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CREDIT SUPPLY AND HOUSE PRICES:
EVIDENCE FROM MORTGAGE MARKET SEGMENTATION

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ABSTRACT

We show that easier access to credit significantly increases house prices by using exogenous changes in the conforming loan limit as an instrument for lower cost of financing. Houses that become eligible for financing with a conforming loan show an increase in house value of 1.16 dollars per square foot (for an average price per square foot of 220 dollars) and higher overall house prices controlling for a rich set of house characteristics. However, these estimated coefficients are consistent with a local elasticity of house prices to interest rates that is lower than some previous studies proposed (below 10). In addition, loan to value ratios around the conforming loan limit deviate significantly from the common 80 percent norm, which confirms that it is an important factor in the financing choices of home buyers. In line with our interpretation, the results are stronger in the first half of our sample (1998-2001) when the conforming loan limit was more important, given that other forms of financing were less common and substantially more expensive. Results are also stronger in zip codes where personal income growth is low or declining, and in regions with lower elasticity of housing supply.

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An online appendix is available at:
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1 Introduction

One of the central debates in finance is about the role of credit on the level of asset prices and the creation of bubbles (Kiyotaki and Moore, 1997; Allen and Gale, 1998; Bernanke and Gertler, 2001; Kindleberger, Aliber, Solow, 2005; Mian and Sufi, 2009; Brunnermeier, Eisenbach and Sannikov, 2012). A salient recent example is the US housing market: many observers of the crisis have proposed that easy access to credit, and the reduced cost of credit, were the central factors fueling the increase in housing prices as well as the subsequent reversal in house price growth when credit dried up (Favilukis, Ludvigson, and Van Nieuwerburgh, 2010; Hubbard and Mayer, 2008; Khandani, Lo, and Merton, 2009; Pavlov and Wachter, 2010; Mayer, 2011). Proponents on the other side of the debate argue that cheap credit alone cannot explain the house price boom and bust, and that other forces are likely to have been at play (Glaeser, Gottlieb, and Gyourko, 2010).

The key difficulty in measuring the effect of credit on the price of housing is establishing the direction of causality between credit conditions and house price growth: On the one hand, easier and cheaper access to credit might reduce borrower financing constraints and increase total demand for housing, which in turn would lead to higher prices. On the other hand, however, credit conditions might be responding to expectations of stronger housing demand and, as a consequence, higher house prices. In this latter scenario, cheaper credit is not the driver of house price increases, but a byproduct of increased demand for housing, since housing as collateral becomes more valuable. As we see in the existing literature, it has been very difficult to separate these two effects.[1]

In this paper, we use annual changes in the conforming loan limit (CLL) as an instrument for exogenous variation in the cost of credit, and in the availability of credit itself. The CLL determines the maximum size of a mortgage that can be purchased or securitized by Fannie Mae or Freddie Mac. This implicit (and since 2008, explicit) government support for loans below the conforming loan limit provides easier access to credit for a wide range of borrowers and reduces the cost of credit relative to jumbo loans. The difference in interest rates between conforming loans and jumbo loans (those that are above the conforming limit) is estimated to be up to 24 basis points in the 90's and 2000's (McKenzie, 2002; Ambrose, LaCour-Little, and Sanders, 2004; Sherlund, 2008; Kaufman, 2012). Loutskina and Strahan (2009, 2011) show that lenders screen borrowers for jumbo loans more intensely than they do those that apply for conforming loans, which implies that mortgages below the conforming loan limit are both cheaper and easier to obtain than above this limit.

[1] A recent paper by Favara and Imbs (2011) uses branching deregulation in the 1990s to identify the causal link between credit supply and house prices and finds that states where there is deregulation subsequently experience larger house price increases.

The key idea of our identification strategy is that changes in the conforming loan limit (CLL) from one year to the next are exogenous not only to individual transactions, but also to the conditions of local housing markets or the local economy, since this change is based on the countrywide average appreciation in house prices. Our empirical approach involves comparing transactions with a price that can be financed more easily using a conforming loan, and houses that are more expensive and for which buyers need to obtain larger loans to maintain the same loan-to-value ratio. We track houses in this price range in the year that the limit is in effect as well as the subsequent year, once the limit is raised and all houses in the sample become eligible for conforming loans. This setup enables us to cleanly identify the effect of the cost and access to credit based on the exogenous variation of the CLL and control for any overall trends in house prices.

The threshold that we use to define houses that are "easy" to finance with a conforming loan in a given year is the conforming loan limit divided by 0.8.[2] By construction, buyers of houses with a price below this threshold can get a conforming loan that makes up 80 percent of the price of the house, whereas, if the price of the house is above 125 percent of the CLL, it can no longer be financed at 80 percent with a conforming loan.[3] Above this price threshold, borrowers either finance their home with an 80 percent first mortgage using a jumbo loan (i.e. a loan above the CLL) at a higher interest rate, or, if they want to take advantage of the lower interest rate below the CLL, they have to use savings or alternative forms of financing to make a larger down payment.

The intuition behind this estimation strategy is that transactions that fall just above the threshold are harder to finance, and thus, their prices have been bid up less relative to the underlying fundamentals of the house. An increase in the conforming loan limit should affect this part of the house price distribution more strongly since it allows borrowers who previously were not able to get attractive financing to enter this segment of the market. Importantly, our sample includes all transactions in this price range independent of the financing choice of each borrower. This excludes any bias from our estimates that might come from the endogenous choice of financing of a specific transaction, e.g. richer people who can afford to put more money down might also purchase houses that are more expensive based on (unobservable) quality dimensions.

We first confirm that the conforming loan limit has a significant impact on borrowers'

[2] Kaufman (2012) uses this threshold for appraisal values to study the effect of the conforming status of a loan on its cost and contract structure.

[3] 80 percent loan to value ratios (LTV) are widely used in the industry as an important threshold for first lien mortgages. Loans with an LTV ratio below 80 are associated with more attractive terms and conforming loans above 80 percent require private mortgage insurance in order to qualify for purchase by Fannie Mae or Freddie Mac (Green and Wachter, 2005).

choice of financing. The data shows that the norm in the mortgage market during this period was to borrow at an LTV of *exactly* 0.8 (on average 60 percent of transactions are at an LTV of 0.8). However, for houses that transact just above 125 percent of the CLL, a much larger fraction of purchases are at an LTV below 0.8, since many borrowers choose to take out a mortgage exactly at the conforming loan limit. This produces a large discontinuity in the number of loans that are made at exactly the conforming loan limit, suggesting that the CLL is an important determinant of mortgage finance. Borrowers that buy houses with a price above the threshold of 125 percent of the CLL may choose a conforming loan either because they are excluded from the jumbo loan market altogether, or because the combination of a conforming loan and additional financing is a cheaper option. Whatever the reason, these borrowers are *de facto* constrained in their choice of financing relative to borrowers who buy houses at a price below 125 percent of the CLL.

In our main analysis, we test the impact of easier access to credit on house prices instrumented via the change in the conforming loan limit. We run differences-in-differences regressions in which we compare transactions just above and just below the threshold of 125 percent of the CLL in the year that the limit is in effect, and in the subsequent year when all of the transactions can obtain an 80 percent conforming loan.[4] We use three different dependent variables to capture the value of a property: (1) the value per square foot; (2) the residuals of log of house prices from a hedonic regression using a large set of controls for the underlying characteristics of the house, and (3) the residuals of the value per square foot from similar hedonic regressions. [5]

We show that exogenous changes in the availability of credit due to changes in the CLL have a significant effect on the pricing of houses that can be financed more easily using a conforming loan. Specifically, we find that transactions in the "constrained" group of borrowers, i.e. those with transaction values just above 125 percent of the CLL, are made at lower values per square foot than those for the unconstrained group. We see a 1.16 dollar discount per square foot for a mean value per square foot of 220 dollars and mean size of 1935 square feet. This difference is still significant but reduced to 0.65 dollars per square

[4]This is the case for all years between 1998 and 2005. For example, the CLL in 1999 is USD $240,000$, which gives a threshold of USD $240,000/0.8 = 300,000$ for this year. This means that in the regression for 1999, we include houses priced at between $290,000$ and $310,000$ in the years of 1999 (the year the CLL is in effect) and 2000. The CLL in the year 2000 was raised to $252,700$, so the new threshold for that year is $315,875$. Clearly, all the houses we included in the analysis for 1999 can be financed at 80 percent with a conforming loan in the year 2000.

[5]We run the hedonic regressions by year and by metropolitan statistical area (of which we have 10) and we use the set of controls available from deeds registries data, which includes common variables such as number of rooms and number of bedrooms, but also detail on the type of heating, architectural type, building type, among many others (we discuss these controls in more detail in Section 3.2).

foot after we control for house characteristics, suggesting that part of the effect we find can be accounted for by differences in the observable quality of houses above and below the threshold. The effect is smaller (and often insignificant) in the second half of our sample (2002-2005), which is the period when jumbo loans became cheaper and easier to obtain (partly due to the increased ease with which they could be securitized) and also when second lien mortgages became widely available (see Figure 1 in the Online Appendix). Both these effects go in the direction of making the CLL less important in the latter part of the sample, since borrowers above and below the threshold were getting similar access to finance.

Given our estimate for the change in house prices due to changes in credit conditions, we can compute the semi-elasticity of prices to differences in interest rates in the region close to the threshold. As our measure of the cost differential faced by buyers above and below the threshold, we use the differences in interest rates estimated in the prior literature of 10 to 24 basis points between conforming and jumbo loans. We obtain elasticity estimates that range from a low end of 1.2 to an upper range of 9.1 depending on the period and the exact estimate for the interest rate differential between jumbo and conforming loans that we use for our calculations. Therefore, when we use exogenous variations in the ease of credit conditions, we find elasticity estimates that are at the lower end of what has been previously found in the literature. This suggests that it is of first order importance to control for the endogenous decrease in the cost of capital in times when house price appreciation is high.

One basic assumption underlying our analysis is that the CLL provides a significant improvement in the cost of funding and in the access to finance for people buying houses at a price just around the threshold. We expect this effect to be stronger when buyers face other types of constraints at the same time, namely in terms of their income. To test this intuition, we interact the changes in the CLL with the economic condition of the average household in a neighborhood. We find that the effect of credit supply on value per square foot is much stronger in zip codes and years that are below the 10th percentile of the income growth for each individual regression. The point estimate for these areas shows that value per square foot is 2.50 dollars higher in the year that a house becomes eligible to be financed with a conforming loan. This is more than double the size of the average effect that we found in the overall sample.

