Credit Supply and House Prices: Evidence from Conforming Loan Limits

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Abstract

The basic challenge in understanding the impact of credit on house prices is to separate supply and demand effects. We construct an instrument for mortgage credit supply by using county level changes in the conforming loan limit set by agencies. We estimate that an exogenous 1% increase in credit supply leads to a 0.38% increase in house prices. By focusing on transactions close to the border of differentially treated counties and since the conforming loan limits are only sporadically revised, we can plausibly rule out demand based explanations for our results.

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1 Introduction

Consider two assets delivering identical cash flows. Basic finance theory predicts they should have the same price. Nevertheless, if borrowing is constrained and the assets differ in the way they can be financed, this basic result might fail to hold. In the setup of Geanakoplos (2010), for instance, the way an asset can be financed is important for its price if it is hard to sell short and people have heterogeneous beliefs on its value. Both of those conditions are likely satisfied in the housing market. Several studies attempt to link the large increase in house prices from the 1990s to 2006 with the corresponding increase in the amount of credit available to home buyers (Favara and Imbs (2015), Favilukis et al. (2017), Mian and Sufi (2009)). Whether or not credit availability can explain the recent housing boom, evaluating the extent to which an exogenous change in financing conditions affects house prices is important. For instance, it helps understanding the consequences of housing finance reform. Measuring such an effect can also inform models of the housing market: our results imply that financing frictions are central in this context.

Concretely, suppose two properties are in the same location, have the same characteristics, and are sold at the same time. For some reason that has nothing to do with the conditions of the market or the properties, potential buyers can finance the acquisition of one of the two properties on more favorable terms.[1] Does this imply the two properties will sell for different prices? Standard no-arbitrage reasoning suggests they shouldn't. Arbitrage becomes less compelling when we consider houses that might not be exactly the same or are in slightly different places.

Of course, exogenous changes in financing conditions are hard to come by. In this paper, we exploit an institutional feature of the mortgage market to construct plausibly exogenous shocks to the interest rates and availability of credit. Federal housing agencies such as Fannie Mae and Freddie Mac (GSEs) guarantee a large majority of mortgages originated in the United States. They impose a limit on the size of the loans they are

[1] Either they can get a lower rate mortgage, or can borrow an higher amount. We will discuss the differences of those two possibilities in detail below

willing to guarantee, the Conforming Loan Limit (CLL). Those limits were historically set at the national level but, since 2008, the limits have varied by county. Since loans purchased by GSEs carry an implicit government guarantee, conforming mortgages tend to have lower interest rates. This is because originators can offload the default risk from their balance sheet by selling them to GSEs, whi ch historically underpriced the guarantee they provide (Passmore (2005)). Even though estimating the interest rate discount for conforming loans precisely is challenging as borrowers sort into conforming and jumbo[2] loans, the effect on interest rates is clearly visible in the data as shown, for instance, by DeFusco and Paciorek (2017). If a house can be financed through a lower interest mortgage its price might increase, as in traditional user cost models such as Poterba (1984). Moreover, if borrowers or lenders prefer conforming loans, demand for houses that can be financed with conforming loans after a CLL change will increase.

We obtain detailed information on home sales from the Zillow Transaction and Assessment Dataset (ZTRAX). The data merges various administrative sources to provide information on all recorded housing transactions in the United States. We focus on transactions that are close to the border of a pair of counties that had the same CLL up until at least 2007 but subsequently have different ones. In any given year after 2007, there are adjacent county pairs with different CLLs. This effectively generates a spatial discontinuity in the amount of credit that can be guaranteed by agencies. Houses on either side of the border are plausibly similar and the local economic environments are affected by the same factors. Even though we focus on a narrow area around the border and only on county pairs that experienced this sort of CLL changes, given the richness of the data, we are left with 1.2 million transactions, which allow us to estimate an exhaustive specification. In particular, our identification relies on the assumption that, in the absence of the policy change, the average house price, controlling for hedonic characteristics, in the narrow band either side of a shared county border would experience the same percentage appreciation. We discuss in detail the conditions under which this assumptions would be

[2]Jumbo mortgages are those with principal larger than the applicable CLL.

violated in Section 3.3.

We verify that houses transacting on either side of the border are similar on observables and identify the effect of funding on prices using an instrumental variable approach. The instrument we use for the amount borrowed on a given house is the level of the CLL. Once fixed effects are partialed out, identification is sourced from the change in the CLL in its own county compared to the adjacent county. While the CLL is clearly affected by the previous year's price appreciation in a county, it is not related to future price growth directly, given it is set mechanically given past year information. The excess appreciation in the treated county identifies the effect of increased credit availability.

Furthermore, since the policy change should only directly affect borrowing ability for houses with equilibrium prices at least above the national CLL[3] this provides a natural placebo test prediction that no such differential appreciation[4] should be observed below this point in the distribution.

In a closely related study, Adelino et al. (2012) focus on houses that sell at a price just above and just below the national CLL before 2008, in the period in which the CLL did not vary at the county level but was set nationally. They employ a difference in difference strategy around changes in national limit. There are several differences between their approach and ours. In particular, our approach tracks two largely fixed cohorts of houses (one treated, one control) over time, and compares their differential appreciation. By contrast, Adelino et al. (2012) consider a constantly changing cohort (as revision of the CLL is not necessarily equal to appreciation in markets close to affordable with conforming loans), which yields a relatively unclear interpretation. Of particular potential concern is the fact that their treatment and control groups are designated based upon observed ex-post house prices. Selection on the outcome can alter the distribution of the (unobserved) error term differentially for the treatment and control counties, and is particularly problematic if the previous sale price of the house is not controlled for, as

[3]The threshold depends on the relevant loan to value ratios, but for the purpose of this placebo test this is not important

[4]Up to spillover effects that affect differentially houses in the control and treatment counties

any unobserved characteristic of the house is then captured in the error term.

Our empirical results provide an additional piece of evidence that house prices are indeed affected by credit supply, as Adelino et al. (2012) and Favara and Imbs (2015) found. They also shed some light on the impact of government subsidies to housing finance.

The rest of the paper is structured as follows. Firstly, in Section 2, we introduce a simple model of the impact of financing on house prices to motivate the empirical exercise. Section 3 outlines the data and empirical strategy, and Section 4 discusses tests of the experimental validity of the claimed natural experiment. In Section 5 we document the effect of the changes in CLLs on amounts borrowed, essentially estimating the first stage of our specification, Section 6 describes the effect on house prices and Section 7 concludes.

2 Model

There are two main theoretical channels through which an increase in credit supply by relaxing conforming loan limits may impact house prices. First, conforming loans generally have lower interest rates than nonconforming loans, as they are eligible to be purchased by a GSE and thus are effectively subsidized via an implicit government guarantee against default. For a given stream of flow consumption benefits provided by a property, a reduction in mortgage rates increases willingness to pay. Below, we expound on this in more detail. Second, some individuals may be credit constrained by virtue of being unable to borrow in the nonconforming loan market and possessing insufficient liquid wealth to finance their desired purchase with a conforming loan. If some buyers or would-be buyers are credit constrained, relaxing this constraint will induce increased borrowing, with the potential for bidding up prices. We note this channel for completeness, but formal exploration of this second channel is not incldued here.

