CREDIT EXPANSION, COMPETITION, AND HOUSE PRICES*

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Abstract

We document the effect of policy-induced credit supply on fueling asset price that leads to the financial crisis, and broad economic outcomes. By comparing loans that fall under the current conforming loan limit (CLL) to those under the old limit last year in a diff-in-diff setting, we find there is a direct effect of CLL change on credit supply and on prices of homes financed by the expanded credit. Increase in credit supply in the new conforming loan market also crowds out private capital in the jumbo loan market. We also find significant effects of CLL change on broad economic outcomes, such as regional home price, employment and business growth.

Keywords: Conforming loan limit; credit supply; financial crisis

JEL classification: D12; G10; R20

* The views expressed here are those of the authors and do not necessarily represent those of the Federal Reserve Bank of Philadelphia or the Board of Governors of the Federal Reserve System. All errors are our own.

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I. INTRODUCTION

The recent US mortgage crisis was characterized by two distinct features: a rapid rise in house prices in most markets prior to the crisis, and an expansion of mortgage credit in both prime and subprime markets during the same period (Mian and Sufi, 2009; Shiller 2014). A growing literature debates about the role of credit supply induced by government policies on the level of asset prices and the development of bubbles (for example, Agarwal, et al., 2016; Di Maggio et al., 2015; Favara and Imbs, 2015; Hubbard and Mayer, 2009). Most of these papers focus on the private-label securities (PLS) loans, mostly subprime loans, and those originated by commercial banks, but largely overlook the prime conforming loans, the most prevalent mortgage sector since the Great Depression. Compared to the subprime market, the prime loan market is more heavily regulated and frequently intervened by governments through the two government-sponsored entities (GSEs), Fannie Mae and Freddie Mac. Recently, Ferreira and Gyourko (2015) show that while the recent financial crisis started out in subprime sector, it quickly morphed into a much bigger and broader event dominated by prime borrowers losing their homes. Therefore, it is important to study prime lending in order to understand the role of government policies in the development of the crisis. This paper examines how credit supply responds to changes of government housing policies targeted only on a specific (prime conforming) market segment and whether house prices are affected.

Empirically, it is challenging to identify the causal effect of credit supply on asset prices due to the well-known identification issue: the provision of credit and the dynamics of asset prices are both endogenous to current and expected market conditions. For example, easy access to mortgage credit may create more demand for homes while it could well be the result of stronger housing demand and/or optimism of future house price appreciation (see, e.g., Breuckner, Calem and Nakamura, 2016). We overcome the identification difficulties by exploiting changes in conforming loan limits (CLLs) regulated by the US Department of Housing and Urban Development (HUD), and, in a diff-in-diff setting, use CLL change as an instrument for exogenous variation in the cost of credit and credit availability. The CLL defines the maximal loam amounts by property type (e.g., single-family one unit) of mortgages that can be purchased by the GSEs. Conforming loans, those below the CLL, are much more liquid in the capital market through GSEs' securitization and trading of mortgage-backed securities (MBSs) in the secondary market. As noted by Loutskina and Strahan (2009, 2011), conforming loans usually offer lower interest rates than jumbo loans, those above the CLL, and consumers borrowing near the CLL are thus induced to borrow less than they otherwise would. When HUD increases the CLLs, a fraction of would-be jumbo loans are turned into conforming loans, which enjoys higher liquidity and lower rate.

During the course of our analysis, we seek answers to the following questions: (i) Do increases in CLL lead to more credit supply and higher home prices in the new conforming market? (ii) Do increases in CLL adversely affect the adjacent markets (those near the old and new CLLs)? (iii) Do increases in CLL lead to lax loan underwriting? (iv) Do increases in CLL affect economic outcomes at the regional or macro level? To answer these questions, we examine loan applications (approvals), originations and property transactions from 1994 to 2006 when the national housing market reaches its peak. Our diff-in-diff strategy compares the changes in mortgage and housing outcomes year over year between the treated loan cohorts and the control cohorts. The core treatment group in our analysis is those loans above the old CLL but within the new CLL. They are directly affected by the CLL change. Our control group includes those way below the old CLL and those way above the CLL. They are affected very little by the CLL change. We also analyze the market that is adjacent to our core treatment group - the one just below the old CLL and that just above the new CLL. They might be affected by CLL change due to competition and spillover effects. By tightly controlling for county- and bank-fixed effects along with other characteristics, the estimated coefficients reflect the incremental effect of the CLL change on mortgage and housing outcomes. Because the CLLs are adjusted on an annual basis, we classify loans into treated and control groups on a rolling basis and our regressions are run by pairs of every two years, e.g., 2004 versus 2003 and 2005 versus 2004.

Our result shows that following increases in the CLL, mortgage volumes in the new conforming market experience the most dramatic growth during the sample period. From 1995 onward, applications and originations in this cohort expand at an annual rate of approximately 200% every year before they slowed down in 2005. In contrast, volumes in the old conforming market just below the old CLL have very modest growth, by only 17-20% per annum, and those in the jumbo market just above the new CLL decline in absolute terms. It appears that, while the overall mortgage market expands in those years, there is great variation across different market segments due to changes in credit supply and competition.

We then utilize Home Mortgage Data Act (HMDA) data to run the diff-in-diff analysis of loan applications. To mitigate possible borrower self-selection across different cohorts, we take a donut approach to carve out loans at the borders between every two adjacent loan cohorts and focus on the home purchase transactions in the baseline results. We find that, relative to the control cohorts, approval rates in the new conforming loan cohort from 1995 to 2005 increased by 1.4–5.6% over the previous year, with a median of 4.1%. The approval rates in other treated loan cohorts do not increase significantly, either statistically or economically. These results support the contention that an increase in the CLL has a significant positive impact on credit supplies in the targeted new conforming market.

We also estimate the effect on mortgage profiles and performance. Our regression of the note rate confirms the pricing advantage of conforming loans in the capital market. Loans in the new conforming loan cohort experience a reduction in the interest rate of 10–38 basis points (bps) relative to the control cohort, after controlling for a full array of observable pricing variables including FICO and LTV. The rates offered on loans in the other treated cohorts do not experience any significant change in most years. Also, above and beyond the observable risk factors of loans, there is no significant difference in defaults and prepayment risks of treated loan cohorts relative to the control cohorts, suggesting that the rate reduction is not a result of loan quality change but rather an effect of increased credit supply and pass-though of GSE funding advantages associated with government guarantees.

Our ultimate goal is to estimate the effect of increased credit supply on home prices. We do so by using both hedonic price equations and a repeat sales approach. Our results show that the prices of properties in the new conforming loan cohort in 2001 to 2005 increase by an additional 0.5–1.4% per annum relative to the control cohorts. In contrast, the price of properties financed with loans in the other two treated cohorts experienced mostly significant declines and between the two, those financed with jumbo loans just above the new CLL decline more. These results suggest that an increased credit supply due to government policy has an immediate impact on house prices in the targeted submarkets and their neighborhoods.

Because the CLLs are adjusted based on a national home price surveys, we are concerned that the estimated effects may reflect a current home price trend, a precondition for raising the CLL. We address this concern in two ways. First, we focus only on counties in the lowest quartile of home price appreciation during the period. These markets do not have pre-conditions supporting the national loan limit increase and should be immune to endogeneity concerns. Second, we treat the loan limit increase as an event and run month-by-month regressions to nail down the exact timing of the CLL effect. The new CLL applies to loans securitized beginning January 1st each year and thus we should see a significant increase in originations from December to January. Our results based on underperforming markets are very similar to and consistent with those for the overall national sample. The month-by-month regressions support significant effects of the CLL in loans originated only in January, not in any other month.

Finally, we examine the broad effect of changes in CLLs on regional economic outcomes. Our research design exploit regional heterogeneity in the growth of loans classified in three treated loan cohorts. Although the loan limit change is a national event, regions differ in responding to the changes each and every year. We compare outcomes in regions that have a relatively high growth of loans in the new conforming loan cohort—and therefore benefit more from an increased credit supply—to otherwise similar regions with a relatively low growth of such loans. Two types of regions are matched using propensity score matching algorithm based on information prior to 1998 when outcome variables become available. Our results suggest a significant and positive relation between the growth of loans in treated loan cohort and increases in home prices, total employment, payrolls, and total number of business establishments at the zip code level. Each 1% growth of credit supply in the new conforming loan cohort is associated with a 0.06% appreciation in home price, 0.18% employment growth, 0.08% payroll growth, and 0.02% more business openings, all at an annual rate. Considering that these results control for MSA fixed effects and a number of zip code-level variables, the magnitude of the effects is economically very significant.

This paper makes important contributions to the growing literature on the effect of credit supply in the real estate markets. Favara and Imbs (2015) studies the effect of interstate banking deregulation on credit supply in the mortgage market and housing prices. They find commercial banks affected by banking deregulation experience significantly higher deposit growth and lower rates. In areas primarily operated by these institutions, more borrowing take place, and the demand for housing increases. Our instrument for credit supply differs from theirs. We focus on changes in CLL in the secondary mortgage market, which has an important impact on the primary market. Adelino, Schoar, and Severino (2012) also study the effect of CLL on home prices, assuming properties are all financed at an LTV ratio of 80% in the absence of mortgage information. They compare the price changes in properties financed by loans above the limit relative to those financed by loans below the limit. Our analysis is much broader, and we analyze not just the targeted submarkets but also their neighboring submarkets. In addition, we explore broader economic outcomes. In terms of methodology, in addition to the diff-in-diff strategy, we adopt a donut approach to deal with borrower self-selection and address any remaining endogeneity concerns with additional robustness tests.

Since CLL is an important instrument for federal government to regulate GSEs as well as the housing market, this paper also contributes to our understanding of the role of GSEs and government guarantee in the housing market. Early papers have surveyed the U.S. housing finance policies (e.g., Frame et al. 2013; Glaeser and Gyourko, 2008; Levitin and Wachter, 2013) and estimated the average implicit subsidy from the government guarantees (e.g., Passmore et al. 2002; McKenzie, 2002). Some recent papers evaluate the effects of government housing programs including Community Reinvestment Act (CRA) and affordable housing goals in the financial crisis (Agarwal et al 2012, 2016; Avery and Brevoort 2015) and assess the effects of phasing out the GSEs or removing the government guarantee (e.g., Elenev et al. 2015; Gete ad Zecchetto, 2016; Jeske et al. 2013). Recently, Hurst et al. (2016) find that GSEs’ national constant interest rate policy implies substantial redistribution of resources and welfare across regions through the mortgage market. We find that while increase in the CLL improves access to credit, it also crowd out private capital serving the jumbo market to higher-amounted submarkets, creating a ripple effect of credit supply.

Our findings have important policy implications for regulators and policy makers on how to re-design the housing finance system and to implement housing policies. Since 1995, US government has set homeownership as a national priority and implemented hundreds of policies to booster homeownership rate until the market crashed in 2007. In the wake of the crisis, government has also implemented various housing programs to jump-start the economy. Our results suggest that the GSEs and government programs often achieve their intended consequence of increasing credit supply, but they can also distort lending behavior.

The remainder of the paper is structured as follows. Section II explains the data and our identification strategy. Section III presents our main results and Section IV reports results of robustness tests to address endogeneity concerns. Section V discusses broad impacts on regional economic outcomes. Section VI concludes the paper.

II. DATA AND IDENTIFICATION

II.1. Data Sources

The data used in the analysis were collected from a number of sources. Our first data source consists of CLL applied to the two GSEs. The Housing and Community Development (HCD) Act of 1980 requires a limit by property type on the size of mortgages that can be purchased by the two GSEs. Mortgages that exceed the CLL, namely, jumbo loans, may not be purchased by GSEs and are usually held in bank portfolios or sold to private investors. The act mandates the CLL to be adjusted annually to changing market conditions. Adjustments to the loan limit are based on home sale prices from October to October published by the Federal Housing Finance Board in its Monthly Interest Rate Survey. The HCD Act of 1992 revises the methodology used to adjust CLL by including broad metrics and designating HUD as the government body that sets and adjusts the CLL. Figure 1 plots both the level and growth trends of CLL for single-family one-unit loans from 1990 to 2007. When it started in 1980, the initial CLL for single-family one-family loans was set at $93,750. It was increased at double-digit rates in the late 1980s and was mostly flat in the early 1990s until 1996, when it had its first significant increase. The limit has increased steadily from 1996 through 2002, when the growth rate of loan limits slowed down from 2003 through 2005. Therefore, our sample period covers the years through 2006.

Our second data source is the public version of HMDA data from 1995 to 2006. The HMDA was enacted by US Congress in 1975 to provide public loan-level data that could be used to determine whether financial institutions are serving the housing needs of the community as well as practicing discriminatory lending. From the HMDA data, we know detailed demographic information of the loan applicant, such as race, ethnicity, gender, and reported income.[3] The HMDA data also contain rich information on mortgage applications and origination: first, whether an application was approved or denied along with major denial reasons and, second, the amount and purpose of the loan, that is, refinancing versus home purchase. Although the LTV is not reported, we can calculate the loan-to-income ratio as an alternative measure of leverage.