We also test whether our instrument has an impact on the number of transactions we observe above the threshold. There is no visible bunching of transactions around the threshold of CLL divided by 0.8, suggesting that the *supply* of housing does not react strongly to the CLL. In our empirical tests we find no significant effect of our instrument on the quantity of houses transacted during the first half of the sample. We do see fewer transactions above the threshold in the period 2002-2005, which may help explain the weaker response in prices that we find in the later period.

Our estimation strategy allows us to estimate the causal effect of changes in credit availability on the valuation of houses. Since house price levels differ across the various states of the United States, the change in the CLL affects different parts of the housing stock across areas depending on the price level of the area. This allows us to control for the possibility that there are differential growth rates within the distribution of house types across the country. For example, one concern would be that middle class families might be the ones buying a certain type of house and, at the same time, have a different income growth from other parts of the economy. Our instrument allows us to rule this out, because the same "type" of house will have different prices depending on the part of the country in which it is located. However, we do not observe the exact mechanism through which credit conditions feed through to house prices. There are at least two alternative ways that credit could affect prices: First, better access to credit increases demand for houses since more people can now bid on properties, and as a result, we see an increase in the price of the transactions. A second and alternative channel would be that borrowers who have easier access to finance bargain less hard for a reduction in property prices relative to borrowers who struggle to find financing. Importantly, in either of these channels a change in access to finance is driving the change in borrower behavior and consequently house prices.

We can rule out an alternative hypothesis related to a selection effect driving our results, whereby buyers of houses "above the threshold" in the year that the conforming loan limit is in effect are different along some unobservable characteristics from the other buyers. Several features of our analysis make selection an unlikely explanation of the results. First, for a selection hypothesis to be a true alternative to our explanation of the results, it would have to involve arguments other than access to credit to explain why buyers were different above and below the threshold. Second, these "special" buyers would both have to be better able to deal with the reduced access to credit (potentially because they are wealthier or have higher income) and bargain harder for houses. It is unclear why wealthier borrowers should pay less for a similar house than poorer borrowers. If wealthier people bought higher quality houses and we did not observe these differences, these unobservable characteristics would create bias in the opposite direction. Third, our identification strategy would require that the selection effect change each year parallel to the change in the size of the conforming loan limit, which is very unlikely. Finally, to help further rule out this selection hypothesis, we run our main regressions excluding borrowers that choose LTVs below 80 percent in the year that the CLL is in effect. If selection was the explanation of the results, these transactions should be by "wealthy" borrowers driving the results. We find that the results do not change materially when we exclude this subset of transactions.

The rest of the paper is structured as follows: Section 2 discusses related literature and the user cost model. Section 3 describes our data and the identification strategy. In Section 4, we lay out the regressions results and robustness checks of our main analysis. Section 5 discusses the findings and concludes.

2 The User Cost Model

In this paper, we are interested in estimating the impact of changes in credit conditions on the price of housing. The existing literature has focused on different versions of the user-cost model of Poterba (1984) to draw conclusions about the role of interest rates and other costs of owning for house prices. In this model, agents are indifferent between owning and renting if the housing market is in equilibrium, where the mortgage interest rate is the main determinant of the cost of owning. Below, we show that different assumptions yield very different conclusions about the role of interest rates in driving the cost of housing and highlight why our estimate of the impact of the cost of credit on prices is an important contribution to this debate. We follow the notation in Glaeser, Gottlieb, and Gyourko (2010) to describe the basic elements of the user cost model. Renting a property involves paying rent equal to R_t in each period. Owning a property, on the other hand, includes making a down-payment θ that is a proportion of the price of the house P_t and obtaining a mortgage that is rolled over each period, such that principal is never paid down completely. The borrower then pays interest on the mortgage at a rate r_t that is deflated by the relevant tax rate ϕ, as well as property taxes and maintenance costs equal to τ that both grow at a rate g. The model assumes that individuals have a private discount factor of ρ_t. If we assume that market interest rates and private discount rates are constant and equal to each other, we can write the indifference condition for users as:

$$\frac{R_t}{P_t} = (1 - \phi)r - g + \tau \tag{1}$$

This is shown in Glaeser et al (2010) and is similar to what is presented in Hubbard and Mayer (2008) as well as a simplified version of the user cost in Himmelberg, Mayer, and Sinai (2005). If the assumptions of this model hold, changes in the user cost (the right-hand side of the equation) should lead to changes in the price to rent ratio according to this formula. For example, if the user cost is 5 percent, then the price of a house should be about 20 times its market rent. In such a world, a drop of 1 percentage point in mortgage rates would lead to a decline of $(1-\tau)$ in the user cost, or 0.75 if we assume a marginal tax rate of 25 percent. The price to rent ratio would then be 23.5, an increase in the price of 17.5 percent. This is the magnitude of the elasticities proposed in Himmelberg et al (2005), and in Hubbard and Mayer (2008). Glaeser, Gottlieb, and Gyourko (2010) dispute some of the simplifying assumptions in the model above, and show that a more realistic model can produce much lower elasticities of prices to interest rates. In particular, if private discount rates are not the same as market rates, then changes in interest rates wont change the way users discount future expected house price appreciation. This change alone can reduce the elasticity to just 8, instead of the initial 17.5. Glaeser et al (2010) propose other mechanisms through which the elasticity could be substantially reduced, namely mean reverting interest rates, which means borrowers anticipate having to sell a home at a time when rates are higher, or the possibility of prepaying a mortgage, which again lowers the predicted elasticity. Our econometric approach allows us to more carefully identify the magnitude of the change in house prices due to changes in the average cost of financing, since we look at exogenous movements in the cost of capital for home buyers. Our empirical results provide estimates for the numerator of the elasticity calculation. In Section 4.4, we discuss the range of elasticities that are consistent with our results.

3 Data and Methodology

The dataset we use in this paper contains all the ownership transfers of residential properties available in deeds and assessors records for the cities that are covered by Dataquick. Our dataset spans 11 years, from 1998 to 2008, and contains all transactions recorded on the deeds registries for seventy-four counties in ten metropolitan statistical areas (MSAs) - Boston, Chicago, DC, Denver, Las Vegas, Los Angeles, Miami, New York, San Diego, and San Francisco. We limit our attention to transactions of single-family houses, which account for the large majority (approximately 78 percent) of all observations.

Each observation in the data contains the date of the transaction, the amount for which a house was sold, the size of the first mortgage, and an extensive set of variables about the property itself. These characteristics include the property address, interior square footage, lot size, number of bedrooms, number of bathrooms, total rooms, house age, type of house (single-family house or condo), renovation status, and date of renovation. Additional, more detailed characteristics, include the availability of a fireplace, parking, the architectural and structural style of the building, the type of construction, exterior material, availability of heating or cooling, heating and cooling mechanism, type of roof, view, attic, basement, and garage. We describe the procedure for cleaning the raw data received from Dataquick in the Online Appendix to the paper.

3.1 Summary Statistics

The sample that we use for this paper contains 3.9 million transactions of single-family houses that are summarized in Tables 1 and 2. We can see in Panel A of Table 1 that the average transaction value in our sample is 308,520 dollars with a standard deviation of 123,930 dollars. The average size of the houses in the cleaned dataset is 1,735 sqft, and the houses have, on average, 3 bedrooms and 2 bathrooms. The average loan to value is 0.81 (including only the first mortgage for each transaction), and the median LTV is 80 percent. The average value per square foot is 194 dollars with a standard deviation of 92 dollars per square foot (first row of Panel B).

Table 1 also shows the summary statistics for the restricted sample we use in the regressions in the final three columns. For the restricted sample[6] , the average price for each house is higher than in the whole dataset, the average value per square foot is 220 dollars with a standard deviation of 93 dollars per square foot, given that this subsample includes only houses that are close to the conforming loan limit. This is consistent with the fact that the conforming loan limit was set to cover substantially more than 50 percent of the mortgages made every year (Acharya, Richardson, Nieuwerburgh, White, 2011). These houses are also, on average, larger and have more bedrooms and bathrooms than the whole Dataquick sample.

Panel A of Table 2 shows marked differences in the summary statistics for each of the ten MSAs included in our data. The table shows that San Francisco is the metropolitan area with the highest valuation, with an average house price of 384 thousand dollars. Denver and Las Vegas represent the areas with the lowest valuation, with an average of approximately 250 and 262 thousand dollars respectively. When we compare values per square foot, we get a similar picture, namely San Francisco is the area with the highest valuation with an average of 266 dollars per square foot, and Las Vegas is the area with the lowest valuation with an average of 137 dollars per square foot.

Table 2 Panel B shows the evolution of prices through time. Here we see the increase in house prices from an average of 240 thousand dollars in 1998 to a peak of 366 thousand dollars in 2006, as well as the increase in the volume of transactions over the same period. The increase in prices and volume is linked to an increase in volatility. The standard deviation of the transactions increased from 102 thousand dollars in 1998 to 122 thousand dollars in 2006. A similar pattern can be observed for the value per square foot measure, where standard deviation is 51 dollars per square foot in 1998, and increases to 106 dollars per square foot in 2006. Finally, the loan to value average (including only the first mortgage)

[6]In this case, transactions that are used as control for one year, and treatment in the following period, are duplicated to reflect the statistics of the regression sample, the augmented data set has 262,671 observations

is stable both across MSAs and through time at around 0.8.

3.2 Hedonic Regression

One of the advantages of using deeds registry data is the richness of the information provided on the property characteristics, which allows us to account for price differences between houses that can be attributed to observable features. Specifically, we will be able to assess whether the price impact we observe due to the changes in the conforming loan limit can be attributed to differences in the quality of the houses, or whether these differences are there even after accounting for quality.

In order to distinguish between these two explanations, we estimate hedonic regressions of value per square foot and log of house price on a number of house characteristics, and estimate the residuals for each of these two left-hand side variables (which we denote by LHS_i). Specifically, we estimate the following regressions by MSA and by year:

$$LHS_i = \gamma_0 + \Gamma X_i + month_i + zipcode_i + \varepsilon_i$$

We use both the price of a transaction as well as the value per square foot as our dependent variables. By estimating these regressions by year and by MSA, we allow the coefficients on the characteristics to vary along these two dimensions. We also use monthly indicator variables to account for seasonality in the housing market, as well as zip code fixed effects. The set of controls X_i is a similar set of controls to that used in Campbell, Giglio, and Pathak (2010) with some additional characteristics. The controls include square footage, high and low square footage dummies, the size of the lot, number of bedrooms and bathrooms, and a number of indicators for interior and exterior house characteristics (eg. fireplace, style of the building, etc.). We describe which variables are included, as well as the detail of the construction of each variable, in the Online Appendix to the paper.

