The Effect of Credit Availability on House Prices: Evidence from the Economic Stimulus Act of 2008*

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Abstract

I estimate the effect of credit availability on house prices using variation in conforming loan limits (CLLs) implemented by the Economic Stimulus Act of 2008. In a novel approach, I use the original asking price of a home to estimate the likelihood that it would be affected by the new CLLs. I find that a CLL increase from $417,000 to $729,750 raises the price of homes most likely to be affected by 6 percent. Owners of affected homes are more likely to refinance following the policy, suggesting that the price increases operate through a reduced cost of ownership.

JEL Codes: R21, R31, G20

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1 Introduction

For most people, especially younger or poorer households with little savings, the size and quality of a house they are able to purchase is greatly restricted by the amount of down payment they can afford and the amount of mortgage financing they are able to obtain. The price and availability of mortgage credit should therefore have strong impacts on housing market outcomes. Policymakers appear to be aware of this, and most major government interventions in the housing market come through agencies that act to increase liquidity in the mortgage market.[1]

Despite this, there are few empirical studies to estimate a direct effect of mortgage credit availability on house prices. A key challenge is the simultaneity between credit conditions and house prices. If high house price growth was observed in some areas along with cheaper and easier access to credit, it remains an open question whether the cheaper credit led to faster growth or whether faster growth led to cheaper credit, as the value of a mortgage contract depends also on the expected future value of its collateral. This is not a trivial problem, and has been subject to much debate, especially in the aftermath of the financial and housing crisis of 2008.

In this paper, I study the effect of mortgage credit availability on house prices by exploiting the variation in credit supply created by conforming loan limit (CLL) increases that were implemented as part of the Economic Stimulus Act of 2008 (henceforth 2008). The conforming loan limit is a regulatory guideline that determines the maximum size of a loan that is eligible for secu-

[1]Understanding the effects of credit availability on house prices has implications that go beyond just housing market outcomes. There is a rich literature demonstrating the propensity of households to consume out of housing wealth (see Campbell and Cocco (2007); Mian and Sufi (2011); Mian et al. (2013); DeFusco (2015)). Therefore, policies that expand or contract mortgage credit can have important general equilibrium consequences through the effect on consumer spending.

ritization by the GSEs, Freddie Mac, Fannie Mae, and Ginnie Mae.[2,3] Because GSEs carry implicit government guarantees on their credit obligations, there is a significant difference in the cost of originating a conforming versus a non-conforming loan.[4] Increases to the conforming loan limit should therefore have a positive effect on the availability of mortgage credit, especially to borrowers who wish to obtain loans that were previously non-conforming but become conforming under new guidelines. Following Vickery and Wright (2013), I will refer to such loans as "super-conforming" loans.

The ESA was signed into law on February 13th, 2008, and it raised the conforming loan limits for a number of metropolitan areas across the U.S. These new limits were announced on March 6th, 2008, but would apply to all loans originated in 2008. Prior to the ESA, the conforming loan limit was \$417,000 across the country. After the ESA, the new limit varied by metropolitan area. For the cities in my analysis, the CLL became \$729,750 for San Francisco and Los Angeles, \$567,500 for Seattle, and did not change in Chicago. Figure 1 shows that the conforming loan limit is indeed binding, and that origination of super-conforming loans by Fannie Mae increased significantly after the policy.

A number of features of the ESA make it a suitable setting for studying the effect of credit supply on house prices. First, the ESA was passed during a time in which the market for non-agency mortgages was not functioning. Figure 2 shows that the securitization of non-agency residential mortgages had already

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[2]Fannie Mae, Freddie Mac, and Ginnie Mae are the common names for the Federal National Mortgage Association, the Federal Home Loan Mortgage Corporation, and the Government National Mortgage Association. These entities exist to provide liquidity to the residential mortgage market by purchasing loans from originators and then repackaging them and selling them to investors as mortgage backed securities (MBS). MBS issued by the GSEs are commonly referred to as "agency" MBS while MBS issued by private companies are referred to as "non-agency" MBS.

[3]The conforming loan limit is also used as the loan limit for FHA (Federal Housing Agency) loans. The FHA exists to provide high-leverage loans to low-income borrowers. Because the FHA limit and the conforming loan limit are the same, any subsequent analysis captures the combined effect of CLLs through both the FHA and through the GSEs. However, GSE loans have a much greater market share than FHA loans.

[4]Prior to the financial crisis, the spread between a conforming and non-conforming loan was estimated at around 15 to 25 basis points (see Ambrose et al. (2004) and Passmore et al. (2005)). During the crisis, the spread widened to over 100 basis points (see Vickery and Wright (2013)).

fallen to zero by the start of 2008. The GSEs therefore played a dominant role in U.S. mortgage markets during this time, which makes the conforming loan limit a meaningful and important measure of credit availability—moreso than during a period in which the GSEs played a smaller role.

Second, there is variation across cities in the size of the CLL increase received, if any. This allows a comparison of effects between cities that received large increases and cities that received small or no increases. It also allows the effect of the CLL increases to be disentangled from the other major provision of the ESA—tax rebates—which were national in scope. Whether or not a city received an increase, and the size of that increase, were determined by formula, which reduces the concern that the CLL increases were deliberately targeted to cities where there would be a positive effect.

Finally, the magnitude of the CLL increases were potentially very large. This allows for the creation of comparison groups of homes within a metropolitan area, based on the likelihood of the home to be purchased with a super-conforming mortgage. This idea motivates a within-city difference-in-differences approach: for each city in my data, I estimate the change in house prices that occurs before and after the implementation of the new CLLs. I do this separately for homes that are likely to be affected by the new CLLs (homes likely to be purchased with super-conforming mortgages; i.e. the treatment group) and for homes that are unlikely to be affected by the new CLLs (i.e. the control group). Over this time period, the prices of all types of homes are declining, but in cities that received large CLL increases, the prices of treated homes should decline less than the prices of non-treated homes. In cities that did not receive large CLL increases, the prices of treated homes should decline at a similar rate to the prices of non-treated homes.

A key difficulty in implementing this methodology is how to assign properties to the treatment and control groups. Using a similar identification strategy, Adelino et al. (2014) use the transaction price of the home to assign it to either treatment or control. Homes that sold above 125% of the old CLL are assigned to the treatment group, and homes sold below 125% of the old CLL are assigned to the control group. The rationale for such an assignment is as follows: homes that sell for less than 125% of the old CLL can be purchased with an 80% LTV conforming loan in both the pre and post periods, whereas homes that sell for more than 125% of the old CLL can be purchased with an 80% LTV conforming loan only in the post period.[5] The problem with using the transaction price to construct a treatment and control group is that the transaction price is not pre-determined before the treatment goes into effect. To see how this can lead to erroneous conclusions, consider a change in CLL that goes from $417,000 to $729,750. Now consider a home that would sell for $523,000 in the post-period. This same home may sell for slightly less in the pre-period, possibly $521,250 (80% of which is $417,000), so that it can be purchased with an 80% LTV conforming loan in the pre-period. This same home, if it sells in the pre-period, would be assigned to the control group; but if it were to sell in the post-period, it would be assigned to the treatment group. This biases up the price of homes in the control group in the pre-period and biases down the prices of homes in the treatment group in the post period (homes close to the threshold get assigned to the control group in the pre-period, resulting in upward bias; homes close to the threshold get assigned to the treatment group in the post-period, resulting in downward bias). Overall, this method for assigning homes to treatment and control groups would bias the researcher away from finding positive effects of CLLs on house prices.

In this paper, I propose using the *original list price* of a home-for-sale as a proxy for the assignment of homes to treatment and control groups. The use of the original list price has two major merits. First, the original list price is determined long before the actual sale of the home. Therefore, as long as the data sample is restricted to homes put up for sale before the announcement of the new CLLs, the properties which would be assigned to treatment and control groups are not different depending on whether they end up transacting in the pre or post period. Second, the original list price is highly predictive

[5]LTV stands for loan-to-value ratio and measures the initial loan amount divided by initial property value. There is a cutoff in the desirability and accessibility of loans at 80% LTV, because borrowers who want to borrow more than 80% of their property value are typically required to also purchase private mortgage insurance, which insures the lender in the event of a default.

of whether or not a home will be purchased with a super-conforming loan. Moreoever, the likelihood that a home is purchased with a super-conforming mortgage is a highly non-linear function of the original list price. The non-linearity allows the list price to continue to enter as a control for the unobserved quality of the home.

