Stimulating Housing Markets*

David Berger
Northwestern University and NBER
david.berger@northwestern.edu

Nicholas Turner
Office of Tax Analysis
nicholas.turner@treasury.gov

Eric Zwick
Chicago Booth and NBER
ezwick@chicagobooth.edu

July 2016

Abstract

This paper studies temporary policy incentives designed to address capital overhang by inducing asset demand from buyers in the private market. Using variation across local geographies in ex ante program exposure and a difference-in-differences design, we find that the First-Time Homebuyer Credit induced a cumulative increase in home sales of at least 382 thousand, or 7.4 percent, nationally. We find little evidence of a sharp reversal of the policy response; instead, demand appears to come from several years in the future. The program likely sped the process of reallocating homes from distressed sellers to high value buyers and stabilized house prices. The response is concentrated in the existing home sales market, implying the stimulative effects of the program were less important than its role in accelerating reallocation.

*We thank Andrew Abel, Jediphi Cabal, Erik Hurst, Anil Kashyap, Ben Keys, Adam Looney, Matt Notowodigdo, Amit Seru, Johannes Stroebel, Amir Sufi, Joe Vavra, Rob Vishny, Owen Zidar and seminar and conference participants for comments, ideas, and help with data. Tom Cui, Prab Upadrashta, and Iris Song provided excellent research assistance. The views expressed here are ours and do not necessarily reflect those of the US Treasury Office of Tax Analysis, nor the IRS Office of Research, Analysis and Statistics. Zwick gratefully acknowledges financial support from the Neubauer Family Foundation, Initiative on Global Markets, and Booth School of Business at the University of Chicago.

A classic debate in economics concerns how policy should respond to periods of capital overhang following investment booms (Hayek, 1931; Keynes, 1936). When booms coincide with credit expansions, high valuation potential buyers often cannot finance distressed asset purchases in the subsequent slump (Shleifer and Vishny, 1992). In this case, an overhang leads to fire sales and inefficient liquidation, amplifying the slump through debt-deflation dynamics and creating a role for welfare-improving policy intervention (Fisher, 1933; Kiyotaki and Moore, 1997; Lorenzoni, 2008; Eggertsson and Krugman, 2012).

This problem reemerged in the aftermath of the Great Recession, with the housing market suffering extraordinary distress as shown in Figure 1. As house price growth slowed, a shortage of prospective buyers for new homes caused housing inventory to double from 2004 to mid-2006 and remain at historic levels in 2007 and 2008. The boom coincided with a rapid and widespread increase in household debt secured by real estate (Mian and Sufi, 2015). When house prices began to fall, defaults, foreclosures, and further downward pressure on prices ensued (Mian and Sufi, 2009; Campbell, Giglio and Pathak, 2011; Guren and McQuade, 2015). By mid-2008, a dramatic shift in the composition of home sales had taken place, with nearly forty percent of home sales classified as distressed or foreclosure sales and vacancies at or near all time highs.

The debt-induced overhang in the housing market prompted many policy proposals and responses, primarily in the form of debt renegotiation interventions designed to repair household balance sheets, government asset purchase programs designed to support financial markets, and monetary and fiscal policy designed to spur demand growth. However, these policies do not directly target the problem of capital overhang, nor do they promote reallocation when assets are no longer in the hands of their first best users.

This paper considers an alternative policy designed to induce demand for assets through providing temporary tax incentives for buyers in the private market. The policy we study, the First-Time Homebuyer Credit (FTHC), was a temporary, refundable tax credit for new homebuyers between 2008 and 2010. We combine data from administrative tax records with transaction deeds data to measure program exposure and housing market outcomes for approximately 9,000 ZIP codes accounting for 69 percent of the US population in 2007. We use a difference-in-differences research design to estimate the effect of the policy on home sales and the housing market more broadly.

We present four main findings. First, the policy proved very effective at spurring home sales. We estimate the FTHC raised home sales during the policy period by at least 157 thousand within sample and 382 thousand nationally. Second, we find little evidence that the surge in home sales induced by the credit reversed following the policy period. Instead, demand appears to come from several years in the future. Third, the policy response came primarily in the form of existing home sales, implying the direct stimulative effects of the program were small. Fourth, we present evidence that the program likely accelerated the process of reallocation from low value sellers to high value buyers, and the health of the housing market, as reflected in house prices, improved accordingly.

The first part of the paper documents the effect of the FTHC on home sales and presents a number of robustness tests. The research design compares ZIP codes at the same point in time whose exposure to the program differs. We define program exposure based on the number of potential first-time homebuyers in a ZIP code, proxied by the share of people in that ZIP in the year 2000 who are first-time homebuyers. Places with few potential first-time homebuyers serve as a "control group" because the policy does not induce many people to buy in these places. The key threat to this design is the possibility that time varying, place-specific shocks are correlated with our measures of program exposure.

We assess this threat in a number of ways. First, graphical inspection of parallel trends indicates smooth pretrends, clear breaks during the policy period and spikes at policy expiration, and a reversion to pretrends in the post-policy period. Second, estimates are robust to the inclusion of state or, in our default specification, CBSA-by-time fixed effects. Third, our results are consistent across different specifications, with varying control sets, weighting schemes, and sample definitions. Fourth, our results are driven by activity in the "starter home" market, with sales in homes with 1 to 3 bedrooms responding strongly while sales in the 4+ bedrooms market do not respond at all. This test provides a within-time placebo that complements the pre- and post-policy trends in confirming the design's robustness. Last, the age distribution of first-time homebuyers shifts considerably toward younger buyers in 2009 and subsequently reverts to its long run average, a pattern that cannot be explained by place-by-time trends.

In the second part of the paper, we explore the value of the FTHC program as housing market stabilizer. Relying on the detail available in housing transaction data, we show that many transactions during the policy period involved sales by investors and institutional sellers, who were likely to be low utility users of the assets. More than a quarter of the homes sold during this time came out of foreclosure or real estate owned (REO) portfolios from financial institutions and government sponsored entities. Similarly, sixteen percent of homes were built in the preceding one to three years and sold by builders or developers during the policy period, and thus were likely vacant before being sold.

Furthermore, many buyers induced by the program were constrained by down payment requirements that the credit helped relax.[1] During 2009 alone, more than 780,000 homebuyers took advantage of low down payment loans insured by the Federal Housing Administration (FHA), despite these loans carrying significantly higher net present value costs. Down payment constraints can also explain why we fail to find evidence of a sharp reversal after the policy expires: absent the policy, induced buyers must wait until they have accumulated the necessary down payment as savings. These facts are consistent with the idea that debt-induced capital overhangs are times when potential high value buyers are unable to finance welfare-improving reallocations in the absence of policy intervention.

Last, we examine the stability of the policy-induced reallocation and find that, although many policy period buyers bought with high loan-to-value ratios, they were not more likely to default in the subsequent three years than other cohorts of homebuyers. Also consistent with a reallocation toward higher value users, the program slowed house price declines in places with higher exposure. The fact that housing demand was being pulled from years rather than months in the future lends further evidence of the program's medium run stabilizing effects.

Our paper contributes to the empirical literature on policy responses to distress in debt markets, especially policies motivated by the Great Recession (Agarwal et al., 2012, 2015). Relative to these, we focus on how policy can address capital overhangs by accelerating reallocation, which is typically slow during periods of industry decline or macroeconomic weakness (Ramey and Shapiro, 2001; Eisfeldt and Rampini, 2006; Rognlie, Shleifer and Simsek, 2014). Our paper complements studies that estimate the effects of fiscal stimulus by contributing a new analysis of an important durable goods stimulus program (Adda and Cooper, 2000; House and Shapiro, 2008; Mian and Sufi, 2012; Green et al., 2014; Berger and Vavra, 2015; Zwick and Mahon, 2016). Taken together, these studies demonstrate how the reversal of durable goods stimulus programs depends on which activity is targeted and who the marginal buyers are. Brogaard and Roshak (2011) and Hembre (2015) conduct policy evaluations of the FTHC. While we share a similar focus on the FTHC, our paper features a stronger research design, higher data quality, and a different scope, as these papers do not focus on the macroeconomic implications of the FTHC and do not study reallocation. Best and Kleven (2015) study the effect of stamp duty taxes notches and temporary tax holidays on housing sales in the UK and find similar effects on home sales. Their study does not explore the broader effects on housing market health and does not consider the question of policy responses to capital overhang.

[1] We explore this fact and the implications for theories of intertemporal demand for durables in a companion paper (Berger, Turner and Zwick, 2016).

1 Policy Background

The First-Time Homebuyer Credit (FTHC) was a temporary stimulus policy introduced in the US between 2008 and 2010 with the aim of supporting weak housing markets. There were three versions of the credit. The first version, enacted on July 30, 2008, in the Housing and Economic Recovery Act, provided an interest-free loan of up to $7,500 on qualifying home purchases made between April 9, 2008, and June 30, 2009. To be eligible for this version of the credit, a single (married) taxpayer needed a modified adjusted gross income below $75,000 ($150,000) and must not have owned a principal residence during the 3-year period preceding the purchase date.

The second version of the credit was enacted on February 17, 2009, as part of the American Recovery and Reinvestment Act. The policy window was extended to include purchases made up to November 30, 2009. Importantly, the maximum credit increased to $8,000 and was changed from an interest-free loan to a refundable tax credit. This feature significantly increased the value of the credit to potential home buyers.

The third version of the credit was enacted on November 7, 2009, as part of the Worker, Homeownership, and Business Assistance Act. The policy window was extended to include purchases closing before July 1, 2010. The expanded policy also added a $6,500 long-time homebuyer credit (LTHC). To qualify for the LTHC, an individual must have owned and used the residence as his or her principal residence for a consecutive five year period during the eight years prior to the date of the new purchase. For both the FTHC and LTHC, the third version raised the income limits so that eligibility began to phase out for a single (married) taxpayer with modified adjusted gross income above $125,000 ($225,000).

To claim the credit, tax filers needed to note the FTHC on their income tax returns (Form 1040) and attach an additional credit claim (Form 5405). Claimants also needed to provide documentation demonstrating the purchase of a home during the policy window, together with mailing documents supporting the claim's eligibility for the credit to the appropriate IRS office.[2] To accelerate payment, filers could amend previously filed tax returns, for example by amending the 2008 tax return for a home bought in 2009.

We focus on the second and third versions of this policy. We do so for two reasons. First, these versions of the credit were considerably more generous and thus more likely to induce new purchases. Assuming a 3 percent real rate of return, the interest free loan was worth $1,400 dollars in present value; the later versions were worth 5.7 times as much. Second, the

[2] Such documents included the settlement statement (typically Form HUD-1), executed retail sales contract (for mobile homes), or certificate of occupancy (for new construction).

later versions of the policy were more broadly publicized at the time of enaction and thus were more likely to induce changes in behavior rather than retrospective claims for past purchases.

Figure 2 presents time series plots that justify our focus on the second and third versions. Panel (a) presents existing home sales on a seasonally adjusted annual basis from the National Association of Realtors and shows that there were significant aggregate spikes at the end of the second and third extensions of the policy. Panels (b) and (c) confirm these spikes within our analysis sample in seasonally adjusted home sales from DataQuick. Panel (d) plots Google search trend data for the terms "first time home buyer" and "home buyer credit" along with vertical markers for policy events. Interest in these credits spiked at the beginning of the second extension, remained elevated throughout both policy periods, and then declined after the end of the third version.

Congress introduced and passed the FTHC with the explicit purpose of inducing demand for home purchases at a time of unusual weakness and helping to spur the economic recovery. In the respective words of Senators Cardin, Shelby, and Salazar, the program would "help the housing market," it would "help get homebuilders and the housing industry back on track," and it would "help us get rid of the glut we currently have in the market."[3] We evaluate this policy as both housing market stabilizer and fiscal stimulus. As stabilizer, the key questions are whether the policy promoted reallocation of underutilized assets from distressed sellers to more productive owners, and whether this reallocation affected market prices. As stimulus, the key question is whether the policy contributed to economic activity by inducing new home sales or through the transaction fees and complementary purchases that accompany an existing home sale. The non-random timing of the policy necessitates the cross sectional approach we pursue to separate the effect of the program from the other factors affecting housing markets at the same time.

A few other papers have studied the FTHC program. Brogaard and Roshak (2011) use city-level data and a cross sectional research design based on differences in housing market and socioeconomic conditions across cities to study the impact of FTHC on house sales and prices. They find that quantity was not noticeably affected and that prices rose only temporarily, returning to pre-legislation levels within two months of credit expiration. Dynan, Gayer and Plotkin (2013) examine the impact of the program as well as concurrent state bridge loan programs on home prices, home sales, and housing construction using cross state variation in program exposure. They conclude that the credit had, "at best, small and mostly temporary effects on housing activity," identifying small positive effects on home sales and prices, but no

[3]Congressional Record, Vol. 154, No. 52 (April 3, 2008) and Congressional Record, Vol. 154, No. 124 (July 26, 2008).

evidence of higher home construction activity. Our approach yields somewhat stronger results than these papers, which are likely driven by the more granular data and sharper design we use. More substantively, we emphasize and study the market-stabilizing role of the program and provide evidence suggesting this role was first order.

2 Data

This section presents an overview of the data sources for our analysis, discusses construction of key variables, and presents summary statistics. Appendix A presents additional information on the data build process, detailed variable definitions, and supplementary sample statistics.

2.1 Data Sources

We develop our measure of place-based ex ante program exposure using the population of de-identified individual tax return data over the time period between 1996 and 2013. These data include information about the age, earnings, marital status, number of dependents, and tax filing ZIP code reported on the income tax return.

We measure homeownership in the tax data through itemized deduction of mortgage interest and property taxes on Form 1040, Schedule A, or through information return Form 1098 submitted by lenders (which includes interest payments, mortgage insurance, and points paid). The panel structure of the data is critical because it allows us to measure whether a taxpayer owned a home in the previous three years. We also use tax data to measure claims of the homebuyer credit filed on Form 5405. This form records the date of purchase, which we use to study the time series of claims. Masked identifiers allow us to link these claims to the individual's tax return, which we use to measure the ZIP code associated with that person's claim.

There are two potential measurement issues with our approach to measuring homeownership. First, we will miss those who own their homes outright and use the standard deduction or do not file a tax return. These groups likely make up a very small portion of first-time homebuyers, who typically buy with a mortgage.[4] And non-filers primarily comprise poor and elderly people. Second, in measuring first-time homebuyers, we may mistakenly label refinance events as purchase events. This will only be the case for homeowners who previously owned their homes without a mortgage. This issue introduces measurement error in predicting program responses but is not an obvious confound.

[4] Based on survey evidence from 8,449 consumers who purchased a home between July 2009 and June 2010, 96 percent of first-time buyers used mortgage financing (National Association of Realtors, 2010).

We collect data on monthly home sales and house prices from DataQuick and CoreLogic. Our measure of home sales comes from the transactions and assessors data from DataQuick. This data set is deed-level data that measures home sales with dates of transfer for each property. The records provide detailed information on the characteristics of the transacted homes, including price, size, age, bedrooms and bathrooms, and so on, as well as detailed information on the type of transaction, including short sales, financial institution-owned sales (REO), foreclosure auctions, and an indicator for whether the transaction is made between related parties or at arm's length.

We use information between 2004 and 2013, which yields a consistent sample of covered places over time. Figure 2 shows that the Dataquick housing data closely match the time-series patterns for publicly available data published by the National Association of Realtors (NAR). On average, the aggregate counts in our filtered data represent between 40 and 50 percent of the levels reported by NAR.

We use house price data from the Federal Housing Finance Agency (FHFA), CoreLogic, and DataQuick. FHFA's price indices are available at the yearly level for the largest set of ZIPs in our sample and are based on repeat sales.[5] CoreLogic's price indices are available monthly and are also based on repeat sales. We compute median price levels for ZIPs within our DataQuick home sales sample, which we use in cross sectional tests based on pre-policy price levels and for back-of-the-envelope calculations.

We construct geographic-level controls from the Census, IRS public use files, and the American Community Survey (ACS). From the Census we draw the fraction of census blocks classified as urban. From the ACS we draw population in 2007 and compute the average unemployment rate, the average of ZIP-level median age, the average of median rent, and the average fraction below the poverty line between 2006 and 2010. From the IRS we draw average gross income in 2005.