The estimated R^2 of each of these regressions (80 in total for each of the two left-hand side variable–10 MSAs in 8 years) is between 40 and 60 percent for the price of the transaction, and 50 to 70 percent when we use value per square foot as a dependent variable[7].

Summary statistics for the residuals from the hedonic regressions for the whole sample are shown in Panel B of Table 1. The average residuals are, by construction, zero. The standard deviation of the errors is about 42 dollars per square foot, and 0.17 thousand dollars for the log of the price of the house. The hedonic regressions are estimated on the whole clean sample[8], so when we restrict our attention to the regression sample, the average

[7]Unreported regressions.

[8]Please see the Online Appendix for a detailed description of what is included in this subset of the data.

error no longer has to be zero. Indeed, for the regression sample, the average residual from the hedonic regressions for the value per square foot is positive at 5.3 dollars, and the average error for the log of transaction value of the house is 0.05 dollars (last three columns of Panel B of Table 1). The standard deviation of the residuals for the regression subsample is similar in magnitude to what we obtain for all the transactions.

3.3 Empirical Approach

3.3.1 Identification Strategy

To identify the effect of changes in credit conditions on house prices, we restrict our analysis to two groups of buyers who all buy houses in a tight price range, but differ in the financing available to them. The sample for our regressions is made up of houses that transact in a band around 125 percent of each year's conforming loan limit, as well as houses in the subsequent year in the same price range. Specifically, we divide houses into two groups: houses *below* the threshold of 125 percent of the year's CLL (i.e. transactions that fall between 125 percent of CLL and 125 percent of CLL minus USD 10,000) and houses *above* that threshold that transact between 125 percent of CLL and 125 percent of CLL+$10,000$. By construction, in the year that the conforming loan limit is in effect, houses above the threshold of 125 percent of the CLL cannot be financed at 80 percent using a conforming loan, whereas the houses below the threshold can be financed. Thus, home buyers that bid for houses priced above 125 percent of CLL cannot finance a full 80 percent of the transaction with the cheaper and more easily available conforming loans. In the subsequent year, the CLL is raised and both groups of transactions can be financed at 80 percent with a conforming loan.[9] Our sample includes all transactions in this price range, independent of the mortgage choice made by each buyer. This way, our estimates are not biased by the endogeneity of the choice of financing of each specific transaction.

The identification strategy is best understood through an example. Consider the year 1999: In that year, the conforming loan limit (CLL) for single-family houses was USD $240,000$. The corresponding threshold for house prices that we use for this year is $300,000$ ($240,000/0.8$ or, equivalently, $1.25 * 240,000$). In this year, the group of houses "above the threshold" have prices between USD $300,000$ and USD $(300,000 + 10,000) = 310,000$ and houses "below the threshold" have a transaction price between USD $(300,000 - 10,000) = 290,000$ and USD $300,000$ (those that transact at exactly USD $300,000$ are included in

[9] While this was no longer true for the years after 2006, in all cases between 1998 and 2005, the limit increases enough from year to year to make up 80 percent of the price of the transactions we have in the sample.

this second group). For the purposes of our main regressions, we track these two groups of houses from 1999 to 2000, where 1999 is the year in which the CLL is in effect and 2000 is the year in which all these transactions could be bought using a conforming loan at a full 80 percent LTV. In fact, the CLL changed in 2000 to USD $252,700$, so the threshold of 125 percent of CLL was now USD $315,875$ and even our "above the threshold" group for 1999 is now eligible to get an 80 percent LTV conforming loan.

One important assumption in our analysis is that borrowers in the group "above the threshold" of 125 percent are constrained in their choice of financing. In order to stay at an LTV of 0.8, they have to take a jumbo loan and these have been found to be more expensive by between 10 and 24 basis points relative to conforming loans (McKenzie, 2002; Ambrose, LaCour-Little, and Sanders, 2004; Sherlund, 2008; Kaufman, 2012). Alternatively, they can also borrow up to the CLL and then cover the rest of the house price with savings or other funding, which means having a first mortgage LTV of less than 80 percent. This additional source of funding is likely substantially more expensive relative to the conforming mortgage rate. For some borrowers, this may, in fact, be the only option, as they may be excluded from the jumbo market altogether because of more careful screening of jumbo loans done by originating banks (Loutskina and Strahan, 2009, 2011). Whether they choose a jumbo loan or they make up the difference using other sources of financing, these borrowers have a higher average cost of capital than the buyers below the threshold. Figure 1 shows the frequency of transactions by price and choice of first mortgage (both in USD 10,000 bins). The figure shows that the most frequent choice on the part of borrowers is to have an LTV of exactly 80 percent (that is, the large mass along the diagonal of the figure). The main exception to this rule occurs exactly at the conforming loan limit, where a significant mass of borrowers chooses an LTV below 0.8 by sticking to a conforming loan (in 2000 the limit was USD 252,700, and in 2004 it was 333,7000). In unreported analyses, we find that in the year in which the CLL is in effect, about 45 percent of the houses *below* the threshold in our sample are bought with an LTV of exactly 80 percent, whereas, for houses above this boundary just 19 percent of borrowers pick 80 percent LTVs (which for these transactions means using a jumbo loan). Additionally, on average 55 percent of the transactions just above the threshold are financed using a conforming loan, which means having an LTV lower than 80 percent. These borrowers end up with an LTV of 77-79.5 percent, which is a very infrequent choice anywhere else in the distribution. Again, these borrowers might have a lower LTV because they choose to stay below the CLL due to the cost of the loan, or because they are excluded from the jumbo market altogether. Whatever the reason, this group of borrowers is "constrained" in the set of options available for financing their house.

3.3.2 Empirical Specification

Our main regressions estimate the size of the effect of the constraint imposed by the conforming loan limit on the valuation of transactions made just above the threshold of 125 percent of the CLL. We run differences-in-differences regressions year-by-year with one indicator variable for houses priced above the conforming loan limit divided by 0.8, another indicator for the year in which the CLL is in effect, and an interaction of these two indicator variables. We also include ZIP code fixed effects in all regressions, so our estimates do not reflect differences between neighborhoods, but rather variation within zip codes.

The sample for each year-by-year regression includes houses within a USD 10,000 band around the conforming loan limit in the year in which the limit is in force, as well as the subsequent year. This implies that the "Above the Threshold" indicator variable takes a value of 1 if the price at which a house transacts is greater than 125 percent of the conforming loan limit of a certain year, and less than that amount plus 10,000 dollars. This same variable is a 0 for transactions between 125 percent of the CLL and 125 percent of the CLL minus 10,000 dollars. The "Year CLL" indicator variable is a 1 in the year in which the CLL is in effect for each regression, and a 0 in the subsequent year. We use a tight band around the threshold so that all transactions in the year after the limit is in effect are eligible for an 80 percent LTV conforming loan. We thus have a group of transactions that is "easy to finance and another one that is "hard to finance in the year that the limit is in effect, but all transactions in the sample are "easy to finance once the limit is raised.

We run regressions of the following form:

$$Valuation\, measure_i = \beta_0 + \beta_1 1_{AboveThreshold} + \beta_2 1_{Year\_CLL} + \beta_3 1_{Above\ Threshold \times Year\_CLL} + \gamma_{ZIP} + \varepsilon_i$$

We estimate this regression for each year between 1998 and 2005. We cannot include 2006 and 2007 in our estimates because the conforming loan limit did not change after 2006 in our data (house prices dropped and the administration left the limit unchanged). After we obtain β_1, β_2, and β_3 for all 8 years (1998-2005), we estimate Fama-MacBeth averages (Fama and MacBeth, 1973) of these coefficients and obtain the standard errors of this average by using the standard deviation of the estimated coefficients and dividing it by the square root of the number of coefficients.

We should point out that our approach is *not a regression discontinuity design*, but rather differences-in-differences for each pair of years. There are a couple of reasons for this: First, the threshold that we use does not imply a sharp discontinuity in the ease of financing a home. For a house just one dollar above the threshold, a homebuyer only has to come up with one additional dollar of equity (and still obtain a conforming mortgage), which means the total cost of financing the house is almost unchanged. As we move progressively away from the threshold, transactions become harder to finance. For our differences-in-differences estimator to be valid, all we need is that houses above the threshold are somewhat harder to finance, though not necessarily discontinuously so.

The second reason for not using a regression discontinuity design is that in the year that the limit is in effect, homebuyers choose to buy houses above or below the threshold, i.e. the position with respect to the limit is not exogenous. On the contrary, our differences-in-differences specification uses the exogenous *change* in the conforming loan limit to compare a group of transactions that are above the limit in a year, but below in the next with a group of transaction that are always below the limit, achieving a clean identification of the effect of credit availability on house prices.

3.3.3 Differences in Financing Choices

As we pointed out above, the equivalent to a first stage in our empirical strategy is to show that the changes in the conforming loan limit have a significant effect on the financing choices of borrowers. In Figure 1 we can see the importance of both the 80 percent LTV rule, as well as the conforming loan limit, in determining financing choices for the whole distribution of transactions. In Figure 2 we focus on the groups of transactions that we include in the regressions. The first panel tracks transactions up to USD 10,000 below 125 percent of the conforming loan limit in each year, whereas the second panel includes transactions up to USD 10,000 above the threshold. We show the total number of transactions (for all years between 1998 and 2006) in each month during the year prior to the limit being in effect, in the year that the limit is valid, and in the subsequent year. We also break down the transactions by the choice of LTV - the transactions at the bottom of each panel have an LTV below 75 percent, the second group includes transactions with an LTV between 75 percent and 79.5 percent, the third has transactions with LTV=80 percent, and the top group has all the transactions with an LTV above 80.1 percent. The main message from Figure 2 is that in the year that the CLL is in effect, the composition of financing choices by borrowers differs very significantly, with the 80 percent group becoming very prominent for the transactions below 125 percent of the CLL, whereas it is small for the transactions above the threshold. At the same time, the borrowers who stick with a conforming loan and buy houses above 125 percent of the CLL become an important fraction of all borrowers (they have an LTV between 75 and 79.5 percent).[10] In the year after the limit is in effect,

[10]The first picture for the group below 125 percent of the CLL also shows a noticeable fraction of borrowers with an LTV between 75 and 79.5 percent in the year before the CLL is in effect. This is because

the choice of LTV across the two groups becomes indistinguishable.