To carry out my empirical strategy, I use matched data on single family home sales and listings between 2007 and 2008, from four major U.S. metropolitan areas—San Francisco, Los Angeles, Seattle and Chicago. I find that the increase in CLLs in San Francisco and Los Angeles raised prices for the homes that are *most likely* to be purchased with super-conforming loans (hereafter referred to as "high-propensity" homes) relative to homes that are not likely to be purchased with super-conforming loans. The magnitude of the effect is significant. In San Francisco, I estimate that, on average, a home that is 10 percentage points more likely to be bought with a super-conforming loan sold at a 0.8 percent higher price as a result of the new conforming loan limits. The equivalent number is 0.9 percent in Los Angeles. In contrast, I find no evidence of positive effects in Seattle and Chicago, as expected. The overall price effect on a home that is most likely to be bought with a super-conforming mortgage (about 70% likely) relative to a home that has a 0 percent chance is 5.6% in San Francisco and 6.3% in Los Angeles. Both numbers are within the interquartile range of the discount between a home's sale price and its original list price. I show that owners of high-propensity homes are more likely to refinance in the post-period, which suggests that one channel for the price increase is through a reduced cost of ownership.

Related Literature

There is a rich theoretical literature on the interactions between mortgage markets and housing markets. For a few classic papers, see Poterba (1984) and Stein (1995), and see Ortalo-Magné and Rady (2006) and Favilukis et al. (2013) for more recent examples. A key point of agreement in these models is that mortgage market conditions, be it interest rates or collateral constraints, are expected to have an effect on house prices.

Empirically, a number of papers have explored the effects of mortgage market conditions on various housing outcomes. Genesove and Mayer (1997) study the role of equity and debt in the cyclicality of housing markets; Mian and Sufi (2009) study the consequences of mortgage credit expansion on subsequent default rates. There are also a number of papers that study the effect of GSE involvement on mortgage market conditions. Ambrose et al. (2004) and Passmore et al. (2005) study the effect of conforming loan limits on interest rate spreads; Vickery and Wright (2013) study the separate role of the GSEs and short-term futures markets for MBS; DeFusco and Paciorek (2014) use CLLs to study the interest rate elasticity of mortage demand; Fuster and Zafar (2014) use survey evidence to examine similar questions.

A number of papers also explore the effect of credit conditions and GSE involvement on house prices.[6] Recently, a few papers have begun to employ quasi-experimental variation in attempting to estimate the effect of credit supply on house prices. Favara and Imbs (2015) use bank branch deregulation in the 1990s as a source of exogenous variation in mortgage interest rates, and find that states where deregulation occurred experienced larger house price increases. Adelino et al. (2014) use nationwide changes to the conforming loan limit between 1998 and 2005 to estimate the effect on prices of homes that are likely to be affected by the change. As discussed earlier, Adelino et al. (2014) use a difference-in-differences method in which properties are assigned to treatment and control groups based on whether they were sold above or below 125% of an old CLL.

Interestingly, I estimate a fairly large effect of CLL increases on house prices whereas Adelino et al. (2014) measure only modest effects. The results are not directly comparable because the data come from different time periods, but the most likely explanation is that the conforming loan limit was much more important in 2008 than from 1998 to 2005. Our estimates of the semi-elasticity of house prices with respect to interest rates, however, are comparable. I estimate that a 100 basis point decrease in interest rates (roughly the conforming-nonconforming spread in 2008) increases the price of

[6]See Himmelberg et al. (2005), Hubbard and Mayer (2009) , and Glaeser et al. (2010).

high-propensity homes by about 6 percent, implying an semi-elasticity of 6, which is in the middle of Adelino et al. (2014)'s range of estimates.

Finally, a number of recent papers are demonstrating the utility of home price listings data in research. Anenberg and Kung (2014) use listings data to provide new evidence on the magnitude and mechanisms behind foreclosure spillovers; Anenberg and Laufer (2014) use listings data to produce more accurate forecasts of short-run house price changes than has previously been available. Haurin et al. (2010) and Han and Strange (2014) study the role of asking price in the home sales process and use listings data to test their theories.

2 Data

I use data on single family home sales and listings from four major U.S. metropolitan areas: San Franscisco, Los Angeles, Chicago and Seattle. The four cities were chosen based on the availability of listings and transaction data, but also because two of these cities (San Francisco and Los Angeles) had their conforming loan limits raised from $417,000 to $729,750, while Seattle's was raised from $417,000 to only $567,500 and Chicago's was not raised at all. This allows me to not only demonstrate a positive effect of the increased CLLs in San Francisco and Los Angeles, but also to test the hypothesis that there should be smaller (if any) effects in Seattle and Chicago.

The listings data is provided by AltosResearch. Each week, AltosResearch records the address and asking price for the universe of homes listed for sale on the Multiple Listing Service (MLS) that week.[7] A limitation of the MLS data is that it does not record whether the property was ultimately sold, nor does it record the ultimate sale price. I therefore supplement the listings data with property sales data provided by Dataquick. Dataquick data is constructed from public records of property transactions. Each time a property is sold, I

[7] The Multiple Listing Service is a national database of homes listed for sale. All broker-assisted listings appear in the MLS, while for-sale-by-owner homes account for less than 6 percent of all arms-length home sales. Therefore, the MLS listings are closely representative of the universe of all homes for sale at any given time.

observe the transaction price and the closing date of the transaction, as well as any loans taken out against the property. In addition, Dataquick data contains the street address of each property, which allows it to be matched to MLS data. The Dataquick data records transactions as far back as 1988 to 2012.

The two datasets are matched based on street address. Because of possible spelling errors in both datasets, the addresses are first cleaned and geocoded, then matched based on house number, street name, city, state, zip, and latitude and longitude. Only single-family residences are considered, but this is not too restrictive because the large majority of home sales in each of the four cities are of single-family homes. Table 1 reports the results of the matching algorithm. The match rate, defined as the percentage of unique properties appearing in the MLS data that are successfully matched to a property ever appearing in Dataquick, ranges from 90% for San Francisco to 78% for Chicago. A property in the MLS may go unmatched for a number of reasons. First, the address cleaning and geocoding procedure may have failed to produce the correct address and location for the property. Second, the property may have been classified as a single-family residence in the MLS but something else in Dataquick. This would result in a failure to find a match because only properties identified as single-family residences in both datasets are considered.

In the rest of the analysis of this paper, I will focus on successfully matched properties that were listed in 2007. I focus only on properties that were first listed in 2007 because the decision rule by which sellers set their list prices may be different before and after conforming loan limits increase. I therefore consider only properties that were originally listed in the pre-period. Taking the set of properties listed in 2007, I then use the data to construct information about the *listing spells* for each property. A listing spell is defined as a period of time in which the property is continuously listed on the MLS, with no break in listings longer than 90 days.[8] If the property is sold within 90 days of the last listing date of a listing spell, I consider that property to have sold as a

[8]If a property is delisted and listed again within 90 days, with no sale in between, I consider both listings to be from the same spell.

result of the listing spell. If the property did not sell within 90 days of the last listing date of a listing spell, I consider the listing spell to have been unsuccessful in selling the property, and that the property was delisted on the last listing date.

Table 2 reports summary statistics for the listing spells in each city. The top panel reports the number of listing spells that began in 2007, and the percentage of them that ultimately sold. Focusing on the listings that actually sold, the bottom half of Table 2 reports summary statistics for sale price, list price, and time-on-market. The cities are quite different in terms of average property value, but fairly similar in other respects, such as the median time-to-sale and the proportion of homes which are sold after the March 6th 2008 announcement of increased conforming loan limits (Seattle has a higher proportion because the MLS data in Seattle does not start until October 2007). One important point to note is that sale prices tended to fall far below original asking prices during this period. For the full sample, the median discount between sale price and list price is -6.5% and the interquartile range goes from -16% to -1.9%.

3 Descriptive evidence

In San Francisco and Los Angeles, the properties that are going to be directly affected by the CLL increase are the ones that sell with a loan size greater than \$417,000 and less than \$729,750, i.e. "super-conforming" loans. In Chicago, no properties are directly affected, and in Seattle only those that sell with a loan size greater than \$417,000 and less than \$567,500 are directly affected. This naturally suggests a difference-in-differences approach where the change in prices, pre and post CLL increase, of homes sold with super-conforming loans are compared to the change in prices of homes sold with non-super-conforming loans. A major problem with this approach is that the loan amount is determined simultaneously with the transaction price of a home. This issue will be addressed with the usage of the list price as a proxy variable in the next section. For now, it is still useful to present some descriptive evidence without controlling for the endogeneity between loan amount and house price.