2.1 Model with Interest Rate Channel

We consider a model where the effect of the policy change operates through borrowing rates. Suppose there is a set of houses split between two areas, A and B. Each property is indexed by its hedonic level h, a measure of its quality. Identical agents have preferences represented by $U(h, c) = u(h) + c$ where c is a composite consumption good. Following Poterba (1984), marginal utility from housing services is equated to the user cost of housing in equilibrium:

$$r(h, x)P(h, x) - \mathbb{E}[\Delta P(h, x)] = u(h) \tag{1}$$

where $x \in \{A, B\}$ is the region. Notice that an important departure from the standard framework is that we allow the interest rate at which next period payments are discounted, to change depending on h and the region x. Effectively we interpret $r(h, x)$ as the interest rate on the mortgage.[5] For simplicity, we assume that the expected price level change $\mathbb{E}[\Delta P(h, x)]$ is constant in x,[6] therefore

$$r(h, A)P(h, A) = r(h, B)P(h, B) \tag{2}$$

Therefore, if the borrowing rate schedule in region B is altered so that $r(h, B) < r(h, A)$ for $h > \underline{h}$, then $P(h, B) > P(h, A)$ for $h > \underline{h}$. The change in $r(h, B)$ captures the effect of an increase in the CLL since even loans that are larger than the jumbo CLL will carry a lower average interest rate due to the policy change as a larger amount can be financed with a conforming first lien.

[5] This is not the standard interpretation in user cost models, but here it is a parsimonious way to capture the effect of interest.

[6] This assumption is satisfied in the unique static equilibrium, where there is zero expected price growth.

3 Institutional Setting and Data

3.1 The Conforming Loan Limit

A substantial fraction of US residential mortgages are guaranteed by the Federal National Mortgage Association (also known as Fannie Mae) and the Federal Home Loan Mortgage Corporation (Freddie Mac). Those Government Sponsored Entities (GSEs) buy residential mortgages, guarantee them against default, and securitize them for sale on the secondary market. GSEs play a prominent role in financing house purchases, GSEs only guarantee *conforming* loans: mortgages that conform to GSE guidelines. Importantly, loan amounts for those mortgages need to be below the Conforming Loan Limit (CLL). Loans above those limits are often referred to as *jumbo* mortgages.

An extensive literature documents a significant difference in interest rates between conforming and jumbo loans (Ambrose et al. (2004),Kaufman (2014)). The estimates

The CLL was set nationally each year until 2008. In 2008, the Economic Stimulus Act (ESA) and the Housing and Economic Recovery Act of 2008 (HERA) introduced different rules for high-cost areas. Since our identification strategy exploits geographical variation of the CLL, we explain in some detail these regulatory changes[7]. ESA only applied to loans originated in 2008 and it set loan limits for high-cost counties to be equal to the minimum of 125 percent of house price medians in the county and 175 percent of the national conforming limit. HERA is the more important legislation, since it determined the limits from 2009 until today. Under HERA, the high-cost county loan limits are the minimum of 115 percent of county price medians and 150 percent of the national limit. For instance, if the median house price in a county were $\$600,000$ in 2008, the CLL for that county in 2009 would have been the lesser of $1.15 \cdot \$600,000$ and 1.5 times the national 2009 CLL. For our identification strategy, it is important that the CLL in a given county is the result of this mechanical formula based on past information: if it were set with more discretion it could plausibly incorporate information about future demand.

[7] A more thorough description can be found on the Federal Housing Finance Administration's website https://www.fhfa.gov/DataTools/Downloads/pages/conforming-loan-limits.aspx

3.2 Data

The data for this paper come mostly from the Zillow Transaction and Assessment Dataset (ZTRAX)[8]. This data include detailed information on transactions and characteristic of houses in the United States, aggregated by Zillow from administrative records. The data comes from two main sources: transactions records filed with county offices from which we obtain information on prices, financing, buyer identity and location, as well as tax assessments, which include hedonic characteristics. Since we want to implement a regression discontinuity around country borders, we collect information on county borders from the US Census Bureau[9]. We also collect CLL data by county and year from Fannie Mae.

We restrict our attention to house within 6 kilometers of a county border. All county pairs which have different CLL history are kept; this involves dropping some county pairs with jumbo CLLs as they have identical histories to their neighbor. In order to restrict to arms-length consumer-to-consumer transactions, we impose restrictions similar to those implemented in the construction of the Case-Shiller index. In particular we remove foreclosures, sales to and by banks and other companies, intrafamily sales and apparent outliers, which could arise from recording mistakes, for instance.

Since, as described below, our methodology relies on accurately predicting house sale prices given previous information we restrict attention to sales within the 2000-2017 window of properties that had a previous sale from 1993 onwards. This allows us to include previous sale price in our forecasting regressions, which improve the prediction precision substantially.

[8]Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author(s) and do not reflect the position of Zillow Group.

[9]https://www.census.gov/geo/maps-data/data/cbf/cbf_counties.html

3.3 Natural Experiment Identification

By explicit construction, the introduction of jumbo conforming loans affected some counties but not others, with conforming loan limits set separately for each county according to a common formula. A simple, naive estimate of the policy change would be to compare the differential in average house price appreciation between affected and unaffected counties following the policy change. However, this would be undesirable for at least two reasons. First, since local economic shocks can have substantial spatially-correlated effects on house prices; such an estimate would conflate the true effect of the policy change with any such shocks. Second, the formula used to calculate jumbo conforming loan limits for a county is based upon its previous-year median property price. This would create an endogeneity problem where areas that experienced house price appreciation would also experience increased lending limits, leading to a spuriously inferred relationship.

In order to address this concern, we implement a regression discontinuity method that restricts attention to the area geographically proximate to the shared county border. The effect of the conforming loan limits on house prices should manifest discontinuously at the county border. By contrast, local economic shocks should affect house prices smoothly at the county border, and more generally for a sufficiently narrow band, should affect both counties approximately equally. Since county boundaries can also determine access to amenities provided by local governments (such as public schools), the discontinuity at the county border may not isolate the effect of credit availability. Accordingly, it is necessary to control for pre-existing differences in prices at the county border. The effect of the change in conforming loan limits is thus isolated by the change in the county border discontinuity following the policy change.

This methodology can be described as a difference in discontinuity differences estimator (i.e. a DiD of an RD), which allows for pre-existing differences between the counties. Accordingly, this estimation approach relies on the identifying assumption that house prices in the regions adjacent to the border would have exhibited parallel trends in the absence of the policy change, such that this border discontinuity would have been un-

changed. For a geographically confined and proximate region either side of a common border, parallel trends is a very plausible assumption.