In Table 3, we present the effect of the changes in the conforming loan limit on the financing choices made by the borrowers included in the sample of our main regressions. In this table, we are verifying what we see in the pictures, namely that borrowers on average end up with lower LTVs when they buy houses above the threshold of 125 percent of CLL. Results for the effect of the CLL on financing choices are shown in Table 3. We find that LTVs are, on average, 0.3 to 0.7 percentage points lower for the group of transactions that happen above the threshold of 125 percent of the CLL in the year that the limit is in effect. This effect is statistically and economically significant given how little variation there is in the modal choice of LTV of borrowers. The second panel on Table 3 shows that borrowers also obtain, on average, smaller loans in the year that the limit is in effect and when the price of the house is above the threshold. The difference in log loan amount is, on average, 0.0056 to 0.0088 dollars, and based upon the findings in our main results, we conjecture that it is the fact that borrowers obtain smaller first mortgages that leads to the difference of approximately 1.16 dollars per square foot (for an average value per square foot of 220 dollars).

3.3.4 Differences in the Number of Transactions

There are several reasons to expect quantities to change due to differential ease in access to credit, including different levels of down-payment (Stein, 1995) or sellers waiting for buyers to obtain better credit conditions (Genesove and Mayer, 1997). In fact, unless the supply elasticity of houses is very low (or zero), we expect the price effect due to a change in the demand for housing to be accompanied by a change in the number of transactions.

As discussed in Section 3.3.2, we do not use a regression discontinuity approach to address the question of the change in the quantity of transactions. Figure 3 confirms that this would produce no significant result. This figure shows the number of transactions relative to the threshold in each year. The figure is centered at "0, i.e. the transactions at exactly 125 percent of the CLL. The figure shows that there is no discontinuity in the number of transactions above and below the threshold. We confirmed this result by splitting the sample into old and new houses, different MSAs, and in the first half and second half of each year (all unreported).

Given that a regression discontinuity would not be appropriate in our setting, we use

these transactions were not eligible for a conforming loan at an 80 percent LTV in the year before the new limit was in effect and were, in general, just slightly above that threshold. This is thus a reflection of the same phenomenon we see for the group above 125 percent of the CLL in the year that the new limit is in place.

a setup similar to our main regressions to look for changes in the number of transactions above and below the threshold. We consider the difference in the share of transactions in our sample that fall above and below the threshold in the year that the limit is in effect and in the subsequent year in a differences-in-differences setup. This test is equivalent to a T-test for the mean of the variable "Above Threshold" that compares the average of this variable in the year that the limit is in effect and in the subsequent year. If our instrument affects the quantity of transactions, we should see an increase in the share of observations above the threshold when the limit is raised, as access to credit become easier for those transactions. We show in Table 4 that this test reveals no changes in the share of transactions above and below the threshold for the first part of our sample (1998-2001), and that there is a statistically significant effect for the second part of the sample. This effect means that the share of transactions above the threshold is approximately 50 basis points lower in the year that the conforming loan limit is in effect during the period 2002-2005. This shows that the effect of cheaper credit provided by conforming loans is reflected only on house prices in the first part of our sample, and that in the second part of the sample, it impacts both quantities and prices, i.e. supply elasticity of houses seems to have been higher in the second part of the sample. This, along with the reasons we give in Section 4.1 on the availability of second liens and jumbo loans, may help explain why the effect we find on prices is smaller relative to the earlier years (when the quantity response was not there).

4 Access to Credit and House Prices

4.1 Main Regression Results

We present the results for our canonical specification in Table 5. This table presents Fama-MacBeth coefficients from year-by-year regressions, as described before in Section 3.3.2. The coefficient of interest in Panel A of Table 5 is that on the interaction variable, and it shows that houses above the threshold of CLL/0.8 transacted at a value per square foot that was lower by about 1.16 dollars in the year that the CLL was in effect. The results are stronger for the first half of the sample, where the point estimate is -1.55 dollars per square foot for this set of transactions.

The other coefficients on the regressions for value per square foot are consistent with what we know about house prices over this period. First, houses that are above the threshold of 125 percent of CLL (i.e. the more expensive houses in the regression sample) are associated with a higher average value per square foot. In unreported analyses, we find that more expensive houses are generally associated with a higher value per square foot (i.e. price rises quicker than house size in the whole distribution of transactions), and here we find that this is also the case for the regression sample. Also, the "Year CLL" dummy variable is associated with a strong negative effect, reflecting the strong increase in house valuations that we saw in this period in the US. Given that the year in which the CLL is in effect is always the "pre" year in the regressions, we expect those transactions, on average, to be associated with a lower value per square foot.

In Panels B and C we use the residuals from the regressions we described in Section 3.2 as the dependent variable to account for differences in quality between houses. The results are qualitatively and quantitatively very similar to the ones we present in Panel A. In Panel B we are using the residuals of a regression of log of house price on a set of characteristics, and we find a point estimate of -0.0017 that translates to residual being lower by 620 dollars for houses above the threshold of 125 percent of the CLL when the CLL binds, considering an average transaction value of 371,340 dollars. This suggests that transactions that cannot be financed at 80 percent with conforming loans are made at lower prices even after we control for a rich set of house characteristics.

Similarly in Panel C of Table 5, we confirm that even when we use the value per square foot as a dependent variable but control for house quality, the interaction term is significant and economically large even though the point estimate of 0.65 dollars for houses above the threshold is slightly lower than the results in Panel A where we do not adjust for house quality. The difference between the point estimate of 1.16 dollars of Panel A and 0.65 dollars in this specification indicates that houses above the limit are of somewhat worse quality than those below the limit in the year that the limit is in effect.

We also show that the estimated effect of the conforming loan limit on house prices is stronger in the first half of the sample than in the second half. This result holds for all three left-hand side variables. This is in line with our expectations, given that borrowers had easier access to second lien loans after 2002 (we show the evolution of the use of second liens in Figure 1 of the Online Appendix). Additionally, more borrowers use jumbo loans, which may reflect a reduction of the cost differential of this type of loan relative to conforming loans, and an increase in the ease of access to this type of loan, possibly driven by an increased ease of securitization of these loans.

4.2 Credit Supply and Income

We now turn to how the effect of credit supply on house prices changes with the growth in income in a zip code. To do this, we obtain data on zip code level average household income each year from 2000 to 2007 from Melissa Data.[11] We create a new variable that is a "1" if a zip code has negative nominal average income growth from one year to the next, and "0" otherwise. We then run similar regressions to what we did before (year-by-year), adding an interaction between our previous variables and this new zip code level "Negative Income Growth" variable. Looking at the coefficient on the triple interaction term (negative income growth, the year that the CLL is in effect, and being above 125 percent of the CLL) allows us to identify how the effect of credit supply differs in times of positive and negative income growth. Our hypothesis is that the effect of credit supply is stronger in times of negative income growth, as households in a certain zip code are more likely to be constrained and there is likely to be less competition for housing, which increases the probability that a seller sells to a constrained buyer.

We show the results for these regressions in Table 6. In the first column of Table 6 we repeat our main regressions for the period 2001-2005 only, as this is the period for which we were able to construct the income growth indicator variable. The results are consistent with those in Table 5. In the second column of Table 6 we show Fama-MacBeth coefficients from the regressions with the income growth interaction term. The triple interaction terms show that the effect of credit supply on value per square foot is significantly stronger in zip codes and years that are below the 10th percentile of income growth for the individual regression. The point estimate shows that value per square foot is 1.55 dollars lower in the year that the conforming loan limit is in effect for houses above 125 percent of the limit when income growth is low in a zip code. We also find that the main effect from our regressions in Table 5 is quantitatively similar to before, implying that the simple inclusion of ZIP code level income does not change any of our main results.

In the Online Appendix we plot the distribution of value per square foot for ZIP codes of different income levels. Those pictures also suggest that the distribution of value per square foot is affected by the conforming loan limit in ZIP codes in the lowest quartile of the income distribution. In particular, the average value per square foot is monotonically increasing for up to conforming loan limit threshold, and from this point onwards the distribution becomes flat. This pattern is not visible for zip codes with higher median incomes.

4.3 Robustness and Refinements

4.3.1 Differential House Price Trends

We want to rule out that our results are driven by differences in secular trends between houses above and below the threshold of CLL/0.8. Specifically, if more expensive houses

[11] Melissa Data obtains this data from the IRS and provides it in an easy-to-read format.

have, on average, lower house price growth from one year to the next relative to less expensive houses, we might obtain the results reported in Table 5, but we might also obtain similar results for samples with transactions above and below other arbitrary thresholds.

In order to address whether the effect that we find is indeed the product of the true conforming loan limits and not due to different trends along the distribution of houses, we run the same regressions described in Section 3.3.2 for "placebo" loan limits. We do this by shifting the true conforming loan limit in USD 10,000 steps from the true value each year. We start at CLL-100,000 and move 20 steps until we reach CLL+100,000. For each of these 21 tests, we first define the "shift" relative to the true conforming loan limits, and then we change the limits for all years by that amount. For example, when we are changing all the limits by -20,000, this means that the "placebo" limit for 1999 is 220,000 dollars instead of the true 240,000 dollars, the "placebo" limit for 2000 is 232,700 instead of 252,700, and so on. We then run the same year-by-year regressions and produce Fama-MacBeth coefficients for each of the 20 alternative "placebo" values for the CLL. The results from this exercise are shown in Table 7.

The table shows that the coefficients of interest we obtain for all three dependent variables (values per square foot, residuals from the transaction amounts, and residuals of values per square foot) are systematically among the lowest of all obtained with the 20 "placebo" trials (the ranking is given in the last two rows of the table). The coefficient on the value per square foot measure is the lowest of the 21 trials whether we use the whole sample, or whether we limit our attention a sample of transactions that all have an LTV between 0.5 and 0.8 (we discuss this subsample in more detail in the Online Appendix). When we use the whole sample and the two residual measures from the hedonic regressions as the left-hand side variables in the regressions, the coefficients for the true conforming loan limits are the second and third lowest. In the restricted sample with LTVs between 0.5 and 0.8, these two measures produce the second lowest and the lowest coefficient out of the 21 trials. If we limit our attention to placebo limits that are below the true limits (i.e. the top half of Table 7), all our measures produce the lowest coefficients out of those trials. We consider these to be true "placebos", because all the transactions used for those regressions are, by construction, below the "eligibility" criteria of 125 percent of the true conforming loan limit both in the year that the limit is in effect, and in the subsequent year. As such, these transactions should not have any changes in credit availability from one year to the next.

