIGER/Line is based on an annual voluntary survey, and so the timing of municipal boundary changes is of poor quality, especially before 2007 (U.S. Census Bureau, 2016b).

We were able to obtain the administrative boundary change data from 189 municipalities (out of 482) across 18 counties (out of 58) in California. Not all counties and cities have their entire boundary history in electronic format which is readily available for our purposes. There is no obvious pattern of selection into having this type of data: both small and large places, relatively wealthy and poor places, and places with few or many boundary changes according to the Boundary and Annexation event counts appear in the data. The sample is somewhat biased towards Southern California and towards more urban places (most rural counties with very few municipalities are missing). We include a complete list of all counties included in the appendix.

Based on the administrative boundary data, we can then partition each city into its

constituent areas according to the year they joined (or originally formed) the municipality. An area can be as small as a few properties (as small as $40m^2$) or as large to encompass several neighborhoods. Unincorporated areas that have not (yet) joined the municipality but are within its sphere of influence are also included. An example of the area data structure for a single sphere can be seen in figure 2.3.

We supplement these boundary data with data on the universe of home transactions obtained by DataQuick from each county's assessor office between 1988 and 2013. The DataQuick data contains information on the characteristics of each home that sold and on each transaction for that home. Home characteristics include the home address, lot size, number of bedrooms, number of bathrooms, square footage, number of stories, and the year the home was built, which do not change from transaction to transaction.⁶ However, we also observe time-varying transaction values and dates. In addition, while year build is time-invariant, it allows us to calculate age at the time of sale, which is time-varying.

While the DataQuick data has the complete address of each home, it does not provide information on if a home is located in a municipality or an unincorporated area. However, having the address allows the home to be geocoded and merged with the boundary data so we can observe if a home transacted in a municipality or not. We keep only transactions that fall within the spheres of influence of the 189 municipalities in our sample. Our merged DataQuick sample contains 4,119,959 transactions on 2,190,313 homes.

To understand the expenditure patterns associated with counties and municipalities, we use the county and city finance data from the California State Controller's Office. It shows complete finances of counties, cities and special districts for 1991-2014. These data allow us to construct local service expenditure measures of counties and municipalities that are comparable. Specifically, we compare the municipality and the county (with a focus on activities targeted at unincorporated areas) along the following dimensions: total per capita expenditures and police protection per capita expenditures. We also compare county and municipal police protection performance by computing adjusted clearance rates using the California Department of Justice's Criminal Justice Statistics Center (CJSC) database (more details below).

Table 2.1 shows summary statistics for spheres, areas, and properties in our dataset. In Panel A we show sphere characteristics, focusing on the extent to which the municipality in the sphere has taken over local governance. We see that most spheres had at least some unincorporated areas (83%) in 1988 and in 2013 (71%). In Panel B, we can see characteristics of areas. Unincorporated areas are typically larger, which is why they have on average more homes and larger built territory. We can also see that prices per lot size are typically lower in unincorporated areas. Finally, in Panel C, we look at properties within 500 meters of a boundary dividing the municipality and the county. This is the relevant sample of properties in our boundary discontinuity design.

⁶In reality, these characteristics may change over time through renovations, but in our data, we observe the characteristics from the most recent transaction only.

Table 2.1: Summary Statistics for Spheres, Areas, and Properties

| <i>Panel A: Sphere characteristics</i> | Mean | P(50) | SD | P(5) | P(95) |
|--|---------|-------|---------|------|--------|
| Any unincorporated areas, 1988 | 0.83 | 1 | 0.38 | 0 | 1 |
| Any unincorporated areas, 2013 | 0.71 | 1 | 0.45 | 0 | 1 |
| Share incorporated areas, 1988 | 0.73 | 0.84 | 0.29 | 0 | 1 |
| Share incorporated areas, 2013 | 0.89 | 0.95 | 0.15 | 0.50 | 1 |
| Number of areas | 55.6 | 14 | 140.8 | 1 | 229 |
| Square miles of total area | 28.8 | 10.8 | 53.9 | 1.62 | 117.5 |
| Municipal expenditure p.c., 1000s | 1.87 | 0.91 | 9.42 | 0.35 | 3.39 |
| County expenditure p.c., 1000s | 0.83 | 1.05 | 0.34 | 0.28 | 1.13 |
| Municipal adjusted clearance rates | 1.15 | 1.12 | 0.33 | 0.72 | 1.72 |
| County adjusted clearance rates | 1.03 | 0.99 | 0.29 | 0.72 | 1.78 |
| N | 189 | | | | |
| <i>Panel B: Area characteristics</i> | Mean | P(50) | SD | P(5) | P(95) |
| Number of homes, annexed areas | 343.3 | 42 | 2949.6 | 1 | 1045.2 |
| Number of homes, uninc. areas | 366.8 | 13 | 1674.3 | 1 | 1716.9 |
| Built acres, annexed areas | 62.6 | 7.65 | 627.9 | 0.22 | 170.7 |
| Built acres, uninc. areas | 110.5 | 8.38 | 486.0 | 0.14 | 445.5 |
| Avg. price per lot size, annexed areas | 37.7 | 30.0 | 32.1 | 6.89 | 90.6 |
| Avg. price per lot size, uninc. areas | 20.2 | 12.4 | 26.1 | 1.52 | 61.5 |
| Avg. price per bldg size, annexed areas | 171.2 | 150.1 | 96.5 | 69.0 | 330.2 |
| Avg. price per bldg size, uninc. areas | 140.0 | 115.3 | 86.3 | 50.9 | 295.6 |
| N | 10,618 | | | | |
| <i>Panel C: Property characteristics</i> | Mean | P(50) | SD | P(5) | P(95) |
| Sales value (thousands) | 226.7 | 175 | 167.7 | 64 | 570 |
| Building square feet | 1885.0 | 1737 | 782.3 | 972 | 3367 |
| Lot size square feet | 9863.9 | 7405 | 12285.0 | 4464 | 20309 |
| Bathrooms | 2.13 | 2 | 0.72 | 1 | 3 |
| Bedrooms | 3.32 | 3 | 0.79 | 2 | 5 |
| Stories | 1.22 | 1 | 0.49 | 1 | 2 |
| Age | 25.1 | 21 | 21.7 | 0 | 62 |
| Price per bldg size | 122.6 | 97.3 | 83.4 | 47.2 | 285.9 |
| Price per lot size | 29.6 | 22.7 | 24.9 | 6.72 | 76.8 |
| N | 148,727 | | | | |

Notes: This table presents summary statistics at the sphere, area, and property levels. $P(x)$ denotes the x th percentile. Spheres always include areas with municipal governance; most spheres also have areas with county governance (unincorporated areas). Properties included here are only those within 500 meters of a municipal boundary.

2.5 Municipal Boundary Discontinuity

As a first step in our empirical analysis, we now proceed to compare properties on either side of the municipal boundary using a boundary discontinuity design. Note that whenever we refer to municipal boundaries throughout this paper, we mean only those that border on unincorporated county territory, excluding municipal boundaries that border on other municipalities. The goal is to understand the nature and the extent of sorting that takes place between municipalities and counties. We interpret the coefficient estimates of the difference in outcomes on either side of the boundary as a reflection of the equilibrium difference in outcomes in municipalities and counties. We address the extent of a causal effect of municipal governance on outcomes in the next section, when we investigate outcomes over time.

This design was developed by Black (1999) in the context of the school quality valuation literature. While an analogy is often drawn between the boundary discontinuity design and the spatial regression discontinuity design, which aims to estimate the causal effect of a spatially discontinuous policy, Bayer et al. (2007) show that the correlation between the policy and unobserved neighborhood characteristics may substantially bias estimates away from the causal parameter. In this sense, we think of the boundary discontinuity estimates as descriptive statistics to study the extent and type of sorting taking place between municipalities and counties.

Econometric Setup

To estimate potential discontinuities in a number of outcomes across municipal boundaries, we use properties indexed by i transacted in year t within 500 meters of a municipal boundary. We then run

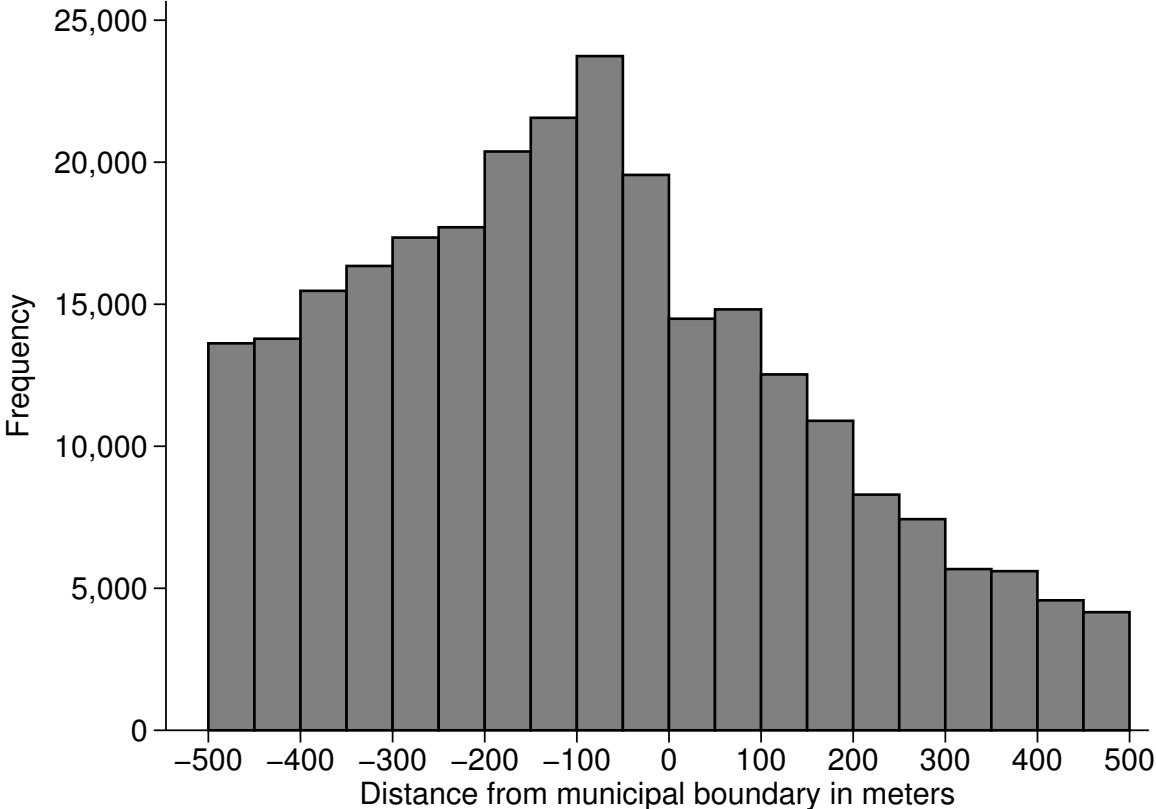
$$y_{it} = \alpha_{b(i),t} + \mathbf{1}[d_{it} \geq 0]\beta + f(d_{it}) + \mathbf{X}_i\gamma + \varepsilon_{it} \quad (2.1)$$

where y_{it} is an outcome of interest such as the sales price, the lot size or the number of bathrooms; $\alpha_{b(i),t}$ is a fixed effect for boundary segment $b(i)$ in year t ; d_{it} is the distance to the nearest municipal boundary, with positive values indicating the property is outside the municipality; $f(\cdot)$ is a set of polynomials on either side of the boundary; \mathbf{X}_i is a set of property characteristics; and ε_{it} is an error term. Our coefficient of interest is β , which measures the magnitude of the discontinuity at the municipal boundary. Throughout all regressions, we use the robust regression discontinuity estimator developed by Calonico et al. (2014).

Results

Before looking at the discontinuities, it is useful to see the distribution of property transactions on either side of the municipal boundary. This is shown in figure 2.4. It can be seen that there is a substantial number of properties within 500 meters of the boundary on either side; however, the density seems to be substantially higher just within the municipality: while the 50-meter bins just outside the municipality have around 14,000 properties,

Figure 2.4: Histogram of the Number of Homes Around Boundary



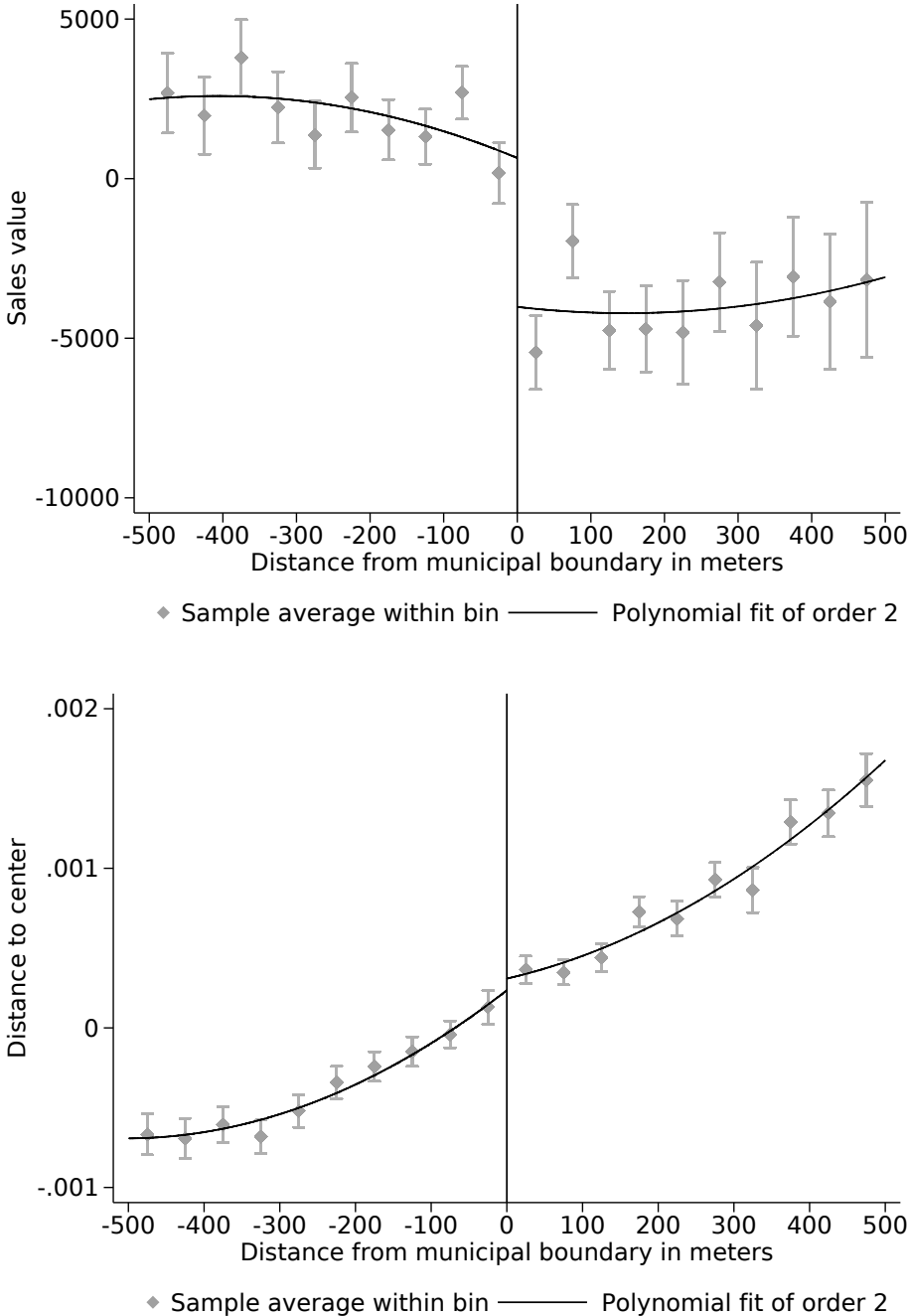
Notes: This figure plots the histogram of the number of homes within 500 meters of a municipal boundary. Properties are only included if a sale occurred on both sides of a given boundary segment in a given year.

there is between 20,000 and 23,000 just on the inside. This suggests, unsurprisingly, that the residential density induced by the presence of a municipality is higher than in its absence.

Next, we examine the standard outcome studied in the boundary discontinuity literature: the difference in house prices. In figure 2.5, we can see that house prices drop by about \$6,000 when crossing the municipal boundary into unincorporated county territory. Compared to estimates in the school quality valuation literature, e.g. Bayer et al. (2007), the municipal discontinuity is about one third the size of the discontinuity between two school attendance zones with a one-standard deviation difference in test scores. The figure also shows that, as expected, there is no such discontinuity in the distance to the center of the sphere of influence of the municipality.