For illustrative purposes, it is useful to think of the ideal thought experiment that this natural experiment approximates. Consider two identical houses situated either side of a street which forms a county boundary. Both houses bring equal flow consumption, and thus they have the same rental value (as explained above, the identification strategy is robust to pre-existing differences in flow utility associated with the county border). A policy that reduces the interest rate on loans for purchasing one of the houses reduces the user cost of possessing the property at any given price, and thus should drive a wedge between the price of the two properties.

This natural experiment identifies this wedge (specifically, the change in the wedge) in prices between the treated and untreated houses produced by the policy-induced change in credit supply. For the purpose of understanding the effect of a uniform liberalization of access to credit (i.e. where the direct effects are as per this policy, but the equilibrium adjustment via supply response is not), this is the key parameter of interest. However, the experiment does not identify the level effect on aggregate house prices actually induced by the policy (for example, in general equilibrium there can be spillover effects on the untreated region).

3.4 Empirical Specification with Pooled County Pairs

A feature of the introduction of jumbo conforming loan limits is that because the policy differentially affects many pairs of counties, the discontinuity can be estimated on more than a single border. In effect, the policy change produces a natural experiment at each of the large number of common borders of county pairs where the policy change differs. This comprises all adjacent counties where one county is treated and the other is untreated, or both counties are treated but the post-period conforming loan limit differs. While the effect of the CLL change can be separately estimated for each county pair, it is more informative and increases precision to estimate the effect of relaxing the CLL averaging

over each of the county pair discontinuity changes.

However, in pooling the samples for affected adjacent county pairs across the US and estimating an average discontinuity effect, it is necessary to carefully design the specification of controls in order to correctly identify the model. For each county pair, identification of the effect of credit supply comes from the difference between house prices as a function of the difference in CLL measures. County fixed effects capture baseline differences between the county border regions (at the border). County pair by CLL policy regime period control for common shocks at time t to house prices in both treatment and control counties, and also net out common changes in the CLL variables. The CLL policy regimes periods refer to the different historical waves of the conforming loan limits (including changes to both the national CLL and the county-based jumbo limits), revisions to which have typically but not universally been implemented annually. These controls are necessary to ensure the identification of the CLL variables only sources from within-county-pair variation, rather than capturing variation across county pairs. For example, county pairs that experience positive local economic shocks will tend to experience faster house price appreciation, and also higher conforming loan limits as CLLs endogenously respond to county-level average dwelling prices; but these changes are silent on the effect of exogenously changing the CLL and thus need to be separated out.

To isolate changes at county borders, the full specification also includes controls for the distance to the common border (linear on each side of the border for each county pair). A rich set of property hedonic characteristics,[10] the previous sale price of the property in the pre-period, and census tract block group fixed effects are added to increase precision and control for compositional changes over time.

Accordingly, the specification estimated is

$$y_{ijt} = \alpha_j + \gamma_{j,j',t} + \beta_C CLL_{jt} + \beta_D^{j,j'} d_{i,j'} + \beta_R y_{ij\tau} + \gamma_{j,j',\tau} + x'_{it}\delta + \iota_j + \varepsilon_{ijt} \tag{3}$$

[10]Those are described in detail in the Appendix.

where y_{ijt} represents some outcome (such as property sale price) for property i in county j (adjacent to county j') in period t. α_j are county fixed effects (for county j) that control for pre-period differences in outcomes at the county border, $\gamma_{j,j',t}$ are county-pair by policy regime fixed effects that net out common shocks to both counties within the pair, and $d_{i,j'}$ is the distance from property i to the border of the adjacent county j'. To properly control for the previous sale price $y_{ij\tau}$ of property i in period τ, fixed effects for county pair by year of previous sale are also added (to normalize past sale prices for different properties to a common year). Finally, x_{it} is a set of hedonic characteristics for property i at time t and ι_j are census tract block group fixed effects.

3.5 Plausibly Affected and Placebo Groups

One of the key results from the models in Section 2 is that the direct effects of the policy change manifest only on the houses in the part of the hedonic distribution where the change in lending supply is potentially binding (i.e. reduced lending rates above the CLL or relaxed credit constraints above the CLL). While houses lower in the price distribution within the treated county are still subject to spillover effects, without making specific assumptions about preferences over county-based amenities, spillovers should be approximately equal for similar houses in the treatment and control counties.

This suggests that the effects of the the conforming loan limit changes be estimated separately for bins of properties defined by their estimated sale price. First, isolating the group of houses most plausibly affected by the policy maximizes the chance of being able to cleanly and tightly identify any treatment effects. Second, houses immediately lower in the price distribution, that could already be purchased with a conforming loan under the national limit, should not be directly affected by the policy change, and thus provide a natural sample for placebo tests.

For each property i in a county pair $p = j, j'$, the estimated sale price (in log dollars)

is generated as

$$\widetilde{y}_{ipt} = y_{ip\tau} + \frac{1}{n_{pt} - 1} \sum_{k \neq i} y_{kpt} - \frac{1}{n_{p\tau} - 1} \sum_{k \neq i} y_{kp\tau} \tag{4}$$

This takes the previous sale price of the property, and adds the difference between the leave-out mean sale price of properties in the county pair in the current year and the year of previous sale. These latter two terms are a proxy (not adjusting for different composition) for the house price appreciation in the county pair since the previous sale. These forecasts are endogenous to the policy change in that if the policy affects house prices, this is incorporated into the forecast. However, since appreciation is approximated at the county pair level, this is a common additive shift that is absorbed by the county pair (or county pair by time) FE, such that this should not induce mechanical bias. For a property to be plausibly affected by the ability to borrow a jumbo conforming loan, it must have a sale price at least equal to the conforming loan limit.[11] To broadly capture effects on borrowers whom the jumbo CLLs directly impact, the *affected* group is defined as all properties with estimated prices within a range of $[1, 2.5] \times$ National CLL_t.[12] To ensure properties are not too dissimilar, the *placebo* group is defined as properties with estimated prices in the range $[0.5, 1) \times$ National CLL_t, such that their purchase is feasible with a conforming loan in all counties within the US.

Using the estimated sale price rather than the ex-post sale price sacrifices precision, but is necessary to avoid the pitfall of selecting a sample based on the value of the dependent variable. In particular, if the policy change truly affects house prices, selection based upon observed ex-post house price can alter the distribution of the (unobserved) error term differentially for the treatment and control counties. Ultimately, as this price forecast is imperfect, not all properties designated to the affected group were truly possibly constrained by the national CLL, nor were all properties in the placebo group truly

[11] The maximum loan-to-Value ratio that is permissible for a conforming loan is 97%. This is a conservative definition; as most borrowers have larger deposits, the national CLL presumably poses no constraint for properties just above this threshold.

[12] Very high priced houses are thus excluded, both because their owners are unlikely to be credit constrained or utilize conforming loans, and because their value tends to be harder to predict in hedonic models, undermining precision.

unconstrained. At worst, this should depress slightly the first-stage effects on lending in the affected group and allow there to be a small true first stage in the placebo group, but should not meaningfully effect the IV estimate for the affected group.