2.2 Analysis Samples, Variable Definitions, and Summary Statistics

We construct a ZIP-by-month panel by collapsing individual transactions from the deeds records into counts for various transaction types. We define the primary analysis sample beginning with counts at the ZIP-by-month level for non-distress sales of existing homes. To ensure estimates are not biased by changes in geographical coverage, only ZIPs with more than 90 percent of their transaction time series complete from 2006 onwards are included. This filtering will tend

[5] Bogin, Alexander N. and William M. Doerner and William D. Larson (2016) describe the construction and source data for these price indices.

to exclude very small ZIPs that have many months during which there are no transactions. All other datasets are filtered to restrict the analysis sample to the same set of ZIPs. The primary sample contains 1,042,213 ZIP-months for 8883 ZIPs scattered across 47 states. These ZIPs account for 69 percent of the US population in 2007.

We seasonalize home sales counts using a within-place transformation for each month. For each ZIP, we also compute the mean of monthly house sales in 2007, which is our primary scaling variable, and total house sales in 2007, which is our primary weighting variable. Our main outcome variable is scaled monthly sales of existing homes, excluding distressed or forced sales. We censor this variable at the 99 percent level to remove outliers. We define program exposure as the ratio of first-time homebuyers to total tax filers in a place in 2000. In all regressions, we normalize exposure by its cross sectional standard deviation to aid interpretation of coefficients.

Table 1 collects summary statistics for the sample in the home sales analysis. The average observation has 19.6 sales per month. This varies from 3.7 sales at the 10th percentile to 41.5 at the 90th. The 10th percentile of the scaled variable is 0.44, the median is 0.92, and the 90th percentile is 1.72.

3 Empirical Approach

Our empirical strategy exploits cross sectional variation across geographies in ex ante exposure to the FTHC program to isolate the effect of the program from aggregate macroeconomic shocks. This empirical approach has been used by Mian and Sufi (2012) and Chodorow-Reich et al. (2012) to estimate the effect of fiscal policy. The main advantage of this approach is that it allows us to construct a counterfactual that can be used to estimate what would have happened in the absence of the policy. Areas with few potential first-time homebuyers act as the "control group" because the credit does not apply to most residents or houses. The difference between treatment and control areas provides an estimate of the causal impact of the program.

We measure exposure to the FTHC by identifying places with more first-time buyers in a time period prior to the policy. Higher exposure may reflect local amenities, such as schools or social attractions, that attract first-time buyers. Or it may reflect a local housing stock that is better suited to these buyers, in terms of affordability, lot size, and so on. The policy primarily targeted first-time homebuyers, so we should expect larger effects in places where the proportion of first-time homebuyers is higher.

We use administrative data from individual tax and information returns to measure the number of first-time homebuyers in each ZIP code in the US. In particular, we mark an individual as a homeowner if she claims a deduction for mortgage interest, property taxes, or mortgage insurance on her tax return, or if she receives an information return from a lender to whom she has paid mortgage interest. First-time homebuyers are people whom we classify as homeowners but who were not homeowners in any of the prior three years. We apply this rule at the household level for taxpayers who file a joint tax return.

Figure 3, panels (a) and (b) show that there is significant variation across areas in this instrument. For each place, we scale the number of first-time homebuyers by the number of tax filers in 2007. Darker areas indicate more exposure to the program. For ease of viewing, panel (a) displays county level variation because we are showing the entire U.S. whereas panel (b) shows ZIP level variation for three major cities. Table 1 shows that there is significant variation in our exposure measure at the ZIP level. Program exposure varies from 1.92 percent at the 10th percentile to 4.15 percent at the 90th. Mean exposure is 3.00 percent.

Consistent with anecdotal accounts of where first-time homebuyers tend to buy, the instrument is relatively concentrated in suburban areas around cities. Table 2 confirms this with a set of bivariate regressions of program exposure on ZIP-level observables. ZIPs with high exposure have higher rents and fewer people below the poverty line. The populations are larger and somewhat younger. Income is weakly correlated with program exposure. Exposure does predict a somewhat larger decline in house prices for high exposure ZIPs prior to the policy. Substantial variation in ex ante exposure within cities allows us to pursue a research design that conditions on city-by-time fixed effects.[6]

Our instrument may not accurately measure exposure to the program, either because the tax data miss non-filing or non-itemizing households, or because places change over time. To address this concern, we show that places with higher ex ante exposure indeed saw more individuals claim the credit. We do so in two ways. Figure 4, panel (a) plots binned bivariate averages ("binscatters") of FTHC claims from tax records versus program exposure. Exposure is strongly correlated with take-up in the cross section. The regression coefficient with CBSA fixed effects and ZIP-level covariate controls is 0.33 with a t-statistic of 54.[7]

Figure 4, panels (b) and (c) show our exposure measure also predicts time series variation in claims in these areas. In particular, we plot counts of FTHC claims by month of home purchase for purchases made between February 2009 and September 2010 along with vertical markers for policy events. The vertical markers correspond to the start of the FTHC loan program,

[6]We use Core-Based Statistical Areas (CBSA) to define city boundaries. Though our instrument varies at the ZIP level, we cluster standard errors at the CBSA level to permit within-city correlation in error terms.

[7]Clustering at the CBSA level yields a t-statistic of 22.

the start of version two of the credit, the scheduled expiration of version two, and the actual expiration of version three, respectively. Panel (b) plots national claim counts month-by-month, while panel (c) plots claim counts for high and low exposure quintiles of ZIP codes sorted by ex ante exposure.[8] Not only does our exposure-based instrument predict that high exposure places claim more credits, but the exposure measure also predicts the spikes in claims that we observe in the national claims data.

While our instrument is strongly correlated with FTHC take-up, a concern is that unobservable characteristics unrelated to the FTHC program are responsible for any differential purchase patterns that we observe. Table 2 shows that places where first-time homebuyers typically buy are not random, which poses a potential challenge to our empirical approach. For example, a risk to our design is that our measure is correlated with the expansion in subprime credit documented by Mian and Sufi (2009), leading to different ZIP-by-time trends within cities as the cycle corrected. We deal with this specific threat in our baseline analysis by measuring the number of first-time homebuyers in a pre-subprime period, the year 2000, to ensure that our instrument is not conflated with the increased purchases by subprime borrowers later in the decade.

We employ multiple strategies to mitigate these threats. First, our baseline analysis always conditions on city-by-time fixed effects and we report results with and without observable controls. This approach removes many potential confounds from our analysis. Second, we explicitly test for parallel trends in the pre-period and perform a within-ZIP placebo test to further assess this concern. Third, we exploit information in the age distribution of first-time homebuyers over time, showing that the median age of first time home-owners falls during the policy period and the age distribution reverts immediately after the policy expires. Moreover, the highest exposure ZIPs account for the largest share of the shift in first-time homebuyer age observed in the aggregate data. Finally, we exploit the short-lived nature of the policy to argue against potential confounds because the sharp changes we observe rule against potential alternative stories. In particular, the dramatic increase in housing sales we observe just before the expiration of both the second and third versions of the program, followed by a large decline in housing sales just after these expiration dates are difficult to explain by confounding trends which operate at lower frequencies.

[8] Quintiles are formed using weights that ensure each quintile has equal population in 2007.

4 The Effect of FTHC on Home Sales

4.1 Main Result

We begin with a simple graphical analysis that demonstrates our main finding, which is that home sales respond sharply to the FTHC program but do not show a sharp, immediate reversal once the program ends.

Figure 5, panel (a) plots the monthly home sales series between July 2007 and September 2011 for ZIPs divided into 100 quantiles and sorted based on ex ante program exposure. We present these data in the form of a calendar time heatmap, which is analogous to the traditional two-group calendar time graph but allows us to plot visually discernible time series for many more groups. In the graph, columns correspond to months and rows correspond to groups of ZIPs sorted by exposure. Exposure is the number of first-time homeowners in a ZIP in 2000 scaled by the number of tax-filing units in 2000. Each cell's shading corresponds to a level of the key outcome variable, which is monthly home sales scaled by average monthly home sales in 2007. The quantiles are formed using weights that ensure each quantile has an equal number of home sales in 2007.

The heatmap yields four conclusions. First, high and low exposure series closely track each other every month prior to the policy, deviating only during the policy window. Note that every sequence of consecutive months in the pre-period provides a placebo test that fails to reject the design's core identification assumption of parallel trends. Second, the smoothly increasing gradient visible at each policy expiration date shows the policy response is monotone in ex ante exposure and not driven by a few outlier ZIP codes. Third, the gradient does not reverse significantly in the seventeen months following the second policy expiration, rather the series return to a pattern of parallel trends; thus the data do not indicate a sharp reversal of the policy response. Last, we will use the lowest exposure quantile as a counterfactual to estimate the cumulative number of sales induced by the program. The heatmap shows that this group is a credible counterfactual, as it indicates no response to the program during the policy period.

Figure 5, panel (a) plots coefficients from regressions estimating the monthly and cumulative effects of the program. Specifically, we run month-by-month regressions of the form,

$$\frac{\text{Home Sales}_i}{\text{Average Monthly Sales}_{i,2007}} = \alpha_{\text{CBSA}} + \beta \text{Exposure}_i + \gamma X_i + \varepsilon_i, \tag{4.1}$$

where Exposure_i is the geographic measure of program exposure for place i and α_{CBSA} is a

CBSA-specific constant.[9] In controls specifications, X_i is a control set that includes log population, the average unemployment rate from 2006 through 2010, and log average gross income in 2005. All regressions are weighted by total home sales in 2007. Note that this approach is approximately equivalent to a panel regression with time-specific coefficients on exposure and the control variables, and ZIP, month, and CBSA-by-time fixed effects.[10] To aid interpretation, we normalize exposure by its cross sectional standard deviation.

Panel (b) plots coefficients for these regressions both with and without controls. The patterns are consistent with those in the heatmap. Exposure patterns do not predict differences in sales activity until the policy window begins and the coefficients spike in accord with the aggregate series. The coefficient of 0.06 for November 2009 implies that a one standard deviation increase in program exposure produces a 6 percent increase in monthly home sales relative to the average level in 2007. This is approximately 0.14 standard deviations of the left hand side variable. Panel (c) plots coefficients for regressions which replace monthly sales with cumulative monthly sales beginning seventeen months prior to the policy. The series is approximately flat prior to the policy window, increases monotonically through the window, and flattens in the post period. The cumulative effects are between 50 and 60 percent relative to the average level of monthly sales in 2007. Again, we see no evidence of a sharp, negative relationship between sales and exposure in the seventeen months following the policy. Regressions with controls do not alter this interpretation.

There is some evidence of reversal starting in the middle of 2012. However at this horizon—two years after the policy expired—our cumulative regressions begin to lose statistical power, as each subsequent month of home sales adds noise and increases standard errors. Thus we draw more measured conclusions over longer timeframes. At the 95 percent level for regressions with controls, we can no longer reject a full reversal starting in late 2012. For regressions without controls, we can still reject a full reversal until our data ends in mid 2013, though the point estimates do indicate some reversal. The balance of the evidence thus shows no reversal for the first seventeen months after the policy ended followed by a gradual reversal.

Table 3, panels (a) and (b) present the average monthly effects of the FTHC on home sales pooled over different policy for a variety of specifications. We run cross sectional regressions

[9] For the 129 ZIPs without an associated CBSA, we assign them a state-specific constant.

[10] This cross sectional approach closely matches the approach taken by Mian and Sufi (2012) to evaluate the Cash for Clunkers program. This allows us to compare our findings to theirs more easily. We have also pursued the more standard difference-in-differences approach in a panel regression, as advocated by Bertrand, Duflo and Mullainathan (2004), and all conclusions are the same.

of the form,

$$\frac{\text{Average Monthly Sales}_{i,t\to T}}{\text{Average Monthly Sales}_{i,2007}} = \alpha + \beta\text{Exposure}_i + \gamma X_i + \varepsilon_i, \tag{4.2}$$

where y_i is average monthly home sales in place i over the relevant time period. We use the same control set, weighting, and specification for exposure as in Figure 5, panels (b) and (c). All regressions are clustered at the CBSA level, or for ZIPs that are not associated with a CBSA, at the state level. Note that each row reports estimates from a separate cross sectional regression.

The time windows are defined as follows. **Pre-policy** includes the seventeen months prior to the second version of the FTHC passed in February 2009. **Policy** includes the seventeen months from February 2009 through June 2010. **Post-policy** includes the seventeen months beginning in July 2010. We then focus on specific intervals of interest within the policy period. **Early policy** includes the eight months from February 2009 through September 2009. **Spike one** includes the three months from October 2009 through December 2009. **Spike two** includes the three months from March 2010 through May 2010.

The results of the pooled regressions confirm the patterns from the figures. In the pre-policy period, there is little sign of differential trends. The policy period shows a significantly greater average effect on monthly sales, and this effect is most pronounced during the two windows leading up to policy expiration. The first spike shows a somewhat stronger but statistically indistinguishable effect relative to the second spike. One potential explanation for this is that the second period included the long-time homebuyer credit, which our instrument is not designed to predict. Last, the post-policy period shows little to no reversal in the seventeen months after the policy ends.

Quantitatively, the results indicate that the average monthly effect of the program was 2.0 to 3.1 percent relative to average 2007 sales—or 34.0 to 52.7 percent over the full seventeen months of the program—for a one standard deviation increase in program exposure. The post-policy coefficients are approximately zero with inconsistent signs and are statistically insignificant. This suggests that the policy was able to significantly increase sales during the policy period and that these sales were not reversed for at least one and a half years.

The lack of a significant reversal for over a year and a half is surprising, since standard intertemporal theory suggests that temporary price subsidies for durable goods simply reallocate demand across time. Consistent with this view, Mian and Sufi (2012) and Green et al. (2014), which both study the Cash for Clunkers (CARS) program, find that while the program was able to stimulate excess demand for automobiles during the policy period, these sales were completely reversed after seven to twelve months. In contrast to CARS, the FTHC targeted new potential homeowners allowing for a second, *extensive margin* effect to be at work. These are home purchases that would not have taken place absent the FTHC. Consistent with our results, Best and Kleven (2015) study a similar policy in the U.K. and find that the extensive margin can be sizeable in the short run. In a companion paper (Berger, Turner and Zwick, 2016), we explore in a structural model how policy design can affect the relative size of the intertemporal and the extensive margin effects.

4.2 Robustness and Placebo Tests

Table 3 presents a number of tests to confirm the robustness of our key findings, including the absence of trends prior to the policy, the size of the effect during the entire policy period and at the two spikes, and the non-reversal in the post-policy window.

Column (1) estimates the specification in 4.2 without CBSA fixed effects and controls. Column (2) adds a control set that includes log population, the average unemployment rate from 2006 through 2010, and the log of average gross income. Column (3) adds CBSA fixed effects. Columns (3) through (6) all use the same control set as column (2) and all include CBSA fixed effects. Estimates are similar with and without CBSA fixed effects, though somewhat more precise in the former specification. In column (4), we respecify the left-hand-side variable in logs. Coefficient estimates mostly do not change in this specification, though there is modest evidence of a partial post-period reversal.

In our main specification, we weigh regressions by total home sales in 2007 in order to provide macroeconomically relevant estimates. Column (5) presents regressions without weights. Unweighted regressions lead to modestly larger estimates during the policy window. Regressions with population weights, which we do not report for brevity, lead to similar conclusions. Following Mian and Sufi (2012), column (6) excludes the sand states: Arizona, California, and Nevada. Excluding sand states only slightly weakens the size of policy period estimates. In general, estimates are very robust across states.

Importantly, the parallel pre-policy trends assumption is rejected in none of the six specifications, and we find very weakly negative or null average post-policy effects. Appendix Figure A.1 presents a placebo test that further confirms these findings. The test estimates the month-by-month regressions and plots coefficients from the non-control specification in Figure 5, panel (b), emphasized with a bold line, along with equivalent regressions shifted backward in time to start in 2005, 2006, and 2007, and shifted forward to start in 2009 and 2010. These placebo series show that the policy coefficients are unusually high while pre- and post-policy coefficients coincide with placebo series. The figure also suggests that seasonal confounds not captured by our seasonality adjustment do not influence our estimates for the spikes.