Our third set of data is loan-level mortgage data from the McDash Analytics servicing database that is not merged to HMDA. McDash contains detailed information on origination as well as dynamic performance, tracked and reported monthly. Because of limited coverage of the McDash data before 1995, our sample covers 1996 to 2006. The information at origination includes the borrower's credit score (FICO), LTV, debt-to-income ratio, loan purpose, occupancy status (e.g., owner occupied versus investment); property type (e.g., single-family houses, condominiums), the level of documentation (low documentation versus full documentation), loan amount, interest rate type (fixed-rate versus adjustable-rate), and so forth. Each loan is tracked in dynamic files until the borrower defaults or prepays the loan. Default is defined as when the borrower misses at least three consecutive payments, so-called serious delinquency (SDQ).

To assess the effect of conforming loan changes on home prices, we leverage the transaction-level data from public deeds and tax assessor data in 2001–2006 aggregated by CoreLogic. These data contain detailed property characteristics, the price and date of each transaction, and the geographic locations. Original public deeds include both cash sales and those financed by mortgages. Since our loan cohorts are defined based on mortgage balances, cash transactions are excluded. We use a rich set of characteristics in hedonic equations to control for property amenities, including gross living area in square feet; lot size in acres; the number of bathrooms; property age based on year built; swimming pool; fireplace; condition (poor, fair, average, good, and excellent); quality of construction; proximity to a golf course or lake and other adverse and favorable views; and parking space. These controls help to isolate other factors that could affect the property sale price. For a subset of transactions, we were able to identify prior sales of the same property. This allowed us to observe the appreciation of repeat sales, as in a Case–Shiller (repeated sales) home price index approach.

[3] Applicants could overstate their information in mortgage applications to be eligible.

We also gather zip code-level economic outcomes from ZIP Code Business Patterns (ZBP) data collected annually by the US Census Bureau. These include total employment, total payroll income in dollars, and the total number of business establishments. We use these data to explore the regional impacts of increases in CLL.

II.2. Identification

This paper explores the effect of increases in the CLL on mortgage, housing market, and broad economic outcomes. We adopt a diff-in-diff strategy to identify the causal effect of limit changes. Based on a comparison of the new CLL effective this year and the old CLL from last year, we classify loans into five cohorts (Table 1 and Figure 2): 1) *Treated* 2 cohort includes loans that were jumbo loans, above the old CLL, last year, but are conforming loans this year. This is the most important treatment cohort. Because of liquidity and pricing advantages, we anticipate an increase in this cohort's credit supply from last year. 2) *Treated* 3 cohort includes loans that are just above the new CLL. They are jumbo loans in both years, but may be affected by the CLL change due to borrowers who were borrowing jumbo loans near the new CLL now have to borrow less than they otherwise would in order to take advantage of lower rate of the new conforming loans. 3) *Treated* 1 cohort includes loans that are just below the old CLL. They are conforming loans in both years, but may be affected due to borrowers borrowing near the old CLL who had to borrow less than they otherwise would can now borrow more up to the new CLL. 4) *Control* cohorts include loans that are further below *Treated* 1 (*Control* 1) and above *Treated* 3 (*Control* 2). They are far from the old and new CLL and should be affected very little by the CLL change. We make the above five segments equal in length on the loan size spectrum.

To further illustrate our loan classification, consider the following example: the CLL for single-family homes is \$275,000 in 2001 and \$252,700 in 2000; there is a 7% increase in the CLL from 2000 to 2001. The *Treated* 2 cohort thus includes loans with a balance between \$252,700 and \$275,000 in both years, the *Treated* 3 cohort includes loans with a balance between \$275,000 and \$294,250 (= \$275,000 × 1.07), the *Treated* 1 cohort includes loans with a balance between \$235,011 (= \$252,700 × 0.93) and \$252,700, the *Control* cohort 1 includes loans with a balance between \$217,322 (= \$252,700 × 0.86) and \$235,011 (=\$252,700 × 0.93), and the *Control* cohort 2 includes loans with a balance between \$294,250 (= \$275,000 × 1.07) and \$313,500 (=\$275,000 × 1.14). To mitigate the issue of potential borrower self-selection between different loan cohorts, we adopt a donut approach to carve out loans right at the borders between two adjacent loan cohorts, as illustrated in Figure 2. Specifically, we first calculate the distance between the new and old CLL as *D*. Then for each of the two adjacent cohorts (e.g., *Treated* 1 and 2 cohorts or *Treated* 2 and 3), we exclude loans falling within 10% x *D* above and below each cutoff point.

Our diff-in-diff research design exploits variations of conforming and jumbo loans from last year to this year under different loan limits. To implement the design, we run regression for every two years, the current and the previous. We thus compare variations of changes in outcome from the previous year to the current year among different loan cohorts. We combine two control cohorts into one - the observable characteristics and outcomes in these cohorts are very similar. Thus, we estimate the effect of increases in the CLL as measured by the difference between the three treated cohorts and one combined control cohort after the CLL change.

More formally, to analyze the effect of the loan limit change on application and mortgage outcomes, we run diff-in-diff regressions of the following form:

$$
\begin{aligned}
Y_{i,t} = \ & \beta_1 \times Treated_1 + \beta_2 \times Treated_2 + \beta_3 \times Treated_3 + \beta_4 \times Post \\
& + \beta_5 \times (Treated_1 \times Post) + \beta_6 \times (Treated_2 \times Post) \\
& + \beta_7 \times (Treated_3 \times Post) + \beta_8 \times Others + \gamma + \epsilon_{i,t} \qquad (1)
\end{aligned}
$$

where $Y_{i,t}$ is the outcome at the application or loan level, $Post$ is a dummy for the year under the new CLL so that β_4 captures changes in the control cohort from the previous to the current year, and β_1 to β_3 capture the time-invariant difference between the three treated cohorts and the control cohort. The interaction terms between the three treated cohorts and the *Post* dummy are the most interesting variables in the analysis. Their coefficients β_5 to β_7 capture the effects of the loan limit change on the treated cohorts. The variable $Others$ includes a number of loan or application-level control variables and γ_t represents county- and bank-fixed effects so that loans in the treated cohorts are compared with those in the control cohort in the same county and originated by the same bank. In all the regressions, standard errors are clustered by MSA.

II.3. Summary Statistics

The key variables used in the analysis are summarized in Table 2. Panel A reports detailed statistics based on the overall sample. Our samples contain about 27.8 million applications, 7.2 million mortgages, and 3.2 million home sales, all restricted to mortgage balances between 80% of the old conforming limit and 120% of the new limit each year. The rationale behind these restrictions is that we focus on loans with different but reasonably close balances so that all the borrowers in the sample still have access to the same mortgage market.

The overall approval rate in the HMDA data is 77%. Application loan amounts have an average of $285,000 and the average borrower income is $118,000, implying a loan-to-income ratio of about 2.5. A total of 22% of the applicants in this period were non-white minorities. Besides controlling for MSA fixed effects in all the regressions, we also control for tract-level income as a percentage of MSA median income. From the McDash data, the average loan balance is about $290,000, with a standard deviation of $57,000. The average note rate is 6.1%. A total of 23% of loans in the sample are different adjustable-rate mortgages. Since McDash contains data on all segments of the mortgage market, government goals originated by the Federal Housing Administration, conforming loans insured by GSEs, and jumbo and subprime loans funded by private capital are all included. The average LTV ratio and FICO score are 80% and 716, respectively. Because of the existence of low-documentation loans (4%) and poor data quality, most of the debt-to-income ratio data are missing from the sample. A total of 57% of loans were to refinance an existing mortgage. Only 2.4% of loans were for investment properties instead of for owner-occupied properties. We define defaults as loans that become SDQ. For both default and prepayment, we condition on performance in the first three years after loan origination to control for the effects of changing economic conditions. Data from the property public records show the average purchase price to be about $390,000 nationwide, with a standard deviation of $86,000. These properties have an average gross living area of 2,182 square feet, a lot size of 0.6 acre, are aged 39 years old, and have 2.2 bathrooms on average.

Panel B reports the statistics by different loan cohorts. The approval rate in the *Treated* 1 and 2 cohorts is the highest of all the cohorts. These two cohorts include borrowers with the highest income in conforming loan markets. Compared to conforming loans with slightly lower balances, jumbo loans in the *Treated* 3 cohort tend to have a lower approval rate, regardless of their higher income. The measure of leverage, the loan-to-income ratio, is the lowest for *Treated* 1 and the highest for *Treated* 3 cohort. This result is consistent with that of the LTV ratio measure based on the McDash data. Loans in the *Treated* 2 cohort have the lowest LTV ratio, at 71%, and the highest FICO score, at 717 on average. The newly eligible conforming loan market tends to attract the highest-quality borrowers. Loans in *Treated* 3 cohort, all jumbo, tend to have the highest LTV ratio, 75%, and the lowest FICO score 704 on average. Without controlling for any risk factor, we find the nominal note rate on mortgages to be high for loans in *Treated* 2 and 3 cohorts, at 6.08% and 6.05%, respectively. Jumbo borrowers appear to pay lower interest rates without controlling for any standard pricing variable. This could reflect their higher share of adjustable-rate mortgages, 47% versus 24% for *Treated* 2, whose note rate is the initial teaser rate. There is a direct relation between loan balances and defaults as well as prepayment rates. The default rate steadily increases from 5.2–5.5% for the conforming loan cohorts to 6.4% for *Treated* 3 cohort.

It is possible that borrowers purchase homes in one year and then refinance next year, a double counting problem. To control for the derived demand, our baseline analysis is based on purchase transactions only. We also report the results based on both home purchases and refinancings in the Appendix. The two results are very similar.

II.4. Discontinuity of Mortgage Credit

Figure 3 plots a histogram of the frequency distribution of mortgage applications from HMDA data as a function of the ratio of loan amount to the CLL (Panel A). The figure first shows that most mortgage applications come in below the CLL, suggesting conforming loans are the dominant segment in mortgage market. Although below and above the point where loan amount is exactly at CLL, applications appear evenly distributed, frequencies of applications at each percentage cohort below the CLL double those above the CLL. Also, there is a sharp spike in the frequency of loan applications exactly at the CLL, suggesting these borrowers are endogenously determined, in part, due to their financial constraints. The discontinuity, as Loutskina and Strahan (2009) first explore, shows that since interest rates are higher for jumbo loans, thus some borrowers with loan demand near the CLL may borrow less than they otherwise would to take advantage of the lower rate. However, once the CLL is raised, these borrowers would borrow more accordingly. This borrower self-selection suggests that the donut approach is necessary to our identification.

Panel B plots the histogram charts by year. The spike in the frequency of loan applications right at the CLL is evident in all years. The magnitude of the spike reaches its peak in 2001 and 2002 with a proportion well above 15% and gradually declines afterwards, reflecting improved liquidity in jumbo market with a flux of capital into PLS following the burst of the Internet bubble. Private capital quickly became a major participant in the mortgage market during those years. Their share in the secondary housing finance market increased from 53% in 2000 to 65% in 2005, while GSEs' along with Ginnie Mae's shares dropped from 47% to 35%, according to *Inside Mortgage Finance*.

II.5. Changes in Mortgage Volume

Figure 4 plots year-over-year growth of loan applications and originations by year (Panels A-C). The magnitudes of growth of purchase transactions are very similar to those of all transactions. Across all charts, applications and originations in the *Treated* 2 cohort, the new conforming loan cohort, experience the most rapid growth every year from 1995 onward following increases in the CLL. Applications and originations in the *Treated* 2 cohort grow at an annual rate of 195% and 209% on average respectively before their growth slow down to 17% in 2005. During the same period, applications and originations in the *Treated* 1 cohort, those just below the *Treated* 2 cohort, as well as two control cohorts have only modest growth, at an annual rate of 17%, 20% and 13% respectively before their declines in 2005. In contrast, loans in *Treated* 3, where loans that were jumbo further above the old limit in the previous year but are now just above the new CLL, actually see a decline in volumes in 1995-2000 and some modest growth in 2001-2004 before growth turns negative again in 2005. The temporary growth in early 2000s is most likely due to the rapid expansion of private label securities. Panel D plots the year-over-year growth of LPS loan volumes used to finance purchases. It shows similar growth pattern among five loan cohorts except the magnitude of growth in purchase loans in *Treated* 2 is much greater. It appears that, while the overall mortgage market grew in those years, there is great variation across different market segments. Borrowers are attracted to the newly eligible conforming loan market from elsewhere because of the broad appeal of the conforming loan market as well as higher balances.

III. EMPIRICAL RESULTS

III.1. Application Outcomes

It is difficult to establish a causal relation between changes in the CLL and the dramatic growth of application and origination volumes based on information shown in Figure 2, since other factors could be driving them. In this section, we estimate the effect of loan limit changes in a diff-in-diff setting while controlling for other factors. Our first set of regressions focuses on loan applications. We control for tract-level median income and county-fixed effects, so we compare loans in different cohorts but that are originated in the same county and within comparable census tracts.