In Figure 3, I plot the average residual of log(Sale Price) − log(List Price) against log(Loan Amount), where List Price here is the original listing price of the home-for-sale. The residuals are generated from city-specific regression of log(Sale Price) − log(List Price) on the original list price fully interacted with a third-order polynimal of time-on-market. The regression therefore controls for the effect of leaving a house on the market longer and for any differential trends for houses of different base quality. The residuals will then shed light on the additional effect of being bought with different loan amounts, in the pre and post periods. From Figure 3 we can see that in San Francisco and Los Angeles, the prices of homes sold with loans close to the super-conforming region are generally higher in the post-period than in the pre-period. Moreover, the difference is largest in exactly the super-conforming region, between $417k and $729k. In contrast, there is no discernable difference between the pre-period and the post-period in Chicago and Seattle, where CLLs increased only a little or not at all. The descriptive evidence so far is consistent with the hypothesis that the CLL increases in San Francisco and Los Angeles had a positive effect on house prices for properties that were likely to be sold with super-conforming loans.

In Figure 4, the average residuals from the same regression are plotted against the transaction date. Figure 4 is another way to tell the same story as Figure 3, but more clearly illustrates the time trends in each city. From Figure 4, we can see that in San Francisco and Los Angeles, the prices of homes sold with super-conforming loans initially follow the same time-trajectory as homes sold with non-super-conforming loans. However, after 2008, the prices of homes sold with super-conforming loans start to increase relative to the prices of homes sold with non-super-conforming loans. This pattern is not observed in Seattle or Chicago. Moreover, the pattern is not solely driven by differential time trends between high-valued homes and low-valued homes. Figure 4 plots the residuals separately for homes sold with conforming loans (loans that are conforming in both the pre and post periods) and for homes sold with jumbo loans (loans that were non-conforming in both the pre and post periods). The trend for each of these types of homes appear to be similar. Again, the descriptive evidence presented here is consistent with the hypothesis that the CLL increases had a positive effect on the prices of homes likely to be purchased with super-conforming mortgages.[9]

Figure 4 shows the time trends for homes purchased with and without super-conforming mortgages, with "super-conforming" being defined according to the conforming loan limits of each city. However, it is also of interest to consider an invariant definition of "super-conforming", so that the effect of the Economic Stimulus Act on houses of a certain price range can be compared across cities. Indeed, this is the appropriate definition to consider if we are interested in testing the hypothesis that the ESA had no effect on the prices of high-valued homes in cities that did not receive large CLL increases. Because this is an important reason for including Chicago and Seattle in the study, future references to the word "super-conforming" shall be interpreted as super-conforming according to the conforming loan limits of San Francisco and Los Angeles.

Figure 5 replicates Figure 4, but separates the residuals based on whether or not the home was purchased with a loan that would be defined as super-conforming in Los Angeles or San Francisco. As expected, Figure 5 reveals that the ESA had no differential effect in Seattle or Chicago on homes purchased with loans between $417,000 and $729,750.

The evidence presented in Figure 5 can be summarized using a difference-in-differences regression. For each city, I run the following regression:

$$y_i = \beta_0 + \beta_1 J_i + \beta_2 T_i J_i + X_i \beta_4 + \epsilon_i \tag{1}$$

[9] An interesting point to note is that the prices of homes bought with super-conforming loans appear to start diverging at the start of 2008, rather than on March 2008, which was when the new CLLs were announced. Although the new CLLs were announced in March of 2008, they applied retroactively to all loans originated since the start of 2008, so the evidence in Figure 4 may suggest that the announcement was anticipated. Indeed, the Economic Stimulus Act was passed on January 29th, 2008, so drafts of the bill were already in circulation at the start of 2008. In the subsequent empirical analysis, I will consider both January of 2008 and March of 2008 as the cutoff date for pre and post indicators.

where y_i is the log sale price minus the log list price for property i, T_i is an indicator for whether property i sold after March 6th 2008, and J_i is an indicator for whether the property is super-conforming (again, as defined in LA and SF). The variables in X_i include log(List Price) interacted with a third-order polynomial of time-on-market, as well as a full set of transaction-month fixed effects. The coefficient of interest is β_2, which measures the change in prices between the pre and post periods for homes bought with super-conforming loans (as defined in LA and SF) relative to the change in prices for homes bought with non-super-conforming loans. The results of this regression, city-by-city, are presented in Table 3. As expected, the difference-in-differences (DD) estimate β_2 is positive and statistically significant in both San Francisco and Los Angeles. The DD estimates are smaller, but still statistically significant in Seattle and Chicago. The estimates for San Francisco and Los Angeles—6.4% and 4.7%—are within the interquartile range for the discount between sale price and list price in these two cities.

4 Using list price to construct the treatment variable

There are two main endogeneity concerns in specification (1). First, loan amount and sale price are simultaneously determined. It is therefore likely that the error term ϵ_i is positively correlated with the indicator J_i, because homes that sell at a higher price are also more likely to be bought with a super-conforming loan. Second, a house can be affected by the new CLLs, due to increased competition between buyers, even if it is not ultimately purchased with an affected loan. Therefore, J_i is a noisy measure of treatment for homes in the price range likely to be affected by CLL increases. The first effect may lead to an upward bias while the second effect may cause an attenuation bias.

It is more appropriate, then, to construct a treatment variable that is predetermined and that measures the *likelihood* that a home would be purchased by a super-conforming mortgage, rather than the actual realization. The original list price of a home is both pre-determined and highly predictive of whether or not the home is ultimately purchased with a super-conforming mortgage. So to construct a better treatment variable, I non-parametrically estimate the probability of being purchased by a super-conforming loan in the pre-period as a function of log(List Price). Recall that log(List Price) was already used as a covariate in regression (1). However, because the expectation of J_i is highly non-linear in log(List Price), the treatment variable can still be constructed. Letting L_i be the log of the original list price, the instrument is constructed by non-parametrically estimating $f(L_i)$:

$$E[J_i|T_i = 0, L_i] = f(L_i) + \eta_i \quad (2)$$

$$E[\eta_i|L_i] = 0 \quad (3)$$

using the data of properties sold prior to the CLL increase, separately for each city. $f(L_i)$ is therefore an estimate of the probability that a house sold in the pre-period would sell with a super-conforming loan. It is estimated using a Nadaraya-Watson kernel regression with Epanechnikov kernel and bandwidth 0.1.

Figure 6 reports the non-parametric estimates of J_i as a function of L_i. Unsurprisingly, the propensity for a home to be purchased with a super-conforming loan is basically zero for low-priced homes, but increases quickly as the homes become higher priced. The location of the sharp ascent in the estimated propensity is around where 80% of the list price would equal $417,000 (the CLL in the pre-period). Similarly the location of the dropoff in the estimated propensity is around where 80% of the list price would equal $729,750.

Having estimated $f(L_i)$, I use it as the treatment variable in the following difference-in-differences regression:

$$y_i = \beta_0 + \beta_1 f(L_i) + \beta_2 T_i f(L_i) + X_i\beta_4 + \epsilon_i \quad (4)$$

As in regression (1), the variables in X_i include log(List Price) interacted with a third-order polynomial of time-on-market, as well as a full set of transaction-month fixed effects. $f(L_i)$ measures the likelihood of a property to be purchased with a super-conforming loan in the pre-period, conditional on list price. The coefficient β_1 therefore measures a baseline for how the discount between sale price and list price changes with respect to likelihood of being purchased with a super-conforming loan, and β_2 measures how this relationship changes in the post-period.

Table 4 reports the results of regression (4).[10] The DD estimates are positive and statistically significant for San Francisco and Los Angeles, and not so for Seattle and Chicago. When comparing Table 4 to Table 3, it would appear that the use of list price to construct the treatment variable increased the magnitude of the estimates. This would suggest that the estimates in Table 3 likely suffer from attenuation bias, and that the increase in CLLs actually affects all homes that are likely to be purchased with super-conforming loans, rather than having a direct effect only on homes that actually are purchased with super-conforming loans.

The estimates imply that a house in San Francisco that is 10 percentage points more likely to be purchased with a super-conforming loan will be sold at a 0.8% higher price in the post-period than in the pre-period, other things being equal. In Los Angeles, that number is closer to 0.9%. In both San Francisco and Los Angeles, the homes most likely to be purchased with super-conforming mortgages have about a 70% likelihood of being so. Therefore, the difference-in-differences estimate in San Francisco between homes most likely to be purchaesd with super-conforming mortgages and homes not likely to be purchased with super-conforming mortgages is 5.6%. That number is 6.3% in Los Angeles. Both numbers are again in the interquartile range of the discount between sale price and list price. The spread between conforming and non-conforming loans in 2008 was about 100 basis points, so this would imply a semi-elasticity of house prices with respect to interest rates of around 6.[11]

[10]I have also computed standard errors for Table 4 using 100 bootstrapped repetitions. Because of the large sample size, the bootstrapped standard errors are not larger than in Table 4, so I do not report them.