The pre-policy coefficients provide strong evidence that our design is valid and low exposure areas can serve as a counterfactual to high exposure areas. But there may still be concern that place-specific shocks coinciding with the policy or place-specific trends beginning in 2009 might confound our estimates. We consider an alternative approach to validating our design with a within-time placebo test. The idea motivating this test is simple: first-time buyers are more likely to buy smaller homes than larger homes, so smaller homes should respond more strongly to the program. If place-specific shocks are driving our results, we should see similar patterns across all types of homes.

Table 4 presents regressions of the same form as those in Table 3. We divide the home sales series into "starter" homes—defined as those with 1, 2, or 3 bedrooms—and large homes—defined as those with 4 or more bedrooms. We run the ZIP-level specifications separately for each series. Because of incomplete reporting across places, the analysis sample here is the subset of the main analysis sample where fewer than 20% of transactions between 2004 and 2013 have missing bedrooms data.

Estimates for the starter home sample closely match those in our full sample, while those for larger homes are weakly negative and statistically insignificant. Thus our main results are concentrated among the starter homes, while larger homes show no response to the program. This provides further evidence in support of the parallel trends assumption in our design.

As a final robustness test, we explore whether the effects are larger in places where initial price levels are low. For homes with prices above \$80,000, the FTHC is fixed at \$8,000. Thus the subsidy is less generous in more expensive cities. In the first row of Table 5, we present these results by estimating a differenced version of 4.2, specified as

$$\frac{\Delta \text{Average Home Sales}_i}{\text{Average Monthly Sales}_{i,2007}} = \alpha + \beta \text{Exposure}_i + \gamma X_i + \varepsilon_i, \tag{4.3}$$

where ΔAverage Sales equals the average number of home sales in place i during the policy period minus the average number of home sales in place i during the seventeen-month pre-period. We first reproduce the results using specifications from Table 3 to confirm the estimates are unchanged.

Columns (6) and (7) in the first row of Table 5 divide the sample of ZIPs into the bottom three ("Low p") and top three ("High p") deciles in median house prices during 2008. The effects are concentrated in the low price ZIPs, which yield a coefficient of 0.027, while the high price ZIPs show no discernable effect with a coefficient of 0.005. These split sample findings provide further evidence that our results are indeed due to the FTHC policy.

4.3 The Age Distribution of First-Time Buyers

The non-reversal of the policy period response following the program's expiration raises the question of where these buyers came from. To address this question, Figure 6 presents direct evidence indicating that, in the absence of the program, many buyers would not have bought homes for several years. Panel (a) plots age distributions of first-time homebuyers identified using income tax return and information return information for the years between 2002 and 2013. We highlight the age distribution for 2009, which shifts substantially to the left relative to the other years. The median age for all first-time buyers in 2009 was 35 in the non-policy years and 33 in 2009. Among those that claimed the credit, the median was 32.

To explore whether the FTHC explains this pattern, panel (b) shows the correlation between the shift in the age distribution in 2009 and program exposure. We decompose the national shift in the age distribution in 2009 into contributions from each ZIP. For each ZIP, we compute the difference between the ratio of buyers aged 30 or younger to total new homebuyers in 2009 versus the average ratio of buyers aged 30 or younger to total new homebuyers in other years. We then plot binned bivariate sums of these ZIP-level contributions against average exposure in each bin. The highest exposure ZIP codes account for the largest share of the shift in first-time homebuyer age observed in the aggregate data.

Thus a noticeably younger cohort of first-time buyers appeared in 2009 alone, driven by the temporary policy incentive to accelerate transition into homeownership. These facts also assuage concerns that place-by-time cyclicality or secular trends can explain the slow reversal.

4.4 New Home Sales

Our analysis thus far has focused on non-distress sales of existing homes. These are the largest category of transactions and are the most reliably recorded in the DataQuick database. Both of these features permit the high frequency analysis we use to validate our research design. Yet in examining the policy as fiscal stimulus intended to spur GDP growth, existing home sales are not the ideal category to study, as they only contribute to output through transaction fees and complementary purchases (such as furniture) made by homeowners.

In Table 5, we explore the effects of the program on new home sales, using the new construction data recorded by DataQuick. To do so, we estimate a differenced version of 4.2, specified as

$$\frac{\Delta\text{Average Construction}_i}{\text{Average Monthly Sales}_{i,2007}} = \alpha + \beta\text{Exposure}_i + \gamma X_i + \varepsilon_i, \tag{4.4}$$

where ΔAverage Construction equals the average number of new home sales in place i during the policy period minus the average number of new home sales in place i during the seventeen-month pre-period. We seasonally adjust the new home sales series prior to averaging. All specifications include CBSA fixed effects.

The results indicate the program had approximately no effect on new home sales. The point estimate is -0.004 and not statistically distinct from zero, as compared to 0.021 for existing home sales. We confirm this finding in several robustness checks. Column (2) equally weighs observations and column (3) excludes the sand states: Arizona, California, and Nevada. Columns (4) and (5) confirm that the results are not driven by outliers or small geographies. Column (4) estimates the relationship on a subsample that censors the left-hand-side variable at the 5th and 95th percentiles. Column (5) restricts the sample to places with average home sales in 2007 above the 10th percentile.

All specifications point to the conclusion that FTHC did not induce additional construction. This finding is not surprising for a time when the national market suffered a significant overhang of recently built homes. Nevertheless, the result implies that the direct stimulative effects of the program were likely second order, despite the substantial increase in existing home sales caused by the program.

4.5 Aggregate Estimates

Following Mian and Sufi (2012), we compute an estimate of the total number of sales caused by the program, exploiting only differences in cross sectional exposure and using the group receiving the smallest shock as a counterfactual. We choose the bottom one percent of ZIPs as the counterfactual group, which corresponds to the bottom row of the heatmap in Figure 5, panel (a). We then compute the effect of the policy for other groups relative to this group. By construction, any time series effect of the policy shown by the bottom group is set to zero and removed from the effect computed for other groups.

Standardized exposure is 0.85 for the bottom group and increases to 7.58 for the highest group. Thus for each exposure group g, the aggregate number of sales induced by the program is

$$\Delta\text{Sales}_g = 17 \times \beta \times (e_g - 0.85) \times s_{g,2007}, \tag{4.5}$$

where β is the coefficient from equation 4.2 for the seventeen-month policy period, e_g and

$s_{g,2007}$ are weighted average program exposure and monthly sales in 2007 for group g, respectively, where the weights are average monthly sales for the ZIPs in each group. We aggregate ΔSales_g across all groups to provide an estimate of the aggregate effect within the DataQuick sample.

We estimate that the FTHC increased existing home sales by 157 thousand within sample during the policy period, or 7.4 percent of all sales during this period. In 2007, our sample covers approximately 41 percent of the national existing home sales market. Extrapolating our estimates to the national market yields an estimated increase of approximately 382 thousand during the policy period. There were 1.58 million claims of the FTHC during this time.[11] Thus under the assumptions that the lowest exposure group is a plausible control group and that our sample is representative of the national market, 24 percent of claims were made for induced sales, as opposed to sales that would have happened in the absence of the policy. Note this is a lower bound estimate if the lowest exposure group also responds to the program.

To produce this aggregate estimate, we have not modeled potential general equilibrium effects, which are subsumed into time fixed effects. In response to the concern about general equilibrium effects, it is comforting that the raw aggregate path of home sales provides a clear demonstration of a policy response. Furthermore, the heatmap does not reveal home sales falling below pre-policy levels in low exposure areas, which would be predicted by binding aggregate resource constraints. In addition, because the policy was implemented at a time when interest rates were at the zero lower bound, the mitigating effect of interest rates was likely small. Nevertheless, without a full model, our aggregate estimate should be considered an imperfect approximation of the total effect.

An important motivation for the FTHC policy was to stimulate real economic activity, particularly in the housing sector. We have seen that the FTHC had a large effect on transaction volume in the existing home market and little effect on new home construction. Thus the primary direct stimulative effect on GDP comes through the transaction fees and complementary purchases associated with the purchase of an existing home.

We follow Best and Kleven (2015) and provide a back-of-the-envelope calculation of these amounts using our estimate of induced home purchases. The realtor fee ranges from 3 to 5 percent of the purchase price. When evaluated at the median purchase price for homes in our sample during the policy period ($190,000), this implies a GDP contribution of between $2.2 and $3.6 billion. Best and Kleven (2015) use survey evidence to estimate that complementary furniture, home improvement, and related expenditures at the time of a new purchase amount

[11] This figure excludes approximately 250 thousand claims for the Long-Time Homebuyer credit.

to approximately 1 to 1.5 percent of the purchase price, which implies a contribution to GDP of approximately $700 million to $1.1 billion. Thus when compared to the cost of versions two and three of the program, which amounted to approximately $11 billion, these effects do not alter our conclusion that the direct GDP effects of the program were relatively modest.

5 The Effect of FTHC on Reallocation

In this section, we consider the value of the FTHC program as a housing market stabilizer. As discussed in the Introduction, the policy came at a time of extraordinary weakness in housing markets across the country. Housing inventories were near historic highs and nearly forty percent of home sales were classified as distressed or foreclosure sales. The aftermath of the Great Recession meant that many prospective homebuyers were constrained because they faced elevated unemployment risk and had suffered wealth losses making it difficult to afford required down payments. Each of these factors placed significant downward pressure on housing prices. Against this backdrop came widespread concern that absent intervention, fire sale dynamics would continue, leading to more vacancies and foreclosures, more destruction of housing wealth, and further downward pressure on prices.

These conditions create several economic rationales for intervening in the housing market, of which we highlight three. First, a large literature documents how a decline in house prices can affect the real economy. Falling house prices can generate pecuniary externalities and financial accelerator effects by destroying household net worth and thus affecting whether firms can borrow to invest and whether households can borrow to consume (Kiyotaki and Moore, 1997; Iacoviello, 2005). These effects would be particularly strong during a liquidity trap, as was the case during the FTHC program. Furthermore, an increase in foreclosures can spill over through the banking sector as losses realized by banks inhibit their ability to borrow and lend (Shleifer and Vishny, 2010). There is considerable evidence that falling house prices following the expansion in home equity borrowing in the mid-2000s led to a large decline in consumption and employment (Mian and Sufi, 2011, 2014).

A second rationale for intervention is to address the pecuniary externality that elevated foreclosures, short sales, and vacancies impose on nearby homeowners. Campbell, Giglio and Pathak (2011) show that prices for houses within 0.05 miles of a foreclosure fall by about 1 percent. Similarly, Whitaker and Fitzpatrick IV (2013) find that an additional property within 500 feet that is vacant or delinquent reduces a home's selling price by 1 to 2 percent. Guren and McQuade (2015) show that these effects can be large in a quantitative, general equilibrium model. Thus policies that stabilize house prices during a period of market level distress can mitigate this market failure.

The third rationale is to correct a credit market failure due to the simultaneous presence of constrained buyers and elevated vacancies. In normal times, buyers purchase homes until the marginal cost and marginal benefit of homeownership equate. However, during the recession many natural buyers of homes were constrained because of elevated unemployment risk and lower incomes. At the same time, because the homes had already been built, the marginal cost of delivering the house was potentially much lower than the marginal benefit to a household of occupying the house. Additionally, vacant homes depreciate faster due to lack of maintenance (Gerardi et al., 2015) and may enable crime (Cui and Walsh, 2014; Ellen, Lacoe and Sharygin, 2013). Thus a policy like the FTHC—which alleviates credit constraints faced by new homebuyers and potentially moves people into underutilized homes—could improve welfare.

Taking these rationales as given, we present evidence suggesting the additional demand induced by the FTHC program indeed facilitated a productive reallocation of underutilized assets. We make this case in three steps. First, many transactions during the policy period involved sales by investors and institutional sellers, who were likely to be low utility users of the assets. Second, many buyers induced by the program were likely constrained by down payment requirements that the credit and concurrent federal lending policies helped relax. In addition, a large fraction of purchased homes were previously vacant or were foreclosures, consistent with positive reallocation and improved utilization of existing assets. Third, and consistent with a reallocation toward higher utility users, the program increased house price growth. Finally, we find the marginal reallocation caused by the program was stable, as program buyers were not more likely to default than other cohorts and the quantity and price responses did not immediately reverse.

5.1 Recently Built Homes and Homes in Distress

We exploit the richness of the DataQuick transaction data, which records the names of buyers and sellers as well as categories for distressed sales, to explore how likely it was that the FTHC sped reallocation of houses from low to high utility users. For each property, DataQuick's county assessor data provide detailed information on the characteristics of the transacted homes, including price, size, age and number of bedrooms and bathrooms. Linked to each property are transaction data that track changes in deed. The data identifies if a transaction is a short sale, a financial institution-owned sale (REO), a foreclosure auction or an exchange of deed on a foreclosed home. DataQuick's proprietary model classifies whether the transaction is made between related parties or at arm's length.

It is important to distinguish sellers who are homeowners from those left holding assets they were unable to sell. A number of negative externalities are associated with the latter case. Empty houses decay more rapidly and can be subject to vandalism or host to other crimes. Foreclosure spillovers associated with forced sales of distressed homes can depress housing values for neighbors and, through subsequent reappraisals, amplify barriers to refinancing.

We divide the total transactions for our main analysis sample during the policy period into categories based on the likelihood that the sellers were not first-best users of the homes they were selling. We investigate the following categories:[12]

1. **Recently Built**: Includes homes built between 2005 and 2010.
2. **Foreclosure or Short Sale**: Includes homes categorized by DataQuick as short sales (i.e., sales involving principal forgiveness by lenders).
3. **Real Estate Owned (REO)**: Includes homes categorized by DataQuick as being sold from a financial institution's portfolio of homes.
4. **Developer Seller**: Includes homes for which the seller is either a home builder or other kind of company, based on the seller's name.
5. **Government Sponsored Enterprise (GSE) Seller**: Includes homes for which the seller is a federal entity—Fannie Mae, Freddie Mac, Ginnie Mae, the Federal Home Loan Banks, or the Veterans Administration.

Figure 7 displays weighted average shares of total transactions for the policy period for these categories. Weighted averages equal the average share of sales in a category in a ZIP with average monthly sales in 2007 as the weight. During this period, there were 4.3 million total transactions for our sample.

During the policy period, there were approximately 739 thousand homes sold that had been built in 2005 or later. This compares to 396 thousand homes classified as new construction by DataQuick. To the extent our new construction marker is too restrictive, these sales indicate the program may have allowed builders to sell homes from their recent inventories. Consistent with this, approximately 1.13 million homes, or 24 percent of all home sales, were sold by developers or builders. Thus nearly two-thirds of homes sold by developers were not new construction.

Recent construction does not contribute to output or employment at the time of sale. However, the importance of recent construction in aggregate sales during this period highlights

[12] Appendix A provides more detail on how we categorize transactions, including regular expressions used to identify builders, developers, and the GSEs.

two macroeconomic issues created by investment overhangs. First, an overhang of previously built assets reduces investment today while the economy redeploys excess inventory. Second, while GDP correctly measures the delivery of new homes during the period of construction, it does not correctly measure the initiation of a stream of consumption services if those assets are subsequently left vacant. The facts suggest programs like the FTHC can work by accelerating redeployment and initiating use of vacant homes.

Figure 7 also shows the importance of distressed sales and sales from financial institution portfolios during this time. Within our sample, there were approximately 561 thousand foreclosures or short sales and 843 thousand REO sales, including 235 thousand sales from the government entities' portfolios of repossessed homes. While not mutually exclusive from the recent construction and developer sales above, these figures again suggest that many of the homes transacted did not involve transfers from one homeowner to another, but instead enabled transitions of underutilized assets to more productive use.

5.2 Constrained Buyers and Mortgage Finance

A large literature documents the importance of down payment constraints in housing markets. Stein (1995) shows that modeling down payment constraints is crucial for matching many empirical features of the housing market. Using the PSID, Engelhardt (1996) shows that young households reduce consumption in years in which they buy a home yet increase consumption back to long-run levels in subsequent years. This suggests that the down payment constraint is binding for many young households. Survey evidence confirms this fact. Fuster and Zafar (2015) administer a survey on the role of down payment constraints on household willingness and ability to buy housing. They find that a reduction in down payments would have a much larger effect on household behavior than a decline in mortgage rates. This result reflects the difficulty many households face in saving for the typical 20% down payment, especially in high home price areas.[13]

The FTHC program coincided with an expansion by the Federal Housing Administration (FHA) of its first-time homebuyer mortgage guarantee program. This program enables mortgage loans of up to 96.5 percent of purchase price for eligible buyers. Given the low down payment requirements, first-time homebuyers make up a significant portion of new originations supported by the FHA, as the government-sponsored enterprises Fannie Mae and Freddie

[13] A recent report by builderonline.com finds that residents making the median income in a state have to save nearly eight years on average to put 10% down for a median price home (builderonline, 2015). Similarly, a recent survey by Trulia.com finds that a full 47% of surveyed renters would consider buying if they had enough savings for the down payment (Kolko, 2012).