When we compute the standard deviation of those coefficients, we find that the coefficients using value per square foot as the dependent variable are statistically significantly different from the average of the other coefficients at a 5 percent level in both the whole sample and in the restricted sample with LTV between 0.5 and 0.8. T-statistics for these tests are shown in the fourth row of Table 7. When we use the value per square foot residual measure as a left-hand side variable, the coefficient has a t-statistic of 1.77 in the whole sample, and above 2.37 in the restricted sample. Finally, the coefficient from the regression that uses the residual from the log of house price hedonic regression as a left-hand side variable is not significantly different from the average of the other coefficients, as the t-statistics are between 1.0 and 1.2 in both the whole sample and in the restricted sample. The fact that the results are directionally the same when using all three left-hand side variables, and that there is no "placebo" limit that consistently produces results that are as strong as the ones from the true limit, further confirms that our coefficients are not obtained by pure chance.

4.3.2 Selection Into Treatment

As discussed in the introduction, there can be at least two alternative mechanisms for the effect of the conforming loan limits on house valuation. The first mechanism is that easier access to credit around the threshold leads to an increase in the demand for houses of a certain type, which then leads to higher valuation of these houses (or, conversely, tighter access to credit reduces the demand for houses above the threshold in the year that the limit is in effect). The alternative mechanism is that different credit conditions above and below the threshold attract a type of buyer in the year that the limit is in effect that is both better able to deal with the worse access to credit (possibly because of higher wealth or income), and is a more effective negotiator than other "typical" buyers. This would still mean that our results are driven by credit conditions being different above and below the threshold, but it would be a different mechanism for our results. This selection effect results from the fact that borrowers can *choose* the level of their LTV. If all borrowers mechanically had to use an LTV of 80 percent, there would not be any possibility for selection.

To understand whether the aforementioned form of selection is important, we divide transactions that are just above the cut off for being eligible for a CLL at 80 percent in a given year into two groups: (1) transactions that nevertheless use a conforming loan and therefore choose to have an LTV below 80 percent (making up the difference with other forms of financing), and (2) transactions that use a jumbo loan with an 80 percent LTV, which means they do not get a conforming loan. The first group isolates the set of borrowers where selection could be an issue. These borrowers might be optimizing around the CLL threshold and could therefore have other unobservable differences from the rest of the borrowers. For example, these "special" buyers could have more wealth or higher income and thus might also differ in other unobservables such as their ability to bargain. By excluding the group of home buyers who choose this type of financing, we can test if these are driving our results, i.e. whether they alone buy *cheaper* houses. As an aside, it is ex ante not clear why those borrowers would buy cheaper houses (based on value per square foot). The fact that they are wealthier would usually lead us to believe that the omitted variable bias goes in the other direction, i.e. they buy houses with higher unobservable quality. The following regressions show that this group of borrowers does not drive our results.

To test the importance of the selection effect, we run differences-in-differences regressions excluding each of the two groups described above at a time (in the year that the limit is in effect) and construct Fama-MacBeth coefficients, as we did in Table 5. The results are shown in Table 8. We find that results do not change much when we exclude the jumbo loans or when we exclude the conforming loans, which implies that our main results are not being driven solely by either one of these groups of transactions. The statistical significance of the results is similar, and the magnitude of the coefficients sometimes is larger for one group and other times for the other, depending on the left-hand side measure we use. Overall, the results point in the same direction for both sets of regressions.

This robustness test shows that the effect of credit conditions on house prices in our setting is not likely to be driven solely by selection of different buyers in our "treated" group. If this were the case, we would expect the borrowers that pick a conforming loan and end up with an LTV below 80 percent to be the ones driving our main result. The fact that we also see similar results when we exclude this subgroup increases the likelihood of our alternative explanation, namely that credit access changes demand for housing, and that this shift in demand for housing drives the change in house valuation. Finally, in the Online Appendix we show that our results are very stable if we use a 5,000 dollar band around the threshold of CLL/0.8 instead of the 10,000, which suggests that the difference in the cost of credit is likely to be similar for these two sets of buyers relative to buyers below the threshold. This is further evidence that the result is not driven solely by buyers who choose to obtain a conforming mortgage and put up additional equity from other sources.

4.3.3 Constraints to Housing Supply

To understand whether the effect of credit supply is amplified by the inability of housing supply to adjust quickly to demand, we divide zip codes into high and low house supply elasticity according to the measure in Saiz (2010). If the supply of housing were perfectly elastic and able to adjust quickly to an increase in demand for houses, the effect on prices should not be there. In this test, we find that the constraint imposed by the conforming loan limit is stronger in zip codes located in more inelastic, metropolitan, statistical areas (MSAs) according to the Saiz measure (Table 9). This result is in line with what we expect and with previous literature (e.g. Mian and Sufi, 2009), namely that better access to credit will feed through to house prices more frequently in regions where the supply of houses cannot adjust as easily. We are cautious to interpret this result, however, because we have limited cross-sectional variation in the elasticity measure in our data. In fact, all of the MSAs in our sample are above the median elasticity found in Saiz (2010) for the whole country, and 7 of the 10 MSAs are in the top 20 percent of MSAs with the least elasticity in the nation.

4.4 Economic Magnitude of the Effect

As we discuss in Section 2, there is significant disagreement as to what the magnitude of the elasticity of house prices to interest rates is. As we discuss in that section, changes to the way a standard user cost model is specified can produce vastly different estimates. To understand the magnitude of our estimated effect, we compute the semi-elasticity of house prices to interest rates, calculated as the percentage change in prices divided by the change in interest rates. The change in the CLL gives us an unbiased local estimate of the numerator of this semi-elasticity. To obtain an estimate of the denominator, we use the differential in interest rates between jumbo and conforming loans that were estimated in the prior literature.

Table 10 shows our estimates for different scenarios. The change in house prices around the CLL that we estimated ranges from 30 to 91 basis points. We obtain the low of 30 basis points when we use the residuals from the hedonic regressions of value per square foot as the dependent variable and include the whole time period (1998 to 2006). [12]. The high end of the estimate (91 basis points) comes from the specification where we constrain the period 1998-2001 and use the raw value per square foot as the dependent variable. We exclude our estimates for the period 2002 to 2005 since we know that the CLL was less important during that time.

There is an extensive literature that provides estimates of the jumbo-conforming spread, see McKenzie (2002), Ambrose, LaCour-Little, and Sanders (2004), Sherlund (2008) and Kaufman (2012). The most common estimates that have been found across all the papers range from a low of 10 basis points to a high of 24 basis points.[13] If we divide our estimated range of house price changes by the range in the jumbo-conforming spread, we obtain

[12] The point estimate in the regressions is 0.65 dollars from Panel C in Table 5, and we scale that by the average value per square foot for the sample to obtain 30 basis point changes in value per square foot.

[13] The paper by Kaufman (2012) obtains an estimate of 10 basis points by using a regression discontinuity approach on the access to conforming loans around the threshold of CLL/0.8 in appraisal values. This estimate is particularly relevant for our purposes given that it explores the part of the distribution of homes that we also consider.

estimates for the elasticity of house prices to interest rates that vary between 1.2 and 9.1 (Table 10). These results are at the lower end of the elasticity that has previously been estimated in the literature (see, for example, Glaeser, Gottlieb, and Gyourko, 2010), and it is hard to justify estimates above 10 without making very aggressive assumptions about the cost differential above and below the threshold.

The prior calculation is our preferred method of obtaining an estimate of the elasticity. However, we can obtain an alternative estimate of the elasticity by considering borrowers who choose to obtain a conforming loan of less than 80 percent LTV above the threshold. This means they put up additional equity which either has to be financed through a third party loan or through savings. On average, given the range of transactions in our sample, these borrowers put up an additional USD 5,000. If we assume that the cost of the additional equity is at least 5 percentage points above the conforming mortgage rate, this is equivalent to a spread of 6-8 basis points in the total cost of financing for these borrowers relative to those who buy a house below the threshold. This then translates into an elasticity of between 4.4 and 11.4, depending on the house price effect we use from our regressions. The assumption for the spread of 5 percentage points over the conforming mortgage rate is not high if we consider that many people use a jumbo loan even very close to the threshold of the CLL, indicating that the cost of additional equity is, at least for some borrowers, very substantial. The fact that we see borrowers stick with a conforming loan and put up additional equity above the threshold may, in fact, be an indication that they are excluded from the jumbo market altogether, rather than evidence that this is a cheaper option. As Loutskina and Strahan (2009, 2011) show, jumbo loans are associated with more careful screening of borrowers, which may mean that many households simply could not use an 80 percent LTV above the threshold of 125 percent of the CLL even if they were looking to do so.

Another way of assessing the economic importance of the effect we find is by comparing the dollar amount of savings through lower interest rates and the house price differential. Assume a loan of USD 300,000, which is approximately the conforming loan limit midway through our sample (2002). If we use the upper end of the jumbo-conforming spread of 24 basis points, we calculate a cost difference of USD 720 in the first year of the life of the loan. The present value of the cost difference over 30 years is USD 8,557 assuming a 6 percent discount rate. If we use the lower end of the jumbo-conforming spread that has been estimated (10 basis points), this cost difference is USD 3,604. Our estimated effect of the conforming loan is a price difference of USD 1.16 per square foot for an average size of a house of 1,935 square feet. This translates into a USD 2,244 difference in the price of the house. Thus, the savings in the present value of interest costs of 3.6 thousand dollars leads to an increase in the value of the house of about USD 2,000.

One factor that is often raised when estimating house price elasticity is that home buyers might expect the conforming loan limit to rise in the subsequent year and would thus refinance their loan shortly after obtaining it. If refinancing were frictionless, buying a house above the threshold would then cost 10-24 basis points more than the conforming loan rate for just one year, because borrowers who took a jumbo loan would immediately refinance into a conforming loan in the following year (once the limit was raised). This would imply a very high elasticity of house prices to interest rates, as the difference in the effective interest rate over the life of the loan paid by a borrower who took a conforming loan and one who took a jumbo loan would be very small. However, this analysis misses the transaction costs of refinancing, and the estimates of these transaction costs that have been found in the literature are very large. A paper by Stanton (1995) finds that transaction costs for mortgage prepayment are around 30 to 50 percent of the remaining principal balance of a mortgage. These transaction costs include both explicit monetary costs (about one-sixth of the total costs) and non-monetary prepayment costs (the remaining five-sixths). A more recent paper by Downing, Stanton and Wallace (2005) produced a lower, but still substantial, average transaction cost of refinancing of 11.5 percent of face value. The bottom line from both these studies is clear - transaction costs are too high for the jumbo conforming spread alone to significantly change the prepayment behavior of borrowers. In other words, the benefit from obtaining lower interest rates by refinancing to a conforming loan in a year or two are too small to overcome the transaction costs of refinancing.