Table 3 reports regressions on the approval rate, where approve is coded as one if the application was approved and originated. Regressions are run by year based on the data for every two years. For example, the 2003 regression is based on 2003 and the next year, 2004. The *Post* dummy is coded as one if the applications were from 2004. Throughout the regression tables, the coefficients of the *Treated* 1, *Treated* 2, and *Treated* 3 cohorts capture the time-invariant differences in approval rate of these loan cohorts relative to the control cohorts. The results show that the loans in the *Treated* 1 cohort, which include conforming loans just below the old CLL, have a higher approval rate than the control cohort, while loans in the *Treated* 2 and 3 cohorts, which include loans that were jumbo loans last year but are now eligible conforming loans, and jumbo loans that are just above the new CLL respectively, have a lower approval rate than the control cohort. These results support the contention that conforming loans have a higher approval rate than jumbo loans do in the same market, given greater liquidity and available credit supply. The coefficient of *Post* captures the change in outcomes for loans in the control cohort from last year. The results show mixed changes: negative in some years while positive or no significant changes in other years.[4]

The coefficients of the three interaction terms of the treated cohorts and *Post* are the most interesting to us because they capture the differences in the change of approval rate over the previous year for the loans in the treated cohorts relative to those in the control cohort following the CLL change. They are therefore interpreted as the incremental effect of change in the CLL on the outcomes in the three treated cohorts. The results show that the loans in the *Treated* 2 cohort have the highest increase in approval rate over the previous year, ranging from 1.4% to 5.6% with a median at 4.1% relative to the control cohort. This result supports the contention that an increase in the CLL by US government has a significant and positive effect on stipulating the credit supply. In contrast, in the same period and market, loans in the *Treated* 1 and 3 cohorts experience a decrease in the approval rate relative to the control cohort in some years while no significant changes in other years. This is because some of their borrowers move to the new conforming loan segment as the cheaper credit becomes available at higher balances.

III.2. Mortgage Characteristics and Performance

There is not a great deal of information on originations available from the HMDA data. We thus leverage McDash's servicing data to estimate the effect of loan limit changes on mortgage outcomes. These results are reported in Table 4. Panel A reports regressions of FICO scores by year. The coefficients on the interaction terms are mostly insignificant, suggesting little change in credit quality of borrowers for the loans in the treated cohorts compared to the control cohorts.

Panel B reports the results for the LTV ratio. The coefficients of the interaction terms show that loans in *Treated* 2 cohort experience a decrease in the LTV ratio relative to the control cohort in all years from 1996 through 2005. This finding suggests that the new conforming loan cohort is able to attract less liquidity-constrained borrowers compared to jumbo loans made last year in this cohort. In contrast, loans in the other two treated cohorts experience no significant change in LTV ratios most years compared to the control cohort.

Panel C reports the effect on the mortgage note rate of the loan limit change after controlling for all mortgage characteristics, so the estimated effect is above and beyond standard pricing information, including the FICO score, the LTV ratio, refinancing, and so forth. There is a consistent and significant reduction in the interest rate in *Treated* 2 (interaction term) relative to the control cohort, ranging from -10 to -38 bps with a median at -17 bps, suggesting the immediate benefit of lower rates to conforming

[4] It is important to note that the control cohorts include loans in both the Control 1 and Control 2 cohorts. We tested the difference between the two cohorts in the regressions and confirm that they are not statistically different.

loan borrowers. This result is obviously due to the pricing advantages of GSEs relative to private capital, as well as the liquidity of agency MBSs (e.g., Vickery and Wright 2013). However, the effects on the note rate in the other two treated cohorts (interaction terms) are not significant in most years, suggesting little difference in borrowers in the *Treated* 1 and 3 loan cohorts between two years.

Does the rate reduction induce any adverse selection of borrowers or the moral hazard of lenders? We next examine the effect of the CLL change on *ex post* performance including default and prepayment risks. An adverse effect would suggest some evidence of lax underwriting due to credit expansion. We restrict default and prepayment conditional on first three years of payment history to limit the effect of changes in market conditions - such as in home prices, interest rates, and the labor market - so that we can attribute the difference in performance more to the effect of differences at origination. Panels D and E report the default and prepayment results, respectively. The coefficients of all three interaction terms are mostly insignificant, suggesting no differences in performance year over year between the treated cohorts and the control cohort. Since we control for a full array of observables, including the LTV ratio and FICO, the results also show no significant effect of unobservables or so-called soft information on mortgage performance.[5] Based on these results, we can conclude that the aforementioned rate reduction effect is not due to unobservable better loan quality. Instead, it is likely due to the supply-side effect.

III.3. Property Prices and Returns

Our ultimate question is whether the credit supply induced by government policies helps fuel property prices. Unlike other studies at a highly aggregate level, we have the advantage of examining the effect on properties financed by different types of mortgages defined by ranges of loan amounts. This allows us to test how prices start to run up until they reach an unsustainable level that leads the housing market to crash, as in 2007–2010. These results are reported in Table 5. Panel A reports the regressions of the logarithm of the property sale price on loan cohorts as well as a full array of property characteristics, including whether the deal was a cash sales versus mortgage financing, house size in square feet, lot size in acres, swimming pool, fireplace, property condition (poor, fair, good, excellent, and other), quality of construction (poor, fair, good, excellent, and other), golf course, lake, and other amenities. We also control for zip code-level fixed effects. The coefficients of the three treated cohorts are all positive, indicating that higher loan balances are generally used to finance more expensive homes. Consistent with home price appreciation in the period, the coefficient of *Post* is significant and very positive. The coefficients of the interaction terms capture the different effects of CLL increases on price appreciation over the previous year in different loan submarkets. Relative to the control cohort, properties financed by loans in *Treated* 2 cohort undergo significant positive (faster) appreciation by anywhere from 0.5-1.4%

[5] Rajan, Seru, and Vig (2015) find that soft information has become more valuable with the expansion of securitization.

with a median at 1.0%, while those financed by loans in *Treated* 1 and 3 cohorts suffer from mostly significant negative (slower) appreciation.

We are able to match subsequent sales for a subset of transactions and construct a sample of repeat sales. With repeat sales data, we can calculate holding period returns and estimate the effects of CLL changes on holding period returns. Our findings are very similar to those of the hedonic equations. Houses financed with mortgages in *Treated* 2 cohort appreciate more by 0.7% at the expense of the other two treated cohorts, by -1.1% and -1.7% respectively, than the control cohorts. Between *Treated* 1 and 3 cohorts, houses in the former cohort decline less.

III.4. Transmission Channels

How do changes in the CLL in the secondary mortgage market affect the characteristics and pricing of loans in the primary mortgage market and property sale prices in housing market? Most residential mortgages in the United States are securitized and sold to investors rather than held in bank's portfolio as whole loans. Loans are funded by GSEs or PLS issuers at the time of delivery in securitization cases while lenders have to fund them using deposits or debts should they decide to retain them in portfolio – a decision process known as the Best Ex(ecution) in the mortgage industry. However, lenders do not know the exact Best Ex option for a particular loan in advance and typically prefer to originate conforming loans that are eligible for both GSEs and PLS pools and carry the maximal liquidity value.

Once HUD publishes the new CLL for the next year (typically in late October), lenders start to underwrite loans eligible under the new limit that would close in late December[6], just in time for delivery to MBS pools beginning January 1. Lenders consider a higher loan balance more desirable because they earn commissions as a percentage of loan balances. In order to take advantage of lower rate on higher-balanced conforming loans, borrowers borrowing near the old CLL who had to borrow less than they otherwise would can now borrow more up to the new CLL and buy more expensive houses. They represent upward movement of lending from *Treated* 1 cohort to *Treated* 2 cohort. On the other hand, borrowers who were borrowing jumbo loans near the new CLL now have to borrow less than they otherwise would in order to take advantage of lower rate of the new conforming loans. They represent downward movement of lending from *Treated* 3 cohort to *Treated* 2 cohort. As seen from Figure 3B, there is always a spike in lending right at the CLL even as it is adjusted each year. The magnet effect is formed as lending activities constantly move from *Treated* 1 and 3 cohorts both to *Treated* 2 cohort, resulting in concentration of lending exactly at the CLL. It thus increases the demand for houses financed by the loans in *Treated* 2 cohort and leads to higher contract prices through likely bidding wars.

[6] It typically takes 30-60 days from approval to closing of a loan. It takes longer after the financial crisis due to the added compliance and regulation requirements.

At the same time, lending in *Treated* 1 and 3 cohorts is both crowded out due to the dominance of new conforming loan market, as seen in Figure 4.

There are however fundamental differences between *Treated* 1 and 3 cohorts. Lenders active in *Treated* 1 cohort can continue to originate loans in *Treated* 2 cohort as both loan segments are conforming loans that are delivered to GSE pools while those active in *Treated* 3 cohort are private jumbo lenders who may not deal with GSEs at all or are funded by private capital such as PLS jumbo pools. Thus gains in *Treated* 2 cohort and losses in *Treated* 1 cohort could be net gain of the same lender while losses in *Treated* 3 cohort are net losses for private capital who can not compete in the new conforming market where GSEs have competitive advantages. This is supported by positive growth of volumes of loans in *Treated* 1 while negative growth in *Treated* 3 cohort in Figure 4. As a result, home price financed by loans in *Treated* 3 cohort suffer more declines than those in *Treated* 1 cohort, -1.7% vs -1.1% from repeat sales regression. The crowded-out private capitals are faced with exiting the market, converting to conforming loan origination or moving up to the higher-balanced jumbo market where they can compete. In Figure 4, applications and originations in *Control* 2 cohort appear to grow faster or decline less than *Treated* 3 cohort in almost all years, suggesting at least some private capitals move up to higher-balanced jumbo market, possibly creating a ripple effect of credit supply.

IV. ENDOGENEITY ISSUE AND ROBUSTNESS ANALYSIS

Since CLL are partly adjusted on the basis of home price appreciation in the prior year, we are concerned about the endogeneity of changes in loan limits, which could bias the estimated effects. We take two measures to address this concern. First, we restrict the sample to the bottom quartile of counties ranked by home price appreciation during the sample period. In doing so, we effectively remove the precondition for loan limit adjustments at the regional level, since the loan limit is adjusted at the national home price level. Second, we focus only on a few representative years and run the same regression month by month to nail down the exact timing of the effects. Since the new loan limit takes effect in January 1 each year, we expect to see a significant effect in December originations as lenders prepare to deliver under the new CLL to GSE pools beginning January 1st. Other changes at the market level and in that year should be superseded in month-by-month regression. There should be no effect in the other months.

IV.1. Underperforming Markets

The results for underperforming housing markets are reported in Table 6. Panel A reports the results for the regression of approval rates on loan cohorts along with other controls, similar to Table 2A. Most of coefficients are significant. The coefficient of the interaction term of *Treated* 2 and *Post*, our main variable, is significant and positive in all years from 1997 to 2003, suggesting a significant increase in the approval rate in this cohort compared to the control cohort. The effect ranges from 1.9% to 6.0% with a median at 4.2%. This magnitude is similar to the estimated effect on national markets. In contrast, the effect on the approval rate in the *Treated* 1 and 3 cohorts is either insignificantly or adversely affected by the loan limit increase.

Panel B reports the effect on mortgage note rates. We find a significant reduction of the interest rate in the *Treated* 2 cohort, ranging from -12 bps and -31 bps with a median at -18 bps, following an increase in the CLL. However, the effects on the note rate in *Treated* 1 is positive in three years while insignificant in all other years. Consistent with results based on national sample there is some evidence of adverse selection of the borrowers left in this cohort who end up are charged higher rate relative to control cohorts. The rate of loans in *Treated* 3 see a significant increase instead in 2003-2004 while insignificant in other years. We report the effects of loan limit changes on default risk in the Appendix. As for the previous results based on the national sample, there is no significant effect, suggesting no evidence of risk increase due to the increase in CLL.

The results for the logarithm of the property sale prices based on the underperforming market sample are reported in Panel C. Comparing with Table 5, we find the coefficients of the three interaction terms to be very similar in signs and slightly greater in magnitudes. Year-over-year prices of properties financed by loans in the *Treated* 2 cohort bear a significant premium relative to the control cohort in 2001–2005, ranging from 1.2% to 2.8%. In contrast, the prices of properties financed by loans in *Treated* 1 are significantly discounted relative to the control cohort in 2005, higher in 2002 -2003, and indifferent in other years. Properties financed by loans in *Treated* 3 cohort do not experience higher appreciation except for 2004. These results suggest that the increased credit supply in the new conforming loan market helps fuel price appreciation in these years, while the reduced demand in the lower-balanced conforming market causes slower price appreciation in these markets.

IV.2. Month-by-Month Effects

Since our baseline results identify the causal effect of CLL changes in a diff-in-diff setting based on year-over-year changes, it is possible for changes in the same market within a year to be driven by other factors. We address this concern by estimating the exact timing of the effect, given the new loan limit usually takes effect on January 1st. Instead of year-over-year comparisons, the regressions are now run based on month-over-month comparisons in a diff-in-diff setting. We therefore anticipate a significant effect of loan limit changes on originations in January compared to December.

All the month-by-month results based on 2002 and 2003 data are reported in Table 7. Panel A shows the effect on the mortgage note rate. For *Treated* 2 loan cohort, there is a significant reduction in the interest rate in the *Treated* 2 cohort only in January 2003 over December 2002, by -10bps, and relative to the control cohort. There is no effect in other months, since month-over-month comparisons should cancel out any changes at the year level in a given market. Panel B reports the effect on the logarithm of the home prices month by month. Properties financed by loans in *Treated* 2 experience significantly more rapid appreciation only in January 2003 over December 2002, by 0.9%. This coefficient is not significant in the other months and for other treated cohorts. We also find similar results from month-by-month regressions of other years. These results confirm that the significant effect on home price is due to the increase in the CLL that takes effect in January 1st.