Mac typically require larger down payments. According to the Department of Housing and Urban Development (HUD), FHA supported 781 thousand first-time homebuyers during 2009 and 882 thousand during 2010, or approximately 56 percent of the first-time buyer market during these years.[14]

These low down payment loans are not costless: a lower upfront payment trades for higher subsequent interest payments plus required mortgage insurance premiums. A simple calculation highlights the trade-off. Consider three different mortgage contracts for a house which costs $200,000: (1) a conventional 30-year fixed rate mortgage requiring a 20% down payment, (2) a conventional 96.5% LTV FHA loan where the household pays off the Upfront Mortgage Insurance (UMI), 1.75% of price, within the down payment, (3) a 96.5% LTV FHA loan that shifts the UMI into the principal. The FHA loan also includes a 0.55% mortgage insurance premium. Assume the interest rate is 4.8%, the average conventional mortgage rate from November 2009.[15]

Under these assumptions and no discounting, the first mortgage would cost $301,000 over 30 years. Notice that to receive this contract the buyer makes a $40,000 down payment at origination. The second mortgage would cost $368,000 over 30 years but the buyer would only make a $7,000 + $3,500 down payment upfront. Finally, the third mortgage including UMI would cost about $375,000 over 30 years and the buyer would make a $7,000 down payment. In 2009, the average interest rate on FHA loans was 1.4% higher than a conventional mortgage, which increases the cost of the FHA loans to $428,500 and $436,000 respectively. Thus the FHA mortgages are considerably more expensive over the life of the contract than a conventional mortgage. That so many households chose an FHA mortgage despite the higher future cost suggests that down payment constraints were highly relevant for these households during the sample period.

In a companion paper, we explore in detail the role of down payment constraints in amplifying the response to the program (Berger, Turner, and Zwick, 2016). The data strongly suggest that many potential homebuyers face down payment constraints, as these buyers took advantage of low down payment loans at the expense of higher monthly payments. The FTHC program helped relax these constraints; as a result, many marginal transactions likely involved purchases by high utility users of the assets. When combined with the facts about home sellers, this further suggests that induced sales entailed productive reallocation.

[14]See Figure 6 in HUD's "Annual Report to Congress" (Department of Housing and Urban Development, 2011).

[15]For the sake of simplicity we abstract from mortgage prepayment and exclude the tax benefits of interest payments and insurance premiums.

5.3 Default Rates for Policy Period Buyers

Given the high origination loan-to-value ratios of policy period homebuyers and the literature suggesting such LTVs can lead to subsequent distress, it is critical to ask what happened to these buyers subsequently. We use the DataQuick transaction data to shed light on this question. DataQuick records track a distressed property as it goes through each step of the default process, as early as a short sale and as late as the REO disposal following foreclosure. We use chronological changes in ownership classified as distress sales by DataQuick to identify homebuyers who later defaulted on their loan. Specifically, we follow policy period cohorts of buyers and compare them to cohorts both before and after the policy.

Figure 8, panel (b) plots cumulative distress cohorts for purchases made during the policy period and compares these to cohorts based on 2006, 2007, and 2008 sales as well as cohorts based on 2011 sales. Our data allow us to compare cohorts for at least 36 months from the month of purchase. Both the 2009 and 2010 policy cohorts show no difference in default rates relative to the 2011 post-policy cohort. At 36 months out, each of these groups shows distress transition rates of approximately ten per thousand purchases. Furthermore, all three of these groups display considerably lower rates of transition into distressed sales than the pre-policy groups. Thus the data do not indicate the FTHC program induced unusually risky buyers into the market, despite the very high LTVs at which these buyers entered. In this sense, the reallocation of homes appears to have been stable.

5.4 Vacant Homes and Household Formation

To further assess the likelihood of beneficial reallocation, we complement our exploration of low utility sellers from Section 5.1 with information from the de-identified tax returns of FTHC claimers. Unfortunately, the tax data do not record information about the people or entities from whom FTHC claimers bought their homes. However, it is possible to use information about mailing addresses to ask two related questions about claimer transitions into homeownership. The first question is whether the home occupied by the claimer at the time of purchase was occupied in recent years or instead a vacant home. The second question is whether the transition induced household formation in the sense that claimers move from a multiple occupancy household to a single occupancy household.

We attempt to measure vacancy and household formation using mailing address information attached to an individual's tax return.[16] To measure changes in vacancy status, we ask

[16]Specifically, we use the mailable point information encoded in the 12-digit ZIP code. We restrict analysis to

whether the new address associated with the FTHC purchase had been occupied two years prior to the purchase. To measure changes in household formation, we count the number of tax returns filed from a particular address and compare it to the number of tax returns filed at the FTHC claimer's address two years prior to the purchase. In both cases, we choose the period two years prior because the new address may be assigned to the prior year's tax return if a tax filer amended the prior return to claim the credit. We focus on claims made for purchases in 2009 to separate first-time homebuyers from the long-time homebuyers.

From the FTHC claims in 2009, we find that 42 percent move into an address that had no filers in 2007 and 33 percent transition into a single tax filer address from living in a multiple filer address in 2007.[17] We have also computed these statistics for first-time homebuyers in the non-policy years between 2002 and 2013. The data suggest that FTHC claimers are not more likely to move into vacant homes and only slightly more likely to form new households relative to first-time buyers in other years (33.1% transition to single family in 2009 relative to 30.5% in other years). Thus to the extent the program sped reallocation of underutilized assets, this came primarily through increasing the level of home purchases. Nevertheless, the data do rule out the possibility that FTHC purchases merely resulted in people "swapping" houses. In addition, the data also suggest imputed owner-equivalent rental income as another indirect GDP effect of the program.

5.5 House Prices

To explore the effect of the FTHC on house prices, we use data from the Federal Housing Finance Agency (FHFA) and CoreLogic. Both datasets rely on a repeat sales methodology to estimate price indices at the ZIP level from the present to as far back as the mid 1970s. The FHFA indices use all mortgages guaranteed by Fannie Mae and Freddie Mac, and offer the greatest geographic coverage but are only estimated annually. CoreLogic's index uses its proprietary database to estimate monthly values.

We follow the same empirical strategy as for the home sales regressions in Table 5, exploiting within-CBSA variation in exposure to the program. For the FHFA data, the left-hand-side variable is cumulative annual log price differences during 2009 and 2010 minus cumulative annual log price differences during 2007 and 2008. We present estimates for raw changes in

valid ZIP-12s with fewer than 100 simultaneous tax returns attached to them, as these likely refer to apartment buildings or clusters of post office boxes in low density areas.

[17]The household formation statistic restricts the sample to those filers for whom we have a valid previous address. As a validation check of the vacancy data, we have confirmed that the vacancy share of FTHC claims at the ZIP level is strongly correlated with the share of home sales that are foreclosures or short sales.

price growth and for market-adjusted changes. In the case of market-adjusted changes, we first estimate ZIP-specific housing market betas in the ten-year window from 1997 to 2006 and then subtract beta times the market return to compute a ZIP-level excess return.[18] This allows us to control for differential exposure of high exposure ZIP codes to the national cycle driven by higher risk in these areas. For the CoreLogic data, the left-hand-side variable is raw cumulative monthly log price differences during the policy period minus cumulative monthly log price differences during the 17-month pre-period. In all cases, we multiply the left-hand-side by 100 so the treatment effect units are percentage points of growth per standard deviation change in program exposure.

Table 6 presents results from these regressions. In our preferred specification, which uses the market-adjusted FHFA series, we find the program caused an increase in cumulative price growth of 77 basis points per standard deviation increase in exposure. At the median initial price level of \$222,000 in our sample, this implies an increase in prices of \$1,720 ($\approx .00774 \times 222{,}000$). This figure is plausible given the \$8,000 size of the credit and considerable excess inventory in the market. It also implies that even the highest exposure places did not see house prices increase by more than the credit.[19]

As shown in the table, the estimate is robust to different weights, sample definitions, and censoring of the left hand side variable. In addition, the estimates vary little between the FHFA and CoreLogic samples and do not depend on the market adjustment for ZIP-specific cyclicality. As with the quantity results, we estimate more precise and qualitatively larger effects in the ZIPs with lower initial house prices, though these differences are only statistically significant in the CoreLogic sample.

Figure 9 allows us to explore the extent to which the price effects reverse in the years following the policy. We estimate cross-sectional regressions each year of first differences in market-adjusted price growth from the FHFA. The coefficients show weakly negative pre-trends in years prior to the program and strong trend breaks during the two program years, which match both qualitatively and quantitatively the positive long difference effects estimated in Table 6. In the year immediately following the program, price growth retreats somewhat, undoing at most one quarter of the increase caused by the program. This evidence is consistent with our finding an incomplete reversal of the home sales response in the post-policy period.

A growing empirical literature documents large, causal responses of non-durable consump-

[18] For the market return, we use the national annual FHFA house price index, which is estimated using a similar methodology to the ZIP level indices.

[19] As house prices were falling on average during this time, these effects may be interpreted as saying the program slowed the rate of price declines.

tion to house price movements.[20] Using different identification strategies, these studies estimate an elasticity of non-durable consumption in the range of 0.15 to 0.3. Given that the FTHC had a significant effect on house prices, natural questions to ask are: first, did the FTHC indirectly stimulate consumption through its effect on house prices, and second, how large are these effects? While a complete treatment of these questions is beyond this paper's scope, we apply the sufficient statistic approach of Berger et al. (2015) to derive a ballpark estimate.

The central theoretical result of Berger et al. (2015) is that despite the myriad of ways in which a change in house prices affects an individual's decision problem, the change in consumption due to an unexpected, proportional change in house prices is given by a simple "sufficient-statistic" formula:

$$\frac{\Delta C_i}{\frac{\Delta P}{P}} = MPC_i \cdot (PH_{i-1}(1-\delta)), \tag{5.1}$$

where MPC_i is the individual marginal propensity to consume out of transitory income shocks and $PH_{i-1}(1-\delta)$ is the value of the individual's home after depreciation. Given estimates of these objects, we can aggregate them across households and places and evaluate the size of the indirect effects under a variety of assumptions.

We proceed as in section 4.5 by choosing the bottom one percent of ZIPs as the counterfactual group and computing an aggregate house price effect for other groups relative to this group. Recall that the standardized exposure is 0.85 for the bottom centile and increases to 7.58 for the highest one. Thus for each exposure group g, the aggregate percentage change in prices for that group induced by the program is

$$\frac{\Delta P_g}{P_g} = \beta_p \times (e_g - e_{g,low})$$

where β_p is the coefficient (.00774) from the long difference price regression in Table 6. We apply this price growth factor to the average median house price in each group and accumulate over all owner-occupied housing units in each group under an assumed value for the marginal propensity to consume We then apply equation 5.1 to infer the aggregate change in consumption induced by the policy. These calculations are certainly rough but still informative for the magnitude of potential indirect effects.

If one assumes that the MPC is 0.10[21] and all of the housing stock is affected, then aggregate

[20] See, e.g., Mian, Rao and Sufi (2013), Stroebel and Vavra (2016), and Kaplan, Mitman and Violante (2016).

[21] This figure is a rough average between the numbers found in the fiscal transfer literature (0.2-0.3) and the value implied by the permanent income hypothesis (0.05)

consumption would have increased by $23 billion on account of the FTHC. If one assumes instead that only one to three bedroom homes are affected by the policy, the effect falls to approximately $12 billion. The point of this exercise is not too take any particular number seriously, but rather to illustrate that these effects can be as large or possibly much larger than the program's direct stimulative effects.

6 Conclusion

This paper asks whether policy can accelerate the process of reallocation in times of debt-induced capital overhang following an investment boom. We study temporary tax incentives targeted at marginal asset buyers in the housing market. Unlike debt renegotiation programs, financial market support, and fiscal and monetary stimulus, the policy we study directly targeted the capital overhang, while aiming to keep underused assets in private hands.

We find that the program proved effective at spurring home sales and that these effects did not immediately reverse once the program ended. This stable demand shock to the market likely accelerated the process of reallocation of vacant homes from institutional investors, banks, and the unsold inventories of home builders into the hands of higher value users. House prices increase and buyers induced by the program were not more likely to default than previous or subsequent cohorts of buyers. When targeted correctly, these results suggest policy can accelerate purchases and mitigate the debt-deflation dynamics associated with capital overhang.

The policy is less appealing when considered as fiscal stimulus. This is especially true in the absence of a capital overhang when the spillover effects of reducing distressed sales and supporting asset prices are smaller. If not preceded by a housing boom, new construction might respond more to such a policy, but not without considerable leakage of the response into the resale market. While resales do increase GDP through increased realtor fees and complementary purchases, the implied overall multipliers and size of the program are relatively small.

Given this evidence, was the FTHC a successful policy? The evidence is mixed. If judged solely on its direct stimulative impact then the policy would receive a low grade. Though the increase in housing demand was large and persistent, the costs of the program considerably exceeded the potential GDP gains. However, taking a wider view of the policy as a housing market stabilizer leads to a more generous appraisal. The policy enabled beneficial reallocation of unoccupied housing stock toward higher value users and stabilized house prices. This made the FTHC complementary to other principal and payment renogotiation programs, such as HAMP and HARP, that aimed to repair household balance sheets and improve mortgage affordability.[22]

The FTHC policy was also successful at stimulating homeownership. This is notable because the U.S. government spends at least $70 billion a year on the mortgage interest deduction trying to encourage homeownership. While the mortgage interest deduction may have some effect on inducing marginal households into homeownership, it also induces households already planning to buy a home into buying larger homes, which has limited social benefits (Glaeser and Shapiro, 2003). One lesson from the FTHC is that if increasing homeownership rates is a policy goal then directly targeting potential homeowners and the constraints they face may be a more cost-effective way to achieve this policy goal. More generally, this example highlights the importance of policy design when conducting fiscal policy. As we explore more thoroughly in a companion paper (Berger, Turner and Zwick, 2016), policies which target relevant constraints and marginal agents lead to more cost-effective durable goods stimulus policy.

[22]The key difference is that the FTHC addresses a later stage in the foreclosure chain, namely, when the house was already owned by the bank.

References

Adda, Jérôme, and Russell Cooper. 2000. "Balladurette and Juppette: A Discrete Analysis of Scrapping Subsidies." *Journal of Political Economy*, 108(4): 778–806.

Agarwal, Sumit, Gene Amromin, Itzhak Ben-David, Souphala Chomsisengphet, Tomasz Piskorski, and Amit Seru. 2012. "Policy Intervention in Debt Renegotiation: Evidence from the Home Affordable Modification Program." *NBER Working Paper No. 18311*.

Agarwal, Sumit, Gene Amromin, Souphala Chomsisengphet, Tomasz Piskorski, Amit Seru, and Vincent Yao. 2015. "Mortgage Refinancing, Consumer Spending, and Competition: Evidence from the Home Affordable Refinancing Program." *NBER Working Paper No. 21512*.

Berger, David, and Joseph Vavra. 2015. "Consumption Dynamics During Recessions." *Econometrica*, 83(1): 101–154.

Berger, David, Nicholas Turner, and Eric Zwick. 2016. "Temporary Durable Goods Incentives: Theory with Evidence from the First-Time Homebuyers Credit." *Working paper*.

Berger, David, Veronica Guerrieri, Guido Lorenzoni, and Joseph Vavra. 2015. "House Prices and Consumer Spending." *NBER Working Paper 21667*.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. "How Much Should We Trust Differences-In-Differences Estimates?" *Quarterly Journal of Economics*, 119(1): 249–275.

Best, Michael Carlos, and Henrik Jacobsen Kleven. 2015. "Housing Market Responses to Transaction Taxes: Evidence From Notches and Stimulus in the UK." *Working paper*.

Bogin, Alexander N. and William M. Doerner and William D. Larson. 2016. "Local House Price Dynamics: New Indices and Stylized Facts." *FHFA Working Paper No. 16-01*.