[11]The semi-elasticity is computed as the change in house prices as a percent divided by the change in interest rate in percentage points.

In Table 4, the policy date is taken to be March 6th, 2008, because this is when the increase in CLLs due to the ESA were formally announced. It is not necessarily the case, however, that this is the appropriate date to focus on. First, the ESA allowed the GSEs to begin securitizing super-conforming mortgages that were originated on or after January 1st, 2008. If the increase in CLLs were anticipated, then the effect of the policy may have begun before its official announcement. On the other hand, Figure 1 shows that Fannie Mae acquisitions of super-conforming loans did not begin in significant quantities until May of 2008. In Table 5, I re-estimate regression (4) using the alternative policy dates of January 1st, 2008 and May 1st, 2008. The results are not significantly different from the results using March 6th, 2008 as the policy date. Consistent with Figure 5, it appears that the divergence in house prices between high-propensity and low-propensity homes began at the start of 2008—even though Fannie Mae purchases of super-conforming loans did not begin in earnest until May 2008. This has implications for what might be the underlying mechanisms behind the price support for high-propensity homes, and I explore this issue further in Section 4.2.

4.1 Interpreting the result as a causal effect

Interpreting Table 4 as a local average treatment effect requires a number of assumptions. First, there cannot be any omitted variables that change over time, in which the changes are correlated both with the discount between sale price and list price and with the propensity of a home to be purchased with a super-conforming loan. Such an omitted variable may arise, for example, due to selection if high-propensity properties that sell in the post period are of higher unobserved quality than low-propensity properties that sell in the post period. Alternatively, it may be the case that sales in the pre and post periods are coming from neighborhoods of different quality.

To test the robustness of my results against these possiblities, I re-run regression (4) for each city, this time including the property characteristics square feet, number bedrooms, number bathrooms, and year built as controls.

The results are reported in column 1 of Table 6. It appears that including property characteristics does not significantly alter the baseline results. In fact, the coefficients on the property characteristics are generally not statistically significant, suggesting that the original list price is already a good control for both observed and unobserved house quality. In columns 2 and 3, I further include zip-code or census-tract fixed effects. Again, the results are not much changed (the estimates actually go up in Los Angeles)—so it is not the case that the results in Table 4 are driven by the selection of different neighborhoods into the pre and post periods. Finally, in column 4 I include a fourth order polynomial of log(List Price). The purpose of this specification is to test the exclusion restriction that log(List Price) does not affect the outcome variable in highly non-linear ways (outside of the effect that comes through the propensity to be purchased with super-conforming loans). In all cities, the inclusion of a fourth order polynomials of log(List Price) barely change the estimates at all.

Table 6 shows that the results in Table 4 are not being driven by omitted variables. Another requirement for interpreting Table 4 as reporting causal effects is that high-propensity homes do not trend differently from low-propensity homes in the absence of the new CLLs. While Figure 5 provides some visual evidence that there were no pre-existing differential trends between high and low-propensity homes, it is nevertheless useful to formalize this result using our list-price based treatment variable. To do this, I run placebo regressions in which the policy date is purposefully set to an irrelevant date in the pre period. If high and low-propensity homes were trending differently before the actual implementation of the new CLLs, then running regression (4) using the placebo dates will result in positive and significant estimates of β_2. In each regression, the sample is restricted to a four month window surrounding the placebo date. The placebo dates considered are March 2007, July 2007, October 2007, and March 2008 (the actual policy date). Seattle is omitted from these regressions because listings data for Seattle is not available until October 2007, so the placebo tests cannot be performed.

Table 7 reports the results from this exercise. Using the actual policy date, positive and significant effects are estimated for San Francisco and Los Angeles. The estimated effect using the actual policy date is slightly smaller than the effects reported in 4, and this can be attributed to the shorter time window used in Table 7. By contrast, no significant effect is detected in Chicago, which is consistent with the baseline results. Columns 1 to 3 of Table 7 report the estimation results for the placebo tests, and no clear pre-existing trend is detected in either city. Overall, the results are consistent with the assumption that there were no pre-existing differential trends between high and low-propensity homes in San Francisco and Los Angeles.

A third assumption required for interpreting the DD estimates as a local average treatment effect is the stable unit treatment value assumption (or SUTVA; see Angrist et al. (1996)). SUTVA requires that the CLL increases affect only homes that are likely to be bought with super-conforming loans, but have no effect, direct or indirect, on homes that would not be bought with super-conforming loans. SUTVA is required in order to interpret my results as an increase in prices for high-propensity homes caused by the new CLLs, as opposed to a decrease in prices for low-propensity homes caused by the new CLLs, perhaps through competitive equilibrium effects. (For example, if homes-for-sale are competing in a differential products market, an increase in the attractiveness of high-valued homes—through the change in CLL—may drive down demand and prices for lower valued homes). SUTVA will hold in my setting if houses of a given list price are perfectly segmented and do not compete with houses of other list prices. However, this is not likely to be true in reality.

There is no definitive way to address this issue, but I will attempt to alleviate some concerns. First, Figure 5 shows that, over this time period, changes in the prices of homes bought with super-conforming loans were much larger than changes in prices of homes bought with non-super-conforming loans. Moreover, Figure 5 shows a clear trend for the higher-priced homes that begins only during the period in which CLLs were increased. Although not conclusive, none of these features of the data are suggestive that the CLL increases are actively causing low-priced homes to fall further in prices relative to pre-existing trends.

Another way to address the issue is to restrict the sample to homes that are unlikely to have any overlap in their pool of potential buyers. To this end, I re-estimate regression 4 using only homes that are either listed at under $350,000 or over $550,000. The assumption is that homes listed for under $350,000 are not competing in the same market segments as homes that have any positive likelihood of being affected by the CLL increases, whereas homes listed for over $550,000 clearly are. If the effects reported in Table 4 are driven primarily by a decline in prices of lower-valued homes due to competition, then the effects should not appear when comparing only these two segments of the housing market.

Table 8 reports the results from this exercise. The DD estimates in San Francisco and Los Angeles continue to be positive and significant, and are only slightly smaller than the estimates reported in Table 4. The results in Table 8 are inconsistent with the hypothesis that the baseline estimates are primarily being driven by declines in the prices of low-propensity homes due to competition.

4.2 Evidence of an anticipatory effect

Figure 5 and Table 5 show that the price support for high-propensity homes began at the start of 2008, but Figure 1 shows that Fannie Mae purchases of super-conforming loans did not begin in earnest until around May of 2008. Freddie Mac purchases of super-conforming loans began even later. Indeed, Vickery and Wright (2013) shows that the *total* market share of super-conforming loans (including portfolio loans) fell in mid-2007 and did not start increasing again until May 2008.[12] These facts present a puzzling discrepancy, because why should prices have started to increase before super-conforming mortgages started to become more available?

One possible explanation is that buyers and sellers in early 2008—when the Economic Stimulus Act was being passed into law—anticipated the ability to refinance into a super-conforming mortgage at a later date, even if they

[12] Vickery and Wright (2013) also explore why this is the case, explaining that it had most to do with liquidity in the secondary market for super-conforming loans.

were not able to obtain one at the time of transaction. The expectation of a reduced cost of ownership in the near future would raise the valuation of both buyers and sellers, and lead to higher prices. To provide some evidence for this possibility, I use Dataquick data to follow the refinancing activity of each property in my data for 12 months after it either sells or is delisted.[13] (Note that in the case of properties that were sold, a later refinance indicates that the new owner, and not the original seller, did the refinancing.) I first estimate the following linear probability model for the hazard rate of refinancing in a given month:

$$y_{it} = \beta_0 + \beta_1 f\left(L_i\right) + \beta_2 T_t f\left(L_i\right) + X_{it}\beta_3 + \epsilon_{it} \tag{5}$$

where y_{it} is an indicator for whether property i is refinanced in month t. Each property i is followed for up to 12 months after it is sold or delisted, or until it is first refinanced. $f\left(L_i\right)$ is as before, and T_t is an indicator for whether month t is on or after May of 2008. X_{it} includes dummies for the month t and for the number of months t is from the date that property i was sold or delisted.

The results of regression (5) are reported in the top panel of Table 9. The results show that high-propensity homes in San Francisco and Los Angeles are indeed more likely to refinance on or after May of 2008. By contrast, no positive effects are detected in Seattle or Chicago. The magnitude of the estimates is not trivial. The baseline monthly probability for a property to be refinanced in San Francisco and Los Angeles is around 1.4 percent.