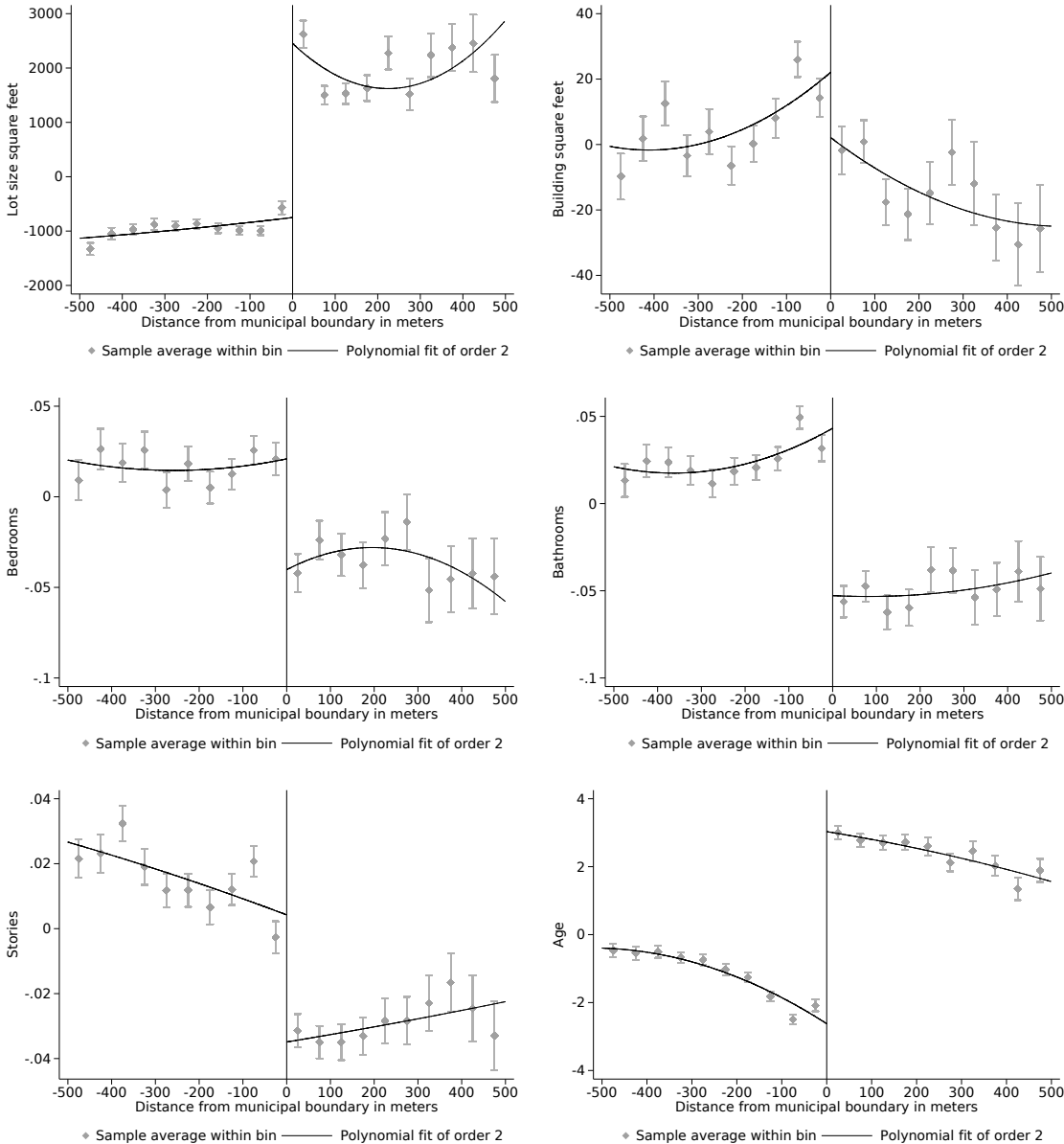
As discussed in the boundary discontinuity literature, if all other house, amenity, and neighborhood characteristics were continuous across the boundary, this price difference would

Figure 2.5: Boundary Discontinuity of Sales Price and Distance to Center



Notes: This figure plots the boundary discontinuities of property sales prices (top) and distance to center of municipal sphere of influence in meters (bottom), using only properties within 500 meters of a municipal boundary. The outcome has been residualized using boundary-segment-year fixed effects.

Figure 2.6: Boundary Discontinuity of House Characteristics



Notes: This figure plots the boundary discontinuities of the following house characteristics within 500 meters of a municipal boundary: lot size, building square feet, number of bedrooms, number of bathrooms, number of stories, and age. The outcomes have been residualized using boundary-segment-year fixed effects.

Table 2.2: Boundary discontinuity estimates.

| | Price per lot size | | Price per bldg size | |
|-----------------------------|--------------------|--------------------|---------------------|----------------|
| | (1) | (2) | (3) | (4) |
| Discontinuity at boundary | -3.44*** (0.60) | -2.68*** (0.59) | -0.52 (1.16) | 0.10 (1.18) |
| Boundary-segment-by-year FE | X | X | X | X |
| Hedonic controls | | X | | X |
| Polynomial | Linear | Linear | Linear | Linear |
| N (both sides) | 248,668 | 238,993 | 171,291 | 162,933 |
| N in municipality | 167,749 | 160,215 | 120,053 | 113,293 |
| N in county | 80,919 | 78,778 | 51,238 | 49,640 |

Standard errors in parenthesis

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

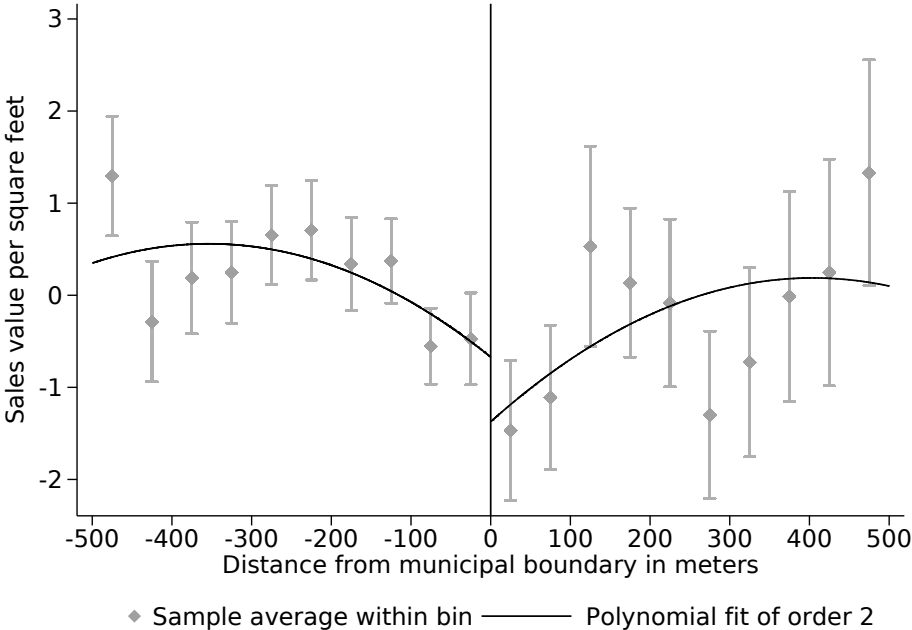
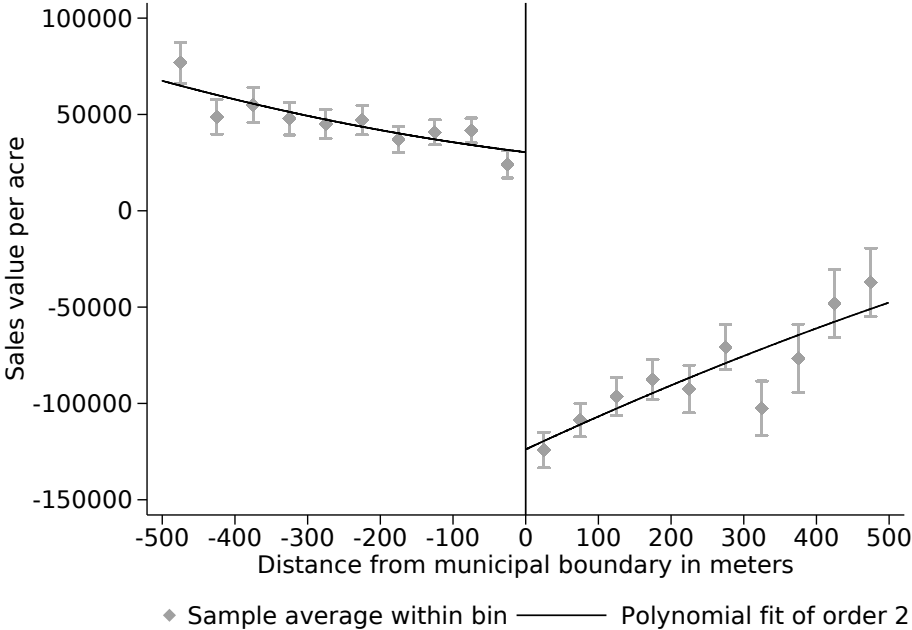
Notes: This table presents results from the robust regression discontinuity estimates using the estimator by Calonico et al. (2014). Hedonic controls include: number of bedrooms, number of bathrooms, number of stories, age, distance to city center. Standard errors are clustered at the sphere level.

correspond to the difference in service provision between the municipality and the county. However, as we can see in figure 2.6 that the housing stock is substantially different on either side of the boundary, reflecting persistent differences in housing supply quality across the municipal boundary. In particular, we see that housing structures just outside the municipality are typically smaller, with fewer bedrooms and bathrooms, fewer stories, and older; however, they are located on substantially larger lots.

We explore the apparent tradeoff between the housing structure and lot size across municipal boundaries further by plotting the sales price per lot acre discontinuity in figure 2.7. At around \$100,000, the discontinuity is about 10% of a standard deviation in sales price per lot acre. The figure also shows that no discontinuity exists in the sales price per square feet of building space. The discontinuity in sales price per acre and the lack of a discontinuity in sales price per building provides further evidence that there is a particular pattern of sorting: relatively lower land prices in the county shift housing suppliers to substitute structure quality with lot size.

To further evaluate the extent of the difference in sales prices per lot acre or building square feet, respectively, table 2.2 shows regression results of the boundary discontinuity, allowing for separate slopes on either side of the boundary. We now express both measures by square feet, such that the discontinuity can be interpreted as the price drop of a square foot of land and a square foot of structure, respectively, when crossing the municipal boundary

Figure 2.7: Boundary Discontinuity of Sales Price Per Lot Acre



Notes: This figure plots the boundary discontinuities of sales price per lot acre (top) and sales price per building square feet (bottom) within 500 meters of a municipal boundary. The outcomes have been residualized using boundary-segment-year fixed effects.

into unincorporated county territory.

Column (1) shows that, without controlling for house characteristics, the price of a square foot of land falls by \$3.44, which is about a quarter of a standard deviation (\$12.06). The difference is highly significant, clustering standard errors on the level of spheres. Once we control for the systematically poorer building quality in counties in column (2), this difference falls to \$2.68, while remaining statistically significant on a 1% level. Column (3) and (4) show the same specifications for the price per building square foot: in both specifications, we see no statistically significant discontinuity when crossing the municipal boundary, and point estimates are very small (\$0.52 and \$0.10). In the appendix in table B.2, we show the same estimates for quadratic polynomials on both sides of the boundary, with qualitatively and quantitatively similar results.

2.6 Annexation Event Study

Having established the basic equilibrium sorting patterns that take place between counties and municipalities, we now take a step towards estimating the causal effect of an extension of municipal services into unincorporated county territory. To do so, we exploit the changes in municipal boundaries over time induced by annexations. To deal with the endogeneity of annexations, we use an event study design, which allows us to control for pre-trends in outcomes before the annexation takes place.

It may be tempting to use repeated sales in individual properties as a means to identify the effect of municipalities on individual properties. However, there are two issues with this approach: first, we can only observe a cross-section of housing characteristics. This means that if a property was subdivided or if the structure was upgraded – the two most salient patterns we identified in the boundary discontinuity design – we are not able to observe these changes. Second, municipal annexations induces a host of other changes to neighborhood and amenity characteristics that are relevant for individual property valuation, in addition to the change in service provision. As we have seen, neighborhood density is likely to increase and the types of structures in the neighborhood may be very different, both of which may have ambiguous effects on prices.

Econometric Setup

Thus, we aggregate housing characteristics to the level of areas indexed by n . We then use the following event study design to evaluate the effect of municipal annexation on area characteristics:

$$y_{nt} = \alpha_n + \mu_{j(n),t} + \sum_{k=-15}^{15} \mathbf{1}[a_n + k = t] \beta_k + \varepsilon_{nt} \quad (2.2)$$

where y_{nt} is an area outcome of interest; α_n are area fixed effects; $\mu_{j(n),t}$ are sphere-by-year fixed effects; a_n is the year of annexation of area n , so that β_k measures the difference in

outcome relative to a reference event year. We use $k = -1$ as the omitted reference year, so we can interpret all effects relative to the year before annexation. ε_{nt} is an error term.

The extent to which pre-trends are absent (i.e. $\beta_k = 0$ for $k < 0$) tells us whether municipal annexation is a precursor or a consequence of changes in the outcome, relative to trends. The absence of pre-trends lends itself to a causal interpretation insofar as systematic changes in outcomes occur once municipal governance has been established in a place. We argue that this is because of the intensification of services taking place after municipal annexation and provide suggestive evidence for this line of reasoning in section 2.7.

In addition to the nonparametric evolution of changes in the aftermath of the annexation event as estimated by the event study methodology, we also use a generalized difference-in-difference design to express the magnitude in a simple one-parameter specification. To this end, we run the following regression:

$$y_{nt} = \alpha_n + \mu_{j(n),t} + \mathbf{1}[a_n \leq t]\beta + \varepsilon_{nt} \quad (2.3)$$

where variable definitions are as before. The coefficient β estimates the average difference after annexation, compared to before annexation, controlling for trends and unobserved area characteristics with the fixed effects.

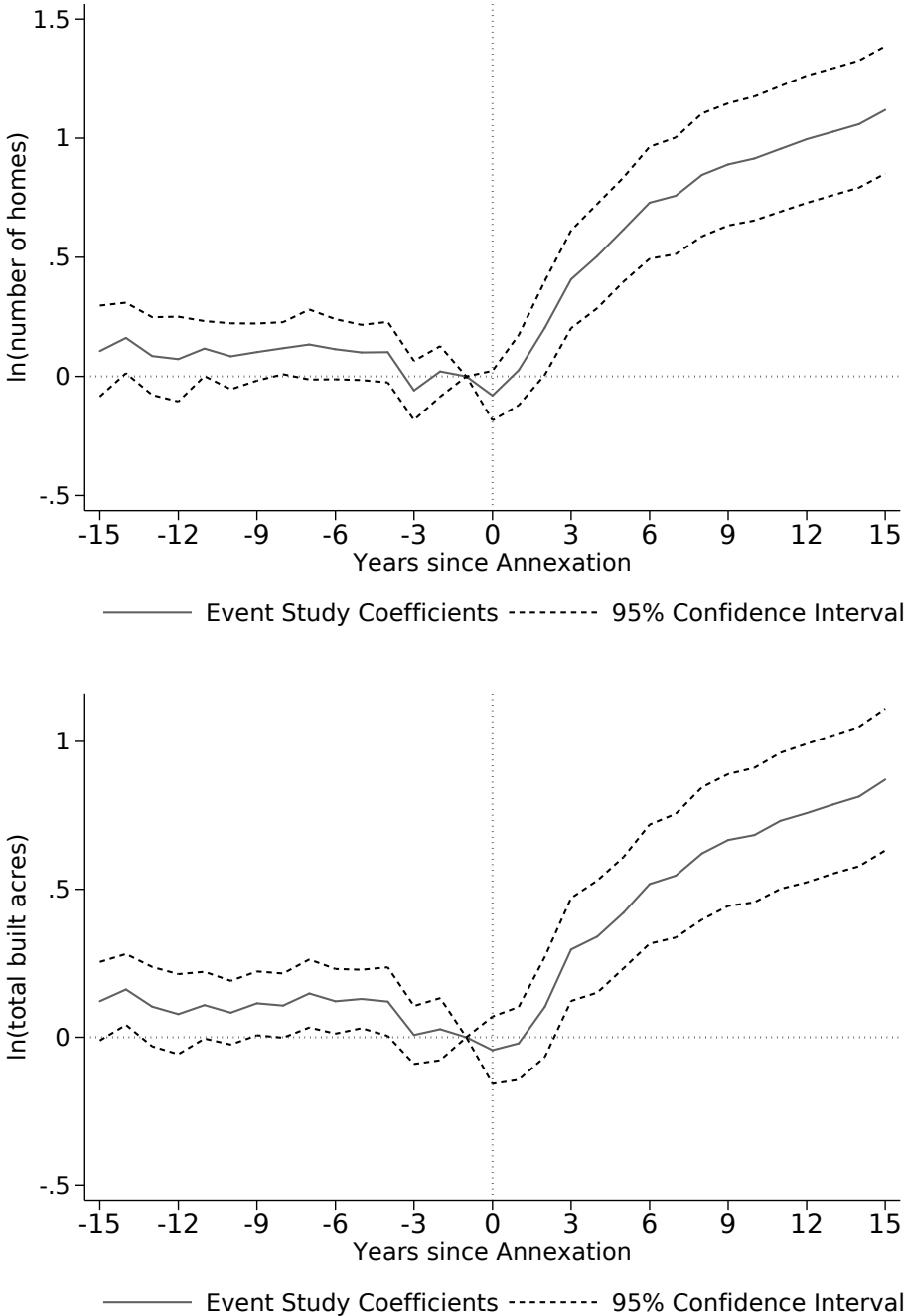
To estimate the event study and the generalized diff-in-diff, we usually include all three types of areas: those that are always in municipalities, those that are unincorporated throughout our data, and those that are annexed and thus switch from unincorporated county governance to municipal governance. Due to the set of sphere-by-year fixed effects we include in all specifications, we identify our parameters of interest from the switching areas; other areas provide precision to estimate the fixed effects.

Results

We begin our results on the causal impact of municipal governance by documenting a strong increase of housing supply starting right after – and not before – annexation. In figure 2.8 we plot the event study coefficients β_k for two outcomes measuring changes in housing supply in a given area: the log of the number of homes in an area, and the total built-up land area. We see that for both measures, the growth in housing supply is largely parallel in municipalities and counties before annexations, but once an area is annexed, its growth in housing supply rapidly accelerates, surpassing supply growth in other areas by more than 30 log points (35%) after three years, relative to before the annexation. Ten years after the annexation, housing supply in an annexed area has grown by more than 150% relative to the year before.

Table 2.3 shows the generalized diff-in-diff parameterization as well as robustness of these estimates to varying comparison groups. In columns (1) of Panel A (log number of homes) and Panel B (total built acres), we include all three types of areas – always in municipalities, always unincorporated, and annexed at some point during 1988-2013. This is the same sample we use to estimate the event study coefficients in figure 2.8. We estimate

Figure 2.8: Event Study of Number of Housing Supply on Annexation



Notes: This figure plots the event study coefficients from regressions of $\log(\text{number of homes})$ (top) and $\log(\text{total built-up acres})$ (bottom) on annexation. The observations are areas (i.e. neighborhoods defined by municipal boundary changes) for the 189 municipalities for which we observe the complete boundary change history in California.