Brogaard, Jonathan, and Kevin Roshak. 2011. "The Effectiveness of the 2008-2010 Housing Tax Credit." *SSRN 1882599*.

builderonline. 2015. "How Long Does a First-Time Buyer Have to Save for the Down Payment on their Dream Home?" `http://www.builderonline.com/money/how-long-will-buyers-save-up-for-the-down-payment-of-their-dream-home_o`, Accessed: 2016-06-15.

Campbell, John, Stefano Giglio, and Parag Pathak. 2011. "Forced Sales and House Prices." *American Economic Review*, 101(5): 2108–31.

Chodorow-Reich, Gabriel, Laura Feiveson, Zachary Liscow, and William Gui Woolston. 2012. "Does state fiscal relief during recessions increase employment? Evidence from the American Recovery and Reinvestment Act." *American Economic Journal: Economic Policy*, 118–145.

Cui, Lin, and Randall Walsh. 2014. "Foreclosure, Vacancy and Crime." *NBER Working Papers 18353*.

Department of Housing and Urban Development. 2011. "Annual Report to Congress Regarding the Financial Status of the FHA Mutual Mortgage Insurance Fund Fiscal Year 2011." `http://portal.hud.gov/hudportal/documents/huddoc?id=FHAMMIFundAnnRptFY11No2.pdf`.

Dynan, Karen, Ted Gayer, and Natasha Plotkin. 2013. "An Evaluation of Federal and State Homebuyer Tax Incentives." *Washington, DC: The Brookings Institution*.

Eggertsson, Gauti, and Paul Krugman. 2012. "Debt, Deleveraging, and the Liquidity Trap: A Fisher-Minsky-Koo Approach." *Quarterly Journal of Economics*, 127(3): 1469–1513.

Eisfeldt, Andrea, and Adriano Rampini. 2006. "Capital Reallocation and Liquidity." *Journal of Monetary Economics*, 53(3): 369–399.

Ellen, Ingrid Gould, Johanna Lacoe, and Claudia Ayanna Sharygin. 2013. "Do Foreclosures Cause Crime?" *Journal of Urban Economics*, 59–70.

Engelhardt, Gary V. 1996. "Consumption, Down Payments, and Liquidity Constraints." *ournal of Money, Credit and Banking*, 255–271.

Fisher, Irving. 1933. "The Debt-Deflation Theory of Great Depressions." *Econometrica*, 337–357.

Gerardi, Kristopher, Eric Rosenblatt, Paul S Willen, and Vincent Yao. 2015. "Foreclosure externalities: Some new evidence." *NBER Working Paper 20593*.

Glaeser, Edward L., and Jesse M. Shapiro. 2003. "The Benefits of the Home Mortgage Interest Deduction." *Tax Policy and the Economy*, 17: 37–82.

Green, Daniel, Brian Melzer, Jonathan A. Parker, and Ryan Pfirrmann-Powell. 2014. "Accelerator or Brake? Microeconomic Estimates of the 'Cash for Clunkers' and Aggregate Demand." *Working Paper*.

Guren, Adam, and Tim McQuade. 2015. "How Do Foreclosures Exacerbate Housing Downturns?" *Working Paper*.

Hayek, Friedrich von. 1931. "The "Paradox" of Saving." *Economica*, 32: 125–169.

Hembre, Erik. 2015. "The Price of Homeowners: An Examination of the First-time Homebuyer Tax Credit." *Working Paper*.

House, Christopher, and Matthew Shapiro. 2008. "Temporary Investment Tax Incentives: Theory with Evidence from Bonus Depreciation." *American Economic Review*, 98(3): 737–68.

Iacoviello, Matteo. 2005. "House Prices, Borrowing Constraints and Monetary Policy in the Business Cycle." *American Economic Review*, 739–764.

Kaplan, Greg, Kurt Mitman, and Gianluca Violante. 2016. "Consumption and House Prices in the Great Recession: Model Meets Evidence." *NBER Working Papers*.

Keynes, John Maynard. 1936. *The General Theory of Employment, Interest, and Money.* London: Macmillan.

Kiyotaki, Nobuhiro, and John Moore. 1997. "Credit Cycles." *Journal of Political Economy*, 105(21): 211–248.

Kolko, Jed. 2012. "Consumer Optimism: Too Much of a Good Thing?" `http://www.trulia.com/blog/trends/trulia-american-dream-survey/`, Accessed: 2016-06-15.

Lorenzoni, Guido. 2008. "Inefficient Credit Booms." *Review of Economic Studies*, 75(3): 809–833.

Mian, Atif, and Amir Sufi. 2009. "The consequences of mortgage credit expansion: Evidence from the US mortgage default crisis." *Quarterly Journal of Economics*, 124(4): 1449.

Mian, Atif, and Amir Sufi. 2011. "House Prices, Home Equity-Based Borrowing, and the U.S. Household Leverage Crisis." *American Economic Review*, 2132–2156.

Mian, Atif, and Amir Sufi. 2012. "The Effects of Fiscal Stimulus: Evidence from the 2009 Cash for Clunkers Program." *Quarterly Journal of Economics*, 1107–1142.

Mian, Atif, and Amir Sufi. 2014. "What Explains the 2007-2009 Drop in Employment?" *Econometrica*, 2197–2223.

Mian, Atif, and Amir Sufi. 2015. *House of Debt: How They (and You) Caused the Great Recession, and How We Can Prevent It from Happening Again.* University of Chicago Press.

Mian, Atif, Kamelesh Rao, and Amir Sufi. 2013. "Household Balance Sheets, Consumption, and the Economic Slump." *Quarterly Journal of Economics*, 1687–1726.

National Association of Realtors. 2010. "Profile of Home Buyers and Sellers 2010."

Ramey, Valerie, and Matthew Shapiro. 2001. "Displaced Capital: A Study of Aerospace Plant Closings." *Journal of Political Economy*, 109(5): 958–992.

Rognlie, Matthew, Andrei Shleifer, and Alp Simsek. 2014. "Investment Hangover and the Great Recession." *NBER Working Paper No. 20569*.

Shleifer, Andrei, and Robert Vishny. 1992. "Liquidation Values and Debt Capacity: A Market Equilibrium Approach." *Journal of Finance*, 47(4): 1343–1366.

Shleifer, Andrei, and Robert Vishny. 2010. "Unstable Banking." *Journal of Financial Economics*, 97(3): 306–318.

Stein, Jeremy. 1995. "Prices and Trading Volume in the Housing Market: A Model with Down-Payment Effects." *Quarterly Journal of Economics*, 379–406.

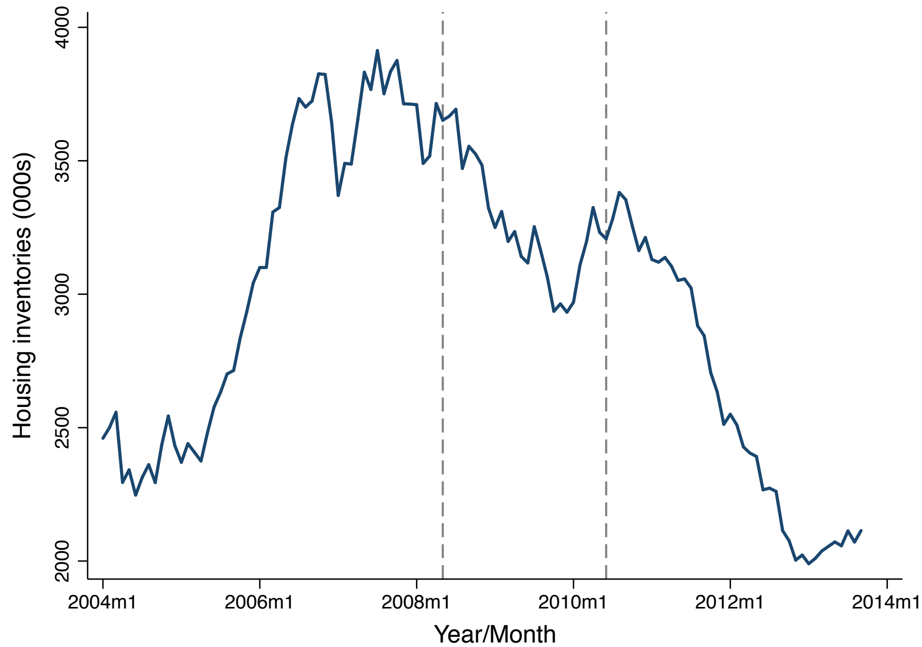
Stroebel, Johannes, and Joseph Vavra. 2016. "House Prices, Local Demand, and Retail Prices." *NBER Working Papers 20710*.

Whitaker, Stephen, and Thomas J Fitzpatrick IV. 2013. "Deconstructing Distressed-Property Spillovers: The Effects of Vacant, Tax-Delinquent, and Foreclosed Properties in Housing Submarkets." *Journal of Housing Economics*, 22(2): 79–91.

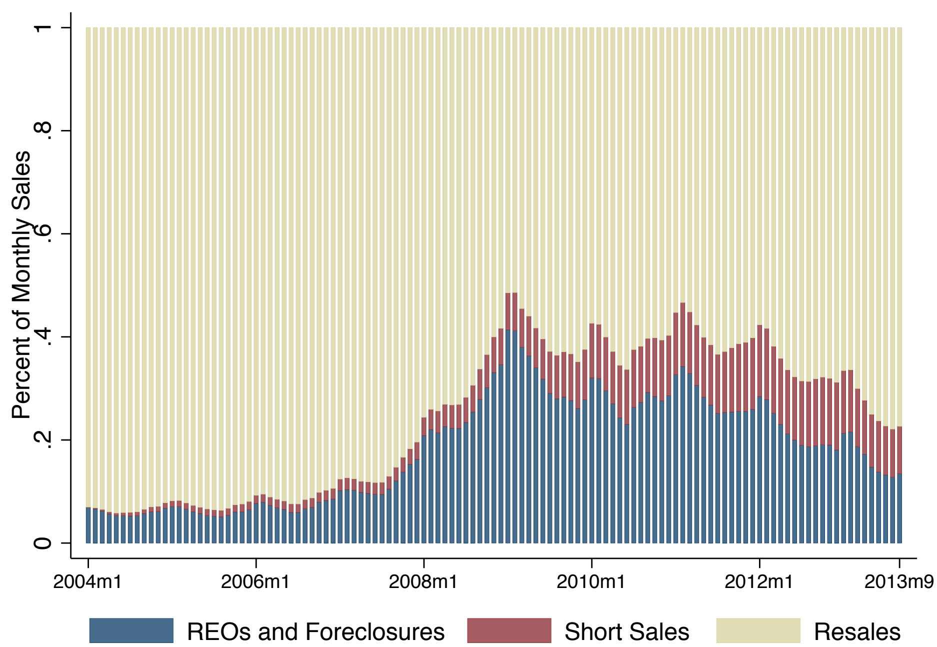
Zwick, Eric, and James Mahon. 2016. "Tax Policy and Heterogeneous Investment Behavior." *NBER Working Paper No. 21876*.

Figure 1: The State of the Housing Market

(a) Inventory (NAR)

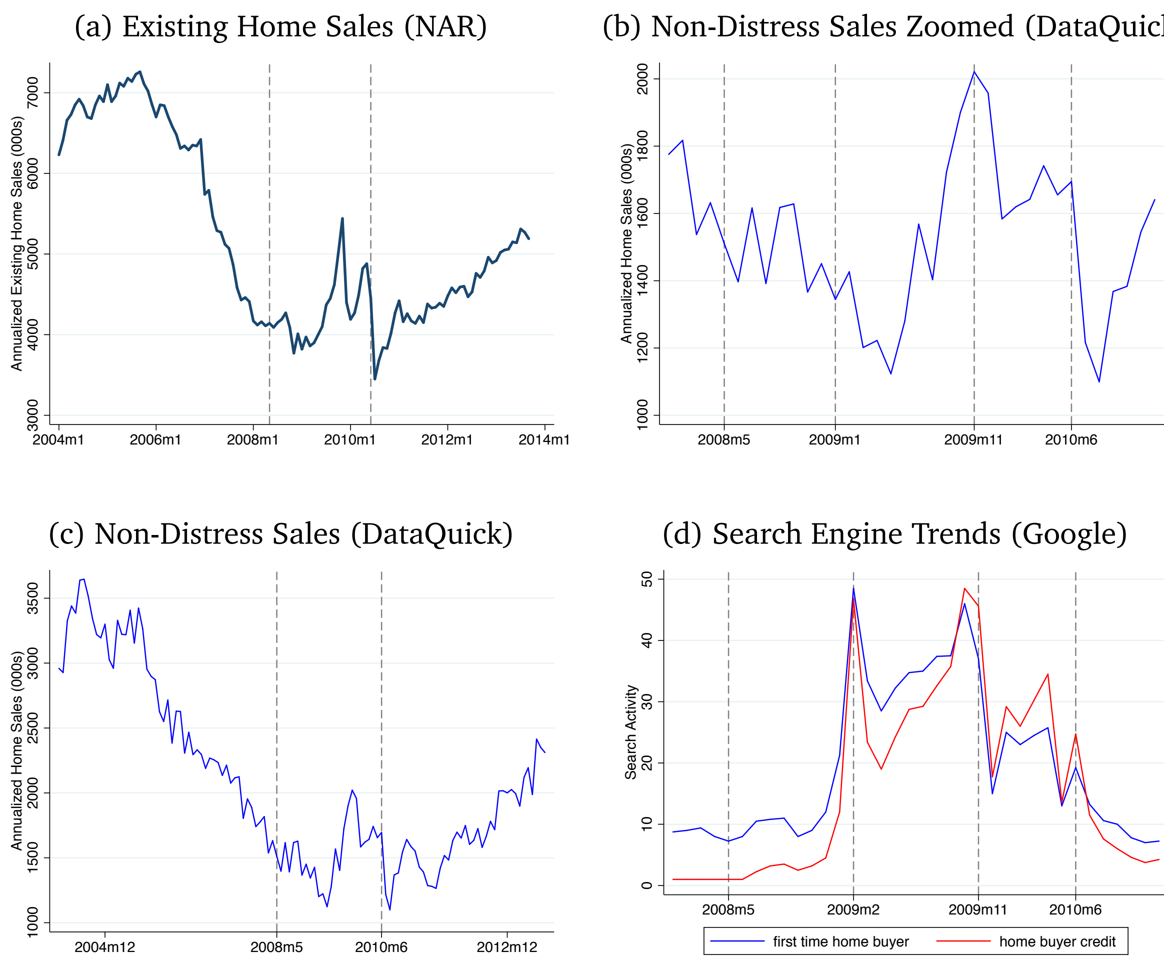


(b) Existing Home Sales Composition (DataQuick)



Notes: Panel (a) plots seasonally adjusted housing inventory, defined as a the number of homes listed for sale, from the National Association of Realtors (NAR). Panel (b) plots the month-by-month share of existing home sales in DataQuick in each of three categories: non-distress resales, short sales, and institution-owned or foreclosures.