4 Descriptive Statistics and Parallel Trends

4.1 Balance Tests

The ZTRAX property transaction data contains a rich set of hedonic characteristics. Considering a given county pair, the identification strategy outlined in Section 3.3 does not require the distribution of observable characteristics to be identical either side of the shared county border.[13] However, there remain several reasons why treatment and control county properties having similar observable characteristics would be advantageous. First, the ceteris paribus thought experiment of cross-border arbitrage is most natural when similar properties can be acquired on either side of the county border. In econometric terms, internal validity requires correctly specifying the hedonic pricing function $P(\underset{\sim}{h})$. If hedonics are very different between treatment and control groups, then even mild misspecification of $P(\underset{\sim}{h})$ could give the illusion of an effect due to the CLL. Second, similarity in observables makes similarity in unobservables more likely, and gives the parallel trends assumption more intrinsic appeal. For example, if a county border were associated with a large jump in average sale price and property characteristics, such that the two areas catered to very different ends of the market, taste or economic shocks that differed by income or wealth could easily violate the parallel trends assumption.

Table 1 presents descriptive statistics for selected variables for treatment and control counties for the 2000-2007 pre-period. While there are differences between the samples in the pre-period, they are relatively small. Treatment county house prices are approximately 1% higher, lot land area and building floor area are slightly larger, and buildings

[13] Notably, unlike with many typical regression discontinuity settings, manipulation of the running variable is not a concern as existing housing is typically not mobile. As a result, non-smoothness in the density of the running variable or any other covariate is not indicative of manipulation.

Table 1: Pre-Period Descriptive Statistics by County *First Treated* Status

Variable	**Treatment Sample**			**Control Sample**		
	N	*Mean*	*St. Dev.*	*N*	*Mean*	*St. Dev*
Sale Price ($)	234,414	317396	237046	232,447	314783	244917
Log Lot Size (Sqr. Ft.)	198,640	9.44	1.25	187,741	9.35	1.25
Building Area (Sqr. Ft.)	234,025	2042.42	1149.08	231,317	2171.69	1213.05
Bedrooms	167,477	3.06	0.93	170,993	3.07	0.91
Bathrooms	184,941	2.13	0.78	189,759	2.11	0.76
Building Age (Years)	232,897	30.20	28.69	231,146	29.28	28.63
Single Family House	170,031	0.62	0.48	175,807	0.64	0.48
Conforming Loan Limit ($)	234,413	340550	53262	232,447	343426	52608

Restricted to pre-period sample from 2000-2007. Sample excludes observations with missing or zero value recorded for respective outcome (except zero values are retained for Single Family House indicator), as zero values typically represent missing data. For means and standard deviation calculations, observations are weighted within each county pair to give equal cumulative weight to treatment and control county. County pair weights are equal to share of observations in sample.

are approximately one year older at the time of sale. Other variables, unsurprisingly including the prevailing conforming loan limits, are very similar between treatment and control groups. Differences in variances for these variables also appear small in magnitude. Somewhat unsurprisingly given the geographic adjacency of the treatment and control samples, they appear to contain a very similar set of properties.

4.2 Parallel Trends Tests

The identifying assumption underlying the empirical approach is that, in the absence of the policy change, property prices and loan amount measures would have changed by the same amount in expectation in treatment and control counties. Of course, this counter-factual prediction can never be tested as the policy change did occur. A priori the assumption appears reasonable as treatment and control groups are geographically adjacent, and thus largely subject to the same shocks, and arbitrage is possible. Further indirect evidence is obtainable by examining a closely related question; whether treatment and control counties had parallel trends in the years leading up to the policy change. If county outcomes historically have moved in parallel, this is a reasonable assumption for

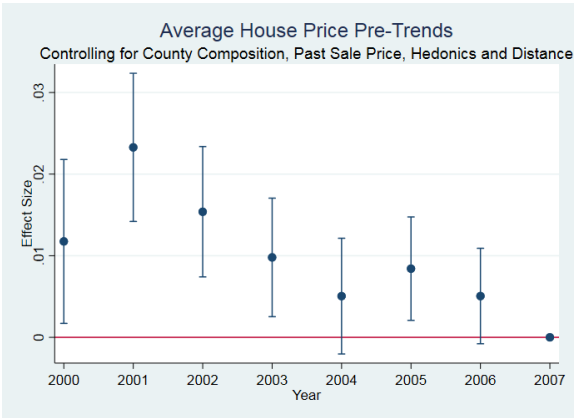
a counter-factual.

Traditional tests of the parallel trends assumption look at whether the coefficient on the treatment group is changing over time in the pre-period. This usually involves a treatment group that is fixed across time in the treatment period, and binary treatment. These conditions do not hold here; jumbo CLLs have been revised multiple times, some counties were initially treated before returning to having only the national CLL, and in a small number of county pairs, the intially treated county lost jumbo status and the initial control county became treated. To deal with this parsimoniously, the parallel trends analysis is conducted by, for each county pair in the sample, assigning a county as *first treated* if it was the first county in the pair to have its CLL be higher than the contemporaneous CLL of the other county.[14] The parallel trends analysis thus calculates coefficients for this *first treated* variable for each year in the 2000-07 pre-period.

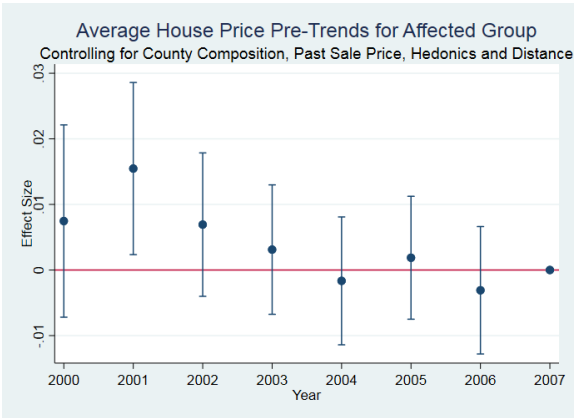
Several different sets of pre-trend analysis follow. For house prices, to analyze whether equivalent houses were diverging in price, the regression specification outlined in Equation 3 is used. This calculates the association between log house prices and first treated status, controlling for changes in county and census tract composition, past sale price and time of past sale, property hedonics, and distance from the shared county border, with standard errors clustered by census tract. Figure 1a shows the coefficients for *first treated* status by year on log house prices for the full pre-period sample, while Figure 1b restricts attention to the plausibly affected group. There is arguably some evidence of a small but noisy, negative trend in Figure 1a. Reassuringly, this pattern essentially disappears when restricting attention to the plausibly affected group, which is the more crucial sample for the analysis.

Examining pre-trends for loan amount variables provides evidence regarding the validity of estimates of the effect of the policy change on loan behavior. For loan amount variables, the assumption of parallel trends is required in order for the estimated effect of the CLL policy change on borrowing variables to be correctly identifying an exogenously

[14] In other words, the first county to have a jumbo conforming loan limit, with ties broken in favor of the county with a higher CLL.