Table 2.3: Generalized Diff-in-Diff of Housing Supply

| <i>Panel A:</i> | log(Number of homes) | | | |
|--------------------------|------------------------|---------------------|---------------------|------------------|
| | (1) | (2) | (3) | (4) |
| post-annexed | 0.396*** (0.101) | 0.423*** (0.095) | 0.397*** (0.104) | 0.139 (0.084) |
| <i>Fixed Effects:</i> | | | | |
| Area FE | X | X | X | X |
| Sphere-Year FE | X | X | X | X |
| <i>Areas included:</i> | | | | |
| Annexed | X | X | X | X |
| Always incorporated | X | | X | |
| Never incorporated | X | X | | |
| <i>Model Statistics:</i> | | | | |
| Area-year N | 156,765 | 17,798 | 147,099 | 8,432 |
| Unique area N | 9,566 | 1,215 | 8,947 | 601 |
| R-squared | 0.97 | 0.95 | 0.97 | 0.92 |
| <i>Panel B:</i> | log(Total built acres) | | | |
| | (1) | (2) | (3) | (4) |
| post-annexed | 0.242*** (0.075) | 0.281*** (0.070) | 0.237*** (0.078) | 0.087 (0.060) |
| <i>Fixed Effects:</i> | | | | |
| Area FE | X | X | X | X |
| Sphere-Year FE | X | X | X | X |
| <i>Areas included:</i> | | | | |
| Annexed | X | X | X | X |
| Always incorporated | X | | X | |
| Never incorporated | X | X | | |
| <i>Model Statistics:</i> | | | | |
| Area-year N | 155,758 | 17,722 | 146,102 | 8,394 |
| Unique area N | 9,446 | 1,203 | 8,832 | 595 |
| R-squared | 0.98 | 0.97 | 0.97 | 0.94 |

Standard errors in parenthesis

* p < 0.10, ** p < 0.05, *** p < 0.01

Notes: This table presents results from the generalized difference-in-difference regressions of the log(number of homes) (Panel A) and the log(total built acres) (Panel B) on a post-annexation indicator. Standard errors are clustered at the sphere level.

an increase in housing supply of around 40% and in total built acres of about 24% relative to before annexation. Since most of the post-event observations are within five years after the annexation event, at a point when the event study estimates are on a steep trajectory but still on a relatively low level compared to before the annexation, the post-annexation estimate is lower than most of the event study coefficients.

In columns (2), we now only include areas that are unincorporated throughout 1988-2013 and that are annexed during this period. This is the classic diff-in-diff setup with two groups in the same condition (under county governance), and one of the groups getting treated at some point during the study period. We see that coefficients are slightly larger, although we cannot reject that they are the same as in columns (1). In columns (3), we restrict to areas that are always incorporated and those that are annexed during 1988-2013. Coefficients are slightly smaller than before but again statistically indistinguishable from those in column (1). Finally, in columns (4), we restrict the sample to areas that are annexed during the study period, exploiting only the timing of annexation. Coefficients are still positive, but they are considerably smaller and statistically insignificant. This is likely due to the fact that there is too little power to pin down sphere-specific time trends without either the areas that are in the municipality or in the unincorporated county throughout 1988-2013.

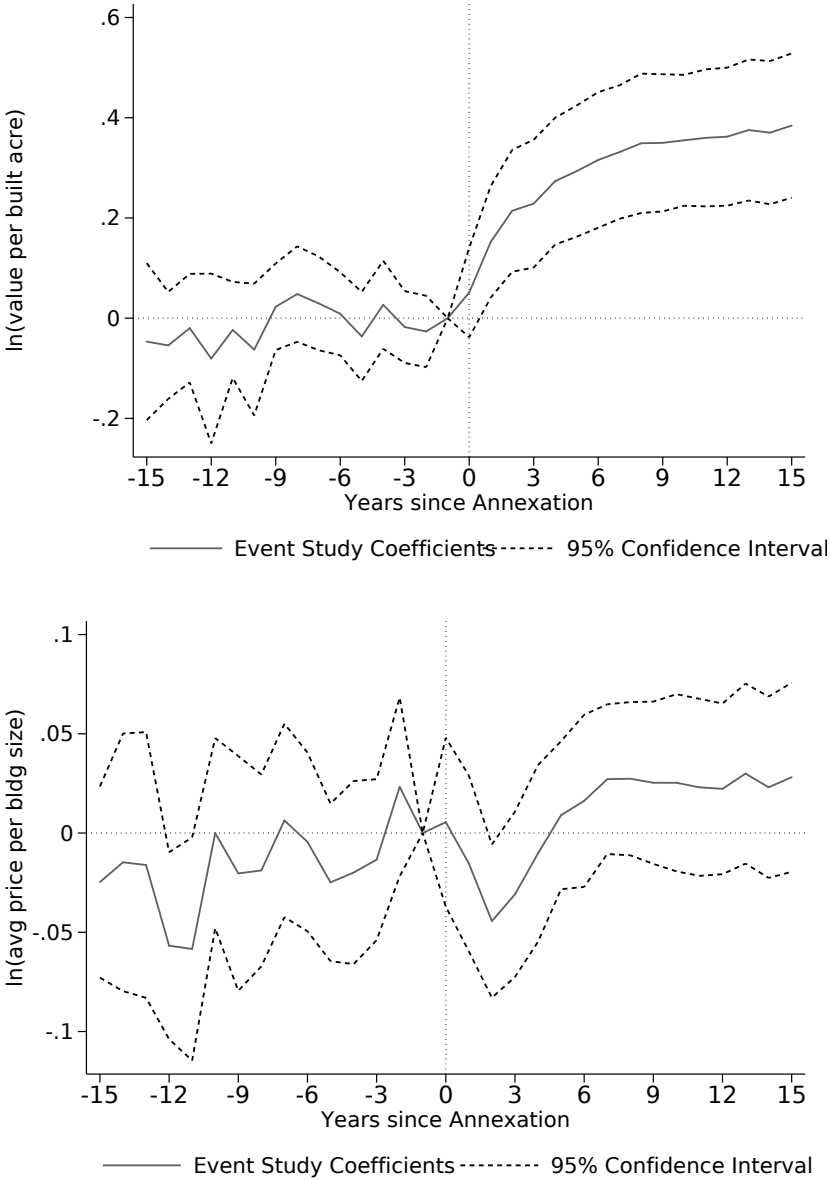
Do we see the differences in housing supply that we observe across municipal boundaries materialize over time as well? To answer this question, we run the event study using the average price per lot size and the average price per building size as outcomes. Figure 2.9 shows these results. Consistent with our findings along the municipal boundary, we see the average price per lot rise sharply after annexation, reaching around 40 log points (49%) after ten years, without any signs of pre-trends. On the other hand, the average price per building size stays largely the same after annexation.

In other words, the average price per unit of land increases due to an increase in housing supply that is skewed much more towards small lots and somewhat larger structures. This is consistent with the idea that municipalities contribute to an increase in land value due to the services and infrastructure they provide.

To quantify this change in land and building prices, we again use our generalized difference-in-difference specification. Results are shown in table 2.4. We see increases in average price per lot size of around 25% across columns (1)-(3), speaking again for a strong increase in land value in the aftermath of annexations. The estimate in column (4) using only the timing of annexation is again somewhat lower at 16%, but it is still significant and has confidence intervals that overlap with those of columns (1)-(3).

In contrast to these strong increases in average price per lot size, the estimates for average price per building size are estimated to be precise zeros. We can reject changes in price per building size larger than 2% for columns (1)-(3) and larger than 3.5% in column (4).

Figure 2.9: Event Study of Prices on Annexation



Notes: This figure plots the event study coefficients from regressions of log(average price per lot size) (top) and log(average price per building size) (bottom) on annexation. The average sales price per lot size is calculated as the sales price for each sale in a given year in a given area divided by the lot size of the respective sale; the average sales price per building size is calculated similarly.

Table 2.4: Generalized Diff-in-Diff of Land and Building Prices

| <i>Panel A:</i> | log(Average price per lot size) | | | |
|--------------------------|--------------------------------------|---------------------|---------------------|--------------------|
| | (1) | (2) | (3) | (4) |
| post-annexed | 0.258*** (0.073) | 0.239*** (0.069) | 0.266*** (0.076) | 0.156** (0.074) |
| <i>Fixed Effects:</i> | | | | |
| Area FE | X | X | X | X |
| Sphere-Year FE | X | X | X | X |
| <i>Areas included:</i> | | | | |
| Annexed | X | X | X | X |
| Always incorporated | X | | X | |
| Never incorporated | X | X | | |
| <i>Model Statistics:</i> | | | | |
| Area-year N | 151,642 | 17,271 | 142,160 | 8,198 |
| Unique area N | 9,479 | 1,205 | 8,867 | 601 |
| R-squared | 0.91 | 0.88 | 0.91 | 0.84 |
| <i>Panel B:</i> | log(Average price per building size) | | | |
| | (1) | (2) | (3) | (4) |
| post-annexed | 0.009 (0.010) | -0.009 (0.010) | 0.009 (0.010) | -0.018 (0.013) |
| <i>Fixed Effects:</i> | | | | |
| Area FE | X | X | X | X |
| Sphere-Year FE | X | X | X | X |
| <i>Areas included:</i> | | | | |
| Annexed | X | X | X | X |
| Always incorporated | X | | X | |
| Never incorporated | X | X | | |
| <i>Model Statistics:</i> | | | | |
| Area-year N | 156,992 | 17,779 | 147,351 | 8,433 |
| Unique area N | 9,624 | 1,220 | 9,004 | 605 |
| R-squared | 0.93 | 0.89 | 0.93 | 0.88 |

Standard errors in parenthesis
* p < 0.10, ** p < 0.05, *** p < 0.01

Notes: This table presents results from the generalized difference-in-difference regressions of the log(average price per lot size) (Panel A) and log(average price per building size) (Panel B) on a post-annexation indicator. Standard errors are clustered at the sphere level.

2.7 Mechanism: Upgrading Public Goods

Having shown large increases in housing supply and land values in the aftermath of annexations, we now discuss the key channel we have in mind that may be mediating these effects. Recall that municipal governance induces a number of changes along several dimensions: police protection services are handed over from the county sheriff to the city police department (in case the service is not outsourced to the county); fire protection and emergency medical services may be provided by the municipality instead of the county; properties may now be connected to the municipal sewer system instead of a septic system or a county sewer district; municipalities typically have ordinances requiring the construction of sidewalks and streetlights; and property owners now have the right to participate in municipal elections and run for office in the municipality.

As a first step to understanding this channel, we estimate the magnitude of per capita expenditure increase typically experienced in the aftermath of annexations. As a second step, we zoom in to the most important service municipalities provide: police protection. We discuss each of these in turn.

To study the effect on per capita expenditures devoted to an area before and after annexation, we proceed as follows. We construct expenditures y_{nt} for area n in year t as

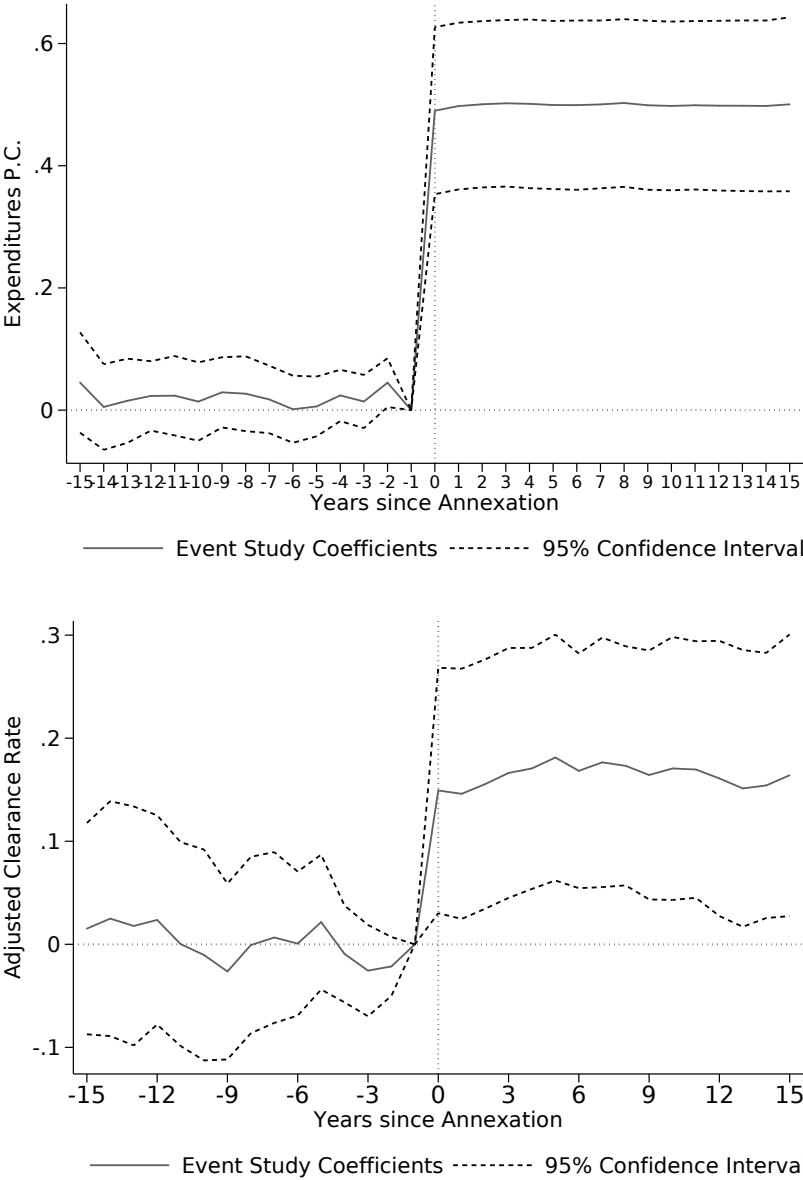
$$y_{nt} = \mathbf{1}[t < a_n]y_{j(n),t}^{\text{County}} + \mathbf{1}[t \geq a_n]y_{j(n),t}^{\text{Municipality}} \quad (2.4)$$

where $y_{j(n),t}^{\text{County}}$ and $y_{j(n),t}^{\text{Municipality}}$ are per capita expenditures by the county and the municipality, respectively. We discuss how we construct expenditure numbers for unincorporated areas in the county in the appendix. In the top panel of figure 2.10, we plot event study coefficients from this regression. The large jump in the year of annexation is not surprising given that municipalities typically spend more per person than counties do on unincorporated areas; a simple comparison of means between municipalities and unincorporated county territory would convey a similar message. However, the magnitude of around \$500 is informative and can be used to compare the change in per capita expenditures to the gain in property values in the aftermath of annexations, and the event study estimation weighs observations similarly to those of the gains in price per lot size.

To see whether municipal services may be superior to county services along similar dimensions, we compute adjusted clearance rates for both municipal and county police service providers in each city sphere. We start off with the complete history of crime counts and clearance counts for various types of crimes (both violent and property crimes) and for each type of service provider (county sheriff and municipal police). We then compute clearance rates (the share of reported crimes that get cleared, i.e. someone gets charged with having committed it) adjusted for the crime-specific likelihood of clearance.⁷ This is a direct me-

⁷In the appendix in figure B.2, we show the same graph for unadjusted clearance rates, which is qualitatively similar. Since sheriff offices are more often policing rural areas, they face very different types of crime, often with significantly higher clearance rates. This is why the adjustment makes crime rates more directly comparable.

Figure 2.10: Event Study of Public Goods



Notes: This figure plots the event study coefficients from regressions of the expenditures per capita (in thousands) (top) and adjusted clearance rates (bottom) on annexation. The per capital expenditures relevant for the area is measured for the county before annexation and the municipality afterwards. The adjusted clearance rate is measured for the county sheriff before annexation and typically the municipal police department afterwards. See the text for a description of adjusted clearance rates. We use only switching areas for the adjusted clearance rate graph.

asure of the quality of police services: for each reported crime case, it gives the adjusted probability of a perpetrator being charged for committing it.

The bottom panel in figure 2.10 provides evidence that a change in jurisdiction is associated with a change in public service quality: the adjusted clearance rate increases by about 16% in the aftermath of annexations. In other words, controlling for the likelihood of a given type of crime to be cleared, municipal police departments are significantly more likely to do so than county sheriff's offices. This is also consistent with evidence shown in Fujioka (2014), which analyzes staffing levels and emergency response times of the Los Angeles Sheriff's Office and finds that response times are about 20% longer in unincorporated areas than in municipalities.

Table 2.5 again quantifies these changes in public goods using the generalized diff-in-diff estimator. Expenditure per capita increases by around \$480 across all four sample specifications. Adjusted clearance rates are more sensitive to sample variations with estimates of rate increases of between 10% and 17%. Overall, these estimates provide evidence for changes in service intensity and quality to be responsible for the changes in housing supply and land prices we observe.

2.8 Conclusion

Municipal boundary changes via annexations provide an unexplored setting to test for the extent of Tiebout sorting and the consequences of municipal governance for housing supply, house prices, and public goods. Since municipal government and county governments provide different levels of local public goods, when an area becomes annexed, we can expect a change to both the neighborhoods in the annexed land and a change to the public goods provided to that area. We find that as a result of annexations, there is growth in housing supply and an increase in the value of land in the annexed areas. Ten years after annexation, the housing supply grows by 150% and the average price per lot size grows by 49% relative to one year before annexation. Furthermore, we find that there is also an increase in the intensity of public goods after annexation. Public goods spending per capita increase by \$500 and adjusted clearance rates, a measure of the quality of police services, increases by 16% right after annexation.

Does the increase in housing supply and the concurrent increase in land values induced by annexations pay for the increase in public goods? The answer to this question informs us about whether municipal governance is underprovided or overprovided. To give a precise answer would require us to nest our reduced form findings into the structural model of Tiebout sorting to estimate the equilibrium value of annexations.