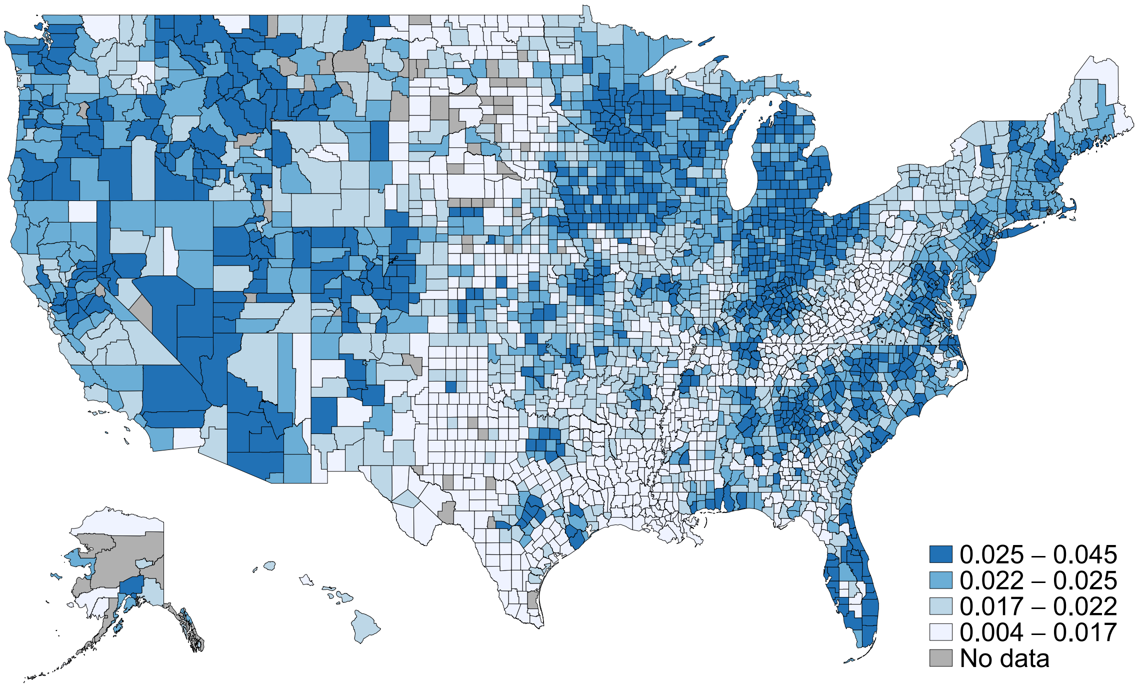
Figure 2: Aggregate Home Sales and the Policy Window



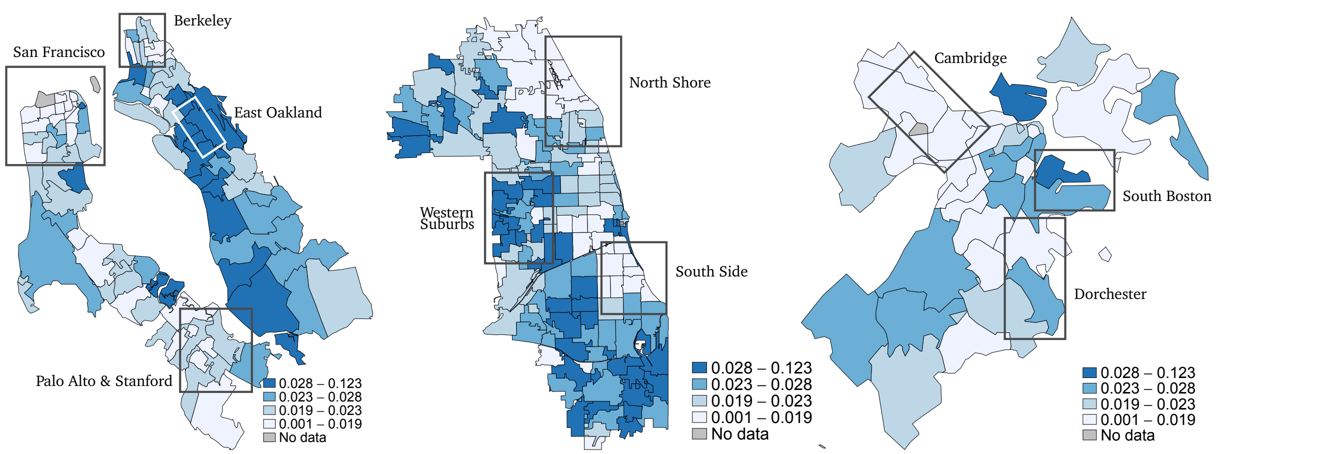
Notes: Panel (a) plots existing home sales on a seasonally adjusted annual basis from the National Association of Realtors (NAR). Panels (b) and (c) plot seasonally adjusted, monthly home sales from DataQuick along with vertical markers for policy events. These data exclude distress transactions and new construction. Panel (d) plots Google search trend data for the terms “first time home buyer” and “home buyer credit” along with vertical markers for policy events. The vertical markers in panels (b) and (d) correspond to the FTHC loan program, the start of the FTHC grant program, the scheduled expiration of the FTHC grant program, and the actual expiration of the FTHC grant program, respectively. The markers in panels (a) and (c) correspond to the FTHC loan program and the actual expiration of the FTHC grant program, respectively.

Figure 3: Maps of FTHC Program Exposure

(a) National Exposure



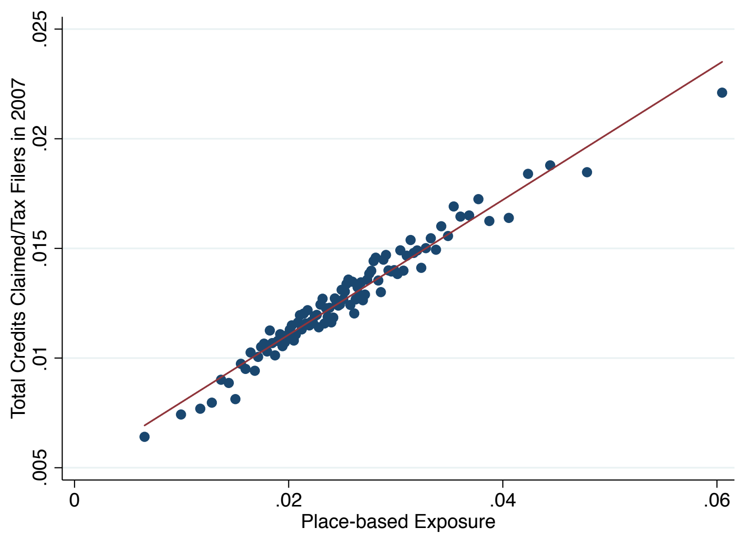
(b) Exposure in major metropolitan areas



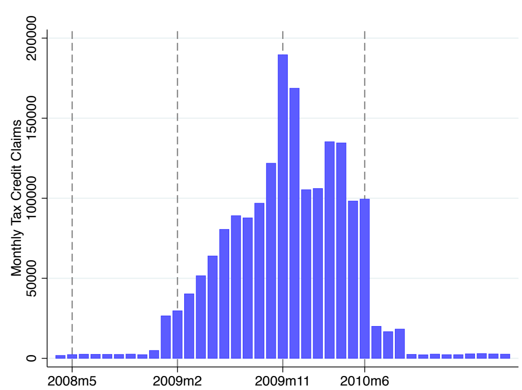
Notes: Panel (a) presents a county map of program measure of exposure, defined as the number of first-time homebuyers in a place in the year 2000 divided by the number of tax filers in 2000. Panel (b) presents ZIP5 maps for three metro areas: from left to right, the San Francisco Bay Area, Chicagoland within Cook County and Boston and Cambridge. Boxes mark particular cities or neighbourhoods in each metro area. Darker shadings reflect higher exposure.

Figure 4: Program Exposure and FTHC Claims

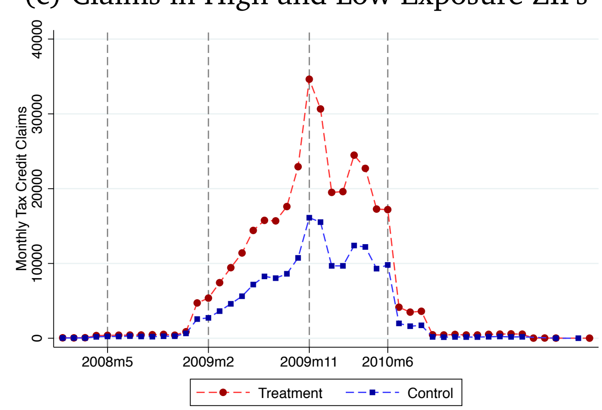
(a) Claims versus Exposure, ZIP



(b) National Claims



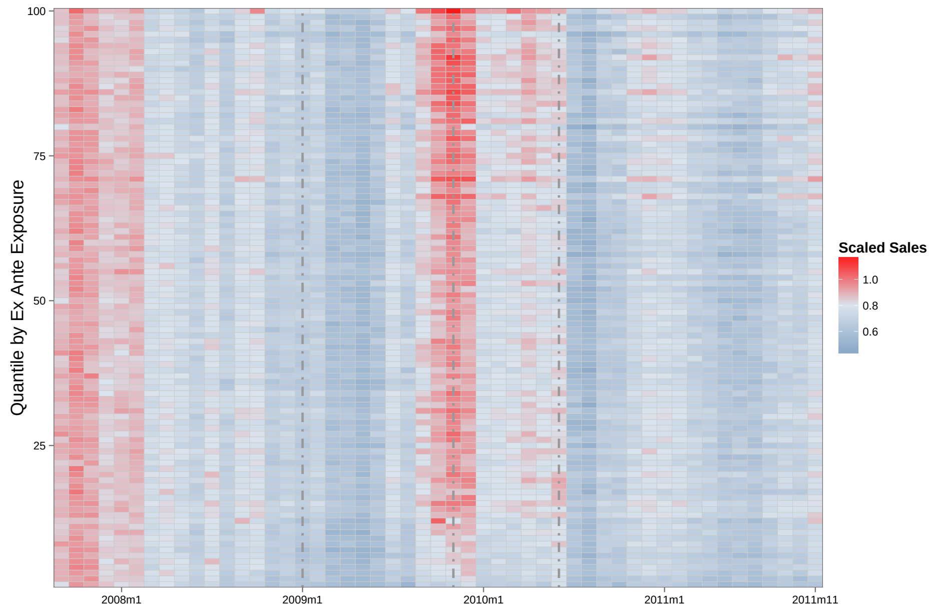
(c) Claims in High and Low Exposure ZIPs



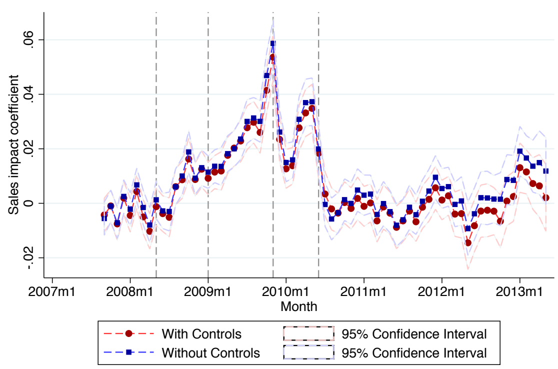
Notes: Panel (a) plots binned bivariate means (i.e., a "binscatter") of ZIP-level FTHC claims from tax records scaled by the number of tax filers in 2007 versus program exposure. Exposure is defined as the number of first-time homebuyers in a place in the year 2000. Panel (b) plots national counts of FTHC claims by month of home purchase for purchases between February 2009 and September 2010 along with vertical markers for policy events. The vertical markers correspond to the FTHC loan program, the start of the FTHC grant program, the scheduled expiration of the FTHC grant program, and the actual expiration of the FTHC grant program, respectively. Panel (c) plots claim counts for high and low program exposure quintiles of ZIPs sorted using the program exposure. The quintiles are formed using weights that ensure each quintile has equal population in 2007.

Figure 5: The Effect of the FTHC on Home Sales

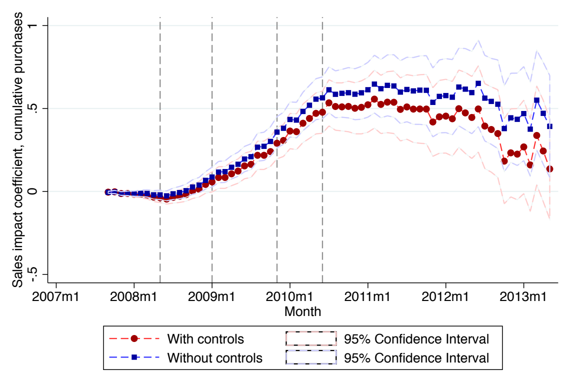
(a) Difference-in-Differences Calendar Time Heatmap



(b) ZIP with CBSA Fixed Effects



(c) Cumulative ZIP with CBSA Fixed Effects



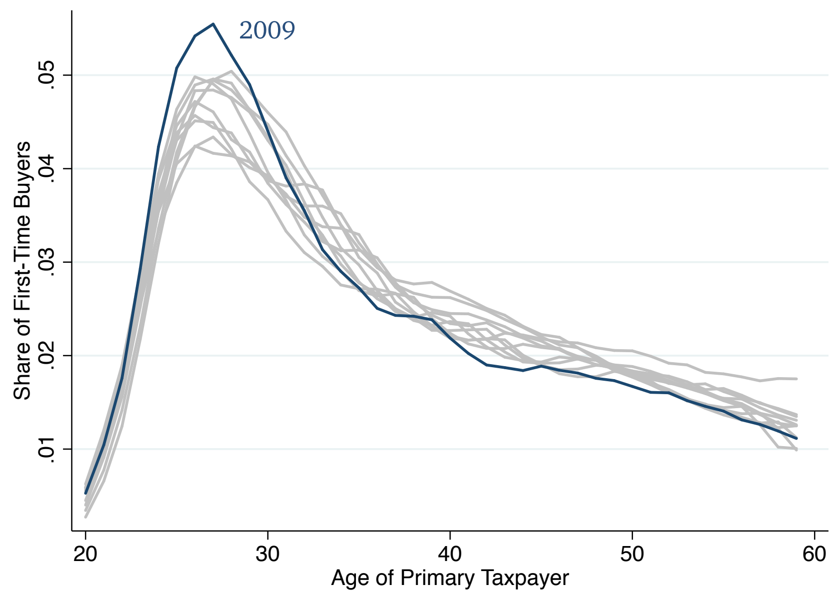
Notes: These figures plot the monthly and cumulative effects of the FTHC on non-distress resales at the ZIP level. Panel (a) plots a difference-in-differences, calendar time heatmap of monthly sales between July 2007 and September 2011 for ZIPs divided into 100 quantiles and sorted based on program exposure. Columns correspond to months and rows correspond to groups of ZIPs sorted by exposure. Exposure is the number of first-time homebuyers in a ZIP in 2000 scaled by the number of tax filing units in 2000. Each cell's shading corresponds to a level of the key outcome variable, which is monthly home sales scaled by average monthly home sales in 2007. The quantiles are formed using weights that ensure each quantile has an equal number of home sales in 2007. Panel (b) plots coefficients for monthly home sales regressions both with and without controls. Panel (c) plots coefficients for cumulative sales regressions. We run month-by-month regressions, weighted by total home sales in 2007, of the form:

$$\frac{y_i}{\text{Sales}_{i,2007}} = \alpha_{\text{CBSA}} + \beta \text{Exposure}_i + \gamma X_i + \varepsilon_i$$

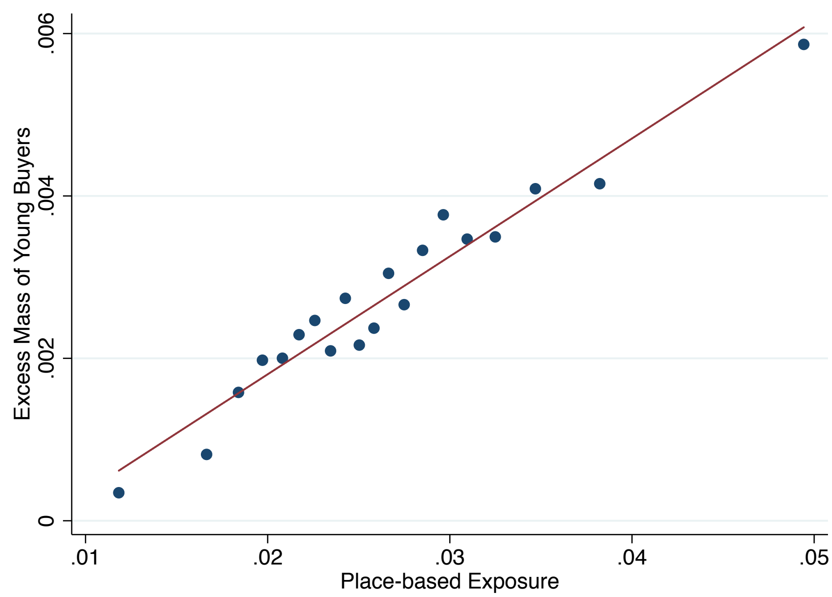
where y_i is either monthly home sales in place i or cumulative monthly home sales in place i beginning 17 months before the program. X_i is a control set that includes log population, the average unemployment rate from 2006 through 2010, and the log of average gross income. Program exposure is normalized by its cross sectional standard deviation.