V. REGIONAL OUTCOMES

Previous application- and loan-level regressions are designed to estimate the incremental and causal effects of loan limit increases on the credit supply and property prices in very specific mortgage segments. The diff-in-diff strategy, however, cannot test the effects on macroeconomic outcomes. In this section, we explore the impact of changes in CLL on regional outcome variables such as home prices, employment, income, and the number of business establishments at the zip code level. Our research design exploits regional heterogeneity in the average growth rate of loans classified in all three treated cohorts, especially *Treated* 2 cohort, year over year from 1998 to 2005. Although the loan limit change is a national event, regions differ in responding to the changes each and every year. We compare the outcomes in regions that have a higher growth of loans in *Treated* 2—and which therefore benefit more from an increased credit supply—to otherwise similar regions with a lower growth of such loans. The growth variables of *Treated* 1 and 3 loans are also included as a comparison.

Our identification assumption is that, in the absence of change in CLL and controlling for a host of observable risk characteristics, the economic outcomes in zip codes with a higher share of *Treated* 2 loans would evolve similarly to those with a lower share. This approach is similar to those used by Mian and Sufi (2010) in evaluating the effects of the "cash for clunkers" program, Agarwal et al. (2012) in evaluating the broader consequences of the Home Affordable Modification Program, and Keys et al. (2014) in evaluating the impact of declining interest rates on household balance sheets and the real economy. Following the approach, we focus only on zip codes that are relatively similar in key observables prior to our study period, that is, 1998, by matching zip codes with high and low growth of loans in the *Treated* 2 cohort using a propensity score methodology and isolating the portion of the propensity core distribution with common support. This approach employs a set of matching covariates, including the zip code-level increase in home prices, employment, payroll income, the number of business establishments, the percentage of minorities, the presence of colleges or higher education, and so forth. This methodology allows for differences in the evolution of economic outcomes across zip codes with high and low shares of increased credit supply in the new conforming loan market.

Our empirical strategy of exploring the impact of conforming loan increases on regional outcomes relies on zip code-level data. Our main data source for zip code-level outcomes is the US Census ZBP, which collects zip code-level total employment, total payroll income, and total number of business establishments. To account for general trends in economic outcomes over time, we focus on the logarithm of the difference of economic outcomes during the period of loan limit adjustments. The decennial census also collects a rich set of socioeconomic and demographic information. We also calculate average FICO scores and average interest rates from the McDash data. We control for MSA fixed effects as well as this zip code-level information to isolate any other potential forces. We focus on the two distinct periods for the outcome variables: average annual changes from 1999 to 2006 characterized of consistent home price growth as well as strong broad economy, and from 2007 to 2011 characterized of home price declines and very weak economic conditions.

The regression results of economic outcomes are reported in Table 8. The results suggest a significant and positive relation between the growth of *Treated* 2 and increases in home price, total employment, total payroll, and total number of business establishments. Each 1% increase in the loans of *Treated* 2 cohort is associated with a 0.06% home price appreciation, 0.18% employment growth, 0.08% payroll growth, and 0.02% business openings, all at an annual rate, from 1999 to 2006. Considering that these results control for MSA fixed effects and a number of zip code-level variables, the magnitude of the effects is striking. There is also a significant negative relation between the growth of *Treated* 2 cohort and home price declines in 2007-2011. Each 1% growth of loans in *Treated* 2 cohort from 1998-2005 is associated with a 0.21% home price declines in 2007-2011, confirming the fueled home price by credit expansion is not quite sustainable. The correlation with business establishments is significant and positive. However, there is no significant relation between share of *Treated* 2 cohort and employment and payrolls growth in 2007-2011. There is also a significant and positive relation between growth of loans in *Treated* 3 cohort, which are the jumbo loans right above the expanded conforming loan market, and most economic outcomes in the pre-crisis period. Since private capitals in this loan cohort has been crowded out and declined overall in these years, zip codes with high growth of loans in this cohort could be where private capitals compete well as well as with better economic conditions. There is however no relation between growth of *Treated* 3 cohort and economic outcomes in the post-crisis period; so is growth of *Treated* 1 cohorts in either period.

How does credit supply affect broad economy such as employment and business openings? A large literature has documented positive impact of homeowners' housing wealth on individual consumption and savings (e.g., Campbell and Cocco, 2007; Gan, 2010; Hurst and Stafford, 2004; Scholnick, 2013). Keys et al. (2014) also document significant real economy implications of home equity lending. There are also evidences that housing wealth is an important lending channel for businesses (e.g., Chaney et al., 2011). Therefore, as home price rises, accumulated housing wealth can be spent on opening businesses, investing and consuming.

VI. CONCLUSIONS AND DISCUSSIONS

Government policies play an important role in the credit supply of the housing market and fueling the housing crisis in 2007–2011. We test this conjecture using an exogenous shock to the credit supply due to annual adjustments of the CLL. We adopt a diff-in-diff strategy to identify the causal effect of limit changes. Based on comparisons of the CLL effective in the current to previous years, we segment conforming and jumbo loan markets into three treated and two control cohorts. This approach allows us to determine the effects on mortgages and home prices in different segments and explore how national price was fueled to an unsustainable level.

We find strong evidence that increases in the CLL led lenders to increase the credit supply in the new conforming loan market just above the old CLL. The new conforming market not only expands with an increase in approval rates but also is able to attract borrowers with lower leverage. We find no evidence of increases in default and prepayment risks due to unobserved borrower and loan characteristics. A greater credit supply is associated with more demand for homes financed by these loans and therefore the higher sale price and higher holding-period returns.

There is also a competitive effect due to the increase in CLL on the loans just below the old CLL and above the new CLL. In order to take advantage of lower rate on higher-balanced conforming loans, borrowers borrowing near the old CLL who had to borrow less than they otherwise would can now borrow more up to the new CLL and buy more expensive houses. On the other hand, borrowers who were borrowing jumbo loans near the new CLL now have to borrow less than they otherwise would. The movements from both sides toward the new conforming loan segments account for the constant concentration of lending exactly at the CLL even as it is adjusted every year. It thus increases the demand for houses financed by the new conforming loans and leads to higher contract prices in this cohort. At the same time, lending in the loan cohorts just below the old CLL and just above the new CLL is crowded out. The effect on the latter cohort represents net losses for private capital who can not compete in the new conforming market where GSEs have competitive advantages. As a result, home price financed by these jumbo loans suffer more declines than elsewhere. Some of the crowded-out private capitals move up to higher-balanced jumbo market where they can compete, possibly creating a ripple effect of credit supply.

Since CLL are partly adjusted on the basis of home price appreciation in the prior year, we are concerned that the estimated effects may reflect the pre-market conditions that justify the changes in CLL. We take two measures to address this concern. First, we restrict the sample to the bottom quartile of zip codes ranked by home price appreciation during the period, which effectively removes the precondition of CLL adjustment. Our results based on a smaller sample are very similar and consistent with those based on a national sample. Second, we focus only on a few representative years and run the same regression month by month to determine the exact timing of the effects. As anticipated, there is a significant and immediate effect in loans originated in January each year under the new CLL, while no effect in all the other months.

Finally, although the loan limit change is a national event, regions differ in responding to the changes each and every year. We compare the outcomes in regions that have a higher growth of loans affected by the increased credit supply to otherwise similar regions with a relatively lower growth of such loans. Increases in loan limits have significant effects on broad economic outcomes, including home price appreciation, employment growth, payroll income, and business establishments in the pre-crisis period.

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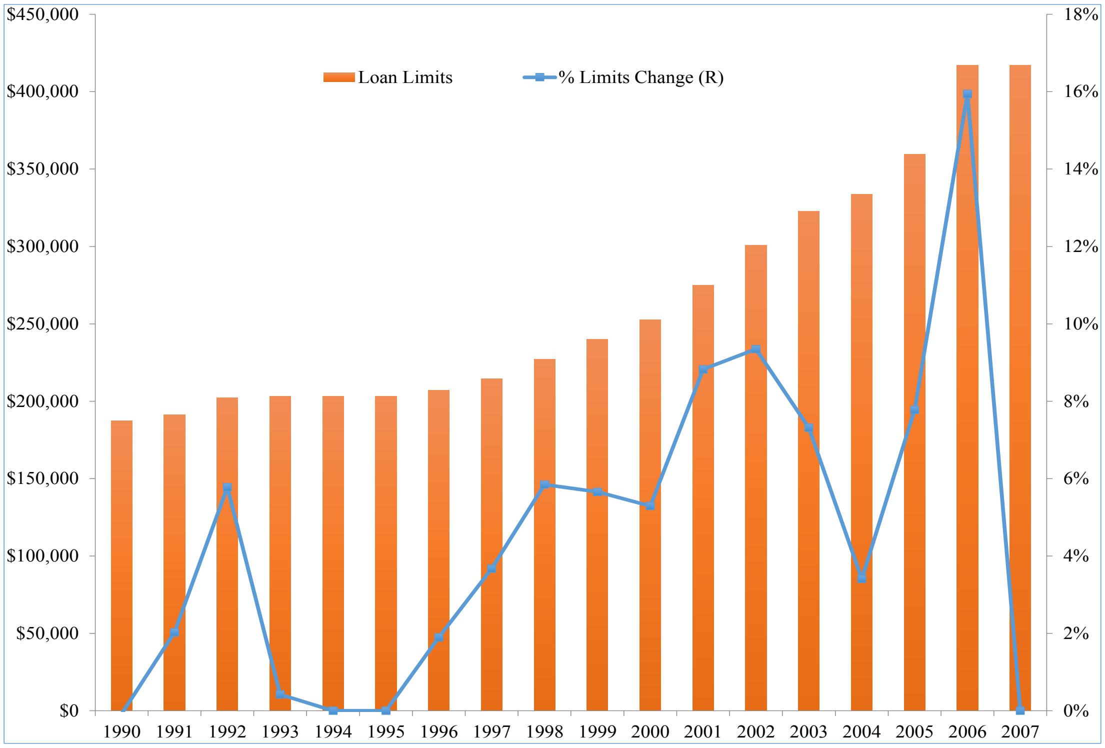
Figure 1. Conforming loan limits, 1990–2006.



Note: This figure plots the loan limits in dollars (left scale and columns) and percentage change from previous year (right scale and line) by year. The horizontal axis is the calendar year when the CLL is applied.