For now, a simple back-of-the-envelope calculation may be informative: consider the \$6,000 discontinuity in sales prices at the municipal boundary, and compare this to the \$500 increase in annual per capita expenditure increase, which at 3% interest rates (about the average during the time period we study) is more than \$16,000 dollars in present discounted value terms. So the change in valuation and public goods on the individual annexed house-

Table 2.5: Generalized Diff-in-Diff of Public Goods

| <i>Panel A:</i> | Expenditures p.c. | | | |
|--------------------------|-------------------------|---------------------|---------------------|---------------------|
| | (1) | (2) | (3) | (4) |
| post-annexed | 0.481*** (0.068) | 0.514*** (0.080) | 0.488*** (0.066) | 0.472*** (0.072) |
| <i>Fixed Effects:</i> | | | | |
| Area FE | X | X | X | X |
| Sphere-Year FE | X | X | X | X |
| <i>Areas included:</i> | | | | |
| Annexed | X | X | X | X |
| Always incorporated | X | | X | |
| Never incorporated | X | X | | |
| <i>Model Statistics:</i> | | | | |
| Area-year N | 141,699 | 16,242 | 133,042 | 7,849 |
| Unique area N | 9,482 | 1,195 | 8,879 | 595 |
| R-squared | 1.00 | 0.94 | 1.00 | 0.97 |
| <i>Panel B:</i> | Adjusted clearance rate | | | |
| | (1) | (2) | (3) | (4) |
| post-annexed | 0.141** (0.063) | 0.105 (0.071) | 0.135** (0.064) | 0.171*** (0.057) |
| <i>Fixed Effects:</i> | | | | |
| Area FE | X | X | X | X |
| Sphere-Year FE | X | X | X | X |
| <i>Areas included:</i> | | | | |
| Annexed | X | X | X | X |
| Always incorporated | X | | X | |
| Never incorporated | X | X | | |
| <i>Model Statistics:</i> | | | | |
| Area-year N | 156,425 | 17,753 | 146,872 | 8,485 |
| Unique area N | 9,620 | 1,222 | 9,003 | 608 |
| R-squared | 0.94 | 0.83 | 0.98 | 0.89 |

Standard errors in parenthesis
* p < 0.10, ** p < 0.05, *** p < 0.01

Notes: This table presents results from the generalized difference-in-difference regression of expenditures per capita (in thousands) (Panel A) and adjusted clearing rates (Panel B) on a post-annexation indicator. The per capital expenditures relevant for the area is measured for the county before annexation and the municipality afterwards. The adjusted clearance rate is measured for the county sheriff before annexation and typically the municipal police department afterwards. See the text for a description of adjusted clearance rates. Standard errors are clustered at the sphere level.

hold does not justify the annexation. However, considering the increase in housing supply by 150% in the aftermath of annexations, it is possible that the additional benefits generated cover the costs of increased municipal service provision. Exploring this in more detail is left for future research.

Chapter 3

The *Good* China Syndrome: Effects of Chinese Housing Investment in the United States

3.1 Introduction

The surge of housing purchases by foreign Chinese in the U.S. over the past decade has grabbed many headlines in the press. According to the National Association of Realtors, Chinese have taken the lead among all foreign buyers of U.S. real estate, as measured by value and quantity, by a wide margin, and they tend to concentrate the purchases in certain regions such as California.¹ While these purchases have been widely reported by the media, to the best of our knowledge, no academic study has provided a formal quantification of the phenomenon and explored its implications for the U.S. real economy.² The objective of this paper is to analyze the effects of residential housing purchases by foreign Chinese on U.S. local economies, specifically local housing markets and labor markets.

To give a sense of the significance of Chinese purchases, we first show two time-series trends on housing purchase behavior by Chinese using detailed transaction-level data covering all real estate transactions in the three largest core-based statistical areas (CBSA) in

¹As an illustration, Chinese buyers spent \$28.6 billion on residential property in the U.S. in 2014, which is a 30% increase from the previous year and more than two and a half times the amount spent by Canadians, the next biggest group of foreign buyers of real estate in the U.S. Furthermore, a survey published by the California Association of Realtors found that Chinese bought 32% of homes sold to foreigners in California, and a recent RealtyTrac report found that 80% of new construction homes in the city of Irvine were sold to Chinese buyers.

²There are a couple reports that highlight the striking increase in real estate purchases by foreign Chinese in the U.S. In a special report for Asia Society, Rosen et al. (2017) show that the growth in housing purchases by foreign Chinese in the U.S. has been accompanied by a rise in the number of Chinese investors in the EB-5 visa program. A study by Simons et al. (2016) finds that Chinese investors in the EB-5 visa program are primarily interested in obtaining a green card for their children instead of actual returns to their real estate investments.

California. Figure 3.1 plots the share of purchases in the U.S. real estate market by foreigners as measured by dollar amounts over the 2001-2013 period. As shown, while the percentage of all transactions made by Chinese was roughly constant throughout the housing boom period (2001-2006) and comparable to that of other other foreigners, it began to increase sharply in 2007 and overtook all other groups as the lead foreign buyer in the U.S. market. Note, the year 2007 is when home prices in the U.S. began to slump, thus our analysis on the effects of housing purchases by foreign Chinese on U.S. local economies is also delving into the question of whether housing investments by foreign Chinese played a stabilizing role during the housing market crash of 2007-2011.³

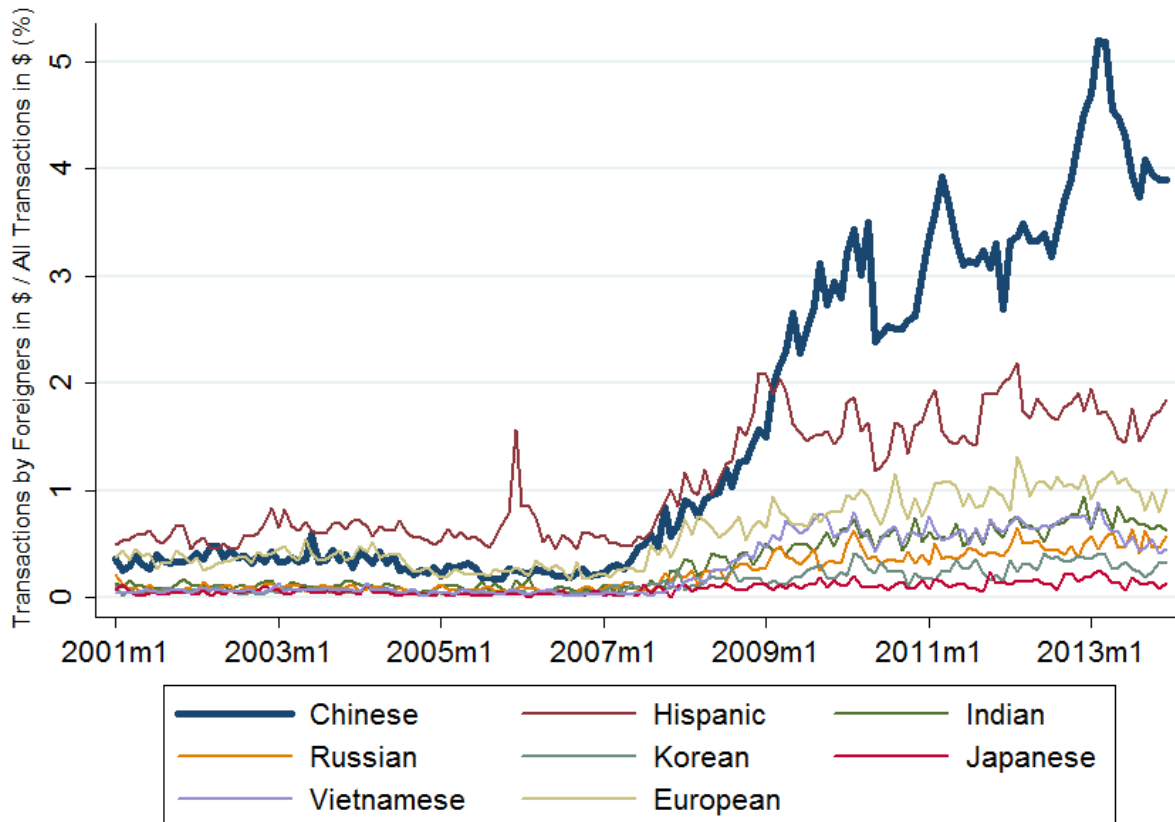
Figure 3.2 dissects Figure 3.1 by plotting the share of purchases in the U.S. real estate market by foreigners for zip codes in the top quartile of Chinese population percentage based on the 2000 Census over the 2001-2013 period. Evidently, the surge of housing purchases by foreign Chinese tend to be concentrated in zip codes that are historically populated by Chinese. As we describe in detail below, this observation helps us to deal with an endogeneity issue when empirically assessing the effects of housing purchases by Chinese on U.S. local economies.

In sum, these two trends reveal two stylized facts about purchase behavior by foreign Chinese in the U.S. housing market: 1) *House purchases by residents from China increased significantly over the 2007-2013 period relative to earlier periods*; 2) *The increase in house purchases by residents from China was concentrated in zip codes that are historically populated by Chinese*. Motivated by these two facts, we study the effects of housing purchases by foreign Chinese on U.S local economies in this paper in two steps. First, we empirically document the causal impact of these purchases on local housing markets and labor markets in the United States. Then we develop a simple model that rationalizes the empirical results by highlighting the channel through which foreign housing purchases impact U.S. local economies.

Empirically establishing the causality from Chinese purchases to local housing markets is challenging due to an endogeneity issue: it is difficult to distinguish if increasing purchases by foreign Chinese are driving up home prices or if foreign Chinese just happen to be buying in areas that are more likely to experience higher home prices. To deal with this issue, we make use of the second stylized fact by exploiting historical cross-market variation in the concentration of Chinese population across zip codes to analyze the effects of the surge in housing purchases by Chinese buyers since 2007 on local housing prices and employment. Given Chinese buyers are more likely to buy homes in neighborhoods that are populated by a higher percentage of Chinese already, we use the percentage of Chinese for each zip code in 2000 as an instrument for the volume of Chinese purchases. Since this percentage was

³Besides the housing market crash in the U.S., the 2007/2008 period is also the time that the real estate market in China began to boom significantly and the Chinese government increased the limit on how much Chinese citizens can exchange yuan to other currencies annually (up from \$20,000 to \$50,000). All of these factors likely played a role in inducing the surge of housing purchases by foreign Chinese in the United States. The focus of this paper is to understand the implications of these purchases on the U.S. economy.

Figure 3.1: Share of Housing Purchases (\$) by Foreigners



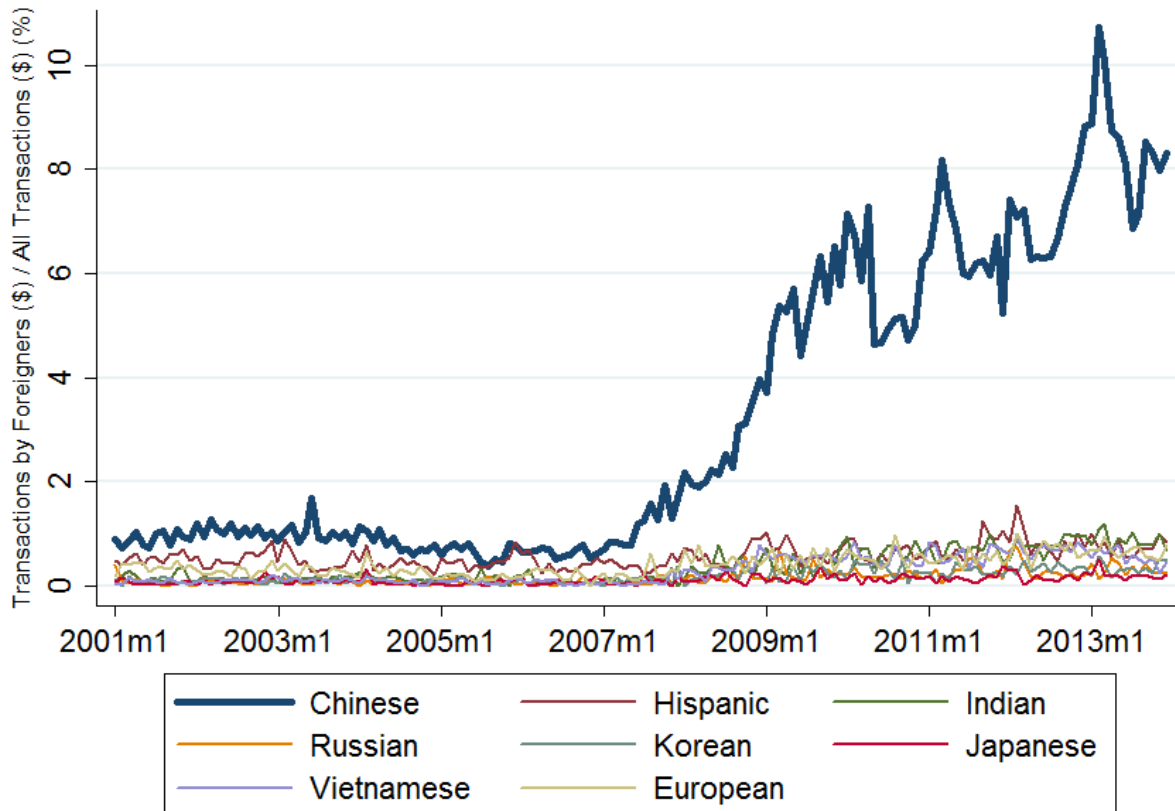
Notes: This figure plots transitions value of home purchases made by each ethnicity monthly in the 3 largest CBSAs in California between 2001 and 2013.

measured in 2000, it is unlikely to be correlated with other factors that could be driving up home prices.

Our results show that zip codes that witnessed a higher volume of real estate purchases by foreign Chinese exhibit higher increases in housing prices; in particular, during the housing market crash of 2007-2011, these zip codes experienced a lower decline in housing prices. We find that a 1% increase in Chinese transaction value causes a 0.074% increase in home prices for a zip code between 2007 and 2011 (the period of the housing market crash) and a 0.102% increase during 2012 and 2013 (the recovery period).

The increase in home prices could then impact local labor markets through the housing net worth channel, a channel first documented by Mian and Sufi (2014). An increase in housing net worth could increase employment by inducing consumer demand either through a direct wealth effect or through less binding borrowing constraints driven by the rise in collateral value. In our estimation of the housing net worth channel in the context of higher

Figure 3.2: Share of Housing Purchases (\$) by Foreigners in Top Chinese Zip Codes



Notes: This figure plots transitions value of home purchases made by each ethnicity monthly in the 3 largest CBSAs in California between 2001 and 2013 for zip codes in the top quartile based on the year 2000 Chinese population percentage.

housing prices driven by higher foreign Chinese purchasing, we find evidence of increased employment in zip codes that experienced a higher volume of foreign Chinese purchases. Our results show that 1% increase in Chinese transaction value induces a 0.102% increase in a zip code's total employment levels during the housing market crash years and a 0.149% increase during the recovery years.

We use a simple model that incorporates the housing net worth channel to aid our thinking about the economic mechanism and interpreting the empirical estimates. This model shows how a nominal shock through housing wealth affects tradable versus non-tradable employment in the local economy. A key prediction of the housing net worth channel is that changes in housing net worth should be positively related to changes in non-tradable employment and not significantly related to changes in tradable employment. The intuition is that a positive housing wealth shock through housing purchases by Chinese nationals will

increase the local demand for non-tradable goods and hence local non-tradable employment because demand for non-tradable goods are centralized in local economies, whereas the increased demand for tradable goods can be supplied by the production elsewhere. Our results support this prediction as we find that foreign Chinese purchases significantly impact employment in the non-tradable sectors but not the tradable sectors.

Our empirical analysis quantifies the effect of foreign investment of a particular form on the local economy and draws a link between international capital inflow and domestic stabilization. Broadly, it highlights the important role of investments by foreigners on the domestic employment, especially in times of economic downturns.

This paper is related to three strands of literature. First, it is related to papers that studies the impact of foreign investments on domestic local economy. A prominent example is Autor et al. (2013) who study the effect of rising Chinese import competition on U.S. local labor markets and find that such competition explains one-quarter of the aggregate drop in U.S. manufacturing employment. Other papers in this strand include Borensztein et al. (1998) and Cvijanovic and Spaenjers (2015). More related to our work, the study by Badarinza and Ramadorai (2015) examines the effects of housing demand by foreigners on domestic housing prices in London and posits that foreign demand is induced by political risks in source countries. Our paper goes beyond the effect on housing prices and examines the effects on local employment through the housing net worth channel. Second, our paper is related to the line of research on the housing net worth channel by Mian and Sufi (2014) and Mian et al. (2013). They show that deterioration in household balance sheets – the housing net worth channel – played a significant role in the sharp decline in U.S. spending and employment during the 2007-2009 financial crisis. Third, it is related to papers that estimate the effects of stabilization policies such as fiscal stimulus on local economies during economic downturns, including Ramey (2011), Nakamura and Steinsson (2014), and Chodorow-Reich et al. (2012).

3.2 Empirical Evidence

Data

We obtain data on the universe of home transactions in the state of California between 2001 and 2013 from DataQuick, who compiles all of the transaction records from each county assessor office. For each transaction, we can observe the address of the home, the transaction price, the transaction date, the buyer and seller name, and home characteristics. We specifically focus our analysis and use transactions from the three largest CMSAs in California — Los Angeles-Long Beach-Riverside, San Jose-San Francisco-Oakland, and San Diego-Carlsbad-San Marcos — as those are the areas that experienced the surge in Chinese investors. In addition we also restrict our sample to only single family residential homes because these homes were more popular among Chinese investors.

Having the name of the buyer helps us to identify the ethnicity of the buyer. With the

assistance of Bill Kerr's ethnic name-matching algorithms, we are able to identify which buyers were of Chinese ethnicity. Kerr (2008) originally created the algorithm to identify the ethnicity of inventors who were granted patents by the US Patent and Trademark Office from January 1975 to May 2009 while Kerr and Lincoln (2010) used this algorithm to investigate the impact of H-1B Visa reforms on Indian and Chinese inventors and patents. This algorithm exploits the fact that certain names are unique or more common to one ethnicity and then assigns each person the probability of being a specific ethnicity, with the sum of the probabilities summing up to 100%. If a full name is unique to one ethnicity, people with that name will be assigned with 100% to one ethnicity. For example, people named Chen and Wong are very likely to be Chinese and would be assigned with 100% probability of being Chinese. For names — especially surnames — that are common among multiple ethnicities, the algorithm uses the demographic breakdown in each MSA to assign a probability of being each ethnicity. For example, someone with the surname of Lee can be either Chinese, Korean, or American, and would be assigned the probability of being each based on the proportion of Chinese, Koreans, and Americans in that MSA. See Kerr (2008) for more comprehensive details on the names matching process and descriptive statistics from their match.