5 Conclusion

In this paper we use the exogenous changes in the annual level of the conforming loan limit as an instrument for easier access to finance and lower cost of credit. We find that a home that becomes eligible for easier access to credit due to an increase in the CLL has, on average, a 1.16 dollar higher value per square foot compared to a similar quality house that is just above the threshold that allows it to be financed with a conforming loan at 80 percent loan to value. The magnitude of the difference that we find is economically important given the average value per square foot of houses that transact around the CLL of 220 dollars, which means that a 1.16 dollar increase constitutes almost a 0.45 percent increase in prices. Under our assumptions for the interest rate differential for transactions above and below the threshold, this corresponds to a semi-elasticity of prices to interest rates of less than 10.

Another way of stating our results is to say that the interest rate subsidy granted by the GSEs and, ultimately, the taxpayer, does not fully benefit the buyers of homes and, instead, partially accrues to the sellers of homes in the form of higher house prices. Also, the results suggest that mortgages are being supplied in a competitive fashion, and that originating banks are not appropriating the mortgage subsidy provided by the GSEs. In addition, we see that the CLL constitutes a first order factor in how houses are financed: there is a significant fraction of borrowers who choose an LTV below 80 percent, between 77 and 79.5 percent, in order to stay below the conforming loan limit. These borrowers either were unable to get a jumbo loan, or are trying to take advantage of the lower interest rate of a conforming loan. But, as a result, many borrowers end up holding a larger fraction of equity in their house than most other borrowers.

In line with our expectations, these results are stronger in the earlier part of our sample when borrowers were less likely to have access to other forms of financing, such as second liens, and when the interest rate differential between jumbo loans and conforming loans was larger. After 2004 in particular, we see that the vast majority of borrowers even above the threshold of 125 percent of the CLL choose an LTV of 80 percent, which supports the idea that access to jumbo loans and other forms of financing became much easier in the second half of the sample. At the same time, the house price impact of the conforming loan limit is also smaller in this time period. This suggests that those houses which were previously just out of reach of being financed by a conforming loan at 80 percent could now be bid up in price since people had easier access to jumbo loans and other forms of finance. The results are also stronger in ZIP codes with the lowest income growth, usually negative, and also in areas with lower elasticity of housing supply. While we can only estimate a local treatment effect around the CLL, this presents a first test of the exogenous effect of cheaper mortgage loans on house prices. We estimate an elasticity of house prices to interest rates that is below 10, implying that the drop in mortgage rates cannot account for the increase in house prices between 2000 and 2006. However, we do show that those credit conditions matter for the formation of prices. Our results do not support a view that credit market conditions purely respond to housing demand, but point instead to a directional effect that easier credit supply leads to an increase in house prices.

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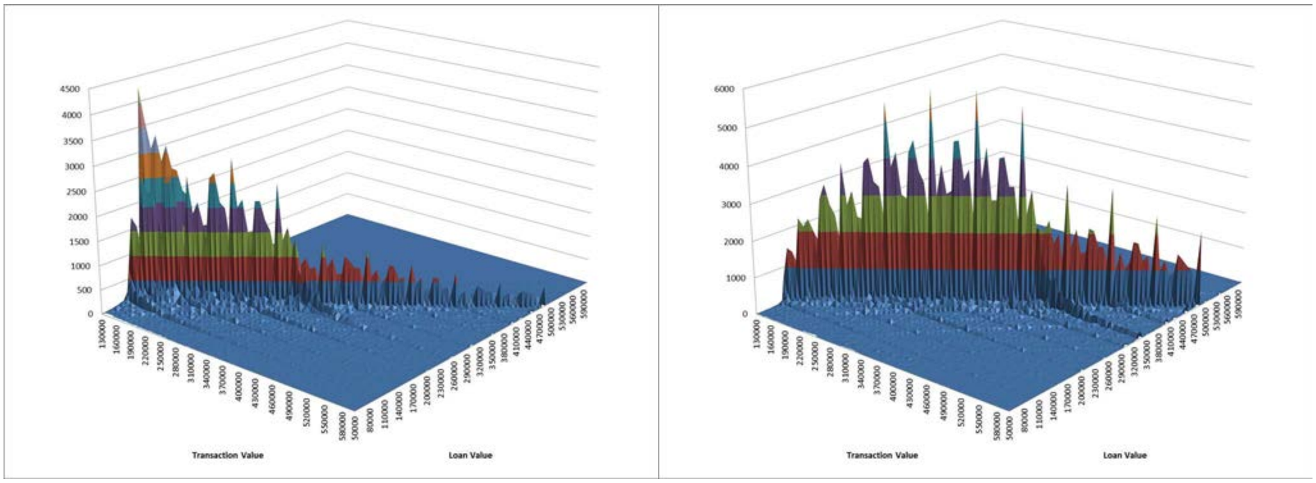
Figure 1: Transaction-Loan Value Surface

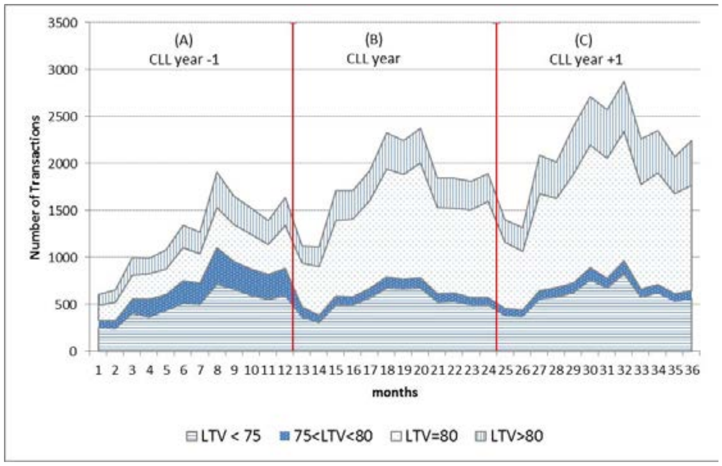
(a) 2000 (b) 2004



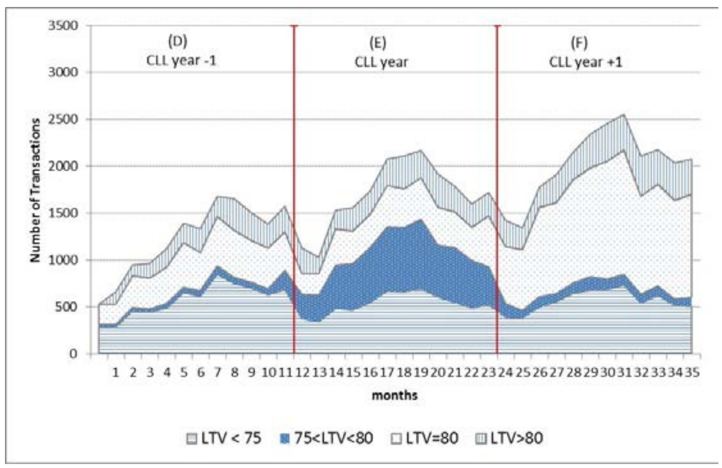
Note: This figure shows the frequency of transactions at each house price-loan value combination for the year 2000 and 2004, and the 10 MSAs covered in our data, where both house prices and loan values were binned at USD 10,000 intervals. The mass of transactions on the diagonal have a loan to value of approximately 0.8.

Figure 2: Borrower Composition for the Regression Sample

(a) Transactions below 125 percent of CLL

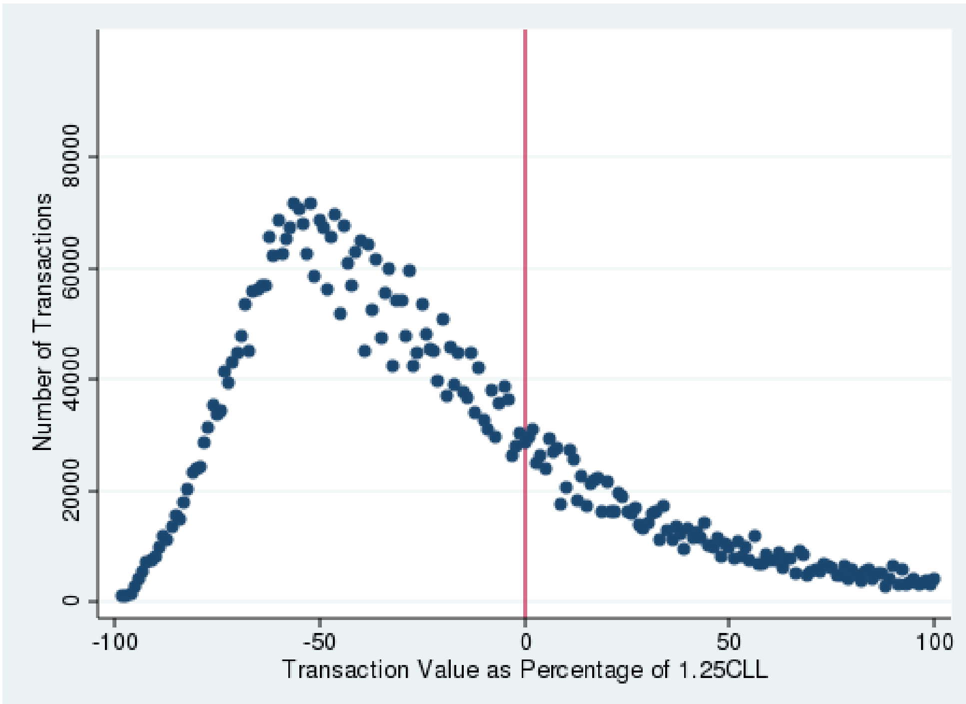


(b) Transactions above 125 percent of CLL



Note: This figure shows the number of transactions by month for transactions within USD 10,000 of the threshold of 125 percent of CLL. Transactions below and above this threshold are tracked from the year prior to the CLL being in effect to the year after the CLL is lifted to its new value. We break down transactions by LTV range to show the differences that emerge between houses above and below 125 percent of the CLL.