Figure 2 An illustration of the donut approach

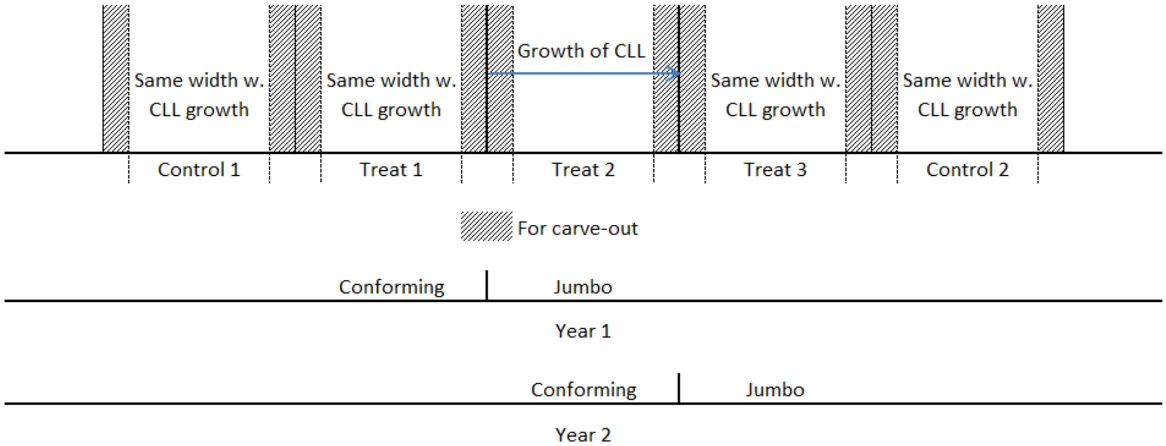
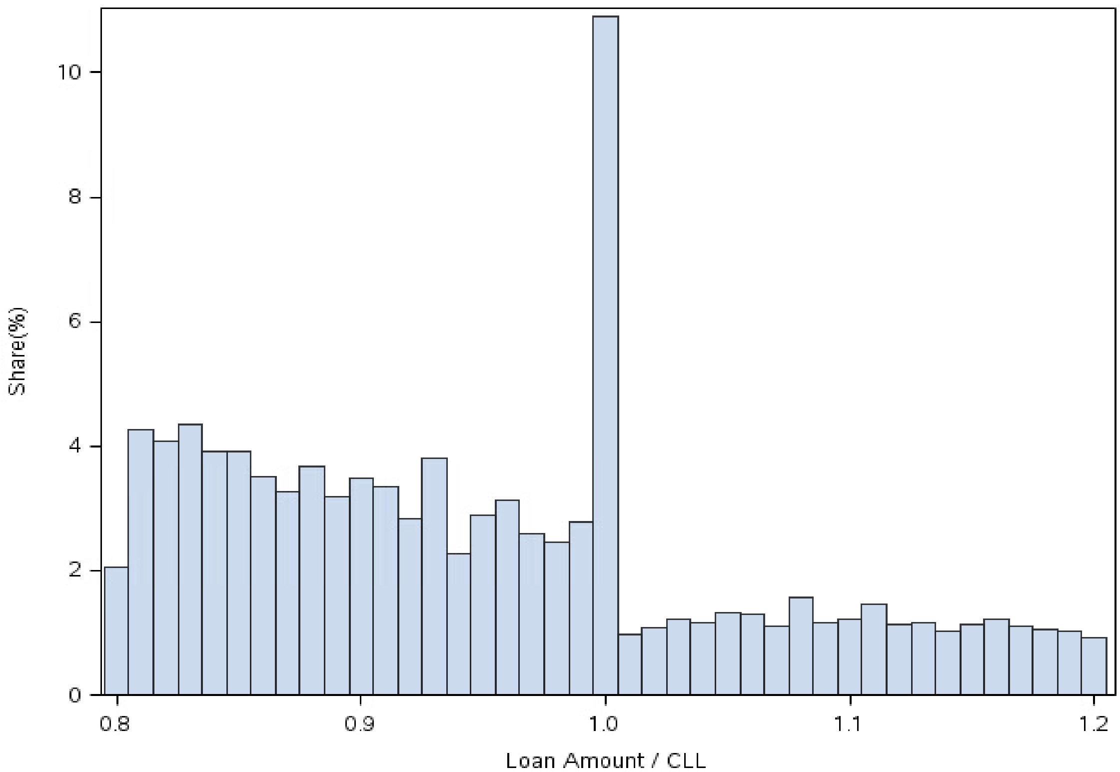
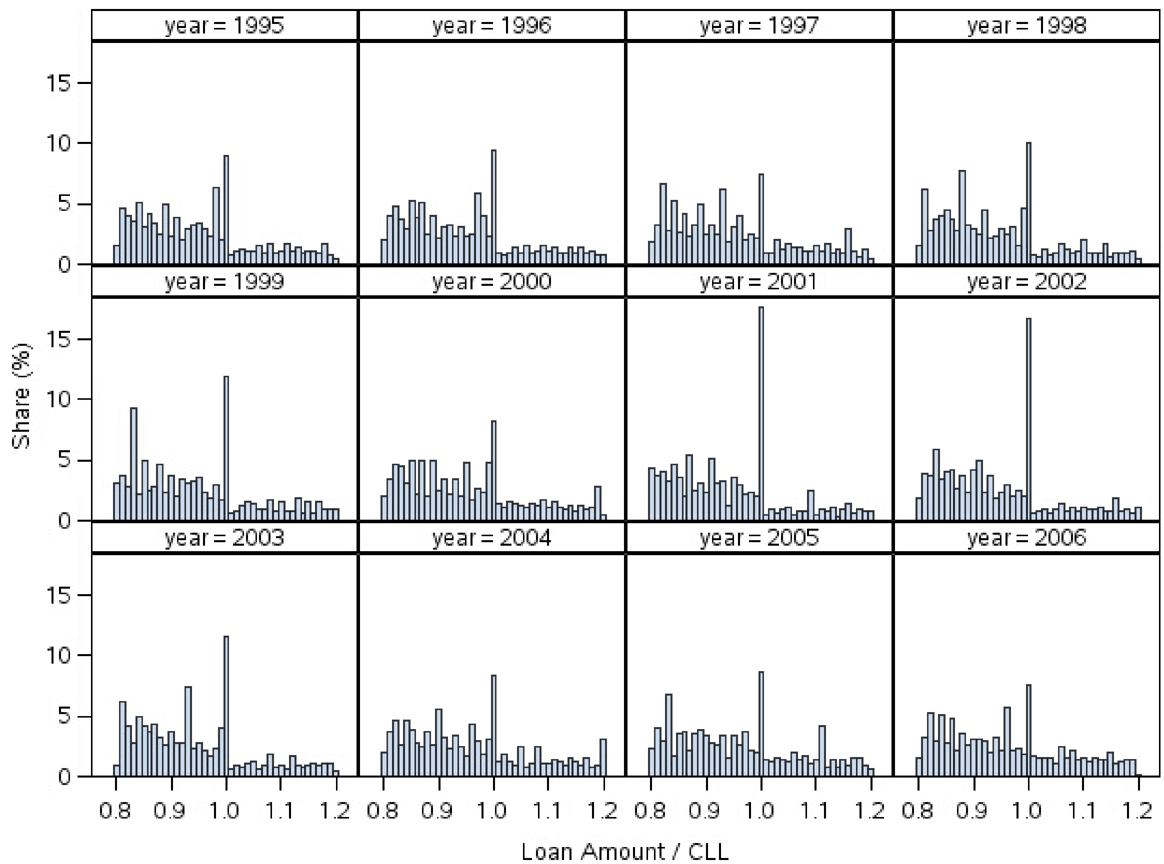


Figure 3. Discontinuity of credit liquidity along ratio of loan amount to CLL

Panel A: Histogram of loan applications for 1995-2006



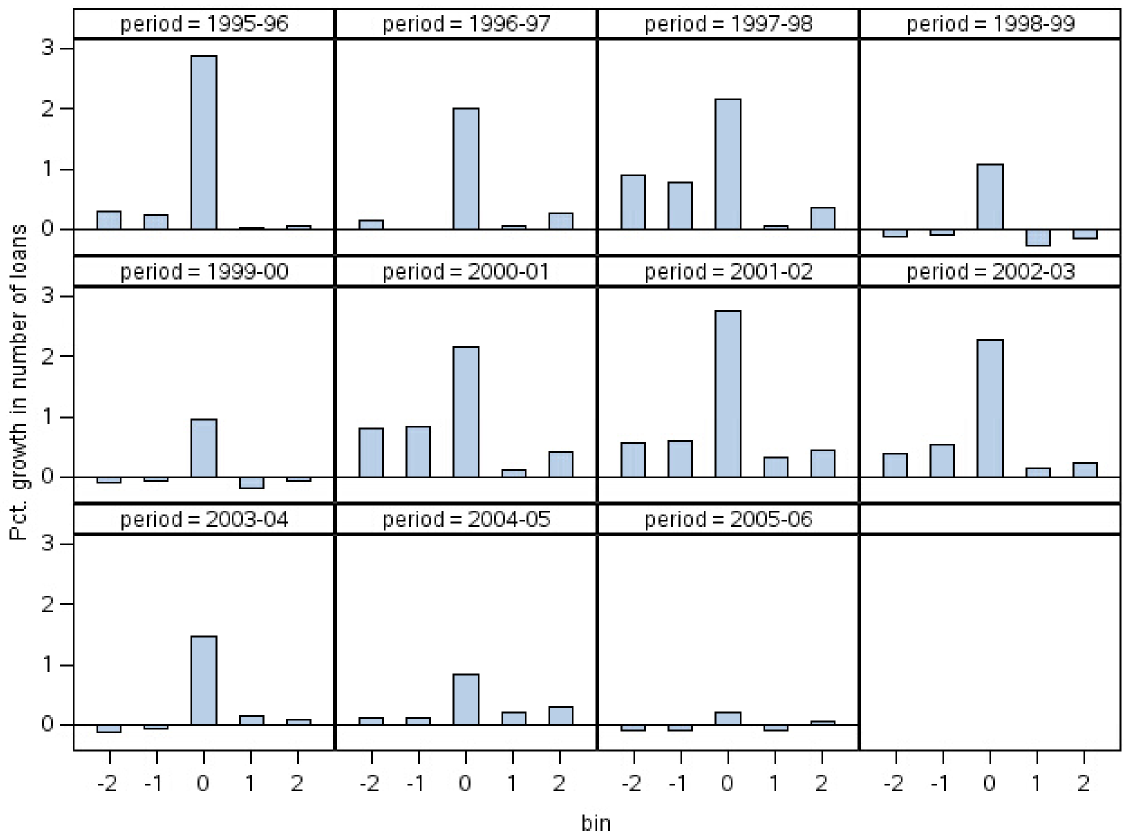
Panel B: Histogram by year



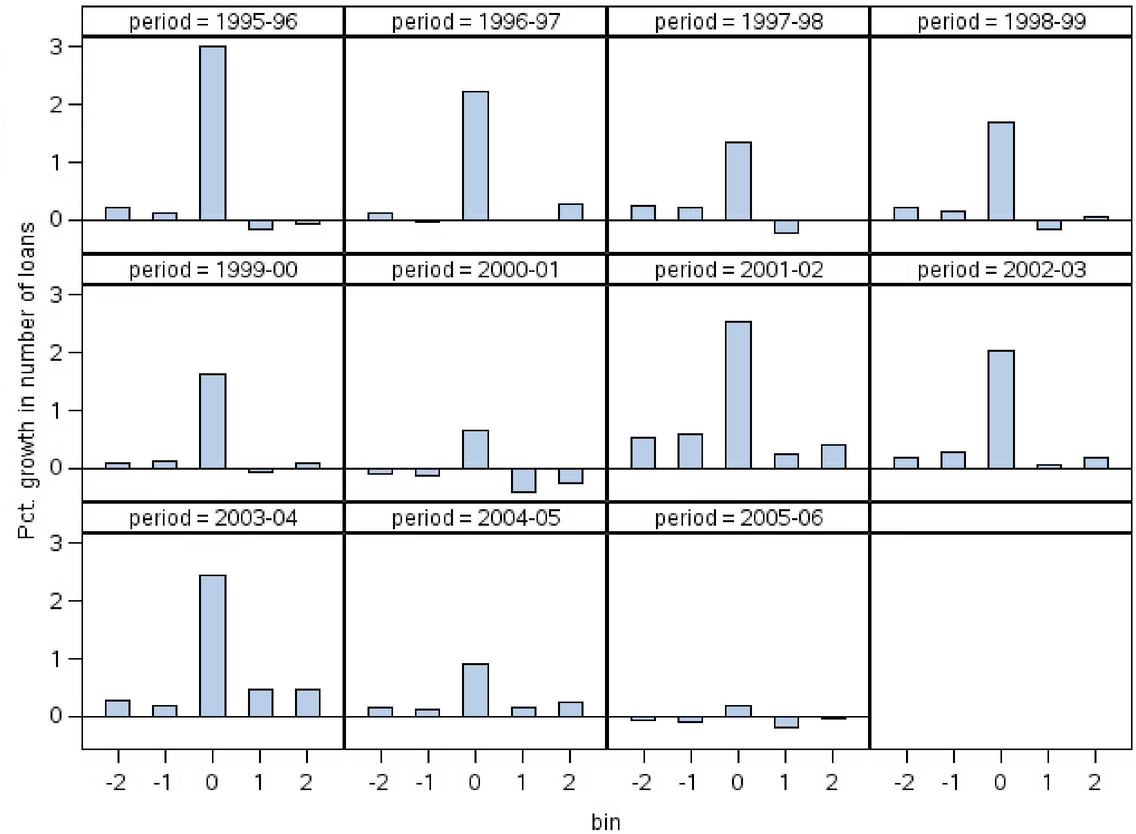
Note: This Figure plots histogram of frequency of all mortgage applications in the HMDA data from 1995 to 2006 by the loan amount of the application divided by the CLL. Each interval of horizontal axis is one percent. For example, 1.0 represents all loan applications with loan amount from 99.01% to 100% of the CLL; 0.9 represents all loan applications with loan amount from 89.01 to 90% of the CLL. Panel A is the histogram based on all applications from 1995 to 2006 and Panel B plots histogram by year.