(a) Full Sample

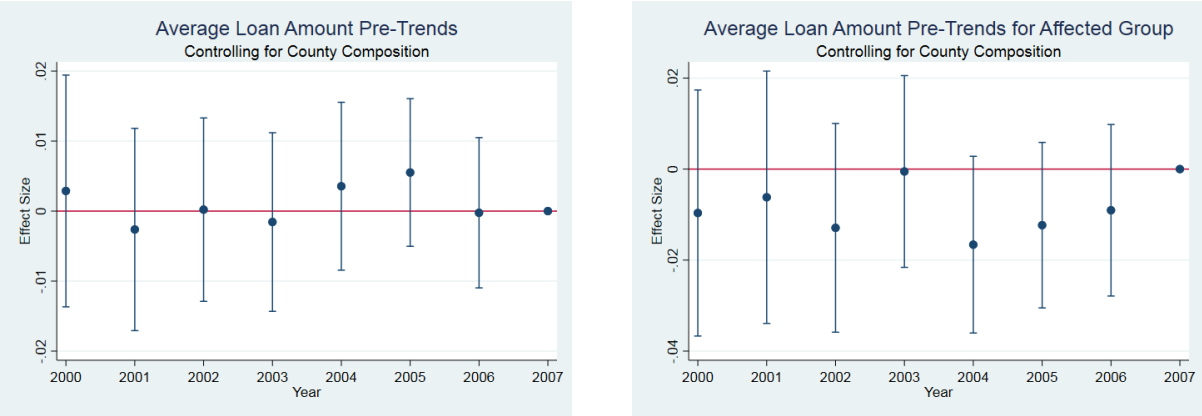


(b) Plausibly Affected Properties

Figure 1: Log House Price Pre-Trend for *First Treated* Counties

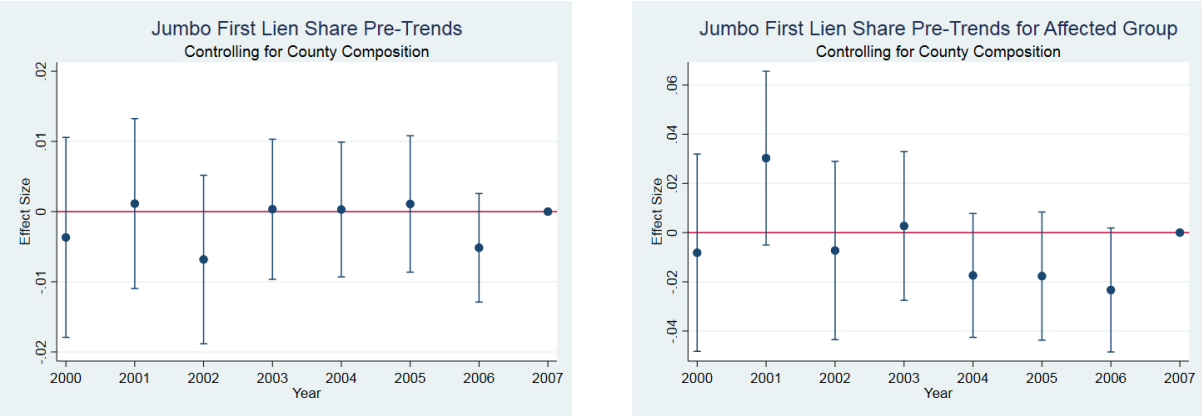
induced change in credit utilization, which is necessary for estimating the effect of credit on house prices. To identify if overall credit access and utilization in treatment and control counties were diverging prior to the natural experiment, focus is placed on trends in average loan measures for a given geography; that is controlling for county composition and census tract but not hedonics or other characteristics.

To provide clear evidence, parallel trends tests for three different loan measures are presented. Figure 2 presents the annual coefficients for being in a treatment county on the average total amount borrowed. Trends in the treatment and control counties looks similar for both the full and plausibly affected samples. Figures 3 and 4 look at the share of borrowers with first liens that are respectively in excess of and exactly at the national CLL. They thus provide sharp measures of the extent to which the national CLL provided a binding constraint on borrowing, which in turn depends on the ease of accessing credit in the private non-conforming market. For both measures, there is no meaningful evidence of differential pre-trends, with no discernable patterns for either the full sample or among plausibly affected properties. Viewed together, these results provide additional credence for the parallel trends assumption.



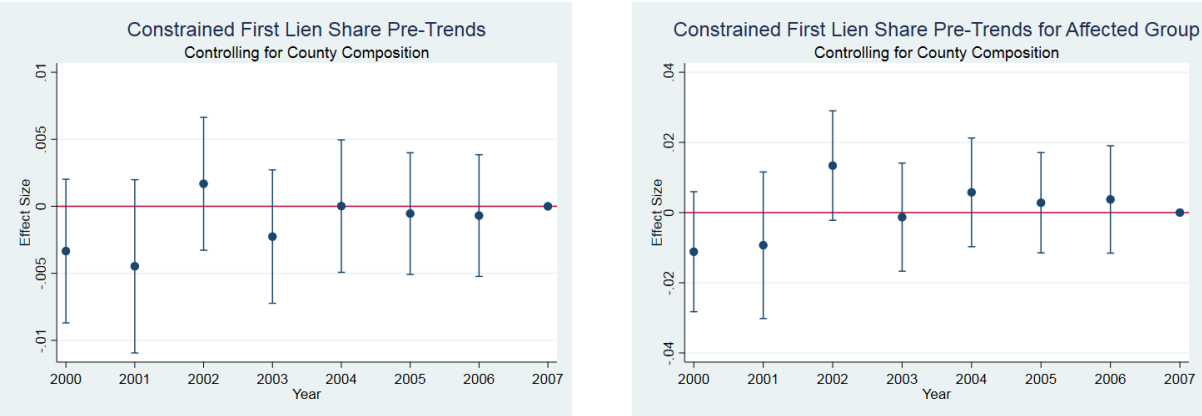
(a) Full Sample **(b)** Plausibly Affected Sample

Figure 2: Log Loan Amount Total Pre-Trend for *First Treated* Counties



(a) Full Sample **(b)** Plausibly Affected Sample

Figure 3: Jumbo Loan Share Pre-Trend for *First Treated* Counties



(a) Full Sample **(b)** Plausibly Affected Sample

Figure 4: Constrained Loan Share Pre-Trend for *First Treated* Counties

5 Loan Amount Results

5.1 Loan Amount Measures

The causal channel of interest, whether it operates by relaxing credit constraints or by reducing mortgage interest rates, should affect house prices only if it affects loan amounts. Before considering the effect on house prices, it is helpful to analyze the existence of an effect upon the amount of credit borrowed. In addition to a predicted positive effect on the average aggregate amount borrowed per property sale, the details regarding the increase in CLLs makes a series of sharp predictions about how effects upon loan amounts should manifest. By allowing borrowers to increase the amount that can be financed with a conforming loan, the policy is predicted to have a positive effect on average first lien amounts but a negative effect on second lien amounts as borrowers substitute towards conforming first liens with lower interest rates. Furthermore, to the extent that the national CLL induces bunching at the conforming loan limit, the policy change is predicted to increase the market share of jumbo loans, while reducing the proportion of loan amounts equal to the national CLL.