Figure 6: Policy Shift in the Age Distribution of First-Time Buyers

(a) Distribution of First-Time Buyers, 2002-2013

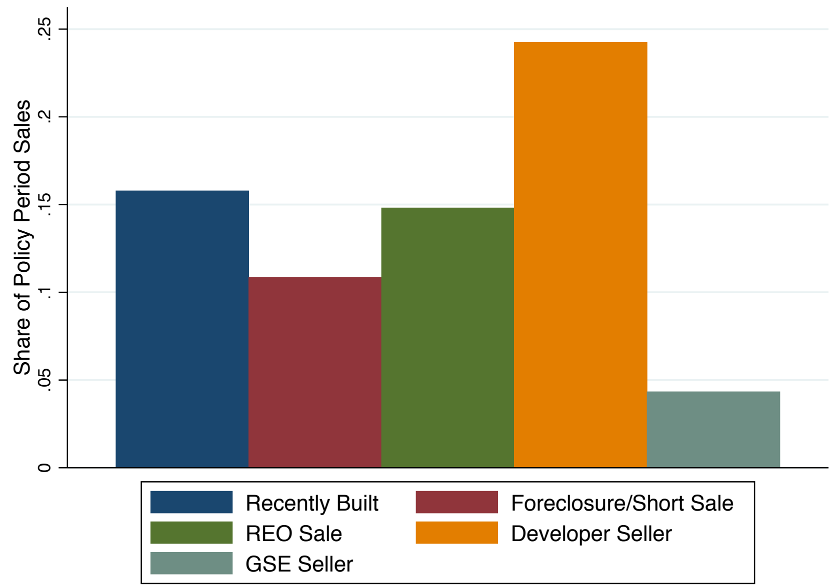


(b) Excess Mass of Young Buyers in 2009 versus Program Exposure, ZIP



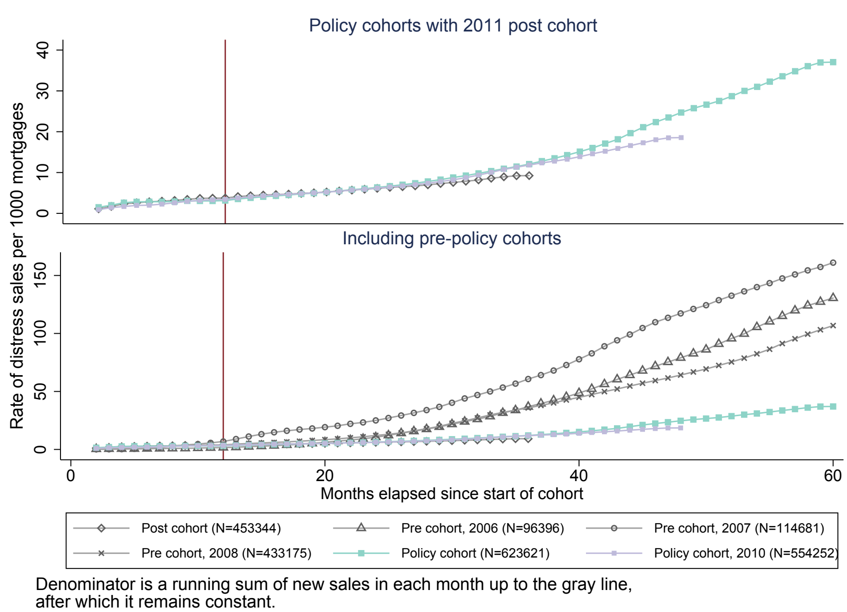
Notes: Panel (a) plots age distributions of first-time homebuyers identified using income tax return and information return information for the years between 2002 and 2013. The FTHC was primarily in effect in 2009, which is highlighted in the graph. All other years are in gray. Panel (b) shows the correlation between the shift in the age distribution in 2009 and program exposure. We decompose the national shift in the age distribution in 2009 into contributions from each ZIP. For each ZIP, we compute the difference between the ratio of buyers aged 30 or younger to total new homebuyers in 2009 versus the average ratio of buyers aged 30 or younger to total new homebuyers in other years. We then plot binned bivariate sums of these ZIP-level contributions against average exposure in each bin.

Figure 7: Transaction Shares for Non-Traditional Sellers During the FTHC Policy Period



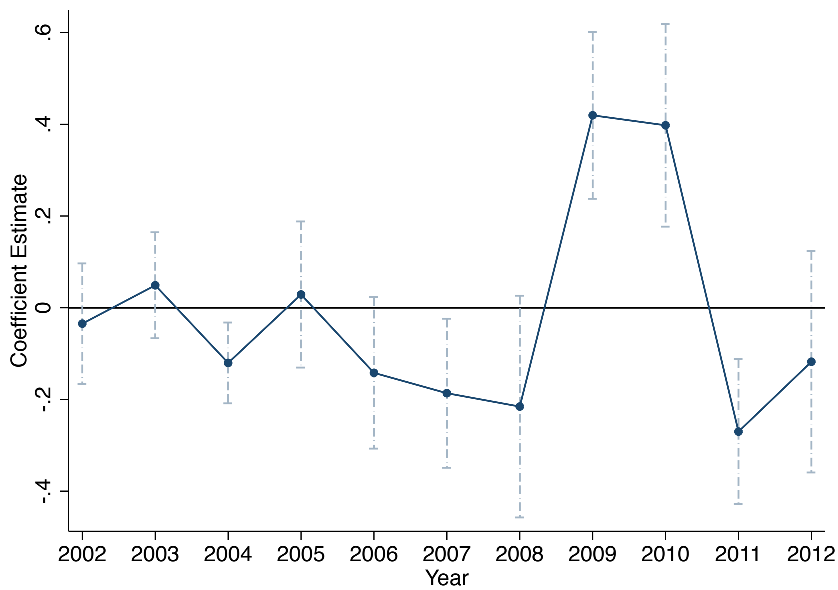
Notes: The figure displays weighted averages of the share of total transactions in each category in a ZIP, with average monthly sales in 2007 as the weight. See Section 5 for category definitions.

Figure 8: Default Rates for Policy Period Buyers versus Other Cohorts



Notes: The figure plots cumulative distress cohorts for purchases made during the policy period and compares these to cohorts based on 2006, 2007, 2008, and 2011 sales. We measure transitions into distress using DataQuick by following properties purchased in a given year and computing the share of properties that become distress sales.

Figure 9: The Effect of the FTHC on House Prices



Notes: The figure plots coefficients for yearly house price growth regressions from market-adjusted house price indices at the ZIP level from FHFA. We run year-by-year regressions, weighted by total home sales in 2007, of the form:

$$\Delta \tilde{r}_i = \alpha_{\text{CBSA}} + \beta \text{Exposure}_i + \gamma X_i + \varepsilon_i$$

where $\tilde{r}_i$ is the first difference in market-adjusted house price growth in place i. X_i is a control set that includes log population, the average unemployment rate from 2006 through 2010, and the log of average gross income. Program exposure is normalized by its cross sectional standard deviation. The left-hand-side is multiplied by 100 so the treatment effects are percentage points of growth per standard deviation in program exposure.

Table 1: Summary Statistics: Home Sales Analyses

	Mean	10th	Median	90th	N
Housing Transactions					
Monthly Home Sales (SA)	19.6	3.7	14.3	41.5	1042213
Home Sales/Average Monthly Sales in 2007	1.01	0.44	0.92	1.72	1007320
Program Exposure (ZIP)					
First-Time Buyers/Tax-Filing Units in 2000 (IRS)	3.00	1.92	2.90	4.15	8883
Cross Sectional Characteristics (ZIP)					
Population, 000s (ACS)	23.26	5.58	20.32	45.06	8883
Average Gross Income, 2005 (IRS)	62.45	32.12	50.43	99.20	8883
Unemployment Rate, 06-10 Average (ACS)	7.83	4.30	7.20	12.20	8883
Median Age, 06-10 Average (ACS)	38.51	31.70	38.40	45.00	8883
Median Rent, 06-10 Average (ACS)	970.93	637.00	910.00	1397.00	8882
Fraction below Poverty Line, 06-10 Average (ACS)	12.05	3.60	9.80	23.80	8883
Fraction of Census Blocks Classified as Urban (Census)	83.30	39.80	99.10	100.00	8883

Notes: This table presents summary statistics for analysis of the FTHC at the ZIP level. Statistics are presented at both the ZIP-by-month and ZIP levels. Monthly Home Sales include non-distress resales. Appendix A provides a detailed description of the data sources used and variable construction and presents more statistics.

Table 2: Correlates of Program Exposure

	LHS is Exposure		
	Coefficient	R^2	N
Exposure Correlates:			
Median Age	-.0519+	0.0027	8882
	(.0305)		
Median Rent	.194**	0.0374	8881
	(.053)		
Fraction below Poverty Line	-.28**	0.0785	8882
	(.0334)		
Fraction Classified as Urban	.0786**	0.0062	8882
	(.0222)		
Controls:			
Log(Population)	.0777**	0.0060	8882
	(.0277)		
Unemployment Rate	-.102**	0.0104	8882
	(.0335)		
Log(Average Gross Income)	.025	0.0006	8882
	(.0345)		

Notes: This table presents bivariate regressions of program exposure on ZIP level observables. Variables have been normalized so the coefficients can be interpreted as a 1 standard deviation change in x produces a β standard deviation change in exposure, where β is the reported coefficient. Standard errors are clustered at the CBSA level.

Table 3: The Effect of the FTHC on Home Sales

	(1) No Controls	(2) Controls	(3) CBSA FE	(4) Logs	(5) No wgts	(6) Ex sand
Pre-policy	0.001	0.001	0.002	0.005	0.002	0.001
2007m9-2009m1	(0.005)	(0.005)	(0.003)	(0.004)	(0.003)	(0.003)
Observations	8882	8882	8882	8882	8882	7540
R^2	0.0	0.014	0.37	0.36	0.303	0.391
Policy	0.025**	0.024*	0.024**	0.031**	0.03**	0.02**
2009m2-2010m6	(0.01)	(0.01)	(0.005)	(0.007)	(0.008)	(0.005)
Observations	8881	8881	8881	8881	8881	7540
R^2	0.012	0.018	0.472	0.492	0.435	0.486
Post-policy	0.014	0.019	0.002	-0.005	0.009	-0.003
2010m7-2011m11	(0.011)	(0.012)	(0.005)	(0.008)	(0.008)	(0.004)
Observations	8878	8878	8878	8878	8878	7539
R^2	0.002	0.042	0.568	0.597	0.513	0.595
Early policy	0.013	0.012	0.017**	0.029**	0.022**	0.014**
2009m2-2009m9	(0.008)	(0.008)	(0.005)	(0.008)	(0.007)	(0.005)
Observations	8881	8881	8881	8881	8881	7540
R^2	0.004	0.008	0.4	0.426	0.347	0.431
Spike 1	0.046**	0.043**	0.04**	0.042**	0.047**	0.036**
2009m10-2009m12	(0.012)	(0.013)	(0.007)	(0.007)	(0.009)	(0.007)
Observations	8841	8841	8841	8841	8841	7535
R^2	0.022	0.032	0.46	0.452	0.417	0.439
Spike 2	0.033**	0.031**	0.032**	0.041**	0.037**	0.028**
2010m4-2010m6	(0.01)	(0.011)	(0.007)	(0.008)	(0.009)	(0.007)
Observations	8852	8852	8852	8852	8852	7536
R^2	0.013	0.019	0.369	0.382	0.337	0.386
Controls	No	Yes	Yes	Yes	Yes	Yes
CBSA FE	No	No	Yes	Yes	Yes	Yes

Notes: This table presents the average monthly effects of the FTHB on home sales for ZIPs pooled over different policy windows with various specifications. We run cross sectional regressions, weighted by total home sales in 2007, of the form:

$$\frac{\overline{\text{Sales}}_{i,t\to T}}{\text{Sales}_{i,2007}} = \alpha + \beta \text{Exposure}_i + \gamma X_i + \varepsilon_i$$

where y_i is average monthly home sales in place i over the relevant time period. In controls specifications, X_i is a control set that includes log population, the average unemployment rate from 2006 through 2010, and log average gross income. Exposure is normalized by its cross sectional standard deviation. Column (3) includes CBSA fixed effects. In column (4), we respecify the left hand side variable in logs. Column (5) presents unweighted regressions. Column (6) excludes Arizona, California, and Nevada. All regressions are clustered at the CBSA level.

Table 4: The Effect of the FTHC on Starter Homes vs. Large Homes

(a) 1-3 Bedrooms, ZIP

	(1) No Controls	(2) CBSA FE
Pre-policy	0.01	0.012*
2007m9-2009m1	(0.008)	(0.005)
Observations	5143	5143
R^2	0.004	0.419
Policy	0.018	0.025**
2009m2-2010m6	(0.011)	(0.006)
Observations	5143	5143
R^2	0.007	0.501
Post-policy	0.009	0.01+
2010m7-2011m11	(0.012)	(0.005)
Observations	5143	5143
R^2	0.001	0.596
Early policy	0.008	0.019**
2009m2-2009m9	(0.009)	(0.005)
Observations	5143	5143
R^2	0.002	0.447
Spike 1	0.033*	0.037**
2009m10-2009m12	(0.014)	(0.008)
Observations	5134	5134
R^2	0.013	0.456
Spike 2	0.024*	0.031**
2010m4-2010m6	(0.012)	(0.006)
Observations	5138	5138
R^2	0.009	0.415
Controls	No	Yes
CBSA FE	No	Yes

(b) 4+ Bedrooms, ZIP

	(1) No Controls	(2) CBSA FE
Pre-policy	-0.008	-0.003
2007m9-2009m1	(0.007)	(0.006)
Observations	5128	5128
R^2	0.004	0.387
Policy	-0.003	0.006
2009m2-2010m6	(0.008)	(0.006)
Observations	5124	5124
R^2	0.0	0.366
Post-policy	-0.007	-0.0
2010m7-2011m11	(0.008)	(0.006)
Observations	5128	5128
R^2	0.002	0.388
Early policy	-0.006	0.004
2009m2-2009m9	(0.007)	(0.005)
Observations	5097	5097
R^2	0.002	0.347
Spike 1	0.0	0.01
2009m10-2009m12	(0.009)	(0.007)
Observations	4944	4944
R^2	0.0	0.347
Spike 2	-0.0	0.008
2010m4-2010m6	(0.009)	(0.008)
Observations	4938	4938
R^2	0.0	0.318
Controls	No	Yes
CBSA FE	No	Yes

Notes: These tables present regressions of the same form as those in table 3. We divide the home sales series into "starter" homes—defined as those with 1, 2, or 3 bedrooms—and large homes—defined as those with 4 or more bedrooms. We run the ZIP level specifications separately for each series. The analysis sample here is the subset of the main analysis sample where fewer than 20% of transactions between 2004 and 2013 have missing bedrooms data.

Table 5: The Effect of the FTHC on Home Sales

	(1) CBSA FE	(2) No wgts	(3) Ex sand	(4) Trimmed	(5) $\overline{\text{Sales}} > \text{P10}$	(6) Low p	(7) High p
	LHS is Long Diff Sales						
Coefficient	0.021**	0.026**	0.02**	0.014**	0.021**	0.027**	0.005
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.005)	(0.007)
Observations	8714	8714	7453	8008	7885	2431	2393
R^2	0.404	0.393	0.368	0.41	0.412	0.589	0.393
	LHS is Long Diff Construction						
Coefficient	0.002	0.01	0.004	0.011*	0.002	0.007	0.02
	(0.007)	(0.007)	(0.008)	(0.006)	(0.007)	(0.015)	(0.016)
Observations	4722	4722	3983	4332	4523	1164	1125
R^2	0.131	0.129	0.129	0.164	0.132	0.205	0.188
	LHS is Long Diff Foreclosures & Short Sales						
Coefficient	0.051*	0.041*	0.056*	0.041**	0.052*	0.077**	0.014
	(0.022)	(0.019)	(0.024)	(0.015)	(0.022)	(0.026)	(0.029)
Observations	8533	8533	7202	7834	7755	2411	2331
R^2	0.366	0.336	0.352	0.43	0.372	0.416	0.3
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents regressions of the average monthly effects of the FTHC on different categories of home sales. We run cross sectional regressions, weighted by total home sales in 2007, of the form:

$$y_i = \alpha_{CBSA} + \beta \text{Exposure}_i + \gamma X_i + \varepsilon_i$$

where y_i is a housing market outcome in place i over the relevant time period. In the first row, the outcome is the difference in average monthly non-distress home resales for the policy period versus the 17 month pre period. In the second row, the outcome is the difference in average monthly new construction sales for the policy period versus the pre period. In the third row, the outcome is the difference in average monthly foreclosures and short sales for the policy period versus the pre preiod. Exposure is normalized by its cross sectional standard deviation. All columns include CBSA fixed effects and controls that include log population, the average unemployment rate from 2006 through 2010, and log average gross income. Column (2) presents unweighted regressions. Column (3) excludes Arizona, California, and Nevada. Column (4) trims the left-hand-side variable at the 5th and 95th percentiles. Column (5) restricts the sample to places with average home sales in 2007 above the 10th percentile. Columns (6) and (7) divide the sample of ZIPs into the bottom three ("Low p") and top three ("High p") deciles in median house prices during 2008. All regressions are clustered at the CBSA level.