Figure 3: Frequency of Transactions as Percentage of CLL Threshold



Note: This figure shows the frequency of transactions by their distance to the threshold of 125 percent of the conforming loan limit. The vertical red line is the threshold and the transactions for all years are centered around that value. The x-axis is represented as one minus the transaction value as a percentage of each year's threshold of 125 percent of the conforming loan limit (e.g. if the threshold is 200,000, a transaction of 150,000 will appear as -25 percent).

Table 1: Summary Statistics

Panel A. House Characteristics.

	Whole Sample N=3,983,575			Restricted Sample N=262,671		
	Mean	*Std. Dev.*	*Median*	*Mean*	*Std. Dev.*	*Median*
Transaction Value (USD 1000)	308.52	123.93	286.00	371.34	54.92	380.00
Loan to value	0.81	0.15	0.80	0.76	0.13	0.80
House Size (sqft)	1,735	672	1,592	1,935	701	1,816
Lot Size (sqft)	10,197	15,495	6,700	11,734	17,923	7,203
Number of rooms	6.84	1.60	7.00	7.23	1.61	7.00
Number of bedrooms	3.20	0.78	3.00	3.33	0.78	3.00
Number of bathrooms	1.93	1.03	2.00	2.11	1.07	2.00
House age (years)	35.40	27.70	34.00	34.74	27.40	34.00

Panel B. House Valuation.

	Whole Sample N=3,983,575			Restricted Sample N=262,671		
	Mean	*Std. Dev.*	*Median*	*Mean*	*Std. Dev.*	*Median*
Value per sqft (USD/sqft)	193.59	91.60	172.03	219.63	93.37	200.20
Value per sqft residual (USD/sqft	0.00	42.30	-0.95	5.29	44.26	3.43
Log of transaction value residual (USD)	0.00	0.17	0.01	0.05	0.14	0.04

Note: Panel A shows the descriptive statistics for all transactions in our data from 1998 to 2008. The data was extracted from deeds records by Dataquick. Panel B shows the different valuation measures we use in the regression analysis. Value per sqft is the transaction amount divided by the size of the house measured in square feet. Both the residual measures are obtained from hedonic regressions run by year and by metropolitan area of value per sqft and transaction value on a set of detailed house characteristics. We give more information on the construction of the residuals in Section 2, Data and Methodology.

Table 2: Summary Statistics by Geography and Year

Panel A. Geographic Distribution

MSA		Transaction Value		Value per sqft		Loan to Value	
	N Obs	*Mean*	*Std. Dev*	*Mean*	*Std. Dev*	*Mean*	*Std. Dev*
Boston	279,261	320.29	112.40	197.67	73.81	0.78	0.16
Chicago	377,031	262.41	108.15	174.37	68.63	0.81	0.15
DC	396,211	329.95	126.16	186.97	85.93	0.82	0.14
Denver	397,293	250.22	94.93	155.84	49.28	0.83	0.15
Las Vegas	345,219	262.24	102.87	136.62	45.38	0.82	0.14
Los Angeles	725,897	332.28	129.71	231.29	108.35	0.81	0.13
Miami	483,541	270.10	111.74	144.80	57.04	0.81	0.14
New York	487,104	341.00	121.13	221.25	92.55	0.78	0.17
San Diego	219,489	353.14	124.63	222.18	94.86	0.79	0.14
San Francisco	272,529	383.59	123.74	266.47	109.26	0.79	0.13
Total	3,983,575	308.52	123.93	193.59	91.60	0.81	0.15

Panel B. Distribution By Year and Thresholds

Year		Thresholds		Transaction Value		Value per sqft		Loan to Value	
	N Obs	*House Price*	*Conf. Loan*	*Mean*	*Std. Dev*	*Mean*	*Std. Dev*	*Mean*	*Std. Dev*
1998	134,200	283,938	227,150	239.78	102.07	133.84	50.59	0.81	0.15
1999	350,827	300,000	240,000	246.38	104.88	139.33	54.03	0.81	0.15
2000	354,071	315,875	252,700	257.67	109.21	149.65	61.64	0.81	0.16
2001	365,814	343,750	275,000	265.16	108.82	156.74	63.81	0.82	0.15
2002	397,527	375,875	300,700	283.79	114.34	171.06	71.85	0.81	0.15
2003	423,939	403,375	322,700	303.37	118.32	187.40	80.05	0.81	0.15
2004	525,407	417,125	333,700	331.81	121.20	212.65	90.51	0.79	0.14
2005	475,723	449,563	359,650	357.51	121.71	237.24	100.72	0.78	0.13
2006	376,182	521,250	417,000	366.27	121.89	247.02	105.50	0.79	0.13
2007	293,329	521,250	417,000	359.24	122.53	237.79	101.57	0.82	0.14
2008	286,556	521,250	417,000	325.11	119.84	206.92	91.62	0.84	0.15
Total	3,983,575			308.52	123.93	193.59	91.60	0.81	0.15

Note: This table uses all the deed registry data on house transactions for 10 MSAs. Panel A shows the mean and standard deviation by city of (i) house price, (ii) value per sqft and (iii) loan to value. Panel B the mean and standard deviation by year for the same three variables.

Table 3: Verification of the Impact of the CLL on Financing Choices

Panel A: Loan to Value

	All years	*1998-2001*	*2002-2005*
Above Threshold	-0.004***	-0.006***	-0.002***
	(0.001)	(0.002)	(0.001)
Year CLL	-0.008***	-0.005**	-0.011***
	(0.002)	(0.002)	(0.001)
Above Threshold x	-0.004***	-0.004*	-0.003*
Year CLL	(0.001)	(0.002)	(0.002)
No. Obs.	242,753	100,870	141,883

Panel B: Log Loan Amount

	All years	*1998-2001*	*2002-2005*
Above Threshold	0.0227***	0.0240***	0.021***
	(0.0016)	(0.0030)	(0.001)
Year CLL	-0.0129***	-0.0087***	-0.017***
	(0.0024)	(0.0027)	(0.003)
Above Threshold x	-0.0060**	-0.0069*	-0.005
Year CLL	(0.0024)	(0.0038)	(0.003)
No. Obs.	242,753	100,870	141,883

Note: This table shows Fama MacBeth coefficients computed from year by year regressions that use two measures of financing choice as the dependent variable in each of the two panels. The sample includes all transactions within USD 10,000 of each year's conforming loan limit, as well as transactions of the same amount in the subsequent year. Above the Threshold refers to transactions up to USD 10,000 above the conforming loan limit divided by 0.8 (i.e. the transactions that were "ineligible" to be bought with a conforming loan at a full 80 percent LTV) and Year CLL is the year in which the conforming loan limit is in effect.

Table 4: Impact of CLL on Number of Transactions

	All years	*1998-2001*	*2002-2005*
Year CLL	-0.003***	0.000	-0.006***
	(0.000)	(0.001)	(0.001)
No. Obs.	262,671	109,496	153,175

Note: This table shows Fama MacBeth coefficients computed from year by year regressions that use a dummy variable for whether a transaction happens above the threshold of 125 percent of the CLL as the dependent variable. The sample includes all transactions within USD 10,000 of each year's conforming loan limit, as well as transactions of the same amount in the subsequent year. Year CLL is the year in which the conforming loan limit is in effect. Zip Codes fixed effects are included on each regression

Table 5: Effect of the CLL on House Valuation Measures

Panel A: Value Per Square Foot

	All years	*1998-2001*	*2002-2005*
Above Threshold	1.261**	1.669***	0.852
	(0.494)	(0.573)	(0.836)
Year CLL	-22.869***	-14.851***	-30.886***
	(4.047)	(2.314)	(5.314)
Above Threshold x	-1.162***	-1.553***	-0.771**
Year CLL	(0.264)	(0.297)	(0.369)
No. Obs.	262,671	109,496	153,175

Panel B: Log of Transaction Value Residual from Hedonic Regressions

	All years	*1998-2001*	*2002-2005*
Above Threshold	0.0129**	0.0154***	0.0104
	(0.0013)	(0.0015)	(0.0009)
Year CLL	0.0387***	0.0356***	0.0417***
	(0.0041)	(0.0047)	(0.0072)
Above Threshold x	-0.0017***	-0.0020***	-0.0013**
Year CLL	(0.0008)	(0.0015)	(0.0004)
No. Obs.	251,431	103,535	147,896

Panel C: Value Per Square Foot Residual from Hedonic Regressions

	All years	*1998-2001*	*2002-2005*
Above Threshold	1.733***	2.060***	1.407**
	(0.360)	(0.425)	(0.595)
Year CLL	4.103***	3.935***	4.270***
	(0.644)	(0.495)	(1.293)
Above Threshold x	-0.651***	-0.940***	-0.362
Year CLL	(0.238)	(0.351)	(0.291)
No. Obs.	251,764	103,709	148,055

Note: This table shows Fama MacBeth coefficients computed from year by year regressions that use three alternative measures of valuation as the dependent variable in each of the three panels. The hedonic regressions that produce the residuals for panels B and C are described in Section 3.2. The sample for each year's regression includes all transactions within +/- USD 10,000 of that year's conforming loan limit, as well as transactions in the same band in the subsequent year. All year by year regressions include ZIP code fixed effects. Above the Threshold refers to transactions up to USD 10,000 above the conforming loan limit divided by 0.8 (i.e. the transactions that were "ineligible" to be bought with a conforming loan at a full 80 percent LTV) and Year CLL is the year in which the conforming loan limit is in effect.