One complication when dealing with loan amount variables is that a considerable fraction of purchases (approximately 21% of the dataset) are made by cash buyers who do not take out any mortgage. Changes in credit utilization thus reflects both extensive margin effects from any change in the share of cash buyers and intensive margin changes in the amount borrowed by a hypothetical given individual to purchase a given house. If cash buyers are included in the analysis, then a small extensive-margin change in the share of cash buyers may dominate the analysis of overall loan activity. Further, the causal channel of interest, namely that relaxing the CLL reduces the cost to a borrower of financing a mortgage and thus increases their willingness to pay for a property, pertains exclusively to the intensive margin channel. This suggests an approach of restricting attention to the subsample with nonzero loan amounts. This, or a similar approach of adding a dummy variable for a cash buyer purchase as a control, are appropriate either if

cash buyer purchases are completely random, or if they are independent of unobservable property characteristics (i.e. independent, conditional upon observables).[15] This may fail, and induce bias in the loan amount estimates, if, for example, higher priced properties are more likely to be purchased with cash and an increased CLL reduces the share of cash buyers.[16] Subject to further testing, any selection bias seems plausibly fairly small, since either component may be weak.

An alternative that is more satisfactory given the underlying economic theory is to focus on the loan that *could* be taken out to finance a given property purchase. Just as auction mechanisms yield expected revenues equal to the expected willingness to pay of the second highest bidder, an individual's cost of financing a house, and thus their willingness to pay, depends on the price and quantity of credit they could access, irrespective of whether a loan ends up being taken out. Accordingly, a potential loan measure is constructed to capture this idea. For houses purchased with a mortgage, it is set equal to the observed loan, as this is a good proxy for the potential optimal mortgage amount. For those purchased by cash buyers, the potential loan amount is set to 80% of the property's predicted sale price. This is a natural choice as 80% is both the modal LTV ratio and close to the mean in the dataset amongst borrowers. Given this construction, the effect of the policy change on the potential loan amount can be estimated without additional issues on the full sample.

5.2 Regression Results

These hypotheses are tested by regressing the loan amount variables on measures of the CLL policy change. Three different measures of the CLL are utilised in turn. The first is the log of the conforming loan limit in county j at time t, and allows the elasticity of various loan amount variables to the conforming limit to be estimated. A second is a

[15] In the later IV results, hedonic characteristics and past sale price are included as regressors, and thus this assumption is more plausible.

[16] In other words, the properties where the extensive margin effect manifests in the treatment group are unrepresentative, causing a composition-driven change in average amounts borrowed.

dummy for a county having a CLL above the national minimum in a given period, while the third is a dummy for having a higher CLL than the neighbouring county j'. These are hereafter referred to as *high CLL* and *higher CLL* dummies. These two additional measures provide immediate estimates of the treatment effect of the policy change (on average) on loan outcomes.

To ensure the effects of the jumbo CLL policy change are identified, fixed effects for county, county pair by regime date, census tract, and month (and year) of sale are included, while standard errors are clustered by census tract to allow for local spatial correlation in unobservable shocks. However characteristics specifically pertaining to the individual property, such as past sale price, and hedonic characteristics, are not controlled for. Accordingly, these regressions identify the effect of the policy change on average loan attributes, without isolating whether any changes are due to changes in the composition of houses sold, house prices, or credit utilization for a given house and sale price. Results for the various loan outcomes for properties with estimated sale prices that make them plausibly affected by the policy change are shown in Table 2 through Table 4.

Table 2: Effect of Conforming Loan Limits on Loan Amount for Plausibly Affected Properties

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Potential Loan (Log)			Loan Amount (Log)			Cash Buyer		
Conforming Loan Limit (Log)	0.0849***			0.1096***			-0.0056		
	(0.0182)			(0.0219)			(0.0184)		
High CLL County		0.0296***			0.0382***			-0.0085	
		(0.0057)			(0.0066)			(0.0069)	
Higher CLL County			0.0238***			0.0348***			-0.0147**
			(0.0074)			(0.0068)			(0.0073)
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Pair x CLL Regime FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Census Tract FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.1882	0.1882	0.1881	0.1790	0.1790	0.1789	0.1431	0.1431	0.1431
Observations	239499	239499	239499	201556	201556	201556	239499	239499	239499

Sample consists of properties with estimated sale prices $\bar{y}_{ipt} \in [1, 2.5] * \text{National CLL}_t$. Full sample first restricted to county pairs where each county has at least 100 observations in each cell (2000-2007 pre-period, 2008-2017 post-period). Columns 4-6 are restricted to observations with nonzero total loan amounts. Standard errors in parentheses; robust standard errors clustered by Census Tract. * p<0.10, ** p<0.05, *** p<0.01.

These regression estimates provide strong evidence that borrowing behavior was affected by the introduction of jumbo conforming loans. In Table 2, Column 1 reports an estimated elasticity of potential loan amounts to the conforming loan limit of 0.085, while focusing only on the subsample that excludes cash buyers, the point estimate in Column 4 of the elasticity of loan amounts to the CLL is 0.110. The coefficients on the high CLL and higher CLL dummies in Columns 2-3 and 5-6 suggest that increased limits associated with the introduction of jumbo CLLs increased average aggregate mortgage borrowing by 2.4-3.8%. Columns 7-9 suggest that the increased availability of mortgage credit may have had a slight negative effect on the cash buyer share, although most of the point estimates are insignificant. Such an effect could either reflect potential cash buyers being outbid more frequently or individuals substituting into taking out loans.

Table 3: Effect of Conforming Loan Limits on Loan Component Amounts for Plausibly Affected Properties

	(1)	(2)	(3)	(4)	(5)	(6)
	First Lien Amount (Log)			Second Lien Amount (Log)		
Conforming Loan Limit (Log)	0.1294***			-0.9527***		
	(0.0204)			(0.2106)		
High CLL County		0.0443***			-0.3075***	
		(0.0064)			(0.0673)	
Higher CLL County			0.0426***			-0.4191***
			(0.0080)			(0.0804)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
County Pair x CLL Regime FE	Yes	Yes	Yes	Yes	Yes	Yes
Time Controls	Yes	Yes	Yes	Yes	Yes	Yes
Census Tract FE	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.1660	0.1659	0.1657	0.2233	0.2232	0.2232
Observations	201556	201556	201556	201556	201556	201556

Sample consists of properties with estimated sale prices $\bar{y}_{ipt} \in [1, 2.5] *$ National CLL_t. Full sample first restricted to county pairs where each county has at least 100 observations in each cell (2000-2007 pre-period, 2008-2017 post-period). All regressions are restricted to observations with nonzero total loan amounts. Standard errors in parentheses; robust standard errors clustered by Census Tract. * p<0.10, ** p<0.05, *** p<0.01.