Table 6: The Effect of the FTHC on House Prices

	(1) CBSA FE	(2) No wgts	(3) Ex sand	(4) Trimmed	(5) $\overline{\text{Sales}} > \text{P10}$	(6) Low p	(7) High p
	LHS is Long Diff Price Growth (FHFA Mkt Adjusted)						
Coefficient	0.774**	0.807**	0.693**	0.565**	0.786**	0.719**	0.325
	(0.151)	(0.188)	(0.159)	(0.126)	(0.152)	(0.249)	(0.283)
Observations	8364	8364	7127	7692	7582	2269	2291
R^2	0.607	0.563	0.606	0.613	0.612	0.598	0.604
	LHS is Long Diff Price Growth (FHFA Raw)						
Coefficient	0.789**	0.846**	0.696**	0.594**	0.8**	0.696**	0.421
	(0.154)	(0.186)	(0.161)	(0.126)	(0.155)	(0.248)	(0.289)
Observations	8381	8381	7147	7708	7601	2287	2292
R^2	0.61	0.567	0.604	0.61	0.615	0.582	0.625
	LHS is Long Diff Price Growth (CoreLogic)						
Coefficient	0.656**	0.639**	0.605**	0.561**	0.663**	1.163**	0.561
	(0.185)	(0.175)	(0.212)	(0.177)	(0.188)	(0.289)	(0.396)
Observations	5759	5759	4700	5288	5633	1354	1679
R^2	0.647	0.678	0.572	0.608	0.644	0.663	0.683
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents regressions of the cumulative effects of the FTHC on different measures of house price growth. We run cross sectional regressions, weighted by total home sales in 2007, of the form:

$$y_i = \alpha_{CBSA} + \beta \text{Exposure}_i + \gamma X_i + \varepsilon_i$$

where y_i is a housing market outcome in place i over the relevant time period. The first two rows use price index data from FHFA. In the first row, the outcome is the market-adjusted, cumulative annual log price differences during 2009 and 2010 minus cumulative annual log price differences during 2007 and 2008. We first estimate ZIP-specific housing market betas in the ten-year window from 1997 to 2006 and then subtract beta times the market return to compute a ZIP-level excess return. In the second row, the outcome is the unadjusted version of the price series from the first row. In the third row, the outcome is raw cumulative monthly log price differences from CoreLogic during the policy period minus cumulative monthly log price differences during the 17-month pre-period. In all cases, we multiply the left-hand-side by 100 so the treatment effect units are percentage points of growth per standard deviation change in program exposure. All series are seasonally adjusted prior to aggregation. Exposure is normalized by its cross sectional standard deviation. All columns include CBSA fixed effects and controls that include log population, the average unemployment rate from 2006 through 2010, and log average gross income. Column (2) presents unweighted regressions. Column (3) excludes Arizona, California, and Nevada. Column (4) trims the left-hand-side variable at the 5th and 95th percentiles. Column (5) restricts the sample to places with average home sales in 2007 above the 10th percentile. Columns (6) and (7) divide the sample of ZIPs into the bottom three ("Low p") and top three ("High p") deciles in median house prices during 2008. All regressions are clustered at the CBSA level.

For Online Publication

A Data Build and Discussion

The analysis combines a large number of proprietary and public use data sources. In this appendix, we describe each source in detail, describe variable construction, and walk step-by-step through sample selection.

A.1 Data Sources and Sets

1. **Tax records, IRS/OTA**: These data are anonymized individual-level data collected by the IRS for the purposes of administering the tax collection process. They are made available through collaboration with the Office of Tax Analysis in the US Treasury Department and the IRS Division of Research, Analysis, and Statistics.

   We compile the following items:

   (a) ZIP-5 level cross sections of FTHC claims from Form 5405. These include claims for versions 2 and 3 of the FTHC program (i.e., the grant program) and also claims for the Long-Time Homebuyer Credit.

   (b) First-time homebuyer and tax filer counts from individual tax returns and information returns for the years 1998, 1999, and 2000.

   (c) Tax filer counts for the year 2007 from individual tax returns.

   Tax credit claims cover 18,073 ZIPs, the homebuyer counts from 2000 cover 24,923 ZIPs, and the 2007 records cover 35,647 ZIPs.

2. **DataQuick deed records**: We clean and merge data retrieved from DataQuick's assessor file, which contains information on individual properties used to assess property taxes, and DataQuick's recorder file, which tracks ownership changes and loans secured by properties.

   We begin with records from 2000 to 2013, up to the company's acquisition by CoreLogic. Assessor data cover 1,819 counties accounting for 91.8% of the US population. While not all counties tracked by DataQuick provide recorder data, 942 counties do and these match to 88% of all deeds tracked in the assessor data.

   Once the two files are joined, we produce our "canonical" list of transactions by applying the following filters:

   (a) Including only resales and new construction (types R and S in SR_TRAN_TYPE) which are arm's length[23];

[23] To our knowledge, the arm's length flag in DataQuick is the output of a "model" that classifies whether resales are genuine arm's length transactions. It automatically excludes refinances and intermediate documents in a distress sale process. It attempts to exclude transactions made between related parties at non-market prices, for example, because of divorce or bequest.

(b) Removing transactions between institutional buyers, such as two developers or a developer with a GSE;

(c) Removing middle-man sales where a property buyer sells the property on the same day;

(d) Keeping only the transaction with the highest transfer value if there are duplicates with the same property, transaction date, buyer and seller, with the intent of removing incomplete duplicate records.

We use the filtered, merged data to create three output datasets:

(a) **DataQuick transaction counts.** We aggregate the transactions in the recorder data to the (geographic unit)×(month)×(transaction type)×(distress type) level, where the geographic unit is a ZIP or county. For each level, we count the number of transactions.

This initial dataset is divided into six distinct datasets used during different parts of the analysis:

i. all sales (transaction types R and S, distress types ∈ NULL, A, I, S and O),
ii. non-distress resales (transaction type R, null distress indicator)[24]
iii. new housing/subdivision sales (transaction type S),
iv. purchases at foreclosure auctions (distress type A),
v. DataQuick-inferred short sales (distress type I),
vi. REO liquidations (distress type S).

A variation of the above aggregation splits counts to the (geographic unit)×(month) ×(transaction type)×(distress type)×(bedroom) level, where bedroom information comes from the assessor file. We mark each transaction as having missing bedroom information, 1 to 3 bedrooms, or 4 or more bedrooms. Geographies with more than 20% of months lacking transactions on properties with bedroom data, as well as geographies in states with consistently poor bedroom data collection, are dropped.

A second variation requires processing individual records further before aggregation:

i. Transactions on recently built housing are marked by checking the SA_YR_BLT column in the assessor file.
ii. Transactions on properties owned by developers or owned by government-sponsored enterprises are marked through running regular expression searches on the SR_BUYER column in the recorder file. The regular expressions for builders, developers, and GSEs are:

```
builder_re_list =
  ('HOME|BUILDER| BUILD|BLDR| HM(\s|S\s)| CONST |REAL ESTATE|',
   'PULTE|RYLAND|NVR|DR HORTON|CENTEX|LENNAR|MERITAGE|',
```

[24] Distress type O transactions appear to be classified as distress by DataQuick, but shows no other sign of being a distress sale, such as an unusually low sale price or institutional seller. We exclude them by default, but have confirmed the robustness of our results by including them as non-distress transactions. They make up at most 2% of the sample in policy period months.

```
    'STANDARD PACIFIC')
developer_re_list =
  ('LLC|CORP|COMPANY| INC$|INTL| LAND |PROPERT(Y|IES)|TRUST|',
   'INVEST(OR|MENT)|LP')
gse_re_list =
  ('FNMA|FHLM|HUD-HOUSING|FEDERAL (H|NATIONAL)|FANNIE|',
   'SECRETARY OF (H|VET)|VETERANS AD')
```

iii. Transactions on properties considered recently in distress are marked through searching over a property's transaction history and checking if the property was distressed less than two years prior to the current transaction.

The exclusion of type L sales from the resales and the new housing datasets is due to DataQuick classifying properties with a later delinquent mortgage as type L, even if the sale associated with that mortgage was a non-distress sale. The label therefore cannot be used to infer whether the original transaction was distressed, not distressed, or otherwise unusual. However, these type L sales account for many transactions in the pre-crisis period, up to a majority in some places in California. For these reasons, they are included when constructing these latter datasets.

(b) **DataQuick price data:** For every geographic unit-by-month unit, we compute the group's median price based on the SR_VAL_TRANSFER column [for non-distressed properties]. We exclude transactions for which price data are missing, about 10% of all transactions, from the computation. We code as missing any units where fewer than five transactions are available. These price data serve to complement the CoreLogic price indices in places when the CoreLogic data are not available. Price regressions, with percent changes in prices as the dependent variable, use both sources.

(c) **DataQuick loans data:** DataQuick counts mortgages attached to the paperwork on a closed sale as loans related to that transaction, and allow up to three liens connected to one transfer. However, these rows miss loans taken out by the buyer from different banks or subsequent to the purchase, which may include second liens and "piggyback loans" used to cover the cost of the down payment.

We connect second liens, which are categorized as refinances in DataQuick, to transactions by sorting data on SR_PROPERTY_ID and SR_DATE_TRANSFER, and then linking all refinances between property transactions with the date of the preceding transaction. Loan values on refinances taking place less than 100 days after the preceding transaction are added to the loan value on that transaction.

3. **CoreLogic Prices**: CoreLogic Home Price Index (HPI) data from the national to the ZIP level were made available through the Initiative on Global Markets at Chicago Booth. Unlike the DataQuick price data, which records nominal values, the HPI is a variant of the Case-Shiller index measuring price changes in repeatedly transacted properties. The structure of the data is a balanced panel, available for 7,169 ZIP codes and 1,267 counties.

4. **Covariate data**: We construct a covariate dataset from the 2010 American Community

Survey and 2000 Census. The ACS data contains five-year averages (2006-2010) of demographic indicators estimated over the ZIP, county, and CBSA levels.

5. **GSE/FHA loans**: We use three datasets made publicly available after the conservatorship of Fannie Mae and Freddie Mac: the Fannie Mae Single-Family Loan Performance Data, the Freddie Mac Single Family Loan-Level Dataset, and the Ginnie Mae MBS Loan-Level Disclosure Data.

   The Fannie Mae and Freddie Mac data contain data on all 30-year fixed loan mortgages guaranteed by the GSEs from 2000 to the present. The Ginnie Mae data include both fixed rate and variable rate mortgages insured by the FHA, the VA and other agencies. Respectively, the data track 14.41 million, 11.16 million, and 11.05 million mortgages.

   The GSE data are split into "origination data" that record each mortgage's initial parameters and "performance" data that track each mortgage's repayment status until full repayment or default. The Ginnie Mae data can be sliced into two in this way, though mortgages were not tracked unless they were still active on October 2013, the first month Ginnie Mae released the data.[25]

A.2 Sample Selection

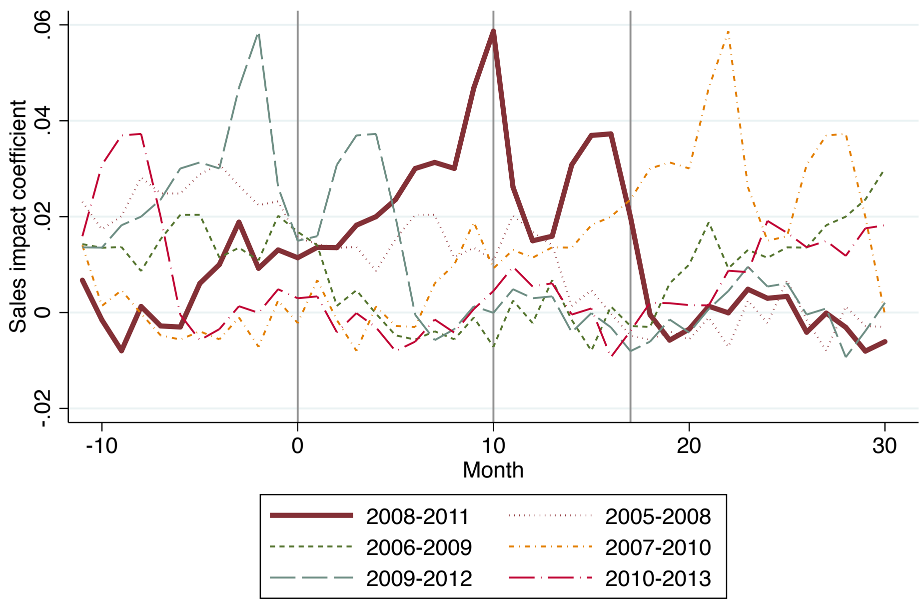
We define the main analysis sample beginning with the non-distress resales dataset described above. To ensure estimates are not biased by changes in geographical coverage, only ZIPs or counties with more than 90% of their transaction time series complete from 2006 onwards are included. This will tend to exclude very small ZIPs which have many months during which there are no transactions. All other datasets are filtered through an inner join that restricts the analysis sample to the same set of ZIPs.

For this sample, we seasonalize home sales counts using a within-place transformation for each month. For each geographic unit, we also compute the mean of monthly house sales in 2007, which is our primary scaling variable, and total house sales in 2007, which is our primary weighting variable.

Appendix Table A.1 details the creation of the non-distress resales dataset.

[25] Ginnie Mae origination data with agency type F is the source for statistics on FHA-insured mortgages.

Figure A.1: Placebo Coefficients



Notes: This figure presents a placebo test for whether seasonality accounts for the spikes in the home sales distribution. The test estimates month-by-month regressions and plots coefficients from the non-control specification in Figure 5, panel (b), emphasized with a bold line, along with equivalent regressions shifted backward in time to start in 2005, 2006, and 2007, and shifted forward to start in 2009 and 2010.

Table A.1: Total number of observations in dataset through each filter

	ZIPs	Counties	CBSAs
Geo-month observations (transaction counts in parentheses)			
Matched between assessor and transaction data	2,716,338	150,859	55,348
	(124.4 M)	(124.4 M)	(117.5 M)
+ Arm's length transactions w/ valid geo, month data	2,540,700	145,776	53,575
	(70.51 M)	(70.51 M)	(67.26 M)
+ Cleaned resales & new sales over 2004-2013	1,422,986	85,724	29,509
	(37.28 M)	(37.28 M)	(35.37 M)
+ Non-distress resales	1,374,995	85,576	29,487
	(22.76 M)	(22.76 M)	(21.41 M)
+ Time series 90%+ complete over 2006-2013	1,046,132	75,697	27,111
	(21.48 M)	(22.15 M)	(21.09 M)
+ Matched to exposure variables and covariates	1,023,008	75,580	26,760
	(20.99 M)	(22.15 M)	(20.83 M)
+ Matched to Corelogic/DataQuick price data	926,108	75,180	26,643
	(20.55 M)	(22.14 M)	(20.64 M)
Unique geographic units in dataset			
Matched between assessor and transaction data	19240	973	295
+ Arm's length transactions w/ valid geo, month data	18351	970	295
+ Cleaned resales & new sales over 2004-2013	17671	941	295
+ Non-distress resales	17459	941	295
+ Time series 90%+ complete over 2006-2013	9117	664	235
+ Matched to exposure variables and covariates	8917	663	232
+ Matched to Corelogic/DataQuick price data	8784	663	231

Notes: The number of unique geographic units in the dataset (ZIPs, counties or CBSAs) are in parentheses.