Table 6: Effect of the CLL on House Valuation in Different Income Growth Areas

Panel A: Value Per Square Foot

	2001-2005	*2001-2005*
Above Threshold	0.731	0.601
	(0.667)	(0.638)
Year CLL	-28.869***	-29.364***
	(4.706)	(4.510)
Above Threshold x	-0.846***	-0.953***
Year CLL	(0.257)	(0.210)
Above Threshold x		-1.548**
Year CLL x Low Inc. Growth		(0.652)
No. Obs.	179,828	179,828

Panel B: Transaction Value Residual from Hedonic Regressions

	2001-2005	*2001-2005*
Above Threshold	0.0109***	0.0108***
	(0.0008)	(0.0009)
Year CLL	0.0418***	0.0439***
	(0.0056)	(0.0057)
Above Threshold x	-0.0016***	-0.0022***
Year CLL	(0.0003)	(0.0006)
Above Threshold x		-0.0018
Year CLL x Low Inc. Growth		(0.0051)
No. Obs.	173,347	173,347

Panel C: Value Per Square Foot Residual from Hedonic Regressions

	2001-2005	*2001-2005*
Above Threshold	1.396***	1.347***
	(0.453)	(0.412)
Year CLL	4.314***	4.806***
	(1.017)	(1.072)
Above Threshold x	-0.504**	-0.750***
Year CLL	(0.250)	(0.158)
Above Threshold x		-0.319
Year CLL x Low Inc. Growth		(0.651)
No. Obs.	173,550	173,550

Note: This table shows Fama MacBeth coefficients computed from year by year regressions that use three alternative measures of valuation as the dependent variable in each of the three panels. The sample for each year's regression includes all transactions within +/- USD 10,000 of that year's conforming loan limit, as well as transactions in the same band in the subsequent year. Above the Threshold refers to transactions up to USD 10,000 above the conforming loan limit divided by 0.8 (i.e. the transactions that were "ineligible" to be bought with a conforming loan at a full 80 percent LTV) and Year CLL is the year in which the conforming loan limit is in effect. This specification interacts the diff-in-diff specification with a dummy variable that uses changes in income at a zipcode level as proxy for good and bad times. Specifically, the dummy is 1 if the changes in the average zipcode income are below the 10th percentile of each particular diff-in-diff regression and 0 otherwise. We use tax income data at zipcode level available from 2000-2006, which restricted our sample to 2001-2005

Table 7: Placebo Test for Coefficient of Interest

	All Transactions			*0.5<LTV≤0.8 Transactions*		
	Value Per Square Foot	*Log of Transaction Value Residual*	*Value Per Square Foot Residual*	*Value Per Square Foot*	*Log of Transaction Value Residual*	*Value Per Square Foot Residual*
CLL(0)	-1.162	-0.002	-0.651	-1.257	-0.002	-0.931
Average 20 coefficients	0.045	0.001	0.222	-0.107	0.000	0.110
St. Dev.	0.467	0.002	0.494	-0.107	0.002	0.440
T-Statistic	2.586	1.206	1.770	2.626	1.009	2.365
CLL Rank	1	4	2	1	3	1
CLL Rank below only	1	2	1	1	1	1

Note: This table shows the average and standard deviation of a series of 20 placebo tests we perform by shifting the conforming loan limit in USD 10,000 intervals from CLL-100,000 until CLL+100,000 (i.e. the limits of all years are first changed by -100,000, then by -90,000, etc.). We use these placebo loan limits to run year-by-year regressions and form Fama-MacBeth coefficients like those in Table 5 for each set of "false" loan limits. The t-statistic is for the difference between the coefficients when we use the true conforming loan limit and the average of all the other coefficients, using the standard deviation given by the 20 trials. The three dependent variables are the same we use in Table 5. The coefficient of interest is on the interaction between our "above threshold" variable and the year in which the conforming loan limit is in effect. As in the previous tables, the sample for each year's regression includes transactions within +/- USD 10,000 of that year's CLL, as well as transactions in the same band in the subsequent year. The first three columns include all such transactions, whereas in the last three columns the sample is constrained to transactions with an LTV between 0.5 and 0.8. All year by year regressions include ZIP code fixed effects. The last two rows show the ranking of the coefficient when we use the true CLL, first for all 21 coefficients and then when we only consider the placebo tests below the true CLL.

Table 8: Effect of the CLL on the Valuation of Different Groups of Transactions

Panel A: Value Per Square Foot

	Keeping Conforming			Keeping Jumbo		
	All years	*1998-2001*	*2002-2005*	*All years*	*1998-2001*	*2002-2005*
Above Threshold	0.939**	1.580***	0.297	0.868*	1.530***	0.207
	(0.472)	(0.568)	(0.666)	(0.481)	(0.545)	(0.701)
Year CLL	-24.539***	-15.953***	-33.126***	-24.874***	-16.040***	-33.708***
	(4.351)	(2.564)	(5.712)	(4.454)	(2.596)	(5.813)
Above Threshold x	-0.967**	-1.314**	-0.621	-2.177***	-2.618**	-1.736**
Year CLL	(0.416)	(0.572)	(0.634)	(0.639)	(1.119)	(0.724)
No. Obs.	177,227	72,048	105,179	160,342	62,905	97,437

Panel B: Log of Transaction Value Residual from Hedonic Regressions

	Keeping Conforming			Keeping Jumbo		
	All years	*1998-2001*	*2002-2005*	*All years*	*1998-2001*	*2002-2005*
Above Threshold	0.0117***	0.0145***	0.0090***	0.0119***	0.0146***	0.0091***
	(0.0014)	(0.0018)	(0.0007)	(0.0013)	(0.0016)	(0.0008)
Year CLL	0.0367***	0.0335***	0.0398***	0.0370***	0.0337***	0.0402***
	(0.0038)	(0.0041)	(0.0067)	(0.0039)	(0.0042)	(0.0068)
Above Threshold x	-0.0027**	-0.0019	-0.0034***	0.0004	-0.0020	0.0028**
Year CLL	(0.0011)	(0.0022)	(0.0009)	(0.0015)	(0.0025)	(0.0012)
No. Obs.	170,808	68,719	102,089	154,848	60,114	94,734

Panel C: Value Per Square Foot Residual from Hedonic Regressions

	Keeping Conforming			Keeping Jumbo		
	All years	*1998-2001*	*2002-2005*	*All years*	*1998-2001*	*2002-2005*
Above Threshold	1.573***	1.947***	1.199***	1.583***	1.991***	1.175**
	(0.290)	(0.357)	(0.414)	(0.308)	(0.333)	(0.470)
Year CLL	3.514***	3.485***	3.543***	3.529***	3.552***	3.507***
	(0.579)	(0.431)	(1.175)	(0.573)	(0.409)	(1.168)
Above Threshold x	-1.399***	-1.216**	-1.583***	0.225	-0.462	0.911**
Year CLL	(0.344)	(0.535)	(0.493)	(0.418)	(0.536)	(0.464)
No. Obs.	170,946	68,790	102,156	154,949	60,165	94,784

Note: This table shows Fama Macbeth coefficients computed from year by year regressions that use three alternative measures of valuation as the dependent variable in each of the three panels. The hedonic regressions that produce the residuals for panels B and C are described in Section 3.2. The sample for each year's regression includes transactions within +/- USD 10,000 of that year's conforming loan limit. All year by year regressions include ZIP code fixed effects. We divide the transactions that happen at a price above 125 percent of a year's CLL in the year that the limit is in effect into two groups: those with a conforming loan and those with a jumbo loan. We then run the same regressions including just one of these two groups at a time. The first three columns include the transactions with a conforming loan and the last three columns include transactions with a jumbo loan. Above the Threshold refers to transactions up to USD 10,000 above the conforming loan limit divided by 0.8 (i.e. the transactions that were "ineligible" to be bought with a conforming loan at a full 80 percent LTV) and Year CLL is the year in which the conforming loan limit is in effect.

Table 9: Effect of the CLL on House Valuation in Low Supply Elasticity Areas (Elasticity$\leq$1)

Panel A: Value Per Square Foot

	All	*All*	*1998-2001*	*1998-2001*	*2002-2005*	*2002-2005*
Above Threshold	1.261**	1.221	1.669***	3.069***	0.852	-0.628
	(0.494)	(0.799)	(0.573)	(0.374)	(0.836)	(0.749)
Year CLL	-22.869***	-15.282***	-14.851***	-8.015***	-30.886***	-22.550***
	(4.047)	(3.920)	(2.314)	(0.843)	(5.314)	(5.981)
Above Threshold x	-1.162***	-0.430	-1.553***	-2.100**	-0.771**	1.239
Year CLL	(0.264)	(0.831)	(0.297)	(0.817)	(0.369)	(0.832)
Above Threshold x		-0.870		0.726		-2.466**
Year CLL x Low Elasticity		(0.977)		(1.332)		(0.992)
No. Obs.	262,671	262,671	109,496	109,496	153,175	153,175

Panel B: Log of Transaction Value Residual from Hedonic Regressions

	All	*All*	*1998-2001*	*1998-2001*	*2002-2005*	*2002-2005*
Above Threshold	0.0129***	0.0106***	0.0154***	0.0182***	0.0104***	0.0030*
	(0.0013)	(0.0030)	(0.0015)	(0.0012)	(0.0009)	(0.0016)
Year CLL	0.0387***	0.0263***	0.0356***	0.0306***	0.0417***	0.0219***
	(0.0041)	(0.0037)	(0.0047)	(0.0044)	(0.0072)	(0.0055)
Above Threshold x	-0.0017**	0.0008	-0.0020	-0.0018	-0.0013***	0.0033*
Year CLL	(0.0008)	(0.0022)	(0.0015)	(0.0037)	(0.0004)	(0.0018)
Above Threshold x		-0.0032		-0.0002		-0.0063***
Year CLL x Low Elasticity		(0.002)		(0.004)		(0.0020)
No. Obs.	251,431	251,431	103,535	103,535	147,896	147,896

Panel C: Value Per Square Foot Residual from Hedonic Regressions

	All	*All*	*1998-2001*	*1998-2001*	*2002-2005*	*2002-2005*
Above Threshold	1.733***	1.338**	2.060***	2.623***	1.407**	0.054
	(0.360)	(0.524)	(0.425)	(0.278)	(0.595)	(0.319)
Year CLL	4.103***	1.811**	3.935***	3.316***	4.270***	0.305
	(0.644)	(0.716)	(0.495)	(0.270)	(1.293)	(0.898)
Above Threshold x	-0.651***	-0.503	-0.940***	-1.620***	-0.362	0.615
Year CLL	(0.238)	(0.546)	(0.351)	(0.306)	(0.291)	(0.684)
Above Threshold x		-0.241		0.843		-1.325
Year CLL x Low Elasticity		(0.740)		(0.744)		(1.104)
No. Obs.	251,764	251,764	103,709	103,709	148,055	148,055

Note: In this case the dummy is 1 for low elasticity places. For this specification that corresponde to the lowest MSA (Miami, San Francisco, San Diego, Los Angeles, New York, Chicago and Boston). The areas with elasticity higher than 1 are Las Vegas, Denver and DC

Table 10: Elasticity Estimates

	Jumbo-Conforming Spread	
Δ House Prices in bp	*Min (10 bp)*	*Max (24 bp)*
Max: 91.2	9.1	3.8
Min: 29.7	3.0	1.2

Note: This table shows elasticity calculations for different scenarios of both the house price increase estimated in the regressions and the interest rate differential implied for transactions above and below the threshold of 125 percent of the conforming loan limit. We use the jumbo-conforming spread in interest rates as the denominator in the elasticity calculation.