These estimates suggest that high-propensity homes in San Francisco and Los Angeles are more likely to be refinanced after May of 2008, but do they necessarily imply that homes that were sold or delisted after January 2008 were the ones being refinanced? This would seem to be the implication if buyers and sellers in early 2008 were expecting to refinance in the near future. To explore this possibiliity, I now ask whether high-propensity properties that were delisted or sold after January of 2008 were more likely to be refinanced

[13] In addition to transactions data, Dataquick has a record each time a new lien is taken out against a property, allowing me to observe each time a property is refinanced.

within 12 months, relative to high-propensity properties that were delisted or sold before 2008, and relative to low-propensity properties. To do this, I run a simple difference-in-differences regression of the following form:

$$y_i = \beta_0 + \beta_1 f(L_i) + \beta_2 T_i f(L_i) + \beta_3 T_i + \epsilon_{it} \tag{6}$$

where y_i is an indicator for whether the property was refinanced within 12 months after sale or delisting. $f(L_i)$ is as before, and T_i is an indicator for whether the property was sold or delisted on or after January of 2008. The results of this regression are reported in the second panel of Table 9. The results clearly indicate that high-propensity properties in San Francisco and Los Angeles that sold or were delisted in 2008 were more likely to refinance within 12 months, relative to low-propensity properties and relative to those properties that were sold or delisted in 2007. No positive or significant effects are detected in either Seattle or Chicago.

Taken together, the results suggest that the increase in conforming loan limits may have been able to provide price support to high-propensity homes, even in early 2008 when super-conforming mortgages remained hard to obtain, because buyers and sellers anticipated a future reduction in ownership costs through the possibility of refinancing.

5 Conclusion

Few papers have convincingly demonstrated the effect of GSE conforming loan limits on house prices. In this paper, I use plausibly exogenous variation in the propensity for a home to be affected by new CLLs, based on the initial asking price of the home, to estimate the effect of the 2008 CLL increases on house prices in San Francisco, Los Angeles, Seattle, and Chicago. I find positive and significant effects on high-propensity homes in San Francisco and Los Angeles, and no positive effects in Seattle and Chicago. The results are consistent with the interpretation that the Economic Stimulus Act offered effective price support to high-propensity homes in San Francisco and Los

Angeles, by increasing the conforming loan limit.

Methodologically, the paper emphasizes the use of the list price as a treatment variable because it satisfies two necessary conditions: first, it is predetermined before the start of the policy and second, it is highly predictive of whether a home would be affected by the policy. The paper illustrates that without using the listings data, one is likely to find an attenuated estimate of the effect of CLLs on house prices.

The results suggest that regulatory features of the mortgage market that expand or contract credit can have significant effects on house prices. This is relevant to the ongoing policy debate about winding down the federal conservatorship of Freddie Mac and Fannie Mae. Because changes to house prices can have much larger general equilibrium effects through their effect on consumer spending, any institutional change to the GSEs must therefore be approached with care.

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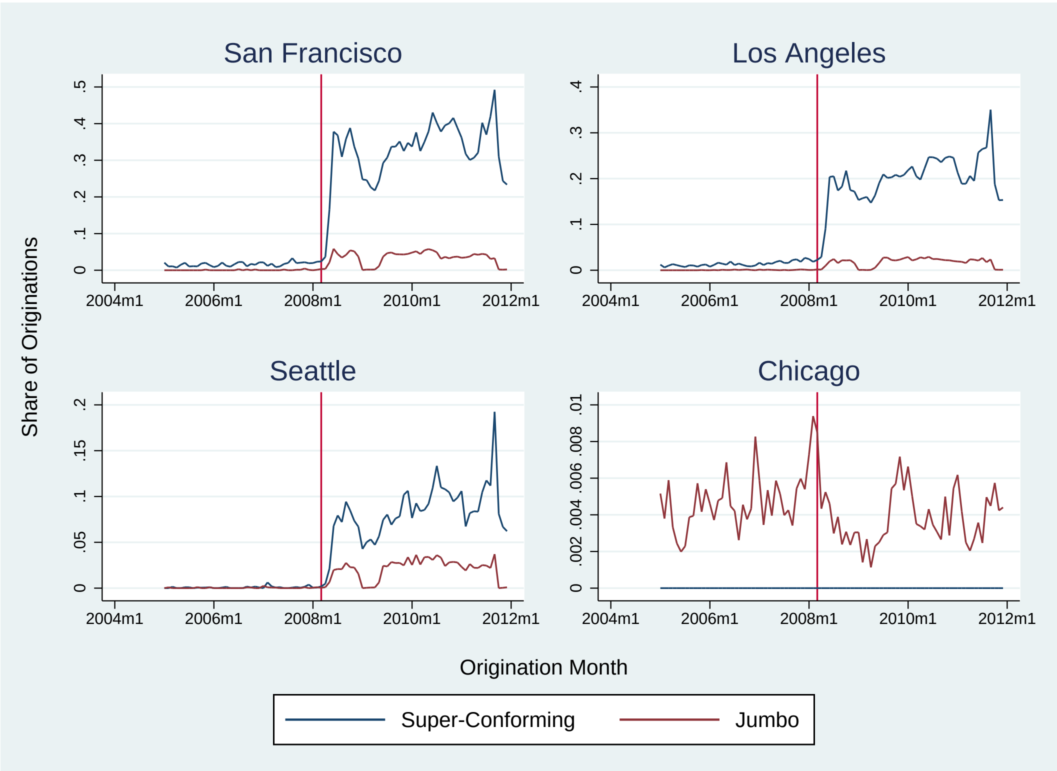
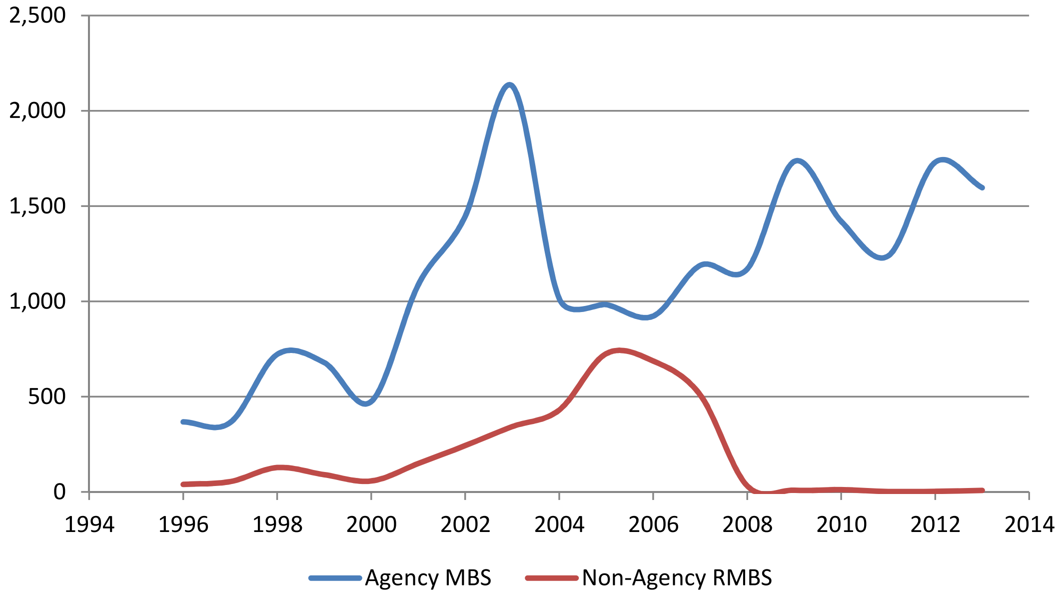
Figure 1: Share of Fannie Mae Originations by Loan Type