We supplement the transactions data with multiple zip code level data from other sources. The 2000 Census provides us with a historical measure of the percentage of Chinese people in each zip code. Zillow provides a home price index at the zip code level. We get income data from IRS and employment data from the Census Zip Code Business Patterns. Similar to Mian et al. (2013), we breakout employment as being tradable, non-tradable, and other by the four-digit industry classification code.

Table 3.1 presents the summary statistics for our data at the zip code-year level. In total, we have 9,986 zip code-year observations. We then break out the statistics into the housing boom (2001-2006) and housing bust (2007-2011) period. Looking at the top four rows, we can see that there was a dramatic increase in Chinese transactions over time. The average zip code had 0.8 Chinese transactions for a total value of \$0.45 million per year between 2001-2006 and 4.6 Chinese transactions for a total value of \$1.87 million per year between 2007-2011. The percentage share of Chinese transaction counts and values also increased from 0.28% to 1.92% and from 0.28% to 1.81% respectively. The bottom four rows show that the economic conditions, as measured by home prices, employment and average income, were not as different on average between the two periods.

Methodology

We are interested in understanding and quantifying the economic impact of this increase in Chinese transactions on various local economic factors. Because Chinese are buying homes and increasing the demand for homes, we would expect home prices to increase as a first order effect. In addition, through the housing net worth channel demonstrated by Mian and Sufi (2014), a change in home prices can also impact local economies. Thus, we are also

Table 3.1: Summary Statistics

| | 2001-2006 | | 2007-2011 | | N (total) |
|---------------------------------------|-----------|---------|-----------|---------|-----------|
| | Mean | SD | Mean | SD | |
| Chinese Transaction Counts | 0.80 | 2.21 | 4.62 | 10.92 | 9,986 |
| Chinese Transaction Value | 0.450M | 1.302M | 1.868M | 4.489M | 9,986 |
| Chinese Transaction Counts (%) | 0.28 | 0.73 | 1.92 | 3.62 | 9,986 |
| Chinese Transaction Value (%) | 0.28 | 0.75 | 1.81 | 3.52 | 9,986 |
| Zillow Single Family Home Price Index | 543,777 | 359,549 | 584,553 | 403,615 | 9,986 |
| Log of Non-Tradable Employment | 7.34 | 1.26 | 7.40 | 1.24 | 9,973 |
| Log of Tradable Employment | 5.88 | 1.99 | 5.70 | 2.00 | 9,736 |
| Average Household Income | 68,562 | 57,776 | 77,047 | 66,045 | 9,157 |

Notes: This table presents summary statistics and counts of key variables for both the housing market boom period and the housing market crash period. The unit of observation is at the zip code by year level.

interested in testing if regions that experienced a larger share of Chinese transactions had an increase in employment.

Figure 3.1 showed that the increase in Chinese home purchases occurred after the housing market crash in 2007 so we are particularly interested in quantifying the impacts of these purchases after 2007. We want to know if foreign investments in the US housing market helped stabilize home prices during the crash. To analyze the impact on home prices, we estimate the following regression:

$$\ln HNW_{zt} = \alpha + \beta_1 \ln CHTV_{zt} + \beta_2 \ln CHTV_{zt} \times \mathbb{I}\{year \geq 2007\} + \gamma X_z + \eta_{ct} + \varepsilon_{zt} \quad (3.1)$$

where $\ln HNW_{zt}$ is the log Zillow Home Value Index for zip code z in year t , $CHTV_{zt}$ is the log value of Chinese housing transactions, $\mathbb{I}\{year \geq 2007\}$ is an indicator variable if the year is 2007 or later, X_z are time-invariant zip code level controls, and η_{ct} are county-year fixed effects.

However, before we can run any regressions, we first need to obtain a more accurate measure of $CHTV_{zt}$. In the transactions data, we can only observe if a buyer's ethnicity is Chinese, so we do not know if that person is a foreign Chinese or an American Chinese. We perform the following three procedures in order to calculate a modified measure of $CHTV_{zt}$ that is more representative of the total value of transactions made by foreign Chinese:

1. First, we use the results of the ethnic-name matching process to determine which transactions had a Chinese buyer. In order to ensure the highest accuracy, we consider a transaction as being made by a Chinese buyer if the ethnic-name matching process assigns that buyer as 100% Chinese.

2. Next, we filter on whether a transaction with a Chinese buyer was made in cash or not because foreign Chinese cannot qualify for a U.S. mortgage when buying homes, so they must pay in cash. Any transactions with a Chinese buyer that has mortgage are assumed to be made by a Chinese American and not included in the calculation of $CHTV_{zt}$.
3. When actually calculating $CHTV_{zt}$, we need to adjust Chinese cash purchases rates for changes in cash purchase rates made by Americans. Paying cash for a home is a necessary but not sufficient condition for a purchase made by a Chinese national because American Chinese can also pay cash for a home. Thus, we need to try to remove all American Chinese cash purchases from our calculation of $CHTV_{zt}$. To do so, we assume that American Chinese behave similarly to Americans.

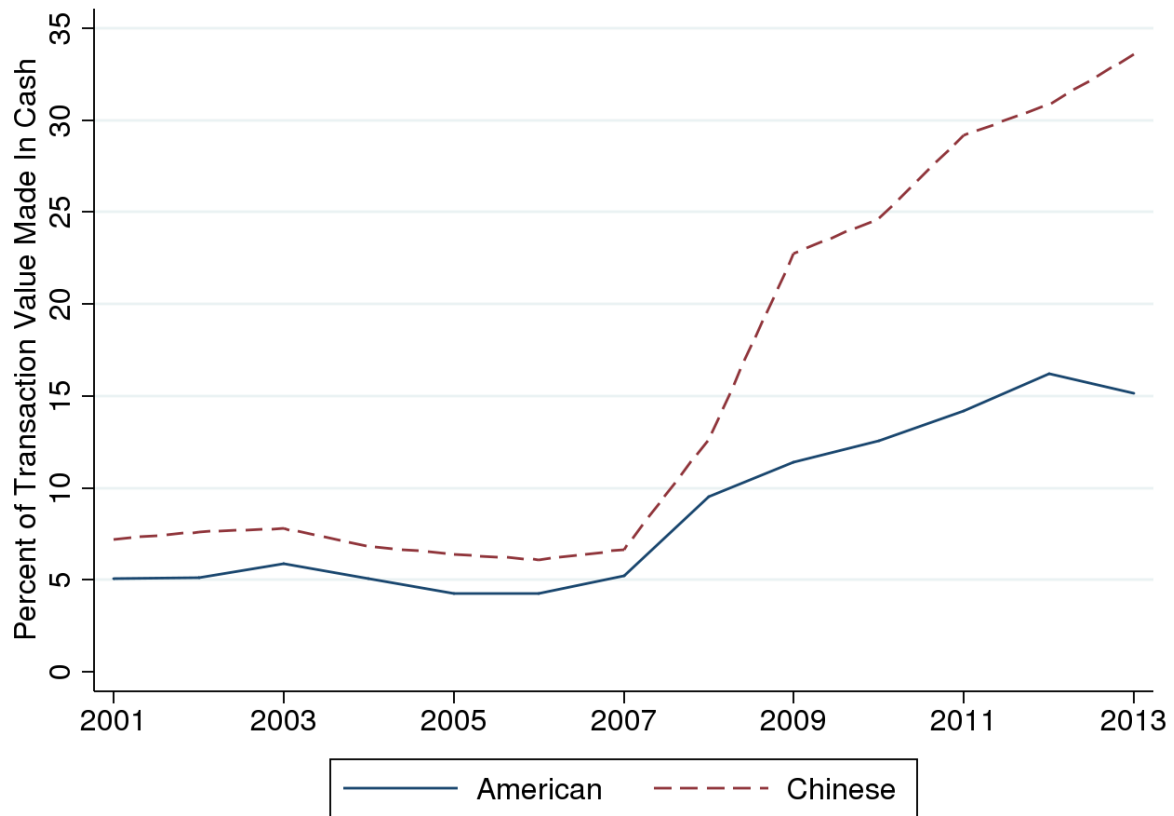
Figure 3.3 shows what percentage of all transactions are made in cash for Americans and for Chinese. The rates for the two ethnicities were comparable prior to 2007. After 2007, the probability of a cash transaction increased much faster for Chinese than for Americans, which we claim is due to the increase in cash purchases by foreign Chinese. By assuming that American Chinese behave similarly to Americans, we can calculate an adjusted measure of $CHTV$ to remove the share of cash purchases made by American Chinese by multiplying total Chinese cash transaction value by 1 minus the probability that a purchase made by an American was paid in cash for a given zip code-year.

Even after performing these three steps to calculate a more accurate measure of $CHTV_{zt}$, we still cannot simply estimate equation 3.1 without getting biased results because of the possibility of reverse causality. It is not clear if an increase in foreign Chinese purchases increased home prices or if foreign Chinese sought to buy homes in regions that were experiencing higher rates of home price appreciation. To address reverse causality, we use the Chinese population percentage reported in the 2000 Census in each zip code as an instrument for $CHTV_{zt}$. The idea behind this instrument is that foreign Chinese prefer to buy homes in regions that have a larger existing Chinese population. Because this population was measured in 2000, it is likely to be independent from factors later on in the decade that affect home prices outside of an increase in housing demand by foreign Chinese. A similar instrument has been used to study the impact of immigrants on the labor markets by Card (2001).

Figure 3.4 provides evidence that this instrument can predict foreign Chinese purchases. In each graph, we split zip codes into deciles based on the percentage of the Chinese population along the X-axis. Along the Y-axis, we normalize total $CHTV$ by total transaction value (the left graph) and total income (the right graph) in each zip code. In both graphs, we see that zip codes in the top two deciles have noticeably higher foreign Chinese housing transaction values.

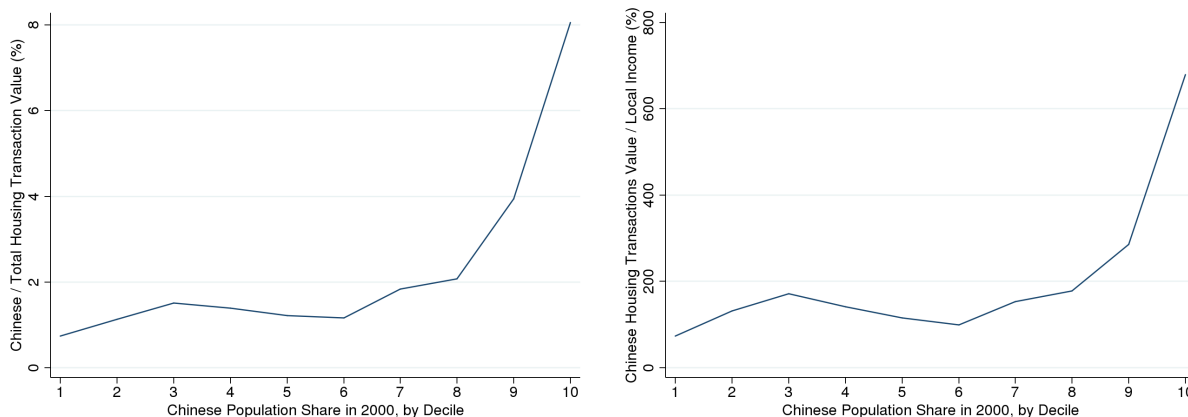
To further show the validity of this instrument, we plots the quarterly Zillow Home Value Index, normalized to 1 in 2007 quarter one, for a group of treated and control zip codes in

Figure 3.3: U.S. and Chinese Cash Purchase Trends



Notes: This figure plots the percentage of home transaction values bought in cash by both Americans and Chinese between 2001 and 2013. Homes were classified as being purchased by American or Chinese if Kerrs ethnic name matching process assigns a 100% match for the buyer. Based on the trends in this graph, we can calculate an adjusted CHTV as: $CHTV_{zt} = ChineseCash_{zt} * (1 - Prob(AmericanCash)_{zt})$

Figure 3.4: Variation in Chinese Cash Purchases by Chinese Population Percentage



Notes: This figure presents two plots of Chinese cash purchases between 2007 and 2013 for each decile of zip codes based on the year 2000 Chinese population percentage. The left figure normalizes Chinese purchases by total transaction value while the right figure normalizes it by total income.

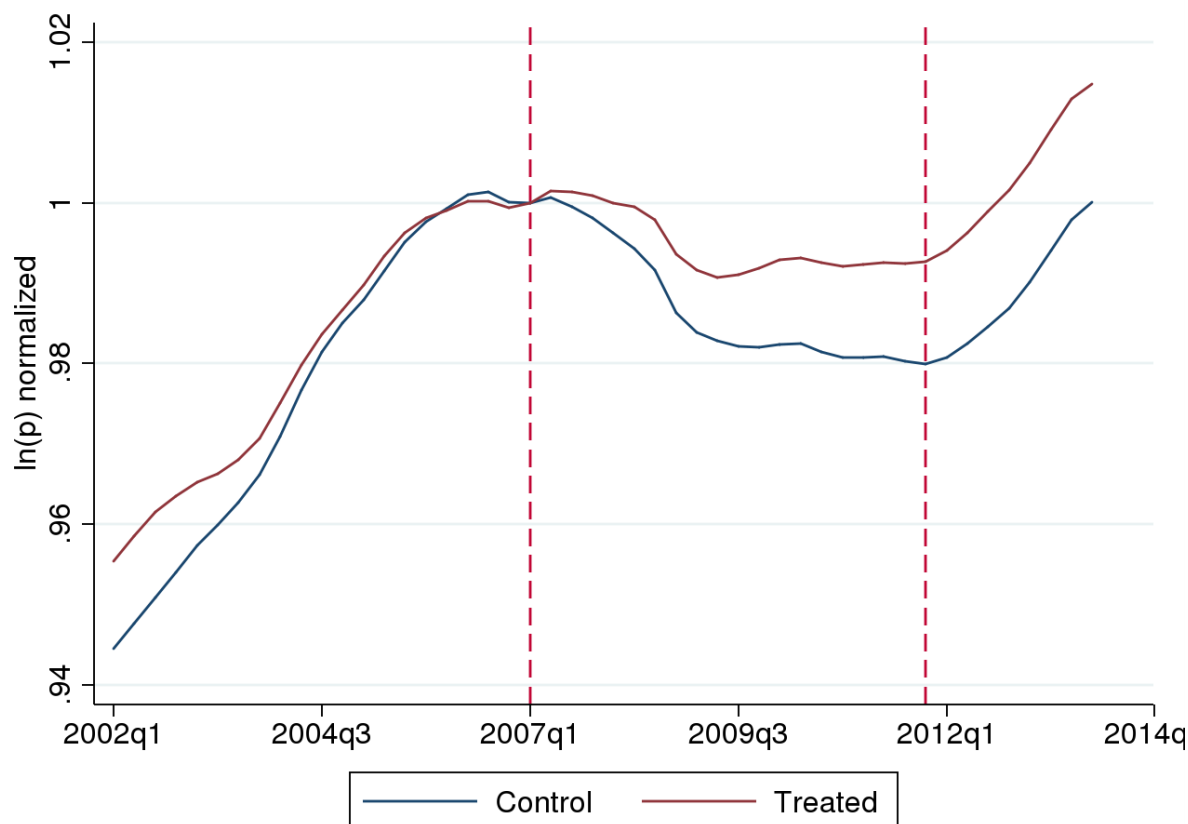
figure 3.5. The treated zip codes are those in the top two deciles by Chinese population percentage while the control ones are in the bottom eight deciles. This figure represents a basic reduced form approach by analyzing if zip codes with a higher Chinese population percentage experienced higher home prices. We can see that while both groups had similar housing booms, the treated zip codes experienced a smaller home price decline during the housing market crash.

Results

Housing Price Effects

The results of estimating equation 3.1 is presented in table 3.2. In all of our estimates, the first stage F-statistic is 85 or higher, which indicates that the Chinese population percentage is a strong instrument. The first three columns present estimates using transactions between 2001 and 2011 to analyze the impact of foreign Chinese investors on home prices during the housing market crash. Comparing column 1 to column 3 highlights in the importance of needing to control for past home price changes because these changes could be driving both future home price changes and also influencing where foreign Chinese want to buy homes. Comparing column 2 to column 3 also highlights the importance of controlling for education (measured by the percentage of the population with a bachelor degree). The estimate on the *CHTV* term in column 3 shows that changes in *CHTV* did not impact home prices at all during the housing boom years of 2001-2006. However, the positive and statistically significant coefficient on the interaction term means that on average, a 1% increase in *CHTV* increases home prices by 0.074% during the 2007-2011 period. Using a mean Zillow Home

Figure 3.5: Home Price Index Variation by Chinese Population Percentage



Notes: This figure presents the average quarterly Zillow Home Value index for a treated group and a control group. The treated group all zip codes in the top twos based on the year 2000 Chinese population percentage while the control room are the bottom eight deciles. For both groups, the index is normalized to 1 in the first quarter of 2007.

Value Index of \$584,553 during the 2007-2011 period, the 0.074% home price increase is equals to an increase of \$432.57 per home.

We can also use this estimate to roughly compare how home prices reacted across zip codes with different levels of changes in *CHTV*. Between 2007 and 2011, while the median zip code experienced an average annual increase in *CHTV* of 47%, a zip code in the 90th percentile experienced an annual increase in *CHTV* of 139%. Having a 92% higher increase in *CHTV* leads to 6.8% higher home prices for the 90th percentile zip code compared to the median zip code.

In addition to comparing the housing boom years to the housing crash years, we also compare the boom years to the recovery years of 2012 and 2013. The results in column 4 shows that a 1% increase in *CHTV* increases home prices by 0.102% during the recovery

Table 3.2: Home Price Effects

| | (1) | (2) | (3) | (4) |
|---|----------------------|----------------------|----------------------|---------------------|
| $\ln(\text{CHTV}) \times \mathbb{I}\{\text{year} \geq 2007\}$ | 0.039** (0.017) | 0.092*** (0.014) | 0.074*** (0.014) | 0.102*** (0.015) |
| $\ln(\text{CHTV})$ | 0.186*** (0.020) | -0.054*** (0.021) | 0.001 (0.021) | -0.004 (0.023) |
| $\ln(\text{Population})$ | -0.198*** (0.024) | -0.018 (0.017) | -0.043*** (0.017) | -0.031* (0.016) |
| $\Delta \ln(\text{HNW}), 00-96$ | 3.050*** (0.194) | | 1.232*** (0.204) | 1.304*** (0.242) |
| Education | | 5.222*** (0.181) | 4.134*** (0.251) | 4.250*** (0.303) |
| County Year Fixed Effects | X | X | X | X |
| Post Period | 2007-2011 | 2007-2011 | 2007-2011 | 2012-2013 |
| Model Statistics: | | | | |
| First Stage F-statistic | 124.07 | 108.98 | 98.95 | 85.53 |
| Observations | 3474 | 3712 | 3474 | 2470 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents results from the IV regression testing for impact of home purchases made by foreign Chinese on home prices as measured by the Zillow Home Value Index. Columns (1) - (3) uses transactions between 2001-2011 while column (4) uses transactions between 2001-2006 and 2012-2013. Standard errors are clustered at the zip code level.

years. During the recovery years, the median zip code experienced an annual increase in CHTV of 36% while a zip code in the 90th percentile experienced an annual increase in CHTV of 174%. This difference leads to a 14.1% difference in home prices.