Table 3 considers how the composition of credit borrowed responded to the increased conforming limits. Since only first liens can be purchased by GSEs such as FNMA and FHLMC, and thus receive an implicit interest rate subsidy, the natural hypothesis is that higher CLLs would induce substitution away from second liens and into larger first liens. This is confirmed by the regression results, focusing on purchases with nonzero loan amounts. The results in Column 1 reports that the estimated elasticity of the first lien amount to the CLL is 0.129. The estimated effects on log first lien amounts in Columns 2-3 are similarly slightly larger than the corresponding coefficients on the total amount borrowed in Table 2. Consistent with this, Columns 4-6 report large negative effects of the increased CLL on log second lien amounts.[17]

[17] A baseline prediction might be that a higher CLL would (locally, considering only positive second liens) reduce second liens dollar for dollar. The coefficient in Column 4 reports the elasticity of the second lien amount to the CLL, and thus does not directly assess this hypothesis.

Table 4: Effect of Conforming Loan Limits on Loan Constrainedness for Plausibly Affected Properties

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Jumbo Loan			Jumbo First Lien			Constrained Loan			Constrained First Lien		
Conforming Loan Limit (Log)	0.2968***			0.3754***			-0.0788***			-0.1308***		
	(0.0317)			(0.0310)			(0.0110)			(0.0132)		
High CLL County		0.1115***			0.1429***			-0.0351***			-0.0578***	
		(0.0099)			(0.0095)			(0.0043)			(0.0057)	
Higher CLL County			0.0993***			0.1321***			-0.0315***			-0.0512***
			(0.0118)			(0.0121)			(0.0049)			(0.0061)
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Pair x CLL Regime FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Census Tract FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.0790	0.0791	0.0783	0.0689	0.0691	0.0678	0.0257	0.0259	0.0256	0.0326	0.0330	0.0323
Observations	201556	201556	201556	201556	201556	201556	201556	201556	201556	201556	201556	201556

Sample consists of properties with estimated sale prices $\bar{y}_{ipt} \in [1, 2.5] *$ National CLL_t. Full sample first restricted to county pairs where each county has at least 100 observations in each cell (2000-2007 pre-period, 2008-2017 post-period). All regressions are restricted to observations with nonzero total loan amounts. Standard errors in parentheses; robust standard errors clustered by Census Tract. * p<0.10, ** p<0.05, *** p<0.01.

Table 4 measures the effect of the increased CLL on bunching of loan amounts at the CLL. The results indicate that increasing the CLL increased the share of borrowers with total loan amounts collectively in excess of the national CLL, with a one percent increase in the CLL raising the share of jumbo total loan amounts by 0.30 percentage points. Approximately a quarter of this effect stems from reducing the share of borrowers with total borrowing exactly equal to the national CLL, with an analogous estimated effect size of -0.08 percentage points.[18] These effects are very large in magnitude; counties exposed to the jumbo conforming limits saw an 11.2% point increase in the jumbo loan share, and a 3.5% point reduction in the constrained share bunched at the national limit, compared to mean prevalence of 13.6% and 1.1% in the overall pre-period sample. These effects are uniformly even sharper when looking at the share of first liens in these two respective categories, which reflects that second liens were more frequently used as a means of borrowing above the CLL prior to the relaxation of the conforming limits.

5.3 Placebo Group Regression Results

As outlined above, if these observed effects truly are due to the change in conforming loan limits, then similar increases in borrowing should largely not manifest for houses in treatment counties with valuations sufficiently low that they can be financed without requiring a non-conforming loan under the national limits.

Tables 5 to 7 show results for loan measure outcomes for the placebo group of houses with contemporaneously expected prices in an interval of $[0.5, 1)$ of the national CLL. For the placebo properties, being in a treatment county is not associated with higher potential loan amounts. Once excluding cash buyers, the effect sizes on total amount borrowed, first lien amounts, and the prevalence of jumbo loans are small, tightly estimated, and largely insignificant. Where significant effects are consistently found, the estimated magnitudes are much smaller than for the plausibly affected group. For example, while the coefficients

[18] This reflects that not all buyers who are effectively constrained by the CLL bunch exactly at the limit.

for the effect of the CLL measures on second lien amounts in Table 6 are significant and negative, they are less than a third as large as the analogous estimates in Table 3. The disparity for other measures is even larger. Table 7 shows the policy change yielded an increase in the share of first liens above the national CLL, and a reduction in bunching at the national CLL both overall and for first liens, but the estimated magnitudes are an order of magnitude smaller than for properties with estimated prices above the national CLL.

Viewed together, this suggests that the introduction of conforming loan limits had only very small effects on borrowing behavior for the placebo group. The dual facts that the loan bunching measures provide the clearest evidence of effects, and the estimates are small in magnitude, suggests the most likely explanation for these estimated effects is forecast error. It is plausible that a small fraction of properties assigned to the placebo group were in fact of sufficiently high value to be affected by the policy change, and that outside of this, the placebo group was essentially unaffected.

Table 5: Effect of Conforming Loan Limits on Loan Amount for Placebo Properties

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Potential Loan (Log)			Loan Amount (Log)			Cash Buyer		
Conforming Loan Limit (Log)	-0.0239*			-0.0124			-0.0427**		
	(0.0144)			(0.0180)			(0.0191)		
High CLL County		0.0019			-0.0021			-0.0273***	
		(0.0062)			(0.0069)			(0.0065)	
Higher CLL County			-0.0063			-0.0019			-0.0353***
			(0.0079)			(0.0086)			(0.0064)
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Pair x CLL Regime FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Census Tract FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.2436	0.2436	0.2436	0.2037	0.2037	0.2037	0.2065	0.2066	0.2066
Observations	460502	460502	460502	372110	372110	372110	460502	460502	460502

Sample consists of properties with estimated sale prices $\bar{y}_{ipt} \in [0.5, 1) * \text{National CLL}_t$. Full sample first restricted to county pairs where each county has at least 100 observations in each cell (2000-2007 pre-period, 2008-2017 post-period). Columns 4-6 are restricted to observations with nonzero total loan amounts. Standard errors in parentheses; robust standard errors clustered by Census Tract. * p<0.10, ** p<0.05, *** p<0.01.

Table 6: Effect of Conforming Loan Limits on Loan Component Amounts for Placebo Properties

	(1)	(2)	(3)	(4)	(5)	(6)
	First Lien Amount (Log)			Second Lien Amount (Log)		
Conforming Loan Limit (Log)	-0.0085			-0.2878**		
	(0.0177)			(0.1258)		
High CLL County		0.0039			-0.0689*	
		(0.0067)			(0.0359)	
Higher CLL County			-0.0005			-0.0878**
			(0.0084)			(0.0398)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
County Pair x CLL Regime FE	Yes	Yes	Yes	Yes	Yes	Yes
Time Controls	Yes	Yes	Yes	Yes	Yes	Yes
Census Tract FE	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.1943	0.1943	0.1943	0.2405	0.2405	0.2405
Observations	372110	372110	372110	372110	372110	372110

Sample consists of properties with estimated sale prices $\bar{y}_{ipt} \in [0.5, 1) *$ National CLL_t. Full sample first restricted to county pairs where each county has at least 100 observations in each cell (2000-2007 pre-period, 2008-2017 post-period). All regressions are restricted to observations with nonzero total loan amounts. Standard errors in parentheses; robust standard errors clustered by Census Tract. * p<0.10, ** p<0.05, *** p<0.01.