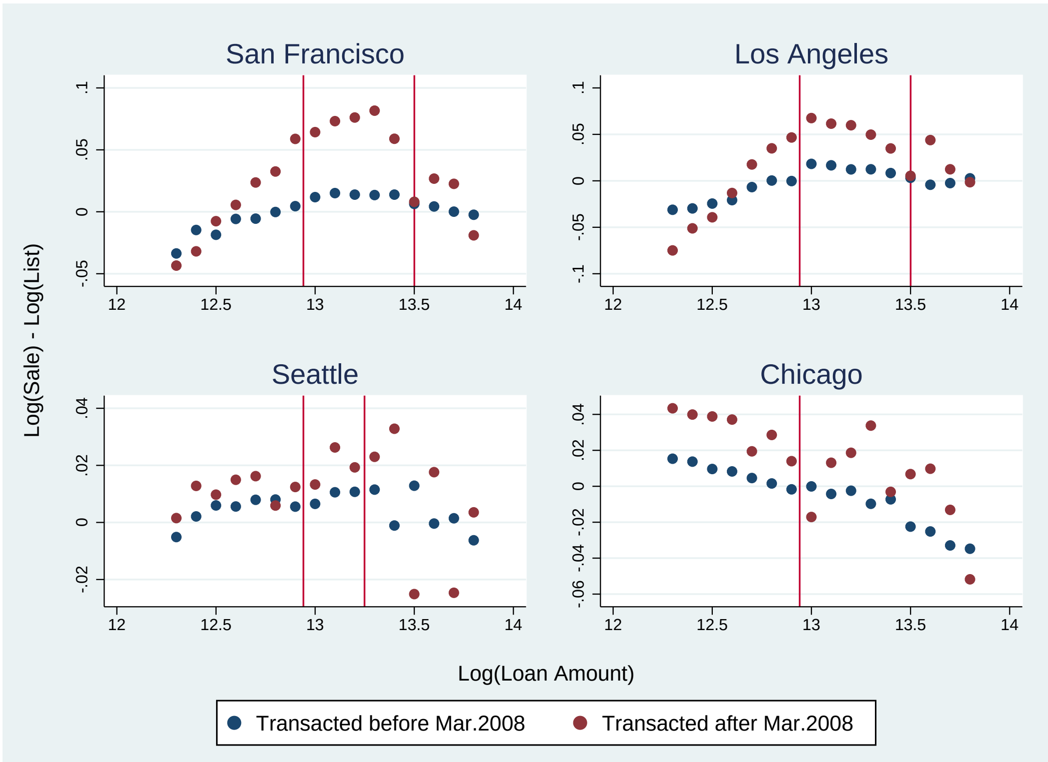
Notes: This figure shows the share of Fannie Mae originations by month that were either super-conforming loans or jumbo loans. Super-conforming is here defined as a loan whose original balance is above the pre-ESA conforming loan limit and below the post-ESA conforming loan limit. Jumbo is here defined as a loan whose original balance is above the post-ESA conforming loan limit. These data are computed from the Fannie-Mae Single Family Loan Performance Data.

Figure 2: Agency and Non-Agency MBS Issuance (USD Billions)



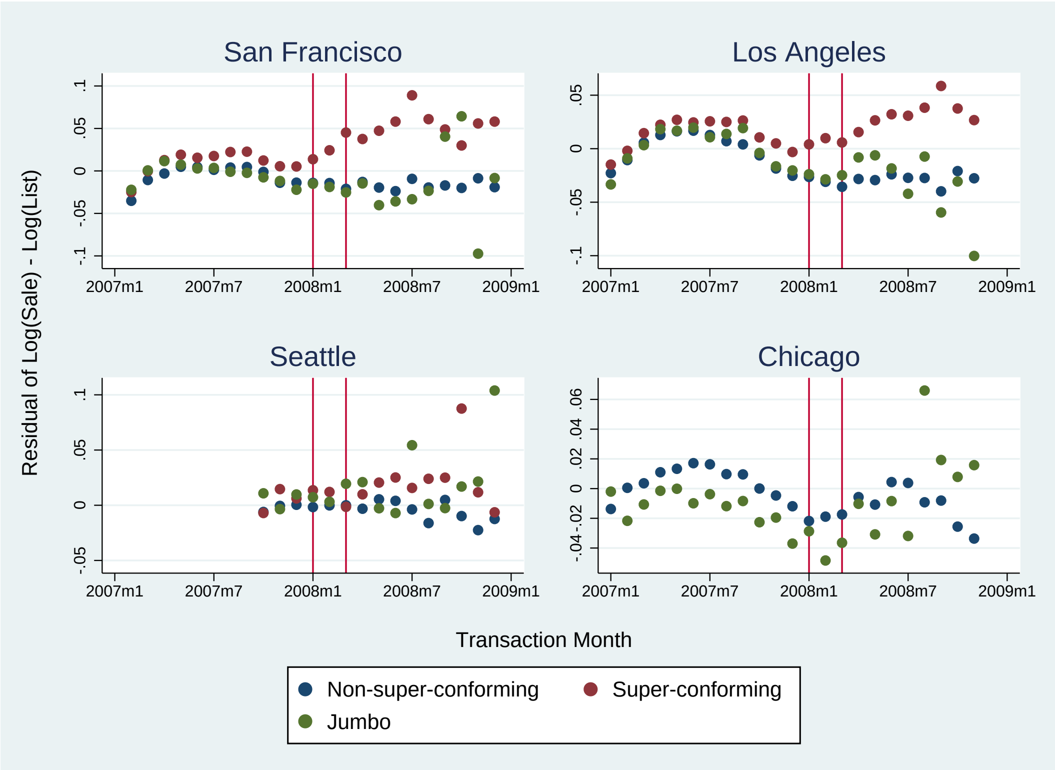
Notes: This figure shows the total volume of agency and non-agency mortgage backed securities issuance from 1994 to 2013. Non-agency issuance of mortgage backed securities all but disappeared in 2008. These data were reported by the Securities Industries and Financial Markets Association (SIFMA). Data can be found at https://www.sifma.org/uploadedfiles/research/statistics/statisticsfiles/sf-us-mortgage-related-sifma.xls.

Figure 3: Residuals of log(Sale Price) − log(List Price) by Loan Amount



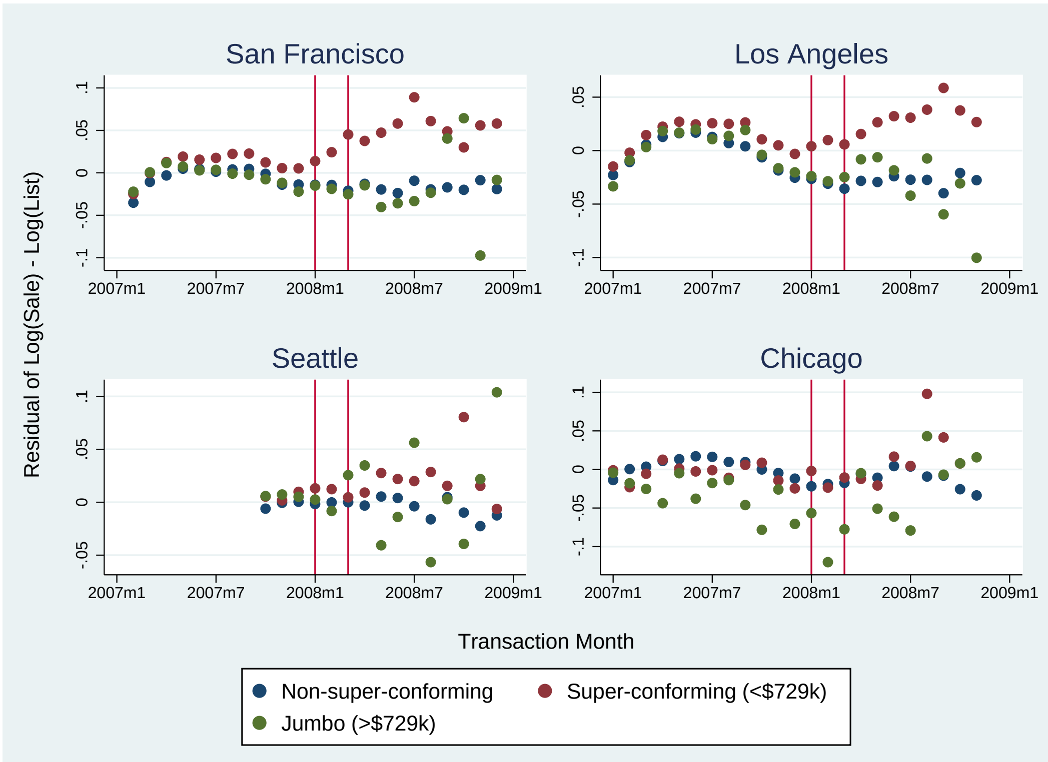
Notes: The residuals are generated from city-specific regressions of log(Sale Price) − log(List Price) on log(List Price) interacted with a third-order polynomial of time-on-market. The red vertical lines on each graph show the region of super-conforming loans for each city. The blue dots represent the average residual for properties sold prior to the announcement of the CLL increases while the red dots represent the average residual for properties which sold after.