Figure 4 Volume trends

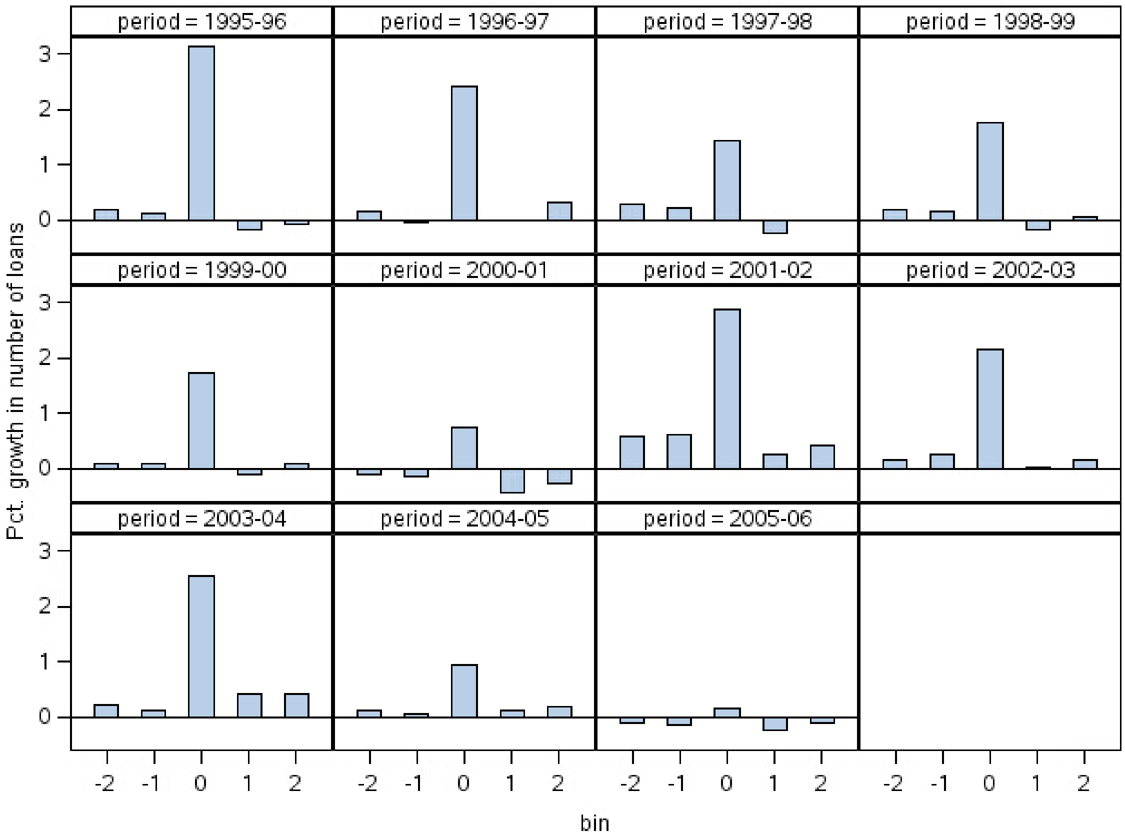
Panel A: Year-over-year growth in HMDA applications (purchase and refinancing)



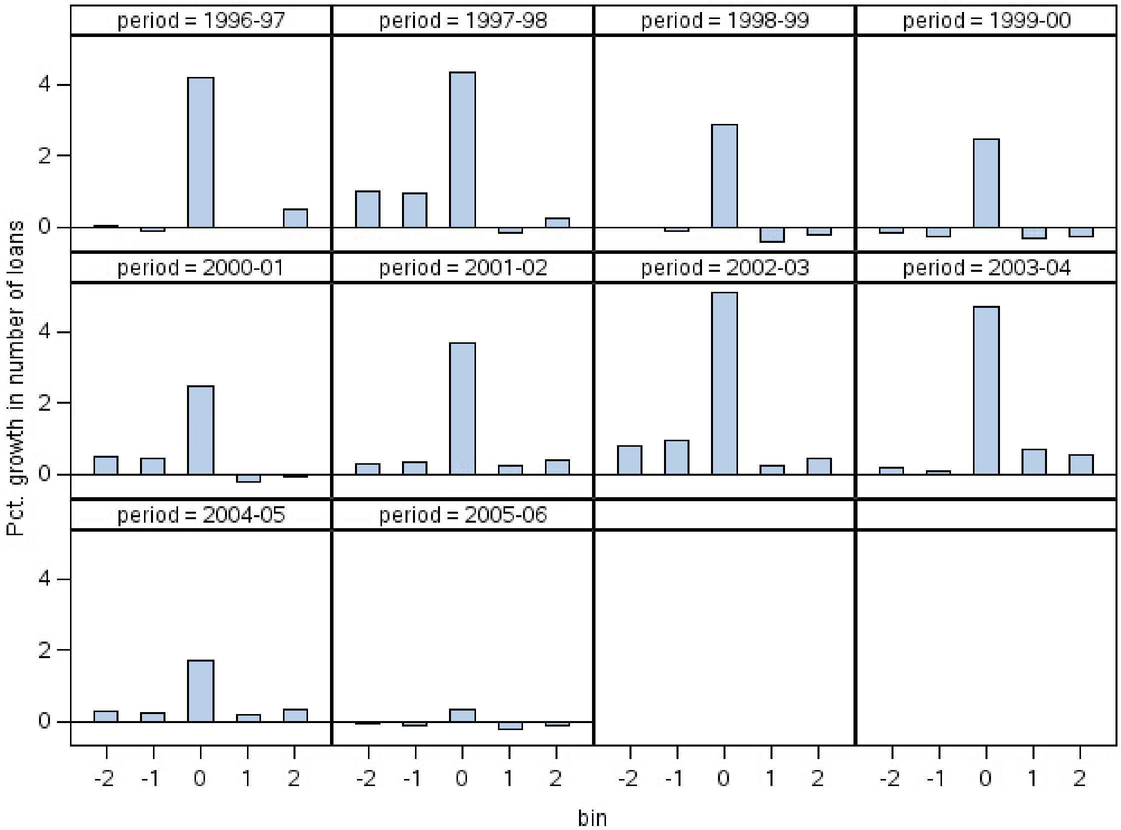
Panel B: Year-over-year growth in HMDA applications (purchase only)



Panel C: Year-over-year growth in HMDA originations (purchase only)



Panel D: Year-over-year growth in LPS loan volumes (purchase only)



Notes: This figure plots the year-over-year growth of loan application and mortgage volumes by year. The vertical axis measures the percentage growth from previous year. For example, 1 represents 100% growth from previous year. The horizontal axis represents five loan cohorts: -2 = control 1 cohort; -1 = treated 1 cohort; 0 = treated 2 cohort; 1 = treated 3 cohort; 2 = control 2 cohort. Panels A-C are based on HMDA data from 1995-2006. Panel D are based on LPS data from 1996 to 2006.

Table 1. Different loan cohorts based on changes in CLL

Group	Definition	Example (2001)	Hypothesized Credit Supply
Control 1	old limit -2 x (new - old limit) <= UPB & old limit - (new - old limit >= UPB	86% x 252,700 <= UPB & 93% x 252,700 >= UPB	Not affected
Treated 1	old limit - (new - old limit) < UPB & old limit >= UPB	93% x 252,700 < UPB & 252,700 >= UPB	Diverged to higher "Conforming" loan balances; Less credit supply and competition
Treated 2	old limit < UPB & new limit >= UPB	252,700 < UPB & 275,000 >= UPB	"Jumbo" replaced by "Conforming"; More credit available
Treated 3	new limit + (new - old limit) >= UPB & new limit < UPB	275,000 < UPB & 107% x 275,000 > UPB	More "Jumbo" credit supply and competition
Control 2	new limit + 2 x (new - old limit) >= UPB & new limit + (new - old limit) < UPB	107% x 275,000 <= UPB & 114% x 275,000 > UPB	Not affected

Notes: This table explains how applications and loans are divided into five distinct cohorts. They are based on comparison between loan amount of individual mortgages and old and new CLL. They are the basis of our identification. Our identification compares loans in three treated cohorts to those in two control cohorts. Outcome variables are the changes in mortgage outcomes in current year compared to previous year.

Table 2. Summary statistics

Panel A: Overall sample

Variable	Source	N	mean	sd	min	p25	p50	p75	max
Origination dummy	HMDA	27,800,000	0.77	0.42	0	1	1	1	1
AmountOfLoan (000)	HMDA	27,800,000	285	56.7	183	240	280	322	499
ApplicantIncome (000)	HMDA	27,800,000	118	164	-3	76	100	133	9999
Ln(Loan to Income)	HMDA	26,400,000	-0.96	0.49	-6.2	-1.26	-1.00	-0.71	3.94
ApplicantMinority	HMDA	27,800,000	0.22	0.42	0	0	0	0	1
Tract Relative Median Income	HMDA	27,800,000	1	0.17	0	1	1	1	4.32
UPB	McDash Analytics	4,398,551	281,739	54,849	189,502	238,000	277,000	320,000	531,700
Note Rate	McDash Analytics	4,398,551	6.43	0.87	1.00	5.75	6.25	7.00	15.8
ARM Indicator	McDash Analytics	4,398,551	0.00	0.00	0.00	0.00	0.00	0.00	0
LTV	McDash Analytics	4,287,014	73	152	0	65	76	80	203000
FICO	McDash Analytics	3,140,061	719	56	1	683	727	764	999
Debt to Income Ratio	McDash Analytics	1,305,960	34	13	1	26	36	42	99
Refinance Flag	McDash Analytics	4,398,551	0.52	0.50	0	0	1	1	1
Investor Flag	McDash Analytics	4,398,551	0.02	0.12	0	0	0	0	1
Low Documentation Flag	McDash Analytics	4,398,551	0.08	0.27	0	0	0	0	1
Default in 3 Years	McDash Analytics	4,398,551	0.04	0.21	0	0	0	0	1
Prepayment in 3 Years	McDash Analytics	4,398,551	0.23	0.42	0	0	0	0	1
Price (Purchase Only)	Public Records	3,224,406	389,552	86,385	189,800	329,042	380,000	437,000	729,500
House Size (sq ft)	Public Records	3,224,406	2,182	830	502	1,542	2,049	2,688	4,999
House Lot	Public Records	3,224,406	0.56	1.82	0.00	0.14	0.20	0.36	50
Property Age	Public Records	3,046,981	39	25	1	18	31	56	115
No of Baths	Public Records	3,224,091	2.15	1.07	0	2	2	3	10
Zip-level Home Price Growth	Corelogic	3,158	8.78	4.09	-4.52	5.27	8.62	12.09	34.48
Zip-level Employment Growth	Census ZBP	3,138	2.48	4.86	-19.96	-0.16	1.68	4.05	75.72
Zip-level Payroll Growth	Census ZBP	3,138	5.69	5.03	-19.81	2.90	4.98	7.59	58.78
Zip-level Establishment Growth	Census ZBP	3,142	2.19	5.16	-7.99	0.26	1.39	2.96	184.31

Panel B: By loan limit cohorts

	Conforming in Prior Year				Jumbo in Prior Year					
	Control 1		Treated 1		Treated 2		Treated 3		Control 2	
Variable	mean	sd	mean	sd	mean	sd	mean	sd	mean	sd
Origination dummy	0.77	0.42	0.78	0.42	0.78	0.42	0.74	0.44	0.72	0.45
AmountOfLoan (000)	252	35	275	44	301	52	331	63	338	67
ApplicantIncome (000)	105	160	116	162	127	166	132	169	135	172
Ln(Loan to Income)	-0.957	0.478	-0.943	0.492	-0.952	0.501	-0.997	0.5	-0.995	0.499
ApplicantMinority	0.224	0.417	0.215	0.411	0.219	0.413	0.244	0.429	0.236	0.424
Tract Relative Median Income	1	0	1	0	1	0	1	0	1	0
UPB	251,930	35,048	281,404	45,712	297,232	50,079	339,214	68,908	352,268	77,371
Note Rate	6.41	0.86	6.42	0.87	6.40	0.85	6.54	0.92	6.56	0.89
ARM Indicator	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
LTV	75.3	71.2	72.7	240.0	70.8	37.1	74.4	25.9	73.1	97.0
FICO	716	59	721	55	722	54	718	57	723	55
Debt to Income Ratio	34.5	13.4	34.1	13.5	34.0	13.3	35.1	12.3	34.3	12.0
Refinance Flag	0.52	0.50	0.52	0.50	0.53	0.50	0.52	0.50	0.52	0.50
Investor Flag	0.02	0.13	0.01	0.12	0.01	0.12	0.02	0.13	0.01	0.12
Low Documentation Flag	0.09	0.29	0.08	0.27	0.07	0.26	0.09	0.29	0.08	0.27
Default in 2 Years	0.04	0.21	0.04	0.21	0.04	0.21	0.04	0.19	0.04	0.19
Prepayment in 2 Years	0.22	0.42	0.23	0.42	0.24	0.42	0.23	0.42	0.22	0.41
Price (Purchase Only)	332,660	62,151	379,464	66,179	422,733	75,015	458,131	81,218	481,071	89,950
House Size (sq ft)	2096	777	2189	826	2238	857	2233	871	2291	889
House Lot	0.544	1.78	0.551	1.78	0.564	1.85	0.577	1.95	0.568	1.87
Property Age	38	24	39	25	39	25	39	25	40	25
No of Baths	2.09	1.03	2.15	1.08	2.19	1.09	2.21	1.09	2.26	1.1
% of sample										
HMDA		35.4		27.2		17.2		10.2		10.1
McDash		41.3		30.3		16.3		5.7		6.4
Public Records		35.2		27.2		17.8		10.7		9.0

Notes: The results presented in the two tables are based on data from four different sources. The data is explained in section 2. The samples used include HMDA loan applications and originations from 1995 to 2006, McDash Analytics mortgages originated from 1996 to 2006, property public records from Corelogic transacted from 2000 to 2006, Census' Zip Business Pattern files from 1993 to 2015. Origination dummy is defined as 1 if the loan application is approved and originated as a loan, 0 otherwise. Ln(Loan to Income) is defined logged ratio of Amount of Loan to Applicant Income. Since there is not measure of loan to value ratio in HMDA, this is an alternative measure of loan leverage. Tract Relative Median Income is defined as ratio of tract-level median household income to MSA-level or state-level median household income, both from US Census. UPB represents unpaid principal balance or original loan balance at origination. ARM Indicator is defined as 1 if loan product is adjustable-rate mortgage, 0 if fixed-rate mortgage. LTV represents loan to value ratio at origination. FICO is the credit score of the borrower. Debt to income ratio is the ratio of borrower's total debt including mortgage payments to total income. Refinance flag is defined as 1 if the mortgage is used to finance a refinancing transaction and 0 if for home purchase Investor flag is defined as 1 if the property is used for investment purpose versus primary residence. Loan documentation flag is an indicator for loans applied with little or no documentations to prove borrower's income and assets. Default represents the credit event when borrowers become delinquent on the mortgage payments. Prepayment represents the event when borrower pay off the existing mortgage with a new mortgage or move out or sell the house. House size is the square footage of the house. Lot is the size of lot on which house is built. Age is number of years from the year it was built to origination.