Furthermore, we also estimate equation 3.1 using actual transaction prices in each zip code year for both the housing market crash years and the recovery years. Results are presented in table 3.3. Because transaction prices can be driven by both home price trends and the characteristics of the homes that sell, we also include the zip code average characteristics for the homes that sold, which includes the number of bathrooms, the square footage, and age of the home. We obtain similar results for both periods, although the coefficient estimates on the interaction term are slightly larger in magnitude compared to the estimates in table 3.2.

Table 3.3: Home Price Effects using Transaction Prices

| | (1) | (2) |
|---|----------------------|---------------------|
| $\ln(\text{CHTV}) \times \mathbb{I}\{\text{year} \geq 2007\}$ | 0.121*** (0.013) | 0.119*** (0.013) |
| $\ln(\text{CHTV})$ | -0.046*** (0.017) | -0.036** (0.017) |
| $\ln(\text{Population})$ | -0.003 (0.013) | 0.009 (0.013) |
| $\Delta \ln(\text{HTV}), 00-96$ | 0.219*** (0.073) | 0.250*** (0.077) |
| Education | 3.475*** (0.169) | 3.335*** (0.172) |
| County Year Fixed Effects | X | X |
| Post Period | 2007-2011 | 2012-2013 |
| Model Statistics: | | |
| First Stage F-statistic | 124.27 | 103.64 |
| Observations | 3699 | 2631 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents results from the IV regression testing for impact of home purchases made by foreign Chinese on home prices as measured by the average transaction values. Columns (1) - (3) uses transactions between 2001-2011 while column (4) uses transactions between 2001-2006 and 2012-2013. Home controls include number of bathrooms, square footage and age. Standard errors are clustered at the zip code level.

Housing Wealth Channel and Employment Effects

Next, we want to analyze if the increase in home prices from an increase in foreign Chinese buyers also had an impact on employment. As originally shown by Mian and Sufi (2014) and Mian et al. (2013), higher home prices can lead to higher rates of employment through the housing net worth channel. When homeowners experienced an increase in home prices, there is more equity for them to borrow against. Therefore, they are able to consume more, which in turn would stimulate the local economy and boost employment. To test for this effect, we estimate equation 3.1 with employment being the outcome variable instead of home prices.

Table 3.4 shows the estimates on aggregate employment. In columns (1)-(3), we find that zip codes experiencing more Chinese home buyers also had higher employment rates during the housing bust years. Unlike with home prices, controlling for the 1996-2000 employment changes and education level does cause the results to change. The results show that a 1%

increase in $CHTV$ increases employment by 0.102% during the housing market crash years and 0.149% during the recovery years.

Table 3.4: Total Employment Effects

| | (1) | (2) | (3) | (4) |
|---|---------------------|---------------------|---------------------|---------------------|
| $\ln(CHTV) \times \mathbb{I}\{year \geq 2007\}$ | 0.098** (0.045) | 0.103** (0.045) | 0.102** (0.044) | 0.149*** (0.051) |
| $\ln(CHTV)$ | 0.128* (0.068) | 0.021 (0.080) | 0.028 (0.079) | 0.018 (0.082) |
| $\ln(\text{Population})$ | 0.640*** (0.065) | 0.794*** (0.073) | 0.752*** (0.079) | 0.741*** (0.090) |
| $\Delta \ln(\text{Emp}), 00-96$ | 0.443** (0.198) | | 0.380* (0.197) | 0.332 (0.210) |
| Education | | 2.310*** (0.686) | 2.246*** (0.680) | 2.402*** (0.714) |
| County Year Fixed Effects | X | X | X | X |
| Post Period | 2007-2011 | 2007-2011 | 2007-2011 | 2012-2013 |
| Model Statistics: | | | | |
| First Stage F-statistic | 145.09 | 108.98 | 110.97 | 93.24 |
| Observations | 3712 | 3712 | 3712 | 2643 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents results from the IV regression testing for impact of home purchases made by foreign Chinese on total employment as measured by the average transaction values. Columns (1) - (3) uses transactions between 2001-2011 while column (4) uses transactions between 2001-2006 and 2012-2013. Standard errors are clustered at the zip code level.

To better understand how the increase in Chinese home buyers impacted employment, we split employment up into tradable and non-tradable employment based on 4-digit SIC codes like Mian and Sufi (2014). The results from the split employment regressions are reported in table 3.5. We find that a 1% increase in $CHTV$ increases non-tradable employment by 0.122% and 0.137% in the crash years and boom years respectively as shown in columns (1) and (3). However, the estimates in columns (2) and (4) show that the increase in Chinese purchases had no statistically significant impact on tradable employment. These results echo the findings of Mian and Sufi (2014) and can be explained by the fact that local demand has a stronger impact on non-tradable industries as oppose to tradable industries, which rely more on national demand.

Table 3.5: Tradable and Non-Tradable Employment Effects

| | (1) | (2) | (3) | (4) |
|--|---------------------|----------------------|---------------------|----------------------|
| | ln(NT Emp) | ln(T Emp) | ln(NT Emp) | ln(T Emp) |
| ln(CHTV) $\times \mathbb{I}\{year \geq 2007\}$ | 0.122*** (0.043) | 0.046 (0.099) | 0.137*** (0.044) | 0.144 (0.116) |
| ln(CHTV) | -0.057 (0.078) | 0.246 (0.175) | -0.060 (0.076) | 0.259 (0.181) |
| ln(Population) | 0.894*** (0.071) | 0.889*** (0.146) | 0.887*** (0.070) | 0.822*** (0.158) |
| $\Delta \ln(\text{NT/T Emp}), 00-96$ | -0.074 (0.136) | -0.153 (0.120) | -0.103 (0.129) | -0.113 (0.121) |
| Education | 2.524*** (0.655) | -4.738*** (1.352) | 2.570*** (0.611) | -5.102*** (1.453) |
| County Year Fixed Effects | X | X | X | X |
| Post Period | 2007-2011 | 2007-2011 | 2012-2013 | 2012-2013 |
| Model Statistics: | | | | |
| First Stage F-statistic | 111.49 | 107.49 | 122.57 | 90.85 |
| Observations | 3708 | 3668 | 4876 | 2607 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents results from the IV regression testing for impact of home purchases made by foreign Chinese on both tradable and non-tradable employment as measured by the average transaction values. Columns (1) - (3) uses transactions between 2001-2011 while column (4) uses transactions between 2001-2006 and 2012-2013. Standard errors are clustered at the zip code level.

One robustness check we want to perform is to make sure that there is no reverse causality between employment and Chinese purchases. We want to show that changes in foreign purchases by Chinese led to an increase in local employment and that an increase in employment did not precede changes in purchases by foreign Chinese. To do so, we estimate the following regression of ex-post Chinese purchases on ex-ante employment:

$$\ln(\text{Emp}, 01-06)_z = \alpha_0 + \beta \ln\left(\sum_{2007}^{2013} \text{CHTV}\right)_z + \gamma X_z + \varepsilon_{zt} \quad (3.2)$$

where $\ln(\text{Emp}, 01-06)_z$ is the zip code level change in employment between 2001 and 2006 and $\ln\left(\sum_{2007}^{2013} \text{CHTV}_z\right)$ is the log of the total value of Chinese purchases between 2007 and 2013 in each zip code. The results in table 3.6 show that ex-post Chinese purchases do not predict

ex-ante employment so there is no evidence of reverse causality between employment and Chinese purchases.

Table 3.6: Placebo Employment Test

| | (1) | (2) |
|-------------------------|----------------------|----------------------|
| ln(CHTV, 07-13) | -0.000 (0.009) | 0.001 (0.010) |
| ln(Population) | -0.074*** (0.028) | -0.077*** (0.030) |
| Education | | -0.049 (0.154) |
| First Stage F-statistic | 413.02 | 328.32 |
| Observations | 717 | 717 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents results from the IV regression testing for impact of total home purchases made by foreign Chinese between 2007-2013 on the growth in aggregate employment between 2001-2006. This specification is used to confirm that foreign Chinese home purchases caused a change in employment and that foreigners were not targeting zip codes had had previously experienced a home price growth. Standard errors are clustered at the zip code level.

While we have shown that an increase in Chinese home purchases leads to an increase in non-tradable employment, another way to test for the impact of the the housing net worth channel is to directly analyze how local home prices affects local employment. To do so, we estimate:

$$\ln(\text{Emp})_{zt} = \alpha_0 + \beta \ln(\text{HNW})_{zt} + \gamma X_z + \varepsilon_{zt} \quad (3.3)$$

where $\ln(\text{Emp})_{zt}$ is the log of either tradable or non-tradable employment and $\ln(\text{HNW})_{zt}$ is the log of the Zillow Home Value Index.

The results presented in table 3.7 verify the earlier results that zip codes with more foreign Chinese home purchases experience an increase in both home prices and employment. Columns (1) and (3) shows that a 1% increase in home prices increases non-tradable employment by 0.664% - 0.69%, and the effect is similar for both the housing crash and recovery periods. At the same time, column (2) shows that home prices do not have an impact on tradable employment, but column (4) suggests that during the recovery years, a 1% increase in home prices increases tradable employment by 0.418%.

The estimated effects on employment presented so far may be underestimated due to the difference in intended use between the seller of the home and the foreign Chinese buyer.

Table 3.7: Housing Net Wealth Effects

| | (1) | (2) | (3) | (4) |
|--|---------------------|---------------------|---------------------|---------------------|
| | ln(NT Emp) | ln(T Emp) | ln(NT Emp) | ln(T Emp) |
| ln(HNW) | 0.690*** (0.167) | 0.560 (0.400) | 0.664*** (0.179) | 0.406 (0.426) |
| ln(HNW) $\times \mathbb{I}\{year > 2011\}$ | | | 0.067 (0.079) | 0.418** (0.211) |
| Δ ln(NT/T Emp), 06-01 | 0.342*** (0.125) | 0.202* (0.122) | 0.341*** (0.125) | 0.202* (0.122) |
| ln(Population) | 0.925*** (0.058) | 1.178*** (0.138) | 0.924*** (0.058) | 1.174*** (0.139) |
| County Year Fixed Effects | X | X | X | X |
| Model Statistics: | | | | |
| First Stage F-statistic | 144.25 | 129.94 | 70.38 | 64.44 |
| Observations | 3406 | 3345 | 3406 | 3345 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents results from the IV regression testing for impact of home purchases made by foreign Chinese on both tradable and non-tradable employment. All specifications use transactions between 2007-2013. Columns (3) and (4) include an interaction specification for the years 2012-2013 to test for differential impacts between the housing crash period and recovery period. Standard errors are clustered at the zip code level.

Glaeser et al. (2017) have shown that vacancy rates in China are much higher than in the U.S. as Chinese buyers in China are not afraid to leave homes vacant after purchasing them; in the major Chinese cities, vacancy rates reached almost 20% in 2012. It would not be surprising if the trend were similar in the U.S. In fact, Rosen et al. (2017) find that in 2015, only about 39% of foreign Chinese home buyers in the U.S. planned to use the home as a primary residence while just 23% planned to rent it out. Higher vacancy rates can lead to an offsetting effect on employment since there may be a decline in consumption due to a smaller population in zip codes with large number of foreign Chinese buyers. However, since the net effect is estimated to be positive, the impact of home prices on employment through the housing net wealth channel must be larger than our estimates.

3.3 A Simple Model

Our simple model below follows Mian and Sufi (2014) and shows how a nominal shock through housing wealth affects tradable versus non-tradable employment in the local economy. This simple framework shows that under nominal rigidity, housing purchases by Chinese nationals during the recession will increase total employment in the local region, mainly through the effect on non-tradable employment, but the effect on tradable employment depends on the overall expenditure shock hitting the entire economy.

Baseline

Consider an economy made up of Z equally sized zip codes indexed by z . Each zip code produces two types of goods, tradable (T) and non-tradable (N). Zip codes can freely trade the tradable good, but must consume the non-tradable good produced in their own zip code. We impose the restriction that labor cannot move across islands but can move freely between the tradable and non-tradable sectors within an island. Each island has D_z units of total (nominal) consumer demand.

Preference Consumers have Cobb Douglas preferences over the two consumption goods, and spend consumption shares $P_z^N C_z^N = \alpha D_z$ and $P^T C_z^T = (1 - \alpha) D_z$ on the non-tradable and tradable good, respectively.

Output All islands face the same tradable good price, while the non-tradable good price may be county-specific since each county must consume its own production of the non-tradable good. Production is governed by a constant returns technology for tradable and non-tradable goods with labor (e) as the only factor input and produces output according to $y_z^T = b e_z^T$, and $y_z^N = a e_z^N$, respectively.

Employment Total employment on each island is normalized to 1 with $e_z^T + e_z^N = 1$. Wages in the non-tradable and tradable sectors are given by $w_z^N = a P_z^N$ and $w_z^T = b P_z^T$, respectively. Free mobility of labor across sectors equates the two wages, making the non-tradable good price independent of its zip code: $P_z^N = \frac{b}{a} P^T$.

Equilibrium Good markets clear: $y_z^N = C_z^N$ in each zip code and $\sum_{z=1}^Z y_z^Z = \sum_{z=1}^Z C_z^T$. We solve the model under the symmetry assumption that, in the initial steady state, all zip codes have the same nominal demand $D_z = D_0$.

$$\begin{aligned} \text{Prices :} \quad & P_z^{*N} = \frac{D_0}{a}; \quad P_z^{*T} = \frac{D_0}{b} \\ \text{Employment :} \quad & e_z^{*N} = \alpha; \quad e_z^{*T} = 1 - \alpha \\ \text{Wages :} \quad & w_z^{*N} = w_z^{*T} = D_0 \end{aligned}$$

Housing Net Worth Shock under Nominal Rigidity

Assume that prices and wages are rigid and stay at their steady state level. This rigidity makes the goods and labor market demand constrained. Suppose each worker owns one unit of housing (for simplicity, let's assume there is no resale of houses).

Now let us introduce heterogeneity in shock to the nominal demand. With a housing shock, the nominal demand becomes

$$D_z = D_0 - \Delta P_{H,z}.$$

The housing wealth shock depends on how much the local housing prices fall during the recession. Regions that see Chinese purchases will experience a smaller shock, compared to other regions.

Now the non-tradable employment becomes

$$e_z^N = \alpha \frac{D_c}{D_0} = \alpha \left(1 - \frac{\Delta P_{H,z}}{D_0}\right).$$

Output and employment in the tradable sector, however, depend on the average demand for tradable goods across all regions:

$$e_z^T = (1 - \alpha) \left(1 - \frac{\sum_z \Delta P_{H,z}}{D_0}\right)$$

With nominal rigidity, non-tradable employment losses depend only on the region-specific household expenditure shock, but tradable employment losses depend on the overall expenditure shock hitting the entire economy.

These predictions are consistent with the empirical results: housing purchases by Chinese nationals have a statistically significant effect on total employment, mainly via the effect on non-tradable employment.

Discussion

The simple framework above is meant to illustrate the intuition on how housing purchases by Chinese nationals affect the local employment through the housing wealth channel. In a recession, residents in regions that see an increase in real estate investment by Chinese nationals (treated regions) experience a smaller drop in their housing wealth, and demand for consumption goods (tradable and nontradable) in those regions will not decrease as much as the demand in control regions. Since nontradable goods are produced locally, nontradable employment in treated regions will drop by less than that in control regions.

Admittedly this model has made many simplifications such as assuming full nominal rigidity and not allowing workers to migrate across regions. A more general framework should relax these assumptions. Suppose workers can move across regions and sectors, subject to migration costs. Then a positive housing wealth shock in the local economy through housing

purchases by Chinese nationals will increase the local demand for non-tradable goods and hence local non-tradable employment through migration to the treated regions, whereas the increased demand for tradable goods can be supplied by the production elsewhere. So the basic intuition is similar to the simple framework above.

3.4 Conclusion

There has been a striking surge in housing purchases by foreign Chinese in the U.S. over the past decade. In this paper, we present empirical evidence on the effects of housing purchases by foreign Chinese on local economies in the United States using an instrumental variable method that exploits cross-local-area variation in the concentration of Chinese population stemming from pre-sample period differences in Chinese population settlement. We find housing investment by foreigners induces higher local area housing net wealth, and it leads to higher local employment in the non-tradable sectors. We then use a simple model that helps to provide and intuition on how housing purchases by Chinese nationals affect the local employment through the housing wealth channel.

In terms of broad implication, our evidence highlight the role of capital inflow and foreign investments on the domestic output and employment, especially in times of economic downturns. The results suggest, during the Great Recession, the improvement in household balance sheet resulting from capital inflow for housing investment in the U.S. played a mitigating role for the domestic economy.

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Appendix A

A Shortage of Short Sales: Explaining the Under-Utilization of a Foreclosure Alternative

A.1 Data Appendix

DataQuick - Home Transaction Data

DataQuick collects transaction data for each home that sells from the local assessor's office to create a nationwide data set. However, coverage is not consistent across the county. I focus my data sample on the 10 largest MSAs across America after filtering out MSAs where DataQuick coverage is lacking and limit my sample to only the largest MSA in each state. As a result, I end up with the following 10 MSAs (with the size rank in parenthesis):

- Los Angeles (2)
- Chicago (3)
- Washington DC (6)
- Philadelphia (7)
- Miami (8)
- Atlanta (9)
- Boston (10)
- Phoenix (12)
- Detroit (14)

- Seattle (15)

My data sample begins in 2004, which is when DataQuick first began flagging short sales, and ends in 2013.