Figure 4: Residuals of log(Sale Price) − log(List Price) by Transaction Month



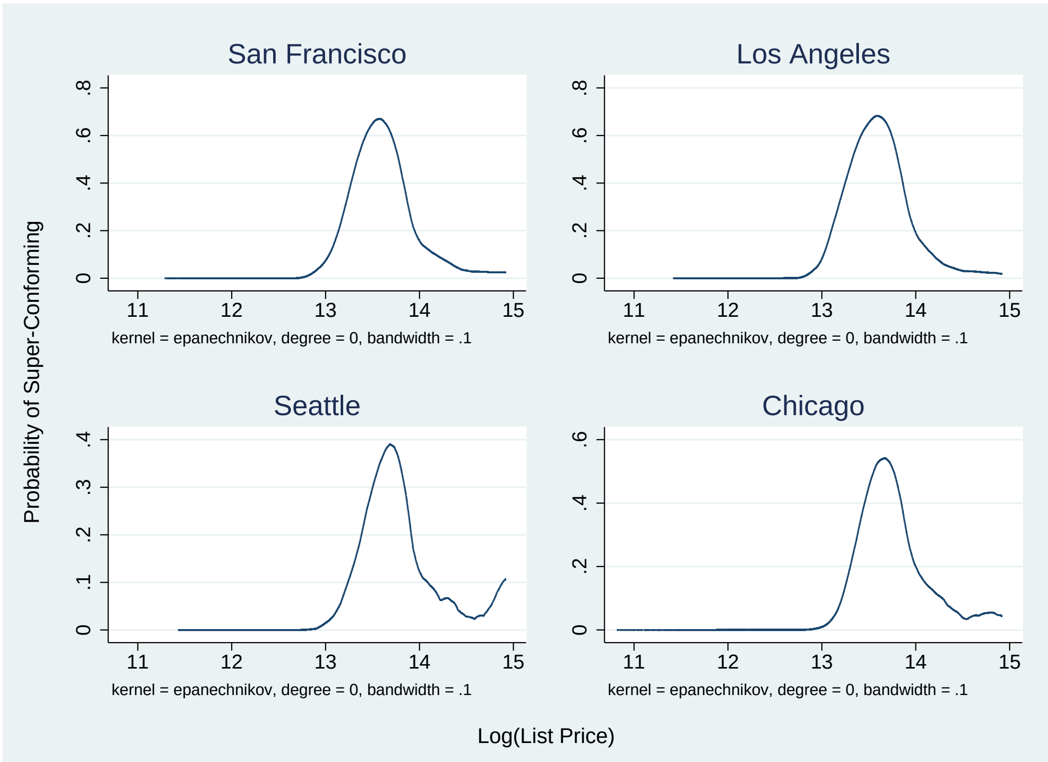
Notes: The residuals are generated from city-specific regressions of log(Sale Price)−log(List Price) on log(List Price) interacted with a third-order polynomial of time-on-market. The red vertical lines on each graph show the period of time between January and March of 2008. Super-conforming loans originated on or after January 2008 would qualify for purchase by the GSEs, and the increase in CLLs were announced in March of 2008. The blue dots represent the average residual for properties that were not purchased with a super-conforming loan, while the red dots represent the average residual for properties that were purchased with a super-conforming loan.

Figure 5: Residuals of log(Sale Price) – log(List Price) by Transaction Month (Alternative Definition of Super-Conforming)



Notes: The residuals are generated from city-specific regressions of log(Sale Price) – log(List Price) on log(List Price) interacted with a third-order polynomial of time-on-markecontrols in X_i include month fixed effects, dummies for the time (in months) since the property was sold or delisted, and an indicator for whether the property was sold or delisted.t. The red vertical lines on each graph show the period of time between January and March of 2008. Super-conforming loans originated on or after January 2008 would qualify for purchase by the GSEs, and the increase in CLLs were announced in March of 2008. The blue dots represent the average residual for properties that were purchased with loans smaller than $417,000 or larger than $729,750, while the red dots represent the average residual for properties that were purchased with loans between $417,000 and $729,750.

Figure 6: Non-Parametric Estimates of $E\left[J_i|T_i = 0, L_i\right]$



Notes: Nadaraya-Watson kernel regression of J_i—an indicator for whether the home was purchased with a loan between \$417,000 and \$729,750—on log(List Price) for properties sold before March 6th, 2008, separately estimated for each city. The kernel is Epanechnikov and the bandwidth is 0.1.

Table 1: MLS-Dataquick Match Quality

	San Francisco	Los Angeles	Seattle	Chicago	Total
# SFR properties in MLS data	170,686	177,246	93,535	156,434	597,901
# successfully matched to Dataquick	153,300	152,415	75,440	122,468	503,623
Match rate	0.898	0.860	0.807	0.783	0.842

Notes: This table shows the raw number of single-family residence properties listed for sale in the MLS data for each city, and the number of these properties that were successfully matched to a property appearing in the tax assessor file of the Dataquick data.

Table 2: Summary Statistics for Matched MLS/Dataquick Data

		San Francisco	Los Angeles	Seattle	Chicago
# listing spells began in 2007		62,766	92,738	21,375	71,582
# sold		30,865	40,754	9,041	28,437
Percent sold		49%	44%	42%	40%
		Listing Spells that Sold			
Original List Price					
	25th pctile	$525,000	$500,000	$296,000	$209,900
	Median	$699,000	$640,000	$388,000	$289,900
	75th pctile	$929,900	$885,900	$549,950	$414,723
Sale Price					
	25th pctile	$430,000	$460,000	$272,900	$190,000
	Median	$651,000	$600,000	$355,000	$265,000
	75th pctile	$900,000	$830,000	$500,000	$375,000
log(Sale/List)					
	25th pctile	-0.1960	-0.1684	-0.1240	-0.1401
	Median	-0.0611	-0.0658	-0.0654	-0.0682
	75th pctile	0.0000	-0.0176	-0.0250	-0.0309
Time-to-Sale (days)					
	25th pctile	41	55	54	70
	Median	82	96	110	117
	75th pctile	182	181	186	203
Sold after Mar.6 2008					
	Mean	0.2427	0.1930	0.4013	0.2231
Super-conforming					
	Mean	0.2981	0.3188	0.0421	0.0000
$417k<Loan Amount<=$729k					
	Mean	0.2981	0.3188	0.0671	0.0403

Notes: This table shows summary statistics for the listing spells that were first listed in the MLS in 2007. The top panel reports statistics for all such spells, and the bottom panel reports statistics for the spells that ended in a sale.

Table 3: Difference-in-Differences Regressions by City

	San Francisco	Los Angeles	Seattle	Chicago
	(1)	(2)	(3)	(4)
Post * 1[$417k < Loan <= $729k]	0.0636***	0.0470***	0.0147*	0.0211**
	(0.0063)	(0.0042)	(0.0078)	(0.0084)
1[$417k < Loan <= $729k]	0.0179***	0.0144***	0.00999**	-0.00845*
	(0.0013)	(0.0011)	(0.0041)	(0.0045)
Observations	29,390	38,277	8,977	27,771
R-squared	0.721	0.630	0.392	0.312

Notes: This table reports the results from city-specific difference-in-differences regressions, as described in equation (1). The treatment variable is an indicator for whether the home was purchased with a loan between $417,000 and $729,750. The post variable is an indicator for whether the home was sold after March 6th, 2008. For each regression, controls include log(List Price) interacted with a third-order polynomial of time-to-sale, and fixed effects for the month of sale. ***, **, * indicate p-values less than 0.01, 0.05, and 0.1 respectively. Standard errors are clustered at the zip code level.controls in X_i include month fixed effects, dummies for the time (in months) since the property was sold or delisted, and an indicator for whether the property was sold or delisted.

Table 4: Difference-in-Differences Regressions by City—Using List Price to Construct Treatment Variable

	San Francisco (1)	Los Angeles (2)	Seattle (3)	Chicago (4)
Post * f(List Price)	0.0804***	0.0880***	0.00143	-0.00131
	(0.0143)	(0.0114)	(0.0222)	(0.0162)
f(List Price)	0.0474***	0.0185***	0.0318***	-0.0733***
	(0.0037)	(0.0038)	(0.0118)	(0.0144)
Observations	29,390	38,275	8,976	27,771
R-squared	0.723	0.631	0.391	0.315

Notes: This table reports the results from city-specific difference-in-differences regressions, as described in equation (4). The treatment variable, f (List Price), is a non-parametric estimate of the likelihood for a home to be purchased with a super-conforming mortgage (as defined in LA and SF) in the pre-period, based on list price. Its construction is described in section 4. The post variable is an indicator for whether the home was sold after March 6th, 2008. For each regression, controls include log(List Price) interacted with a third-order polynomial of time-to-sale, and fixed effects for the month of sale. ***, **, * indicate p-values less than 0.01, 0.05, and 0.1 respectively. Standard errors are clustered at the zip code level.