Table 3. Effect of loan limit changes on loan approval rates

	(1) 1995	(2) 1996	(3) 1997	(4) 1998	(5) 1999	(6) 2000	(7) 2001	(8) 2002	(9) 2003	(10) 2004	(11) 2005
Treated 1	0	0.014**	0.006	0.008***	0.014***	0.001	0.002	0.025***	0.004	0.001	-0.004**
	(0.007)	(0.006)	(0.005)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)	(0.002)
Treated 2	-0.038**	-0.035***	-0.025***	-0.035***	-0.032***	-0.029***	-0.045***	-0.032***	-0.040***	-0.016***	-0.021***
	(0.017)	(0.010)	(0.006)	(0.008)	(0.006)	(0.004)	(0.005)	(0.005)	(0.007)	(0.005)	(0.003)
Treated 3	-0.029**	-0.038***	-0.015**	-0.018***	-0.017***	-0.019***	-0.030***	-0.020***	-0.047***	-0.010**	-0.020***
	(0.013)	(0.007)	(0.006)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.006)	(0.004)	(0.004)
Post	0	0.005	0.017***	-0.003	-0.009***	0.018***	-0.003	0.008***	0	0.003	-0.036***
	(0.008)	(0.005)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(.)	(0.003)	(0.003)
Post											
x Treated 1	-0.003	-0.003	0	-0.012**	-0.009*	0.002	-0.004	-0.021***	-0.005	0.004	0.002
	(0.011)	(0.008)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.006)	(0.004)	(0.003)
x Treated 2	0.056***	0.055***	0.032***	0.039***	0.043***	0.039***	0.055***	0.041***	0.056***	0.026***	0.014***
	(0.018)	(0.012)	(0.006)	(0.008)	(0.007)	(0.005)	(0.006)	(0.006)	(0.008)	(0.006)	(0.003)
x Treated 3	-0.003	0.012	-0.020**	-0.024***	-0.011	-0.016**	-0.008	-0.019***	0.034***	-0.002	-0.025***
	(0.023)	(0.010)	(0.008)	(0.007)	(0.009)	(0.006)	(0.008)	(0.005)	(0.009)	(0.005)	(0.004)
Applicantion Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28751	58327	118216	120350	118004	189338	200163	188607	98942	289046	581395
Adjusted R2	0.064	0.082	0.097	0.11	0.13	0.131	0.114	0.11	0.111	0.124	0.141

Notes: ∗, ∗∗, and ∗∗∗ denote statistical significance at the 10, 5, and 1 percent levels, respectively. The dependent variable is the loan origination dummy. Right-sided variables include three treated variables, post dummy, the interaction terms of three treated dummies and post dummy, log(Loan to Income), Tract Relative Median Income, and MSA fixed effects. All the data used are based on HMDA data from 1995 to 2006. It is estimated using OLS regression with standard errors clustered at MSA. Each column is a regression based on loan applications only in that year and next year to implement the diff-in-diff strategy. For example, the 2003 regression is based on 2003 and the next year, 2004. The *Post* dummy is coded as one if the applications were from 2004.

Table 4. Mortgage characteristics and performance

Panel A: Change in FICO scores

	(1) 1996	(2) 1997	(3) 1998	(4) 1999	(5) 2000	(6) 2001	(7) 2002	(8) 2003	(9) 2004	(10) 2005
Post										
x Treated 1	-21.582**	-3.218	5.775**	3.775*	6.658***	-3.168	-4.008***	2.43	2.259	-1.649
	(10.007)	(5.596)	(2.628)	(2.089)	(2.271)	(2.173)	(1.449)	(1.777)	(2.539)	(1.321)
x Treated 2	15.962	-15.756	1.303	4.881	11.728***	2.334	-1.268	-0.425	-1.124	-0.461
	(31.444)	(12.665)	(3.352)	(3.946)	(4.314)	(2.074)	(2.697)	(2.337)	(2.869)	(2.478)
x Treated 3	67.362*	6.463	-3.607	3.845	4.893	0.595	-1.614	-2.91	1.998	-9.991***
	(39.872)	(9.856)	(4.615)	(3.863)	(3.829)	(3.642)	(2.262)	(2.896)	(3.232)	(2.298)
Borrower /Mortgage Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	695	2646	11221	14081	10676	20861	25454	25220	11141	29293
Adjusted R2	0.077	0.098	0.075	0.082	0.083	0.103	0.114	0.106	0.091	0.107

Notes: *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. The dependent variable is the credit score of the borrower at origination. Right-sided variables include three treated variables, post dummy, the interaction terms of three treated dummies and post dummy, LTV, debt to income ratio, investor dummy, low documentation indicator, and MSA fixed effects. All the data is based on McDash Analytics mortgage data from 1996 to 2006. It is estimated using OLS regression with standard errors clustered at MSA. Each column is a regression based on mortgages originated only in that year and next year to implement the diff-in-diff strategy. For example, the 2003 regression is based on 2003 and the next year, 2004. The *Post* dummy is coded as one if the loans were from 2004.

Panel B: Change in LTV ratios

	(1) 1996	(2) 1997	(3) 1998	(4) 1999	(5) 2000	(6) 2001	(7) 2002	(8) 2003	(9) 2004	(10) 2005
Post										
x Treated 1	0.429	2.591***	-0.606	0.273	0.119	1.132***	0.181	-0.668**	1.116***	1.121***
	(0.894)	(0.533)	(0.390)	(0.355)	(0.430)	(0.436)	(0.336)	(0.306)	(0.394)	(0.314)
x Treated 2	5.498**	0.967	-1.315**	-3.113***	-2.656***	0.935*	-0.751	-1.472**	-3.511***	-2.620***
	(2.559)	(1.103)	(0.568)	(0.472)	(1.015)	(0.478)	(0.521)	(0.618)	(0.905)	(0.574)
x Treated 3	-1.325	1.877*	1.070*	-1.014	-1.608**	-0.174	-0.992	0.251	0.343	-0.97
	(2.122)	(1.001)	(0.608)	(0.873)	(0.678)	(0.643)	(0.716)	(0.683)	(0.777)	(0.661)
Borrower /Mortgage Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4263	8412	23364	24840	16098	29672	33586	32309	14223	37994
Adjusted R2	0.106	0.097	0.07	0.086	0.083	0.105	0.114	0.108	0.107	0.14

Notes: *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. The dependent variable is the LTV at origination. Right-sided variables include three treated variables, post dummy, the interaction terms of three treated dummies and post dummy, FICO, debt to income ratio, investor dummy, low documentation indicator, and MSA fixed effects. All the data is based on McDash Analytics mortgage data from 1996 to 2006. It is estimated using OLS regression with standard errors clustered at MSA. Each column is a regression based on mortgages originated only in that year and next year to implement the diff-in-diff strategy. For example, the 2003 regression is based on 2003 and the next year, 2004. The *Post* dummy is coded as one if the loans were from 2004.

Panel C: Change in note rates

	(1) 1996	(2) 1997	(3) 1998	(4) 1999	(5) 2000	(6) 2001	(7) 2002	(8) 2003	(9) 2004	(10) 2005
Post										
x Treated 1	0.014	0.051**	-0.025*	0.007	-0.021	0.013	0.038***	-0.01	0.017	0.009
	(0.032)	(0.020)	(0.013)	(0.011)	(0.017)	(0.013)	(0.010)	(0.015)	(0.016)	(0.011)
x Treated 2	-0.382***	-0.158***	-0.103***	-0.197***	-0.194***	-0.095***	-0.176***	-0.155***	-0.222***	-0.154***
	(0.087)	(0.056)	(0.020)	(0.028)	(0.042)	(0.025)	(0.025)	(0.021)	(0.027)	(0.025)
x Treated 3	-0.033	-0.086*	0.071**	-0.05	-0.102***	-0.036	-0.087***	0.047*	-0.035	-0.02
	(0.083)	(0.052)	(0.030)	(0.036)	(0.038)	(0.036)	(0.024)	(0.028)	(0.043)	(0.022)
Borrower /Mortgage Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4291	8485	23800	25270	16589	30847	34628	33292	14626	38984
Adjusted R2	0.394	0.404	0.504	0.441	0.552	0.601	0.45	0.539	0.263	0.176

Notes: ∗, ∗∗, and ∗∗∗ denote statistical significance at the 10, 5, and 1 percent levels, respectively. The dependent variable is the note rate of the mortgage at origination. Right-sided variables include three treated variables, post dummy, the interaction terms of three treated dummies and post dummy, FICO, LTV, debt to income ratio, investor dummy, low documentation indicator, and MSA fixed effects. All the data is based on McDash Analytics mortgage data from 1996 to 2006. It is estimated using OLS regression with standard errors clustered at MSA. Each column is a regression based on mortgages originated only in that year and next year to implement the diff-in-diff strategy. For example, the 2003 regression is based on 2003 and the next year, 2004. The *Post* dummy is coded as one if the loans were from 2004.

Panel D: Change in default in the first three years

	(1) 1996	(2) 1997	(3) 1998	(4) 1999	(5) 2000	(6) 2001	(7) 2002	(8) 2003	(9) 2004	(10) 2005
Post										
x Treated 1	-0.004	-0.008	0.004	-0.001	-0.006*	0.004	0.007*	-0.002	-0.008**	0.003
	(0.009)	(0.008)	(0.003)	(0.002)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)
x Treated 2	-0.011	-0.026	-0.001	0.001	-0.008	-0.003	0	-0.004	-0.011	0.001
	(0.023)	(0.018)	(0.004)	(0.004)	(0.006)	(0.005)	(0.005)	(0.005)	(0.007)	(0.006)
x Treated 3	-0.008	0.007	-0.003	0.002	-0.005	0.001	0.001	-0.001	-0.009	-0.013
	(0.014)	(0.008)	(0.006)	(0.006)	(0.006)	(0.008)	(0.007)	(0.006)	(0.006)	(0.010)
Borrower /Mortgage Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4291	8485	23800	25270	16589	30847	34628	33292	14626	38984
Adjusted R2	0.012	0.022	0.017	0.02	0.028	0.023	0.025	0.025	0.03	0.039

Notes: ∗, ∗∗, and ∗∗∗ denote statistical significance at the 10, 5, and 1 percent levels, respectively. The dependent variable is the default dummy conditional on three years of payment history following origination. Right-sided variables include three treated variables, post dummy, the interaction terms of three treated dummies and post dummy, FICO, LTV, debt to income ratio, investor dummy, low documentation indicator, and MSA fixed effects. All the data is based on McDash Analytics mortgage data from 1996 to 2006. It is estimated using OLS regression with standard errors clustered at MSA. Each column is a regression based on mortgages originated only in that year and next year to implement the diff-in-diff strategy. For example, the 2003 regression is based on 2003 and the next year, 2004. The *Post* dummy is coded as one if the loans were from 2004.

Panel E: Change in prepayment in the first three years

	(1) 1996	(2) 1997	(3) 1998	(4) 1999	(5) 2000	(6) 2001	(7) 2002	(8) 2003	(9) 2004	(10) 2005
Post										
x Treated 1	-0.011	-0.027	-0.003	-0.005	0.005	0.008	-0.001	-0.005	0.01	0.011
	(0.033)	(0.021)	(0.013)	(0.013)	(0.016)	(0.014)	(0.011)	(0.014)	(0.013)	(0.011)
x Treated 2	0.081	0.027	-0.023	-0.027*	-0.016	-0.005	-0.036**	-0.033	-0.03	-0.015
	(0.052)	(0.036)	(0.019)	(0.015)	(0.028)	(0.021)	(0.017)	(0.022)	(0.031)	(0.015)
x Treated 3	-0.006	0.038	-0.023	-0.002	0.035	0.033	-0.012	0.011	-0.01	0.004
	(0.057)	(0.039)	(0.023)	(0.021)	(0.028)	(0.032)	(0.020)	(0.020)	(0.033)	(0.017)
Borrower /Mortgage Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4291	8485	23800	25270	16589	30847	34628	33292	14626	38984
Adjusted R2	0.164	0.153	0.098	0.125	0.289	0.173	0.238	0.206	0.081	0.056

Notes: ∗, ∗∗, and ∗∗∗ denote statistical significance at the 10, 5, and 1 percent levels, respectively. The dependent variable is prepayment dummy conditional on three years of payment history following origination. Right-sided variables include three treated variables, post dummy, the interaction terms of three treated dummies and post dummy, FICO, LTV, debt to income ratio, investor dummy, low documentation indicator, and MSA fixed effects. All the data is based on McDash Analytics mortgage data from 1996 to 2006. It is estimated using OLS regression with standard errors clustered at MSA. Each column is a regression based on mortgages originated only in that year and next year to implement the diff-in-diff strategy. For example, the 2003 regression is based on 2003 and the next year, 2004. The *Post* dummy is coded as one if the loans were from 2004.