Table 7: Effect of Conforming Loan Limits on Loan Constrainedness for Placebo Group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Jumbo Loan			Jumbo First Lien			Constrained Loan			Constrained First Lien		
Conforming Loan Limit (Log)	0.0101			0.0263***			-0.0051**			-0.0061**		
	(0.0075)			(0.0050)			(0.0022)			(0.0024)		
High CLL County		0.0043**			0.0076***			-0.0029***			-0.0029***	
		(0.0017)			(0.0013)			(0.0006)			(0.0007)	
Higher CLL County			0.0013			0.0073***			-0.0018***			-0.0022***
			(0.0020)			(0.0015)			(0.0007)			(0.0008)
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Pair x CLL Regime FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Census Tract FE												
R-squared	0.0613	0.0613	0.0613	0.0167	0.0167	0.0166	0.0091	0.0091	0.0091	0.0093	0.0093	0.0093
Observations	372110	372110	372110	372110	372110	372110	372110	372110	372110	372110	372110	372110

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

Sample consists of properties with estimated sale prices $\bar{y}_{ipt} \in [0.5, 1) * \text{National CLL}_t$. Full sample first restricted to county pairs where each county has at least 100 observations in each cell (2000-2007 pre-period, 2008-2017 post-period). All regressions are restricted to observations with nonzero total loan amounts. Standard errors in parentheses; robust standard errors clustered by Census Tract. * p<0.10, ** p<0.05, *** p<0.01.

6 House Price IV Results

Having established that the change in conforming loan limit policy induced an increase in credit utilization, the effect of borrowing on house prices can be analyzed using the CLL policy change as an instrument. From a theoretical perspective, this is not quite the *correct* regression to estimate, as it conflates two different channels by which the higher CLLs may lead to higher prices. For the subset of borrowers who are counterfactually credit constrained by the national CLL, higher CLLs should increase borrowing dollar-for-dollar (up to some point), and this additional borrowing may cause prices to be bid up. Second, as explained in Section 2, CLL relaxation should reduce effective mortgage interest rates, and thus increase willingness to pay, with higher property prices in equilibrium, supported by expanded borrowing (the movement along the demand curve for credit in response to the shift in supply). The coefficient of borrowing on house prices has a natural interpretation for the first, but is strained for the latter, and we aim to separately estimate these two channels in continuing work.

For the time being, the coefficient of borrowing on property prices can be thought of as providing an existence test for the effect of credit supply on house prices. Given that jumbo CLLs induced more borrowing, at the very least, either deposits were crowded out,[19] or property prices increased. The analysis in this section serves to demonstrate that a substantial proportion of the effect manifests upon house prices.

The following tables present results for instrumental variables regressions of log house prices on different loan variables, using the log CLL as an instrument. Controls are as specified in Equation 3, with standard errors clustered by census tract.Two different loan measures are used as the endogenous variable, namely the logarithms of potential loan amount and the total loan amount. An additional control for cash buyers is added to the specification for the latter.

Table 8 shows results for the plausibly affected sample, where the first stage is strong.

[19]Whether for a given borrower, or by changing the composition of buyers towards having less liquid wealth.

Columns 1-2 show results for a base specification without hedonics or distance controls, but with the other controls needed for identification. Irrespective of the loan measure used, the regressions yield similar results, with an estimated elasticity of house prices to credit borrowed of 0.37 using the loan potential measure and 0.40 using the total loan amount measure. Adding hedonics and distance controls has little effect on the elasticity estimates, which are 0.39 and 0.36 respectively in Columns 3-4.

Table 8: Effect of Loan Amount on House Prices

	(1)	(2)	(3)	(4)
	Property Sale Price (Log)			
Loan Potential (Log)	0.3738***		0.3923***	
	(0.0646)		(0.0624)	
Loan Amount (Log)		0.4031***		0.3633***
		(0.0715)		(0.1325)
County FE	Yes	Yes	Yes	Yes
County Pair x CLL Regime FE	Yes	Yes	Yes	Yes
Time Controls	Yes	Yes	Yes	Yes
Hedonic Controls	No	No	Yes	Yes
Distance Controls	No	No	Yes	Yes
Census Tract FE	Yes	Yes	Yes	Yes
First Stage F-Statistic	30.99	32.13	35.48	37.17
Clusters	2363	2363	2363	2363
Observations	222522	222522	222522	222522

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

For comparison, Table 9 provides results for the placebo sample, where the first stage is essentially non-existent, as evidenced by the weak F-statistics. As a result, the point estimates are highly biased and inconsistent, such that relatively little can be inferred from them. Note that the fact that the point estimates are non-zero does not imply a violation of the placebo hypothesis. Indeed, a unit elasticity seems a reasonable prior for an OLS coefficient (i.e. picking up the obvious endogenous channel due to unobservable hedonic attributes) of house prices on loan amounts. The placebo hypothesis is ultimately supported by the weakness of the first stage instruments on the sample with estimated sale prices below the national CLL, rather than the meaningless IV estimates that this then produces.

Table 9: Effect of Loan Amount on House Prices

	(1)	(2)	(3)	(4)
	Property Sale Price (Log)			
Loan Potential (Log)	1.1661***		1.0559***	
	(0.2036)		(0.2590)	
Loan Amount (Log)		1.0698***		-0.1145**
		(0.1734)		(0.0526)
County FE	Yes	Yes	Yes	Yes
County Pair x CLL Regime FE	Yes	Yes	Yes	Yes
Time Controls	Yes	Yes	Yes	Yes
Hedonic Controls	No	No	Yes	Yes
Distance Controls	No	No	Yes	Yes
Census Tract FE	Yes	Yes	Yes	Yes
First Stage F-Statistic	5.84	2.10	3.83	0.40
Clusters	2630	2630	2630	2630
Observations	429758	429758	429758	429758

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

While these results do not speak to the precise channel, they indicate a nontrivial effect of credit utilization on property prices. An immediate implication of this is that the implicit interest rate subsidy given by governments to borrowers through GSEs' implicit guarantee against default at least partially accrues to existing property owners.

7 Conclusion

What is the effect of cheaper access to credit on mortgage borrowing and house prices? Studying the evolution of behavior and outcomes following expansion of Conforming Loan Limits in some counties but not others during the Great Recession, we find strong evidence of increased borrowing for purchases of housing that become eligible for larger implicitly government-backed mortgages. In particular, we find that the increase in borrowing is particularly large for properties where the previous national Conforming Loan Limits was most binding. This increased access to cheap mortgage credit, and increase in borrowing, appears to flow into higher house prices. We estimate that extra induced borrowing increases house prices by approximately 38 cents in the dollar.

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