I clean up duplicates in the same manner as Campbell, Giglio, and Pathak (2011). Then I drop all transactions with a 0 sale price and all non-arms length transactions except REO to lender transactions where the lender takes ownership of a home after it has been foreclosed. Additional cleanings include dropping homes that cannot be accurately geocoded, dropping homes that sold multiple times in a 30 day window, dropping homes that experienced a 4 times price change between transactions, and winsorizing home prices at the 1% and 99%.

When cleaning and tabulating the distress sales, I use the DataQuick distress indicator field to identify short sales and any foreclosure related transaction. Short sales are imputed using a proprietary DataQuick model since they may not always be reported from the assessor office. For homes that are foreclosed on, the home should then either become an REO or get sold at a foreclosure auction to a third party. After a home becomes an REO, then it can be sold as an REO to a third party. These two REO type transactions should occur back to back without any regular transactions in between. I drop homes where I observe a regular transaction immediately before the sale of an REO property or immediately after an REO to lender transaction.

Transactions-Listings Data Merge

I obtain MLS data for the same 10 MSAs that I selected for my DataQuick sample from Altos Research. Every week, Altos Research takes a snapshot of MLS to obtain listing info on all the listed homes. They assign a unique id code for each property based on the address and another unique id code based on the listing. For each snapshot, they provide the snapshot date, the listing price at that time, and the days on market during that week. If a listing is continuously active from week to week, both unique id codes will remain constant.

Home addresses are provided by both data sets and is the only field I can use to merge the two data sets. To simplify the merge, I geocode the addresses from MLS using the same address locator used to geocode DataQuick so I can match on latitudes and longitudes. The advantage of merging on latitude and longitude is that while there are different ways to write the full address of a home, geocoding produces the same coordinates, which leads to more accurate merging. For example, 555 State St can also be written as 555 State St. or 555 State Street but after geocoding the different addresses, they will all produce the same coordinates. For any homes that cannot be geocoded, then I merge on the raw address. Since the listing data does not begin until October 2007, I drop all transactions that occurred before then.

Before merging the data, I first clean up the listing data. For each continuous listing, I collapse the weekly panel into a cross section with one observation per continuous listing and record the first date of listing, starting price, beginning time on market, last date of listing, ending price and ending time on market. Each continuous listing is also given a unique identifier composed of a property id, the unit number, and a list id. Sometimes one

continuous listing may have been split into multiple listings with its own identifier in the data, especially if the address of the home is written a different way or if there is a lapse in coverage in the data. I use the time on market and listing date differences between the multiple listings to determine if they should be one.¹ I combine all these multiple listings into one by assigning them all the same unique identifier. I also combine multiple listings for the same home if the time gap between when one listing ended and the other started is less than 28 days to account for gaps in coverage.

Before merging, I also clean up street names from the MLS data so I can merge homes that cannot be geocoded. The street address should be split into 6 fields: house number, street direction, street name, street type, street post direction, and unit number. Unfortunately, the complete address is not always perfectly parsed out into these different fields so I need to clean and parse out the address as needed. I also abbreviate all street types to make it consistent with the DataQuick field.

After merging the two data sets together, I have a set of all homes that have ever been listed in the MLS data at any point, even if there is not a listing for every transaction. Then, I remove all homes that are not classified as single family homes or have a unit number in DataQuick or have multiple units in the MLS data. The resulting set of homes are the ones that I use as my merged data set to address unobserved home quality.

From this data set, I can identify the foreclosed homes that had a listing. To do so, I first remove all homes that never had a foreclosure-related transaction. Then for the remaining homes, I find the transaction that corresponds to each listing. When a listing matches to multiple transactions, I keep the transaction that occurred most recently after the listing has ended. Then for transactions that match to multiple listings, I keep the last listing to end before the transaction date. Lastly, I drop any listing that ended more than 2 years before a transaction because long foreclosure delays could cause a big time gap between the removal of a failed short sale listing and the sale of the foreclosed home. After having a one-to-one match of listing to transaction, then I label any foreclosure sale as being listed if I find the listing associated with the REO to lender transaction or foreclosure auction transaction that occurred for that foreclosed home.

ABSNet - Loan Performance Data

ABSNet has loan performance data for mortgages that are a part of private-label securitization deals. Coverage is fairly consistent across the county so I do not place any geographical restrictions. Since the focus of my study is on distressed mortgages resulting from the housing crash, I focus my sample on mortgages that originated between 2003-2007 and became 90-days delinquent between 2008 and 2013. Additional filters I apply are: use first lien loans; use loans for single family homes; use loans with LTV at origination between 20% and 100%;

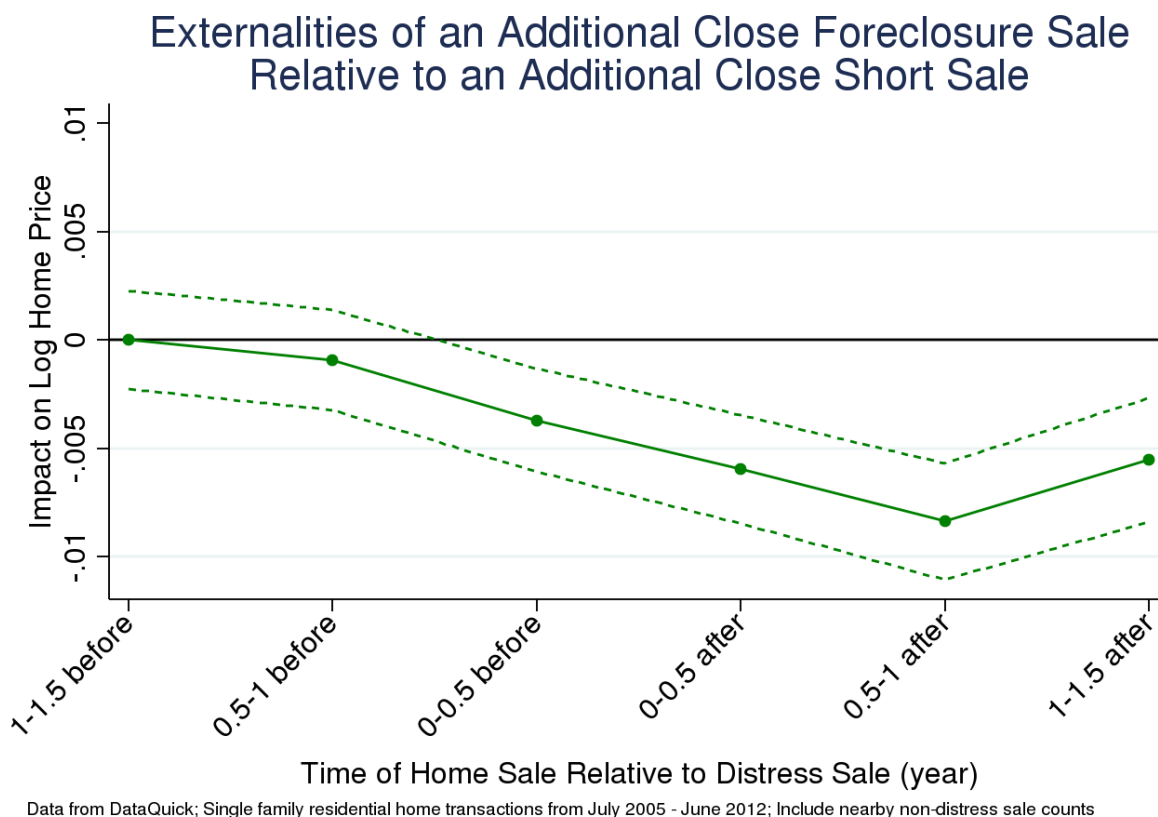
¹For example, suppose a listing for 555 State St exists from Jan 1 to Jan 29 and the time on market for this listing goes from 0 to 28 days. Then there is a listing for 555 State Street from February 5 to February 26 with the starting time on market equal to 35 days. These two listings should be the same continuous listing for the same home, but they were given two different unique ids because the street was written differently.

eliminate loans where the borrower's credit score is missing; eliminate loans in securization deals with no short sales. I also winsorize original loan balance and original interest rate at 1% and 99%.

While the data has a flag for mortgages that end in a short sale, there are none for mortgages ending in foreclosures. The data does provide dates for when a mortgage begins foreclosure, becomes REO, and is liquidated that I can use to infer foreclosures. A loan can begin foreclosure but not end in a foreclosure if the borrower is able to sell the home or resume payments before the foreclosure process ends. As a result, I only classify a mortgage as ending in foreclosure if it has either an REO date or a liquidation date or both in addition to having a foreclosure start date. I assert that mortgages with only a foreclosure start and liquidation date are for homes that sold at a foreclosure auction so the home never became an REO.

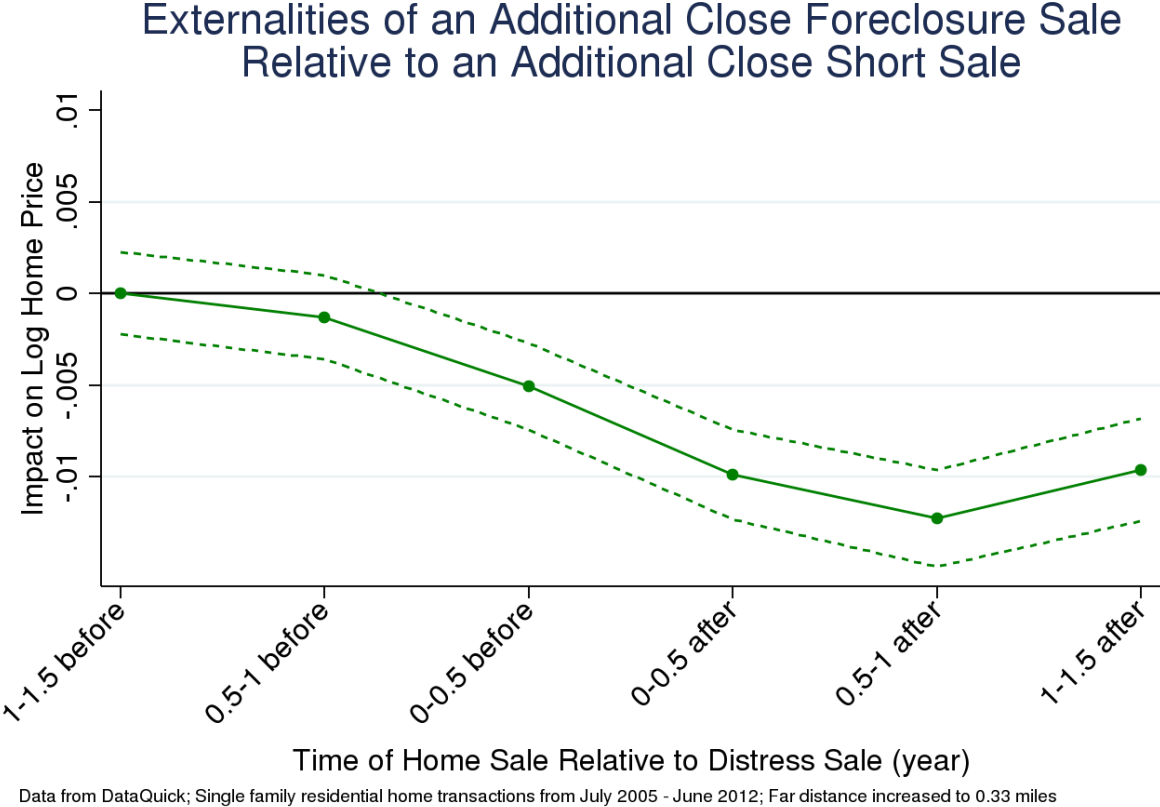
A.2 Robustness Checks

Figure A.1: Relative Foreclosure Externality - Control for all Sale Counts



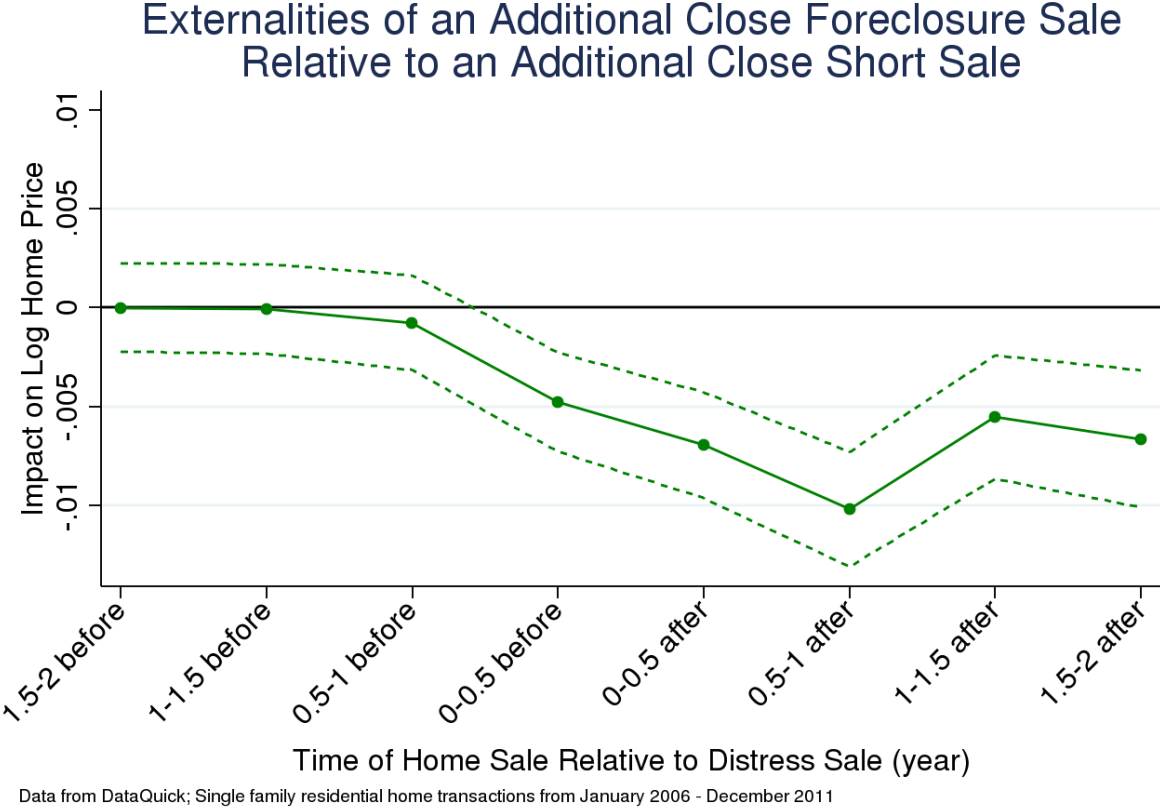
Notes: This figure presents the price externality of a foreclosure sale relative to that of a short sale by plotting the estimates and 95% confidence intervals from a regression of log home prices on close and far foreclosure sale and distress sale counts that occurred within a three year window around the sale of each home. Counts of non distressed sales at both the close and far distance are also included as controls. Close is within 0.10 miles while far is and 0.25 miles. The estimates represent how sale prices are affected by a close foreclosure sale relative to a close short sale that occurred in each six month interval relative to the sale date. All regressions include tract by half year and month fixed effects and property characteristics. Property characteristics are square footage and age and their squared terms. Standard errors are clustered at the census tract by half year level.

Figure A.2: Relative Foreclosure Externality - Far Distance at 0.33 Miles



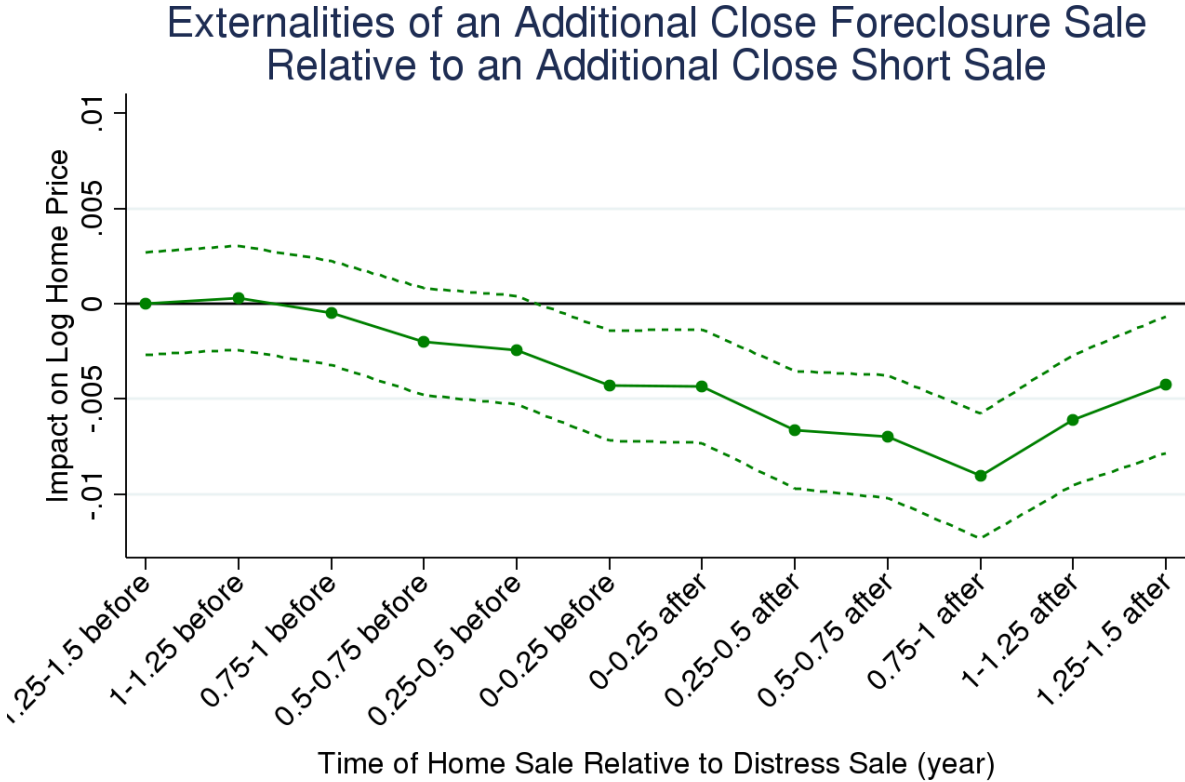
Notes: This figure presents the price externality of a foreclosure sale relative to that of a short sale by plotting the estimates and 95% confidence intervals from a regression of log home prices on close and far foreclosure sale and distress sale counts that occurred within a three year window around the sale of each home. Close is within 0.10 miles and far is within 0.33 miles. The estimates represent how sale prices are affected by a close foreclosure sale relative to a close short sale that occurred in each six month interval relative to the sale date. All regressions include tract by half year and month fixed effects and property characteristics. Property characteristics are square footage and age and their squared terms. Standard errors are clustered at the census tract by half year level.