Table 5: Difference-in-Differences Regressions by City—Alternative Post Dates

Post Date: January 2008

	San Francisco	Los Angeles	Seattle	Chicago
	(1)	(2)	(3)	(4)
Post * f(List Price)	0.0681***	0.0788***	0.0112	-0.00189
	(0.0120)	(0.0106)	(0.0171)	(0.0154)
f(List Price)	0.0432***	0.0138***	0.0250**	-0.0731***
	(0.0034)	(0.0035)	(0.0124)	(0.0143)
Observations	29,390	38,275	8,976	27,771
R-squared	0.723	0.631	0.391	0.315

Post Date: May 2008

	San Francisco	Los Angeles	Seattle	Chicago
	(1)	(2)	(3)	(4)
Post * f(List Price)	0.0759***	0.103***	0.0347	0.0154
	(0.0174)	(0.0139)	(0.0242)	(0.0213)
f(List Price)	0.0541***	0.0227***	0.0231*	-0.0760***
	(0.0040)	(0.0042)	(0.0127)	(0.0152)
Observations	29,390	38,275	8,976	27,771
R-squared	0.722	0.631	0.391	0.315

Notes: This table reports the results from city-specific difference-in-differences regressions, as described in equation (4). The treatment variable, f (List Price), is a non-parametric estimate of the likelihood for a home to be purchased with a super-conforming mortgage (as defined in LA and SF) in the pre-period, based on list price. Its construction is described in section 4. The post variable is an indicator for whether the home was sold after January 1st, 2008 (top panel) or May 1st, 2008 (bottom panel). For each regression, controls include log(List Price) interacted with a third-order polynomial of time-to-sale, and fixed effects for the month of sale. ***, **, * indicate p-values less than 0.01, 0.05, and 0.1 respectively. Standard errors are clustered at the zip code level.

Table 6: Difference-in-Differences Regressions by City—Robustness to Omitted Variables

	San Francisco				Los Angeles			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Post * f(List Price)	0.0817***	0.0858***	0.0821***	0.0786***	0.0850***	0.0861***	0.0890***	0.0801***
	(0.0143)	(0.0130)	(0.0098)	(0.0141)	(0.0118)	(0.0114)	(0.0086)	(0.0112)
Property characteristics	x	x	x	x	x	x	x	x
Zipcode fixed effects		x				x		
Census tract fixed effects			x				x	
Higher order polynomials of log(List Price)				x				x

	Seattle				Chicago			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Post * f(List Price)	0.00113	-0.00281	0.00734	-0.00499	-0.00614	-0.0143	-0.0164	-0.0367**
	(0.0224)	(0.0225)	(0.0213)	(0.0227)	(0.0181)	(0.0180)	(0.0161)	(0.0183)
Property characteristics	x	x	x	x	x	x	x	x
Zipcode fixed effects		x				x		
Census tract fixed effects			x				x	
Higher order polynomials of log(List Price)				x				x

Notes: This table reports the results from city-specific difference-in-differences regressions, as described in equation (4). The purpose of this table is to show the robustness of the results in Table 4 to a variety of omitted variables. First, it is shown that including property characteristics (square feet, number bedrooms, number bathrooms, year built) has no significant effects on the estimates. Then, it is shown that neither the inclusion of zip-code nor census-tract fixed effects change the results. Finally, it is shown that higher-order polynomials of log(List Price) do not affect the results either. The regressions continue to include all the controls present in Table 4.

Table 7: Difference-in-Differences Regressions by City—Placebo Tests

	San Francisco (1)	(2)	(3)	(4)
Transaction Date Range	1/2007-4/2007	5/2007-8/2007	8/2007-11/2007	1/2008-4/2008
Post Date	3/2007	7/2007	10/2007	3/2008
Post * f(List Price)	-0.00386	-0.00524	0.0124*	0.0520***
	(0.0174)	(0.0072)	(0.0074)	(0.0134)
Observations	2,783	7,124	7,982	5,090

	Los Angeles (1)	(2)	(3)	(4)
Transaction Date Range	1/2007-4/2007	5/2007-8/2007	8/2007-11/2007	1/2008-4/2008
Post Date	3/2007	7/2007	10/2007	3/2008
Post * f(List Price)	0.000778	0.0111*	0.00269	0.0510***
	(0.0051)	(0.0067)	(0.0069)	(0.0128)
Observations	9,145	8,680	8,186	5,534

	Chicago (1)	(2)	(3)	(4)
Transaction Date Range	1/2007-4/2007	5/2007-8/2007	8/2007-11/2007	1/2008-4/2008
Post Date	3/2007	7/2007	10/2007	3/2008
Post * f(List Price)	0.0360	0.00349	0.0452**	0.0241
	(0.0359)	(0.0113)	(0.0177)	(0.0312)
Observations	3,897	5,553	8,364	4,369

Notes: This table reports the results from city-specific difference-in-differences regressions, using various dates as a placebo policy date. Each regression continues to include all the controls present in 4, but the sample is restricted to a two month period surrounding the placebo date. Column 4 for each city reports the DD regression using data from the time-period around March 2008, the actual policy date. Columns 1-3 report the regression results using placebo dates of March 2007, July 2007 and October 2007. The placebo regressions do not reveal any pattern of differential trends in the pre-period. Seattle is omitted because listings data only starts in October 2007, so there are not enough observations to perform the placebo regressions.

Table 8: Difference-in-Differences Regressions by City—Segmented Sample

	San Francisco (1)	Los Angeles (2)	Seattle (3)	Chicago (4)
Post * f(List Price)	0.0799***	0.0888***	0.0135	0.0161
	(0.0145)	(0.0157)	(0.0234)	(0.0173)
f(List Price)	0.0582***	0.0411***	0.0339**	-0.0915***
	(0.0048)	(0.0036)	(0.0136)	(0.0198)
Observations	22,884	27,435	6,013	21,590
R-squared	0.704	0.619	0.390	0.304

Notes: This table reports the results from city-specific difference-in-differences regressions, where the samples are restricted to homes with original list price less than \$350,000 or greater than \$550,000. The purpose of these regressions is to test the possibility that the difference-in-difference estimates reported in Table 4 are driven by a decline in the demand of low-priced homes due to the increased attractiveness of higher-priced homes. Buyers purchasing homes worth less than \$350,000 are unlikely to be in the market for homes that have any positive probability of requiring a super-conforming mortgage, and buyers purchasing homes worth more than \$550,000 are unlikely to be considering homes worth less than \$350,000, so buyers in these two market segments are unlikely to overlap.

Table 9: Refinancing Activity After Sale/Delist

Dependent Variable: Monthly refinance indicator

	San Francisco	Los Angeles	Seattle	Chicago
	(1)	(2)	(3)	(4)
(Month>=May.2008) * f(List Price)	0.00251**	0.00840***	-0.00569	-0.00858***
	(0.00122)	(0.00114)	(0.00519)	(0.00245)
f(List Price)	0.00402***	-0.00161**	0.0195***	0.0270***
	(0.000858)	(0.000756)	(0.00445)	(0.00175)
Observations	624,665	814,062	225,859	672,595
R-squared	0.002	0.004	0.002	0.003

Dependent Variable: Whether refinanced within 12 months of sale/delist

	San Francisco	Los Angeles	Seattle	Chicago
	(1)	(2)	(3)	(4)
(Sale/delist after Jan.2008) * f(List Price)	0.0491***	0.0954***	-0.0497	-0.0322
	(0.0141)	(0.0132)	(0.0501)	(0.0266)
f(List Price)	0.0418***	-0.00312	0.197***	0.249***
	(0.00811)	(0.00727)	(0.0387)	(0.0162)
(Sale/delist after Jan.2008)	-0.0818***	-0.0969***	-0.0180***	-0.0553***
	(0.00570)	(0.00536)	(0.00642)	(0.00372)
Observations	52,705	69,417	19,038	56,991
R-squared	0.009	0.007	0.003	0.010

Notes: The top panel reports results from regression (5), which regresses a monthly indicator of whether a property was refinanced that month on the interaction of $f(L_i)$ with an indicator for whether the month is on or after May 2008. The results suggest that high-propensity homes in SF and LA were more likely to refinance on or after May 2008. The bottom panel reports results from regression (6), which regresses an indicator for whether the property refinanced within 12 months of its sale or delisting date on the interaction of $f(L_i)$ with an indicator for whether the property was sold or delisted on or after January 2008. The results here indicate that high-propensity homes that were sold or delisted on or after January 2008 were more likely to be refinanced within 12 months.