Table 5. Logged home prices

	(1) 2001	(2) 2002	(3) 2003	(4) 2004	(5) 2005	(7) Appreciation
Post						
x Treated 1	0.004*	-0.002	0.003**	-0.014***	-0.009***	-0.011***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
x Treated 2	0.014***	0.010***	0.009***	0.014***	0.005*	0.007*
	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.004)
x Treated 3	0.003	0.004	0.002	-0.001	-0.001	-0.017***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.004)	(0.004)
Property Controls	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	297776	325424	284663	150247	419348	634617
Adjusted R2	0.431	0.463	0.395	0.25	0.358	0.503

Notes: ∗, ∗∗, and ∗∗∗ denote statistical significance at the 10, 5, and 1 percent levels, respectively. The dependent variable is the logged home price. Right-sided variables include three treated variables, post dummy, the interaction terms of three treated dummies and post dummy, logged property size, logged lot size, property age, age square, no of bathrooms, cash sale dummy, property condition, fireplace, near golf course dummy, near lake dummy, near ocean dummy, near park dummy, other amenities, and MSA fixed effects. All the data is based on Corelogic public records from 2001 to 2006. It is estimated using OLS regression with standard errors clustered at MSA. Each column is a regression based on houses sold only in that year and next year to implement the diff-in-diff strategy. For example, the 2003 regression is based on 2003 and the next year, 2004. The *Post* dummy is coded as one if the transactions were from 2004.

Table 6. Effects in underperforming markets

Panel A: Change in approval rates

	(1) 1995	(2) 1996	(3) 1997	(4) 1998	(5) 1999	(6) 2000	(7) 2001	(8) 2002	(9) 2003	(10) 2004	(11) 2005
Post											
x Treated 1	0.019*	-0.002	0.020***	-0.012**	-0.032***	-0.003	-0.005	-0.035***	-0.014	-0.002	0
	(0.011)	(0.010)	(0.007)	(0.005)	(0.008)	(0.005)	(0.006)	(0.009)	(0.011)	(0.007)	(0.004)
x Treated 2	0.012	0.014	0.034***	0.060***	0.043***	0.019**	0.033***	0.046***	0.042**	0.015	0.014**
	(0.026)	(0.015)	(0.008)	(0.010)	(0.009)	(0.008)	(0.009)	(0.012)	(0.017)	(0.010)	(0.007)
x Treated 3	0	0.007	-0.027***	-0.005	-0.015	0.008	-0.021	0.009	0.042*	-0.008	-0.038***
	(0.019)	(0.017)	(0.010)	(0.009)	(0.013)	(0.013)	(0.013)	(0.015)	(0.024)	(0.012)	(0.010)
Applicantion Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12480	24384	50806	48855	44849	67365	61061	47236	19410	54865	126364
Adjusted R2	0.075	0.092	0.104	0.117	0.144	0.139	0.122	0.121	0.143	0.15	0.165

Notes: ∗, ∗∗, and ∗∗∗ denote statistical significance at the 10, 5, and 1 percent levels, respectively. The dependent variable is the loan origination dummy. Right-sided variables include three treated variables, post dummy, the interaction terms of three treated dummies and post dummy, log(Loan to Income), Tract Relative Median Income, and MSA fixed effects. All the data used are based on HMDA data in underperforming zip codes from 1995 to 2006. It is estimated using OLS regression with standard errors clustered at MSA. Each column is a regression based on loan applications only in that and next year to implement the diff-in-diff strategy. For example, the 2003 regression is based on 2003 and the next year, 2004. The *Post* dummy is coded as one if the applications were from 2004.

Panel B: Change in note rates

	(1) 1996	(2) 1997	(3) 1998	(4) 1999	(5) 2000	(6) 2001	(7) 2002	(8) 2003	(9) 2004	(10) 2005
Post										
x Treated 1	-0.01	0.068*	0.036*	-0.011	0.011	-0.02	0.035*	0.016	0.001	-0.006
	(0.051)	(0.035)	(0.019)	(0.028)	(0.033)	(0.015)	(0.018)	(0.023)	(0.036)	(0.019)
x Treated 2	-0.111	-0.120*	-0.03	-0.174***	-0.205***	-0.152***	-0.174***	-0.176***	-0.305***	-0.183***
	(0.155)	(0.063)	(0.030)	(0.035)	(0.051)	(0.036)	(0.048)	(0.041)	(0.056)	(0.050)
x Treated 3	-0.073	-0.12	0.064	-0.07	-0.107*	-0.08	-0.099	0.149***	-0.263**	-0.053
	(0.141)	(0.074)	(0.042)	(0.050)	(0.057)	(0.062)	(0.062)	(0.052)	(0.121)	(0.065)
Borrower /Mortgage Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1741	3586	11310	11677	7523	12911	12533	9598	3474	10699
Adjusted R2	0.349	0.349	0.482	0.409	0.524	0.588	0.434	0.559	0.307	0.249

Notes: ∗, ∗∗, and ∗∗∗ denote statistical significance at the 10, 5, and 1 percent levels, respectively. The dependent variable is the note rate of the mortgage at origination. Right-sided variables include three treated variables, post dummy, the interaction terms of three treated dummies and post dummy, FICO, LTV, debt to income ratio, investor dummy, low documentation indicator, and MSA fixed effects. All the data is based on McDash Analytics mortgage data in underperforming zip codes from 1996 to 2006. It is estimated using OLS regression with standard errors clustered at MSA. Each column is a regression based on mortgages originated only in that year and next year to implement the diff-in-diff strategy. For example, the 2003 regression is based on 2003 and the next year, 2004. The *Post* dummy is coded as one if the loans were from 2004.

Panel C: Change in home prices

	(1) 2001	(2) 2002	(3) 2003	(4) 2004	(5) 2005
Post					
x Treated 1	0.002	0.007*	0.007**	-0.007	-0.011***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)
x Treated 2	0.013***	0.015***	0.025***	0.028***	0.012**
	(0.004)	(0.004)	(0.005)	(0.007)	(0.005)
x Treated 3	0.006	0.006	0.003	0.021***	-0.003
	(0.007)	(0.006)	(0.006)	(0.008)	(0.006)
Property Controls	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes
Observations	34750	32857	24412	10836	30907
Adjusted R2	0.394	0.425	0.359	0.272	0.318

Notes: ∗, ∗∗, and ∗∗∗ denote statistical significance at the 10, 5, and 1 percent levels, respectively. The dependent variable is the logged home price. Right-sided variables include three treated variables, post dummy, the interaction terms of three treated dummies and post dummy, logged property size, logged lot size, property age, age square, no of bathrooms, cash sale dummy, property condition, fireplace, near golf course dummy, near lake dummy, near ocean dummy, near park dummy, other amenities, and MSA fixed effects. All the data is based on Corelogic public records in underperforming zip codes mortgage data from 2001 to 2006. It is estimated using OLS regression with standard errors clustered at MSA. Each column is a regression based on houses sold only in that year and next year to implement the diff-in-diff strategy. For example, the 2003 regression is based on 2003 and the next year, 2004. The *Post* dummy is coded as one if the transactions were from 2004..

Table 7. Month-by-month effects, 2002 only

Panel A: Change in note rates

	(1) Jan-02	(2) Feb-02	(3) Mar-02	(4) Apr-02	(5) May-02	(6) Jun-02	(7) Jul-02	(8) Aug-02	(9) Sep-02	(10) Oct-02	(11) Nov-02	(12) Dec-02
Post												
x Treated 1	-0.048*	-0.002	0.001	-0.027	0.021	-0.025	0.015	-0.013	-0.008	-0.006	-0.003	0.007
	(0.028)	(0.027)	(0.026)	(0.024)	(0.018)	(0.020)	(0.018)	(0.022)	(0.020)	(0.019)	(0.017)	(0.019)
x Treated 2	0.129	-0.107	0.032	-0.1	0.054	-0.015	-0.01	0.062	0.023	-0.069	-0.053	-0.100***
	(0.079)	(0.089)	(0.080)	(0.064)	(0.038)	(0.040)	(0.041)	(0.047)	(0.039)	(0.042)	(0.037)	(0.027)
x Treated 3	-0.073	0.007	-0.03	-0.024	-0.001	0.026	0.023	0.071	-0.012	0.041	-0.063*	0.017
	(0.066)	(0.059)	(0.043)	(0.035)	(0.030)	(0.024)	(0.026)	(0.044)	(0.044)	(0.039)	(0.032)	(0.042)
Borrower /Mortgage Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5544	6135	6995	7813	8795	9293	9924	9787	10298	10882	11145	11504
Adjusted R2	0.132	0.123	0.132	0.144	0.17	0.183	0.207	0.201	0.19	0.167	0.167	0.176

Notes: ∗, ∗∗, and ∗∗∗ denote statistical significance at the 10, 5, and 1 percent levels, respectively. The dependent variable is the note rate of the mortgage at origination. Right-sided variables include three treated variables, post dummy, the interaction terms of three treated dummies and post dummy, FICO, LTV, debt to income ratio, investor dummy, low documentation indicator, and MSA fixed effects. All the data is based on McDash Analytics mortgage data from January 2002 to January 2003. It is estimated using OLS regression with standard errors clustered at MSA. Each column is a regression based on mortgages originated only in that month and next month to implement the diff-in-diff strategy. For example, the Dec-02 regression is based on Dec-2002 and the next month, Jan-2003. The *Post* dummy is coded as one if the loans were from Jan-2003.

Panel B: Change in home prices

	(1) Jan-02	(2) Feb-02	(3) Mar-02	(4) Apr-02	(5) May-02	(6) Jun-02	(7) Jul-02	(8) Aug-02	(9) Sep-02	(10) Oct-02	(11) Nov-02	(12) Dec-02
Post												
x Treated 1	0.007	-0.004	-0.002	0.004	0.002	0.001	-0.003	0.003	-0.001	0.004	0.005	0.000
	(0.006)	(0.006)	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)
x Treated 2	0.008	0.003	-0.011	0.01	-0.004	0.006	-0.003	0.003	-0.001	0.007	0.001	0.009**
	(0.011)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.004)
x Treated 3	0.001	0.009	0.005	0.003	-0.002	0.003	-0.009	0.005	-0.004	0.001	-0.002	-0.002
	(0.010)	(0.008)	(0.005)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)
Property Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12836	15234	18606	21212	22886	23748	24189	22242	20791	19810	18919	17245
Adjusted R2	0.409	0.418	0.411	0.426	0.426	0.421	0.425	0.433	0.436	0.437	0.424	0.53

Notes: ∗, ∗∗, and ∗∗∗ denote statistical significance at the 10, 5, and 1 percent levels, respectively. The dependent variable is the logged home price. Right-sided variables include three treated variables, post dummy, the interaction terms of three treated dummies and post dummy, logged property size, logged lot size, property age, age square, no of bathrooms, cash sale dummy, property condition, fireplace, near golf course dummy, near lake dummy, near ocean dummy, near park dummy, other amenities, and MSA fixed effects. All the data is based on Corelogic public records from January 2001 to January 2002. It is estimated using OLS regression with standard errors clustered at MSA. Each column is a regression based on houses sold only in that month and next month to implement the diff-in-diff strategy. For example, the Dec-02 regression is based on Dec-2002 and the next month, Jan-2003. The *Post* dummy is coded as one if the transactions were from Jan-2003.

Table 8. Economic outcomes

	(1) Home Price	(2) Employ-ment	(3) Payrolls	(4) Establish-ments	(5) Home Price	(6) Employ-ment	(7) Payrolls	(8) Establish-ments
	1999-2006				2007-2011			
Average Growth of Loans in								
Treated 1 Cohort	-0.007	0.049	0.016	0.020*	0.070	-0.042	-0.024	0.001
	(0.029)	(0.038)	(0.040)	(0.011)	(0.051)	(0.067)	(0.090)	(0.027)
Treated 2 Cohort	0.064**	0.177***	0.075*	0.021*	-0.211***	-0.055	-0.038	0.062**
	(0.030)	(0.039)	(0.041)	(0.011)	(0.049)	(0.065)	(0.087)	(0.026)
Treated 3 Cohort	0.056**	0.084**	0.142***	-0.005	-0.045	-0.023	-0.099	-0.002
	(0.028)	(0.036)	(0.038)	(0.010)	(0.051)	(0.067)	(0.090)	(0.027)
Lagged Dependent Variable	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zip-Code-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1816	1816	1816	1816	1823	1818	1818	1823
Adjusted R2	0.940	0.887	0.906	0.979	0.855	0.655	0.684	0.799

Notes: *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. The dependent variable is the labeled as column title. Right-sided variables include averaged annual growth of loans in each of the three treated cohorts at zip code level, lagged dependent variable, zip-level control variables including median household income, percent of minority, total population and vacancy rate and share of owner-occupied single-family 1-4 unit houses, and MSA fixed effects. Zip codes are restricted to all zip codes with high growth of loans in *Treated* 2 cohort and those with low growth matched to the former group based on propensity score matching algorithm. All the data is based on Census Zip Business Pattern files at zip code level, plus three constructed treated variables at zip level base on McDash Analytics baseline sample from 1999 to 2006. It is estimated using OLS regression with standard errors clustered at MSA.