Figure A.3: Relative Foreclosure Externality - 4 Year Window



Notes: This figure presents the price externality of a foreclosure sale relative to that of a short sale by plotting the estimates and 95% confidence intervals from a regression of log home prices on close and far foreclosure sale and distress sale counts that occurred within a four year window around the sale of each home. Close is within 0.10 miles and far is within 0.25 miles. The estimates represent how sale prices are affected by a close foreclosure sale relative to a close short sale that occurred in each six month interval relative to the sale date. All regressions include tract by half year and month fixed effects and property characteristics. Property characteristics are square footage and age and their squared terms. Standard errors are clustered at the census tract by half year level.

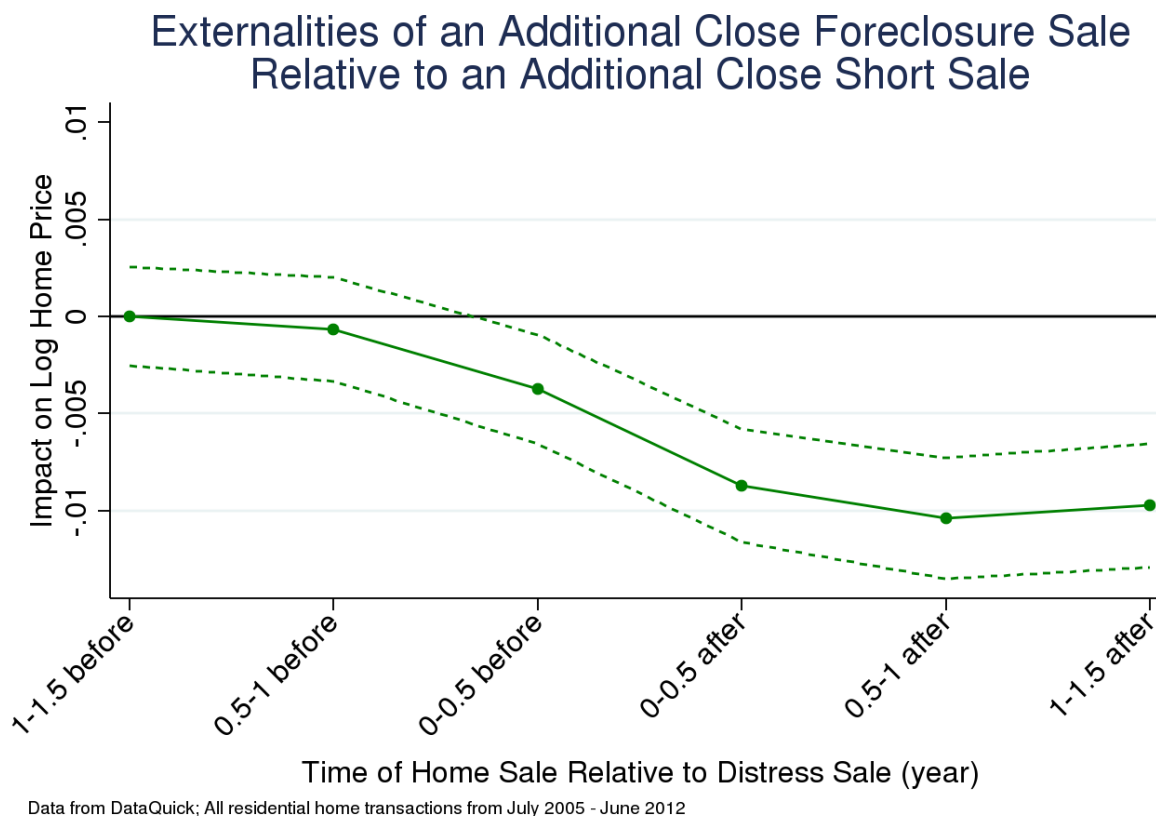
Figure A.4: Relative Foreclosure Externality - Quarterly Periods



Data from DataQuick; Single family residential home transactions from July 2005 - June 2012

Notes: This figure presents the price externality of a foreclosure sale relative to that of a short sale by plotting the estimates and 95% confidence intervals from a regression of log home prices on close and far foreclosure sale and distress sale counts that occurred within a four year window around the sale of each home. Close is within 0.10 miles and far is within 0.25 miles. The estimates represent how sale prices are affected by a close foreclosure sale relative to a close short sale that occurred in each three month interval relative to the sale date. All regressions include tract by quarter year and month fixed effects and property characteristics. Property characteristics are square footage and age and their squared terms. Standard errors are clustered at the census tract by quarter year level.

Figure A.5: Relative Foreclosure Externality - All Home Types



Notes: This figure presents the price externality of a foreclosure sale relative to that of a short sale by plotting the estimates and 95% confidence intervals from a regression of log home prices on close and far foreclosure sale and distress sale counts that occurred within a three year window around the sale of each home. Close is within 0.10 miles and far is within 0.25 miles. The estimates represent how sale prices are affected by a close foreclosure sale relative to a close short sale that occurred in each six month interval relative to the sale date. All regressions include tract by half year and month fixed effects and property characteristics. Property characteristics are square footage and age and their squared terms. All home types are included in data set, and home type fixed effects are included in the regression. Standard errors are clustered at the census tract by half year level.

Table A.1: Foreclosure Sale and Short Sale Discounts by MSA

| | (1) | (2) | (3) | (4) | (5) |
|--------------------------|----------------------|----------------------|----------------------|----------------------|----------------------|
| Foreclosure | -0.271*** (0.002) | -0.328*** (0.003) | -0.339*** (0.002) | -0.226*** (0.002) | -0.331*** (0.002) |
| Short Sale | -0.174*** (0.003) | -0.157*** (0.003) | -0.121*** (0.002) | -0.144*** (0.002) | -0.104*** (0.003) |
| MSA | Atlanta | Boston | Chicago | DC | Detroit |
| N | 739,380 | 500,265 | 497,053 | 773,343 | 398,367 |
| R ² | 0.88 | 0.86 | 0.82 | 0.89 | 0.80 |
| Foreclosure | -0.145*** (0.001) | -0.274*** (0.002) | -0.367*** (0.003) | -0.192*** (0.002) | -0.248*** (0.002) |
| Short Sale | -0.130*** (0.001) | -0.156*** (0.002) | -0.117*** (0.003) | -0.151*** (0.002) | -0.131*** (0.002) |
| MSA | Los Angeles | Miami | Philadelphia | Phoenix | Seattle |
| N | 445,669 | 327,614 | 529,912 | 435,088 | 349,359 |
| R ² | 0.80 | 0.74 | 0.84 | 0.83 | 0.80 |
| Property Characteristics | X | X | X | X | X |
| Tract by Year FE | X | X | X | X | X |
| Month FE | X | X | X | X | X |

Notes: This table presents the estimates and standard errors (in parenthesis) from a regression of log sale price on a foreclosure sale indicator and a short sale indicator to test for the discount associated with foreclosure sales and short sales split by MSA. All regressions include tract by half year and month fixed effects and property characteristics. Property characteristics are square footage and age and their squared terms. Standard errors are clustered at the census tract by half year level.

Table A.2: Foreclosure Sale and Short Sale Discounts by Property Type

| | (1) | (2) | (3) | (4) | (5) | (6) |
|--------------------------|----------------------|----------------------|----------------------|----------------------|----------------------|----------------------|
| fore | -0.265*** (0.001) | -0.262*** (0.001) | -0.259*** (0.001) | -0.214*** (0.004) | -0.358*** (0.007) | -0.228*** (0.001) |
| shortsale | -0.154*** (0.001) | -0.151*** (0.001) | -0.146*** (0.001) | -0.154*** (0.005) | -0.171*** (0.008) | -0.139*** (0.001) |
| Property Characteristics | X | X | X | X | X | X |
| Tract by Year FE | X | X | X | X | X | X |
| Month FE | X | X | X | X | X | X |
| Property Type FE | | X | | | | |
| Property Type | All | All | Single Family Res | Dup, Trip, Quad | Apartment | Condo |
| N | 7,095,948 | 7,095,948 | 4,899,854 | 116,745 | 87,674 | 1,923,065 |
| R ² | 0.85 | 0.86 | 0.88 | 0.87 | 0.86 | 0.90 |

Notes: This table presents the estimates and standard errors (in parenthesis) from a regression of log sale price on a foreclosure sale indicator and a short sale indicator to test for the discount associated with foreclosure sales and short sales split by MSA. All regressions include tract by half year and month fixed effects and property characteristics. Property characteristics are square footage and age and their squared terms. Standard errors are clustered at the census tract by half year level.

Appendix B

Municipal Governance and Annexations in Tiebout Equilibrium

B.1 Data Sources and Preparation

Boundary Data

To obtain the boundary change data, we contacted individual county and municipal offices, who sent us the change shapefile if available. We then standardized all these files by combining them into a single shapefile showing the municipality, the county, the type (annexed or unincorporated), the year of annexation (if annexed), and the acreage. This shapefile provided the basis to which we matched the property locations. Table B.1 shows a list of the counties from which we obtained data as well as the share of spheres we obtained for each of them.

In total, we obtained 189 spheres from across California. Figure 2.3 shows San Jose an example of a sphere with the location of properties. Figure B.1 shows a sample of other spheres. It can be seen that most spheres have unincorporated areas, many of which have properties in them.

Public Goods Data

To construct a measure of per capita expenditures in unincorporated county territory, we do the following: we sum county expenditures for police protection, fire protection, and library services, all of which are generally targeted at unincorporated areas, and divide them by the population in the unincorporated areas. For per capita expenditures in the municipality, we divide total municipal expenditures by the population in the municipality.

To construct adjusted clearance rates, we proceed as follows: For types of crime $k = 1, \dots, K$ and providers $j = \{\text{County, Municipality}\}$ across all reporting agencies (both police

Table B.1: List of Counties in Data

| <i>County</i> | <i>Spheres in Data</i> | <i>Spheres in County</i> | <i>Share</i> |
|-----------------|------------------------|--------------------------|--------------|
| Los Angeles | 88 | 88 | 100% |
| Riverside | 28 | 28 | 100% |
| Kern | 11 | 14 | 79% |
| Ventura | 10 | 10 | 100% |
| Stanislaus | 9 | 9 | 100% |
| Santa Barbara | 8 | 9 | 89% |
| Tulare | 8 | 8 | 100% |
| San Joaquin | 7 | 7 | 100% |
| San Luis Obispo | 7 | 7 | 100% |
| Placer | 6 | 6 | 100% |
| San Bernardino | 3 | 24 | 13% |
| Sonoma | 2 | 9 | 22% |
| Santa Clara | 2 | 15 | 13% |
| Butte | 2 | 5 | 40% |
| Fresno | 2 | 14 | 14% |
| Sacramento | 1 | 7 | 14% |
| San Diego | 1 | 18 | 6% |
| Orange | 1 | 34 | 3% |

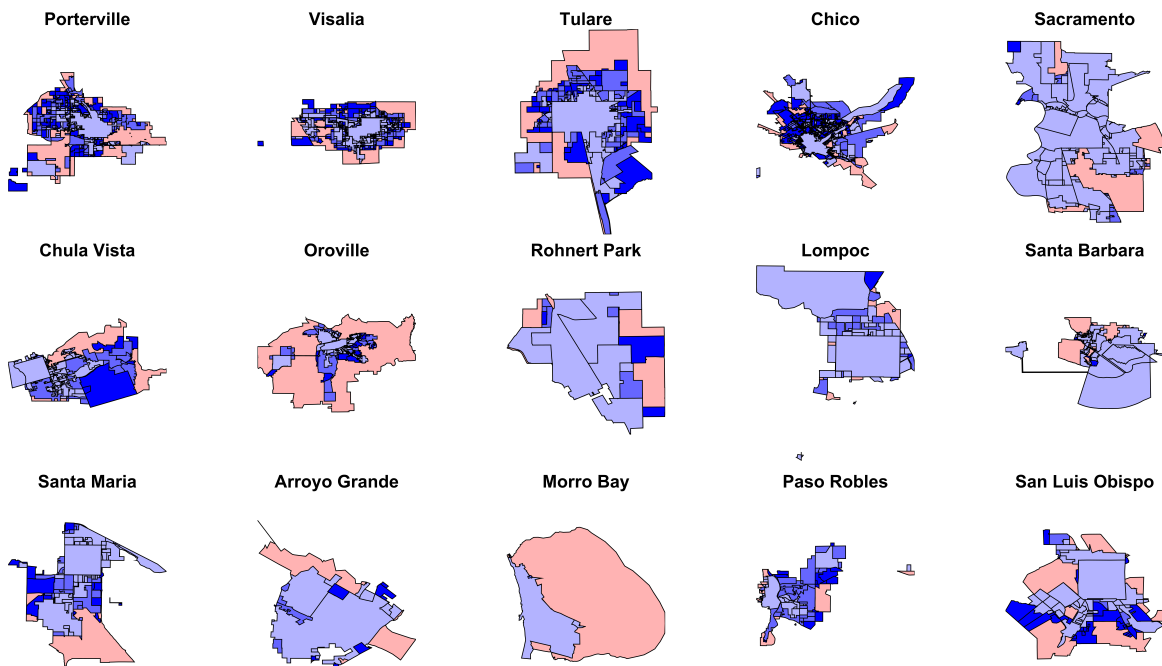
Notes: This table presents the list of counties in the data. Spheres shows the number of spheres in the data. Share is the share of all spheres that exist in the county.

and sheriff) $c = 1, \dots, C$ in California for each year $t = 1985, \dots, 2013$, we compute

$$r_{kt} = \frac{1}{K} \sum_{c=1}^C \frac{x_{ckt}}{y_{ckt}}$$

where x_{ckt} is the number of cleared cases reported by agency c of crime k in year t , and y_{ckt} is the number of reported crimes. Thus, r_{kt} is an adjusted clearance rate of crime k in year t across all agencies. The adjustment takes care of the fact that some crimes are harder to clear than others, such as theft.

Figure B.1: Other Example Maps of City Spheres in the Data



Notes: The figure shows maps of various city spheres in the data. The color scheme is the same as in the San Jose map: red areas are unincorporated throughout the period of study; shades of blue denote different periods of annexation, with the darkest being annexations within the last 20 years.

B.2 Robustness of Results

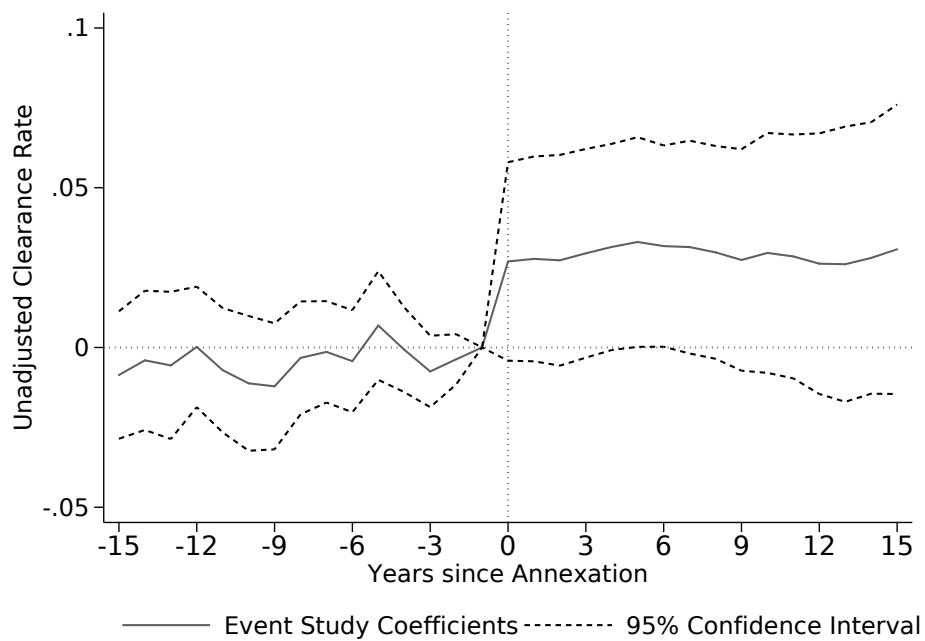
Table B.2: Boundary Discontinuity Estimates with Quadratic Polynomials

| | Price per lot size | | Price per bldg size | |
|-----------------------------|--------------------|--------------------|---------------------|----------------|
| | (1) | (2) | (3) | (4) |
| Discontinuity at boundary | -3.01*** (0.72) | -2.86*** (0.66) | 0.82 (1.31) | 1.53 (1.35) |
| Boundary-segment-by-year FE | X | X | X | X |
| Hedonic controls | | X | | X |
| Polynomial | Quadratic | Quadratic | Quadratic | Quadratic |
| N (both sides) | 248,668 | 238,993 | 171,291 | 162,933 |
| N in municipality | 167,749 | 160,215 | 120,053 | 113,293 |
| N in county | 80,919 | 78,778 | 51,238 | 49,640 |

Standard errors in parenthesis
* p < 0.10, ** p < 0.05, *** p < 0.01

Notes: This table presents results from the robust regression discontinuity estimates with quadratic polynomials using the estimator by Calonico et al. (2014). Hedonic controls include: number of bedrooms, number of bathrooms, number of stories, age, distance to city center. Standard errors are clustered at the sphere level.

Figure B.2: Event Study of Unadjusted Crime Clearance Rates



Notes: This figure plots the event study coefficients from a regression of the unadjusted clearance rate of the responsible jurisdiction. The unadjusted clearance rate is measured for the county sheriff before annexation and typically the municipal police department afterwards. We again use only switching areas for this graph, for the same reason as before.